This Week's Finds in Mathematical Physics

Weeks 251 to 300

May 5, 2007 to *August 11, 2010*

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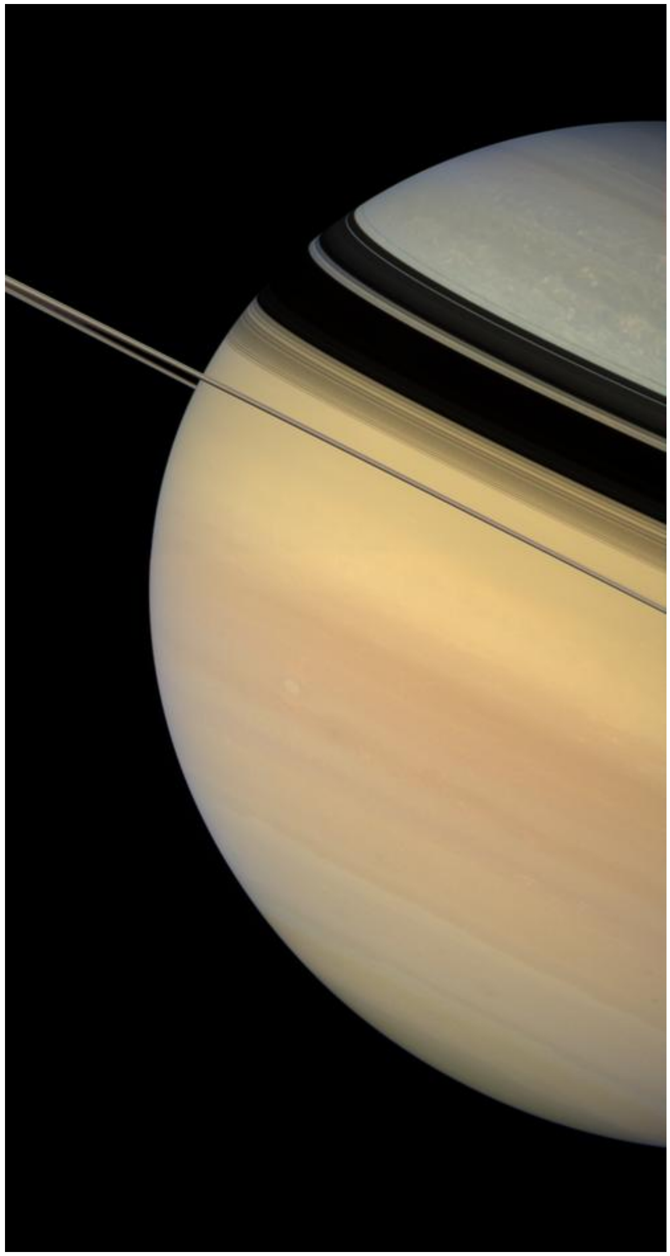
Contents

Week 251

May 5, 2007

Last week I mentioned the conference on "Philosophical and Formal Foundations of Modern Physics" in Les Treilles, an estate near Nice. On our last night there, the chef showed us his telescope. We saw the phase of Venus, mountains on the Moon, and — best of all — the rings of Saturn! They were beautiful. I was reminded of Galileo, who had to make do with a much cruder telescope.

Here's an even better view — a photo taken by the Cassini probe on March 1st, from a distance of 1.2 million kilometers:



1) Cassini-Huyghens, "Tourniquet shadows", http://saturn.jpl.nasa.gov/multimedia/images/image-details.cfm?imageID=2507

I learned some fun stuff about the foundations of quantum mechanics at Les Treilles, so I want to mention that before I forget! I'll take a little break from the Tale of Groupoidification. . . though if you've been following carefully, you may see it lurking beneath the surface.

Lately people have been developing "foils for quantum mechanics": theories of physics that aren't classical, but aren't ordinary quantum theory, either. These theories can lack some of the weird features of quantum theory. . . or, they may have "supra-quantum" features, like the Popescu-Rohrlich box I mentioned last week.

The idea is not to take these theories seriously as models of our universe — though one can always dream. Instead, it's to explore the logical possibilities, so we can see quantum mechanics and classical mechanics as just two examples from a larger field of options, and better understand what's special about them.

Rob Spekkens is a young guy who used to be at the Perimeter Institute; now he's at DAMTP in Cambridge. At Les Treilles he gave a cool talk about a simple theory that mimics some of features of quantum mechanics:

2) Rob Spekkens, "Evidence for the epistemic view of quantum states: a toy theory", *Phys. Rev. A* **75**, 032110 (2007). Also available as quant-ph/0401052.

The idea is to see how far you get using a very simple principle, namely: even when you know as much as you can, there's an equal amount you don't know.

In this setup, the complete description of a physical system involves N bits of information, but you can only know $N/2$ of them. When you do an experiment to learn more information than that, the system's state changes in a random way, so something you knew become obsolete.

The fraction "$1/2$" here is chosen for simplicity: it's just a toy theory. But, it leads to some charming mathematical structures that I'd like to understand better.

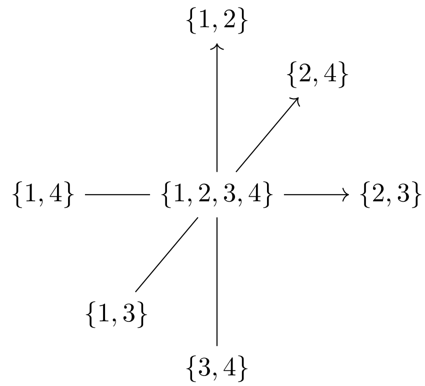
In this theory, the simplest nontrivial system is one whose state takes two bits to describe — but you can know at most one. Two bits of information is enough to describe four states, say states 1, 2, 3, and 4. But, since you can only know one bit of information, you can't pin down the system's state completely. At most you can halve the possibilities, and know something like "the system is in state 1 or 3". You can also be completely ignorant — meaning you only know "the system is in state 1, 2, 3 or 4".

Since there are 3 ways to chop a 4-element set in half, there are 3 "axes of knowledge", namely

Is the system's state in $\{1, 2\}$ *or* $\{3, 4\}$*?*
Is the system's state in $\{1, 3\}$ *or* $\{2, 4\}$*?*
Is the system's state in $\{1, 4\}$ *or* $\{2, 3\}$*?*

You can only answer one of these questions.

This has a cute resemblance to how you can measure the angular momentum of a spin-$1/2$ particle along the x, y, or z axis, in each case getting two choices. Spekkens has a nice picture in his paper:



This octahedron is a discrete version of the "Bloch ball" describing mixed states of a spin-$1/2$ particle in honest quantum mechanics. If you don't know about that, I should remind you:

A "pure state" of a spin-$1/2$ particle is a unit vector in $\mathbb{C}^2$, modulo phase. The set of these is just the Riemann sphere!

In a pure state, we know as much as we can know. In a "mixed state", we know less. Mathematically, a mixed state of a spin-$1/2$ particle is a 2×2 "density matrix" — a self-adjoint matrix with nonnegative eigenvalues and trace 1. These form a 3-dimensional ball, the "Bloch ball", whose boundary is the Riemann sphere.

The x, y, and z coordinates of a point in the Bloch ball are the expected values of the three components of angular momentum for a spin-$1/2$ particle in the given mixed state. The center of the Bloch ball is the state of complete ignorance.

In honest quantum mechanics, the rotation group $\mathrm{SO}(3)$ acts as symmetries of the Bloch ball. In Spekken's toy version, this symmetry group is reduced to the 24 permutations of the set $\{1, 2, 3, 4\}$. You can think of these permutations as acting on a tetrahedron whose corners are the 4 states of our system. The 6 corners of the octahedron above are the midpoints of the edges of this tetrahedron!

Since Spekkens' toy system resembles a qubit, he calls it a "toy bit". He goes on to study systems of several toy bits — and the charming combinatorial geometry I just described gets even more interesting. Alas, I don't really understand it well: I feel there must be some mathematically elegant way to describe it all, but I don't know what it is.

Just as you can't duplicate a qubit in honest quantum mechanics — the famous no-cloning theorem — it turns out you can't duplicate a toy bit. However, Bell's theorem on nonlocality and the Kochen-Specker theorem on contextuality don't apply to toy bits. Spekkens also lists other similarities and differences.

All this is fascinating. It would be nice to find the mathematical structure that underlies this toy theory, much as the category of Hilbert spaces underlies honest quantum mechanics.

In my talk at Les Treilles, I explained how the seeming weirdness of quantum mechanics arises from how the category of Hilbert spaces resembles not the category of sets and functions, but a category with "spaces" as objects and "spacetimes" as morphism. This is good, because we're trying to unify quantum mechanics with our best theory of spacetime, namely general relativity. In fact, I think quantum mechanics will make more sense when it's part of a theory of quantum gravity! To see why, try this:

3) John Baez, "Quantum quandaries: a category-theoretic perspective", talk at Les Treilles, April 24, 2007, `http://math.ucr.edu/home/baez/treilles/`

For more details, see my paper with the same title (see "Week 247").

This fun paper by Bob Coecke gives another view of categories and quantum mechanics, coming from work on quantum information theory:

4) Bob Coecke, "Kindergarten quantum mechanics", available as `quant-ph/0510032`.

To dig deeper, try these:

5) Samson Abramsky and Bob Coecke, "A categorical semantics of quantum protocols", `quant-ph/0402130`.

6) Peter Selinger, "Dagger compact closed categories and completely positive maps", available at `http://www.mscs.dal.ca/~selinger/papers.html#dagger`

Since the category-theoretic viewpoint sheds new light on the no-cloning theorem, Bell's theorem, quantum teleportation, and the like, maybe we can use it to classify "foils for quantum mechanics". Where would Spekkens' theory fit into this classification? I want to know!

Another mathematically interesting talk was by Howard Barnum, who works at Los Alamos National Laboratory. Barnum works on a general approach to physical theories using convex sets. The idea is that in any reasonable theory, we can form a mixture or "convex linear combination"

$$px + (1 - p)y$$

of mixed states x and y, by putting the system in state x with probability p and state y with probability $1 - p$. So, mixed states should form a "convex set".

The Bloch sphere is a great example of such a convex set. Another example is the octahedron in Spekken's theory. Another example is the tetrahedron that describes the mixed states of a classical system with 4 pure states. Spekken's octahedron is a subset of this tetrahedron, reflecting the limitations on knowledge in his setup.

To learn about the convex set approach, try these papers:

7) Howard Barnum, "Quantum information processing, operational quantum logic, convexity, and the foundations of physics", available as `quant-ph/0304159`.

8) Jonathan Barrett, "Information processing in generalized probabilistic theories", available as `quant-ph/0508211`.

9) Howard Barnum, Jonathan Barrett, Matthew Leifer and Alexander Wilce, "Cloning and broadcasting in generic probabilistic theories", available as `quant-ph/0611295`.

Actually I've been lying slightly: these papers also allow mixtures of states

$$px + qy$$

where $p+q$ is less than or equal to 1. For example, if you prepare an electron in the "up" spin state with probability p and the "down" state with probability q, but there's also a chance that you drop it on the floor and lose it, you might want $p+q<1$.

I'm making it sound silly, but it's technically nice and maybe even conceptually justified. Mathematically it means that instead of a convex set of states, you have a vector space equipped with a convex cone and a linear functional P such that the cone is spanned by the "normalized" states: those with $P(x)=1$. This is very natural in both classical and quantum probability theory.

Quite generally, the normalized states form a convex set. Conversely, starting from a convex set, you can create a vector space equipped with a convex cone and a linear functional with the above properties.

So, I was only lying slightly. In fact, a complicated web of related formalisms have been explored; you can learn about them from Barnum's paper.

For example, the convex cone formalism seems related to the Jordan algebra approach described in "Week 162". Barnum cites a paper by Araki that shows how to get Jordan algebras from sufficiently nice convex cones:

10) H. Araki, "On a characterization of the state space of quantum mechanics", *Commun. Math. Phys.* **75** (1980), 1–24.

It's a very interesting paper but a wee bit too technical for me to feel like summarizing here.

Some nice examples of Jordan algebras are the 2×2 self-adjoint matrices with real, complex, quaternionic or octonionic entries. Each of these algebras has a cone consisting of the nonnegative matrices, and the trace gives a linear functional P. The nonnegative matrices with trace $=1$ are the mixed states of a spin-$1/2$ particle in 3, 4, 6, and 10-dimensional spacetime, respectively! In each case these mixed states form a convex set: a round ball generalizing the Bloch ball. Similarly, the pure states form a sphere generalizing the Riemann sphere.

Back in "Week 162" I explained how these examples are related to special relativity and spinors in different dimensions. It's so cool I can't resist reminding you.

Our universe seems to like complex quantum mechanics. And, the space of 2×2 self-adjoint complex matrices — let's call it $\mathrm{h}_2(\mathbb{C})$ — is isomorphic to 4-dimensional Minkowski spacetime! The cone of positive matrices is isomorphic to the future light-cone. The set of pure states of a spin-$1/2$ particle is the Riemann sphere $\mathbb{CP}^1$, and this is isomorphic to the "heavenly sphere": the set of light rays through a point in Minkowski spacetime.

This whole wonderful scenario works just as well in other dimensions if we replace the complex numbers ($\mathbb{C}$) by the real numbers ($\mathbb{R}$), the quaternions ($\mathbb{H}$) or the octonions ($\mathbb{O}$):

- $\mathrm{h}_2(\mathbb{R})$ is 3d Minkowski spacetime, and $\mathbb{RP}^1$ is the heavenly sphere S^1.
- $\mathrm{h}_2(\mathbb{C})$ is 4d Minkowski spacetime, and $\mathbb{CP}^1$ is the heavenly sphere S^2.
- $\mathrm{h}_2(\mathbb{H})$ is 6d Minkowski spacetime, and $\mathbb{HP}^1$ is the heavenly sphere S^4.
- $\mathrm{h}_2(\mathbb{O})$ is 10d Minkowski spacetime, and $\mathbb{OP}^1$ is the heavenly sphere S^8.

So, it's all very nice — but a bit mysterious. Why did our universe choose the complex numbers? We're told that scientists shouldn't ask "why" questions, but that's not really true — the main thing is to do it only to the extent that it leads to progress. But, sometimes you just can't help it.

String theorists occasionally think about 10d physics using the octonions, but not much. The strange thing about the octonions is that the self-adjoint nxn octonionic matrices $\mathrm{h}_n(\mathbb{O})$ only form a Jordan algebra when $n = 1$, 2, or 3. So, it seems we can only describe very small systems in octonionic quantum mechanics! Nobody knows what this means.

People working on the foundations of quantum mechanics have also thought about real and quaternionic quantum mechanics. $\mathrm{h}_n(\mathbb{R})$, $\mathrm{h}_n(\mathbb{C})$ and $\mathrm{h}_n(\mathbb{H})$ are Jordan algebras for all n, so the strange limitation afflicting the octonions doesn't affect these cases. But, I wound up sharing a little cottage with Lucien Hardy at Les Treilles, and he turns out to have thought about this issue. He pointed out that something interesting happens when we try to combine two quantum systems by tensoring them. The dimensions of $\mathrm{h}_n(\mathbb{C})$ behave quite nicely:

$$\dim(\mathrm{h}_{nm}(\mathbb{C})) = \dim(\mathrm{h}_n(\mathbb{C}))\dim(\mathrm{h}_m(\mathbb{C}))$$

But, for the real numbers we usually have

$$\dim(\mathrm{h}_{nm}(\mathbb{R})) > \dim(\mathrm{h}_n(\mathbb{R}))\dim(\mathrm{h}_m(\mathbb{R}))$$

and for the quaternions we usually have

$$\dim(\mathrm{h}_{nm}(\mathbb{H})) < \dim(\mathrm{h}_n(\mathbb{H}))\dim(\mathrm{h}_m(\mathbb{H}))$$

So, it seems that when we combine two systems in real quantum mechanics, they sprout mysterious new degrees of freedom! More precisely, we can't get all density matrices for the combined system as linear combinations of tensor products of density matrices for the two systems we combined. For the quaternions the opposite effect happens: the combined system has fewer mixed states than we'd expect.

This observation lurks behind axiom 4 in this paper:

11) Lucien Hardy, "Quantum theory from five reasonable axioms", available as quant-ph/0101012.

Another special way in which $\mathbb{C}$ is better than $\mathbb{H}$ or $\mathbb{R}$ is that only for a complex Hilbert space is there a correspondence between continuous 1-parameter groups of unitary operators and self-adjoint operators. We always get a *skew-adjoint* operator, but only in the complex case can we convert this into a self-adjoint operator by dividing by i.

Here are some more references, kindly provided by Rob Spekkens. The pioneering quantum field theorist Stckelberg wrote a bunch of papers on real quantum mechanics. Spekkens recommends this one:

12) E. C. G. Stckelberg, "Quantum theory in real Hilbert space", *Helv. Phys. Acta* **33**, 727 (1960).

This is a modern review:

13) Jan Myrheim, "Quantum mechanics on a real Hilbert space", available quant-ph/9905037.

What I find most fascinating is the connection between real quantum mechanics and time reversal symmetry. In ordinary complex quantum mechanics, time reversal symmetry is sometimes described by a conjugate-linear (indeed "antiunitary") operator T with $T^2 = 1$. Such an operator is precisely a "real structure" on our complex Hilbert space: it picks out a real Hilbert subspace of which our complex Hilbert space is the complexification.

It's worth adding that in the physics of fermions, another possibility occurs: an antiunitary time reversal operator with $T^2 = -1$. This is precisely a "quaternionic structure" on our complex Hilbert space: it makes it into a quaternionic Hilbert space!

For more on these ideas try:

14) Freeman J. Dyson, "The threefold way: algebraic structure of symmetry groups and ensembles in quantum mechanics", *Jour. Math. Phys.* **3** (1962), 1199–1215.

15) John Baez, "Symplectic, quaternionic, fermionic", `http://math.ucr.edu/home/baez/symplectic.html`

From all this one can't help but think that complex, real, and quaternionic quantum mechanics fit together in a unified structure, with the complex numbers being the most important, but other two showing up naturally in systems with time reversal symmetry.

Stephen Adler — famous for the Adler-Bell-Jackiw anomaly — spent a long time at the Institute for Advanced Studies working on quaternionic quantum mechanics:

16) S. L. Adler, *Quaternionic Quantum Mechanics and Quantum Fields*, Oxford U. Press, Oxford, 1995.

A problem with this book is that it defines a quaternionic vector space to be a *left* module of the quaternions, instead of a *bimodule*. This means you can't naturally tensor two quaternionic vector spaces and get a quaternionic vector space! Adler "solves" this problem by noting that any left module of the quaternions becomes a right module, and in fact a bimodule, via

$$xq = q^* x$$

But, when you're working with a noncommutative ring, you really need to think about left modules, right modules, and bimodules to understand the theory of tensor products. And, the quaternions have more bimodules than you might expect: for example, for any automorphism of the quaternions:

$$f \colon H \to H$$

there's a way to make H into an H-bimodule with the obvious left action and a "twisted" right action, where q acts on x to give

$$xf(q)$$

Since the automorphism group of the quaternions is $\mathrm{SO}(3)$, there turn out to be $\mathrm{SO}(3)$'s worth of nonisomorphic ways to make H into an H-bimodule!

For an attempt to tackle this issue, see:

17) John Baez and Toby Bartels, "Functional analysis with quaternions", available at `http://toby.bartels.name/papers/#quaternions`

However, it's possible we'll only see what real and quaternionic quantum mechanics are good for when we work in the 3-category $\mathsf{Alg}(\mathbb{R})$ mentioned in "Week 209", taking $\mathbb{R}$ to be the real numbers. Here:

- there's one object, the real numbers $\mathbb{R}$.
- the 1-morphisms are algebras A over $\mathbb{R}$.
- the 2-morphisms $M\colon A \to B$ are (A, B)-bimodules.
- the 3-morphisms $f\colon M \to N$ are (A, B)-bimodule morphisms.

This could let us treat real, complex and quaternionic quantum mechanics as part of a single structure.

Dreams, dreams. . . .

Addenda: In email, Scott Aaronson pointed out this nice webpage:

18) Scott Aaronson, "Lecture 9: Quantum", `http://www.scottaaronson.com/democritus/lec9.html`

He wrote:

> *I talk all about the known differences between QM over the complex numbers and QM over the reals and quaternions (including the parameter-counting difference you mentioned, but also a couple you didn't), and why the universe might've gone with complex numbers.*

His lecture also cites this paper:

19) Carlton M. Caves, Christopher A. Fuchs, and Ruediger Schack, "Unknown quantum states: the quantum de Finetti representation", available as `quant-ph/0104088`.

which Rob Spekkens also pointed out to me.

The quantum de Finetti theorem is a generalization of the classical de Finetti theorem. Both classical quantum de Finetti theorems are about n copies of a system sitting side by side in an "exchangeable" state: a state that's not only invariant under permutations of the copies, but lacking correlations between the different copies!

Here's the quantum de Finetti theorem. Suppose you have an "exchangeable" density operator ρ_n on $H^{\otimes n}$ — that is, one such that for each $N \geqslant n$, there's a density operator ρ_N on $H^{\otimes N}$ which 1) is invariant under permutations in S_N and 2) has ρ as its marginal, meaning that

$$\mathrm{Tr}(\rho_N) = \rho_n$$

where Tr is the partial trace map sending operators on $H^{\otimes N}$ to operators on $H^{\otimes n}$. Then, ρ_n is a mixture of density matrices of the form $\rho \otimes \ldots \otimes \rho$: a tensor product of n copies of a density matrix on H.

This is completely plausible if you know what all this jargon means.

And now for the punch line: *This theorem would **fail** if we did quantum mechanics using the real numbers!*

Of course, this is related to the fact I mentioned this Week, namely that for real quantum mechanics, "the whole is more than the product of its parts" in a more severe way than for complex quantum mechanics.

Bob Coecke wrote:

> *The standard references on quaternionic QM are:*
>
> 20) *D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, "Foundations of quaternion quantum mechanics",* Journal of Mathematical Physics ***3**, 207 (1962).*
> 21) *D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, "Some physical consequences of general Q-covariance",* Helvetica Physica Acta ***35**, 328–329 (1962).*
> 22) *D. Finkelstein, J.M. Jauch, S. Schiminovich and D. Speiser, "Principle of general Q-covariance",* Journal of Mathematical Physics ***4**, 788–796 (1963).*
>
> *A standard structural result in the order-theoretic vein which separates Reals, Complex Numbers and Quaternions from "non-classical fields" is:*
>
> 23) *M. P. Soler (1995) "Characterization of Hilbert spaces with orthomodular spaces",* Comm. Algebra ***23**, pp. 219–243.*
>
> *It does this relative to the order-theoretic characterization of Hilbert spaces:*
>
> 24) *C. Piron (1964, French) "Axiomatique Quantique",* Helv. Phys. Acta ***37**, pp. 439–468.*
> 25) *I. Amemiya and H. Araki (1966) "A Remark on Piron's Paper",* Publ. Res. Inst. Math. Sci. Ser. A ***2**, pp. 423–427.*
> 26) *C. Piron (1976)* Foundations of Quantum Physics*, W. A. Benjamin, Inc., Reading.*
>
> *A nicely written recent survey on this stuff is:*
>
> 26) *Isar Stubbe and B. van Steirteghem (2007) "Propositional systems, Hilbert lattices and generalized Hilbert spaces", chapter in:* Handbook Quantum Logic *(edited by D. Gabbay, D. Lehmann and K. Engesser), Elsevier, to appear. Available at* `http://www.win.ua.ac.be/~istubbe/`
>
> *It is not clear to me how exactly this order-theoretic stuff relates to the* thick *categorical axiomatics for QM John mentioned above. One key difference is that in the order-theoretic axiomatics one failed to find an abstract counterpart to the Hilbert space tensor product. (ie without having to say that we are working in the lattice of closed subspaces of a Hilbert space) On the other hand, the categorical approach starting from symmetric monoidal categories takes that description of compound systems as an a priori. Singling out the complex numbers*

> *is done in terms of two involutions on morphisms, one covariant and one contravariant, where the covariant one capture complex conjugation ie the unique non-trivial automorphism characteristic of complex numbers. The contravariant one captures transposition and together they make up the adjoint.*

Here "thick" refers to working with categories which nice big hom-sets, instead of mere posets or preorders, which are categories with at most one morphism from one object to another.

Rob Spekkens also gives some references on quantum computation in real quantum mechanics. He writes:

> *See also:*
>
> *27) C. M. Caves, C. A. Fuchs, P. Rungta, "Entanglement of formation of an arbitrary state of two rebits", available as quant-ph/0009063.*
>
> *It's also worth noting that quantum computation and quantum cryptography do not require the complex field. Have a look at:*
>
> *28) T. Rudolph and L. Grover, "A 2 rebit gate universal for quantum computing", quant-ph/0210187.*
>
> *I actually know of no information-theoretic task whose possibility is contingent on the nature of the number field.*

More discussion (and pictures!) can be found at the n-Category Caf.

Week 252

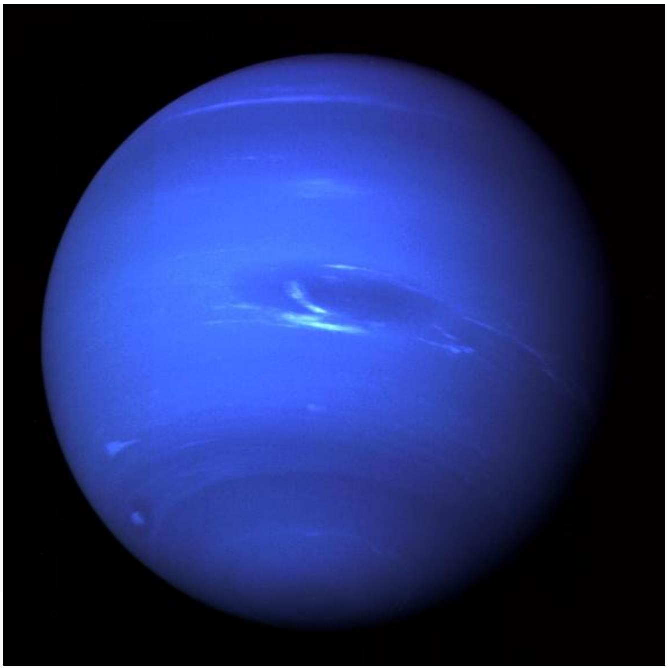
May 27, 2007

Today I want to tell you about the electromagnetic snake at the center of our Galaxy, and continue the Tale of Groupoidification.

But first, the long-range weather forecast. There's a 40% chance of rain on Neptune in 8 billion years! More precisely, that's the chance these authors give for the formation of an ocean on Neptune when its interior cools down enough:

1) Sloane J. Wiktorowica and Andrew P. Ingersoll, "Liquid water oceans in ice giants", available as astro-ph/0609723.

Right now, even though Neptune is named after the Roman god of seas and has a nice blue appearance:



it's mighty dry — at least on top. Spectroscopy detects no water at all in its upper atmosphere! But that's consistent with the presence of water down below, since water vapor is a lot heavier than hydrogen and helium, which make up most of Neptune's upper atmosphere, and the pull of gravity there is mighty fierce.

In fact, scientists believe Neptune has a core of molten metal and rock surrounded by more rock, methane ice, ammonia ice, and water ice — all solid due to high pressures. The overall density of Neptune makes sense if there's a lot of water down there. But, Wiktorowica and Ingersoll argue that the planet can't have an ocean of liquid water.

Surprisingly, this is because Neptune is too *hot* — even though its upper atmosphere is a chilly 50 kelvin! I think their argument goes roughly like this, though I don't understand it well. If you fell down through the atmosphere of Neptune, you'd find that it gets hotter and hotter as you go down, and moister and moister too — but always too hot, given the amount of moisture, for liquid water to be more stable than water vapor.

However, they also indulge in some predictions about the far future!

First let's set the stage:

- In 1.1 billion years the Sun will become 10% brighter than now, and the Earth's atmosphere will dry out.
- In 3 billion years the Andromeda Galaxy will collide with our galaxy. Many solar systems will be destroyed.
- In 3.5 billion years the Sun will become 40% brighter than today. If the Earth is still orbiting the sun, its oceans will evaporate.
- In 5.4 billion years from now the Sun's core will run out of hydrogen. It will enter its first red giant phase, becoming 1.6 times bigger and 2.2 times brighter than today.
- In 6.5 billion years from now the Sun will become a full-fledged red giant, 170 times bigger and 2400 times brighter than today. The Republican Party will finally admit the existence of global warming, but point out that it's not human-caused.
- In 6.7 billion years from now the Sun will start fusing helium and shrink back down to 10 times bigger and 40 times brighter than today.
- In 6.8 billion years from now the Sun will run out of helium. Being too small to start fusing carbon and oxygen, it'll enter a second red giant phase, growing 180 times bigger and 3000 times brighter than today.

But then, about 6.9 billion years from now, the Sun will start pulsating, ejecting half of its mass in the form of solar wind! It'll become what they call a "planetary nebula". Eventually only its inner core will be left. In "Week 223" I quoted Bruce Balick's eloquent description:

> *The remnant Sun will rise as a dot of intense light, no larger than Venus, more brilliant than 100 present Suns, and an intensely hot blue-white color hotter than any welder's torch. Light from the fiendish blue "pinprick" will braise the Earth and tear apart its surface molecules and atoms. A new but very thin "atmosphere" of free electrons will form as the Earth's surface turns to dust.*

This is where Wiktorovika and Ingersoll *begin* their story. So far, Neptune will have warmed up a lot — assuming for the sake of argument that it wasn't thrown out of the Solar System when the Milky Way hit Andromeda. But when the Sun loses mass, Neptune will either collide with Uranus, be ejected from the Solar System, or assume a stable orbit about twice its current size.

In the latter two scenarios, Neptune will chill out, at least when the remnant Sun cools down and becomes a white dwarf. When the surface temperature of Neptune reaches 30 kelvin, they estimate it has a 41.5% plus or minus 4.2% chance of forming oceans.

So, they say: “Billions of years from now, after the Sun has gone, Neptune may therefore become the only object in the Solar system with liquid water oceans”.

This sounds nice — but don’t buy your beachfront property there yet.

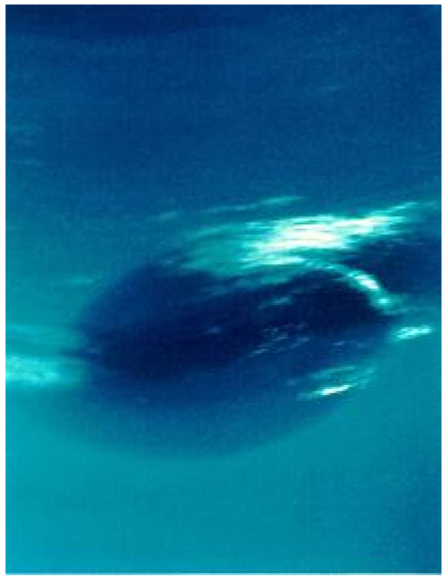
First, they don’t study how the atmospheric composition of Neptune may change when the Sun gets 3000 times brighter than now! Maybe this will *help* Neptune form oceans. After all, light gases like molecular hydrogen and helium, which dominate Neptune’s upper atmosphere now, are more likely than water vapor to be driven off into outer space when it gets hot. But, they don’t even check to see if Neptune will have *any* atmosphere left after this era.

Second, the estimate of a “41.5% plus or minus 4.2% chance” seems strangely precise, given the uncertainties involved. The error bars should probably have big error bars themselves!

Third, it’s worth admitting that the atmosphere of Neptune is a bit mysterious. For example, nobody knows why it’s bright blue. It’s probably because of methane — but Uranus also has methane in its atmosphere, and it’s not as blue:

Also, despite being the coldest planet in our Solar System, Neptune has the fastest winds: up to 2100 kilometers per hour, almost supersonic! Nobody knows what powers these winds.

When the Voyager 2 spacecraft flew by Neptune in 1992, it saw a storm system the size of Eurasia, which was dubbed “The Great Dark Spot”:

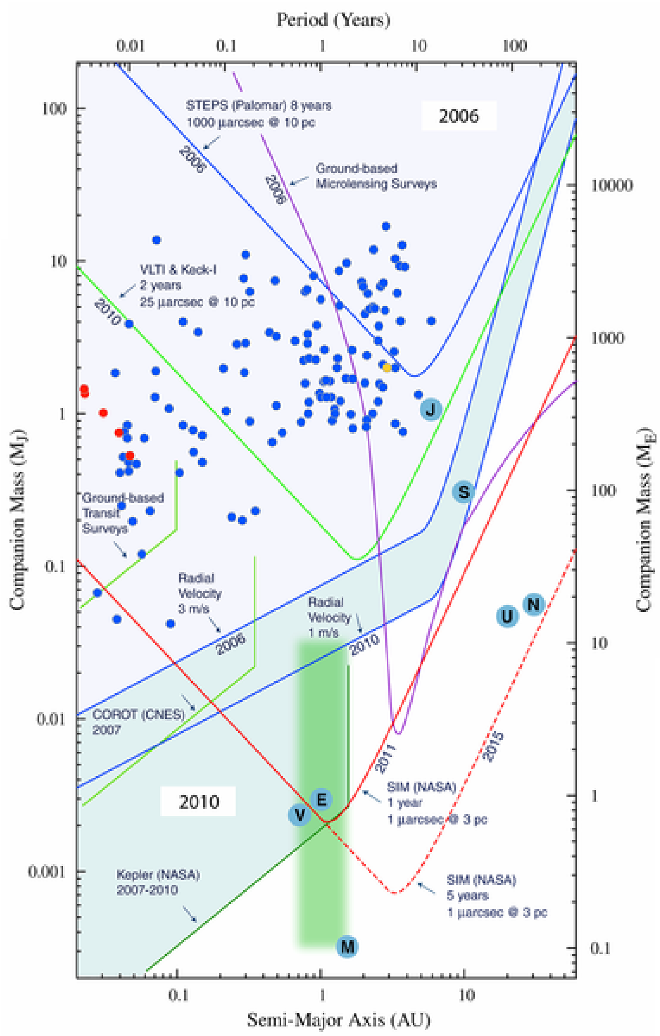


It seemed to resemble the Great Red Spot on Jupiter, which has been around for at least 177 years. But when the Hubble Space Telescope took another good look at Neptune in 1994, the Great Dark Spot was gone! Meanwhile, other storms had formed.

So, the weather on Neptune is dynamic and poorly understood. Doing forecasts for the next 8 billion years seems pretty risky... though fun.

Of course, liquid water oceans are nice if you’re looking for life. And while there probably isn’t life on Neptune, there could be life on similar planets in other solar systems. So far people have found 233 of these “exoplanets”, most of them heavier than Jupiter but close to their suns — because such planets are the easiest to detect by how they pull on their sun.

Here's a chart showing the masses and orbital radii of known exoplanets as of 2004:



2) P. R. Lawson, S. C. Unwin, and C. A. Beichman, "Precursor Science for the Terrestrial Planet Finder", *JPL Pub* **04-014**, Oct. 2004, page 21, fig. 5. Chart at `http://en.wikipedia.org/wiki/Image:Extrasolar_Planets_2004-08-31.png`. Report at `http://planetquest.jpl.nasa.gov/documents/RdMp272.pdf`

For comparison, the letters V, E, M, J, S, U, N, stand for planets in our Solar System, not counting Mercury or the subsequently dethroned Pluto.

As you'll see, there are lots of "hot Jupiters" — planets as big as Jupiter, or even up to 200 times heavier, but closer to their sun than we are to ours. Recently a postdoc at UCLA found evidence for water in the atmosphere of one of these planets:

3) Travis Barman, "Identification of absorption features in an extrasolar planet atmosphere", available as `arxiv:0704.1114`.

This planet is only 7 million kilometers away from its yellow-white Sun, much closer than Mercury is to ours. Its year is only 3.5 of our days! It's bigger in size than Jupiter, but only 0.7 times as heavy. Its surface temperature must be about 1000 kelvin. That's one really hot Jupiter.

People have also been finding "hot Neptunes". In fact, I read about a nice one in the newspaper while writing this! It's called Gliese 436 b. It's the size of Neptune and it's orbiting the red dwarf star Gliese 436, which is 33 light-years from Earth. This star is only 1% as bright as our Sun — but the planet is so close that its year lasts less than three of our days! So, its surface temperature is high: higher than the melting point of lead.

However, because this planet is so big, the pressure down below can still make water into a solid. In fact, its density suggests that it's mainly made of ice!

I think this is the paper that triggered the newspaper reports:

4) M. Gillon et al, "Detection of transits of the nearby hot Neptune GJ 436 b", available as `arxiv:0705.2219`.

It seems hot Neptunes like this could have started as hot Jupiters and then lost a lot of their atmosphere:

5) I. Baraffe, G. Chabrier, T. S. Barman, F. Selsis, F. Allard, and P. H. Hauschildt, "Hot-Jupiters and hot-Neptunes, a common origin?", available at `astro-ph/0505054`.

There could also be cold Neptunes, perhaps with liquid water oceans. We haven't seen these yet, but they'd be hard to see. So, while Wiktorowica and Ingersoll's paper doesn't convince me about the future of *our* Neptune, it raises some interesting possibilities.

Next: the snake at the center of our galaxy!

Gregory Benford is best known for his science fiction, which spans the galaxy, but he's also an astrophysicist at U. C. Irvine. Recently he spent a week at my school, U. C. Riverside, which has one of the world's best SF libraries: the Eaton Collection. Since my wife is involved in the SF program at the comparative literature department here, he had dinner at our place one night. The conversation drifted all over the place, with a heavy focus on fruit flies that have been bred to live twice as long as usual. But when I asked him about his research, he said he'd written some papers about enormous glowing filaments near the center of the Milky Way.

I hadn't known about these! He said the biggest one could be a million years old, perhaps formed by some energetic event, maybe a star falling into the central black hole. Here's an expository paper he wrote about it:

6) Gregory Benford, "The electromagnetic snake at the galactic center", `http://www.ps.uci.edu/physics/news3/benford.html`

I'll just quote a little:

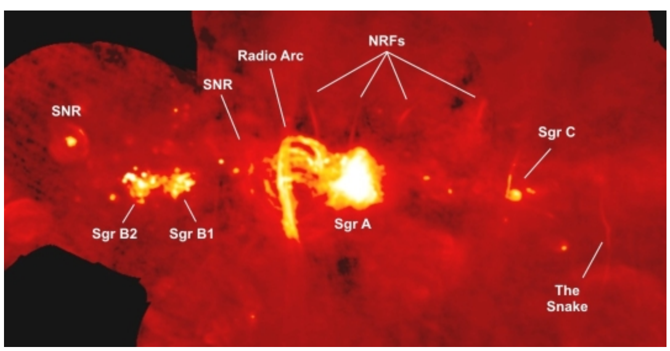
> *Five years ago radio astronomy revealed the oddest and longest filament yet discovered at our galactic center: a uniquely kinked structure about 150 light years long and two to three light years wide — the Snake. Its large kinks are its*

> *brightest parts. There is energetic activity at one end and a supernova bubble at the other, which the Snake appears to penetrate unharmed.*
>
> *How does nature form stable, long-lived magnetic structures which display considerable polarization (about 60% at 10.55 GHz in the Snake)? In 1988 I had modeled others of the dozens of filaments seen uniquely at the galactic center in terms of an electrodynamic view, in which currents set up coherent magnetic pinches. Such self-organizing filaments can exist in laboratory plasmas for long times; the galactic ones could be at least a million years old, as estimated by the time that shear forces would disrupt them.*
>
> *The electrodynamic view uses pinch forces of currents to form filaments, driven by the $E = v \times B$ of conducting molecular clouds moving across a strong milliGauss ambient, ordered field. A return current must then flow at larger radii, making a closed loop which has a springy flexibility, able to withstand the turbulent velocity fields known near the galactic center. The picture then anticipates that aberrant molecular clouds, moving contrary to the general galactic rotation, should accompany each filament. This prediction has held up as more filaments were found.*

I'd like to learn more about these!

Back in "Week 248", I mentioned some of the complex things that electromagnetic fields and plasma do in the Sun. The center of the Galaxy is another place where electromagnetohydrodynamics runs rampant. There's a black hole there, of course, but also these filaments, and a fairly strong magnetic field that contains about 4×10^{47} joules of energy within about 300 light years of the galactic center. By comparison, a supernova emits a mere 10^{44} joules. So, there's a lot of energy around. . . .

The big picture here, created by Farhad Yusef-Zadeh and collaborators, shows the galactic center quite nicely, as viewed in radio frequencies:



7) National Radio Astronomy Observatory, "Origin of enigmatic galactic-center filaments revealed", `http://www.nrao.edu/pr/2004/filaments/`

You can see some supernova remnants (SNRs), the region Sgr A which contains the black hole at the galactic center, various nonthermal radio filaments (NRFs), and the Snake. Some of these filaments come from regions where stars are forming. . . that could be important.

But, this article discusses another piece of the puzzle — the possible role of turbulence in winding up the galactic magnetic field:

8) Stanislav Boldyrev and Farhad Yusef-Zadeh, "Turbulent origin of the galactic-center magnetic field: nonthermal radio filaments", available as astro-ph/0512373.

It's a complicated stew. I don't hope to understand it, just admire it.

And finally: the Tale of Groupoidification! In "Week 250" we reached the point of seeing how spans of groupoids over a fixed group G subsume the theory of G-sets and invariant relations between these — which are traditionally studied using "double cosets".

There is a lot more we could say about this. But, our most urgent goal is to see how spans of groupoids act like twice categorified matrices — matrices whose entries are not just *numbers*, and not just *sets*, but *groupoids*! This will expose the secret combinatorial underpinnings of a lot of fancy linear algebra. Once we've categorified linear algebra this way, we'll be in a great position to tackle fashionable topics like categorified quantum groups, invariants of higher-dimensional knots, and the like.

But, we should restrain ourselves from charging ahead too fast! Everything will hang together better if we lay the groundwork properly. For this, it pays to re-examine the history of mathematics a bit. If we're trying to understand linear algebra using groupoids, it pays to ask: how did people connect linear algebra and group theory in the first place?

This book is very helpful:

9) Charles W. Curtis, *Pioneers of Representation Theory: Frobenius, Burnside, Schur and Brauer*, History of Mathematics vol. **15**, AMS, Providence, Rhode Island, 1999.

Back in 1897, a mathematician named William Burnside wrote the first book in English on finite groups. It was called Theory of Groups of Finite Order.

In the preface, Burnside explained why he studied finite groups by letting them act as permutations of sets, but not as linear transformations of vector spaces:

> *Cayley's dictum that "a group is defined by means of the laws of combination of its symbols" would imply that, in dealing with the theory of groups, no more concrete mode of representation should be used than is absolutely necessary. It may then be asked why, in a book that professes to leave all applications to one side, a considerable space is devoted to substitution groups [permutation groups], but other particular modes of representation, such as groups of linear transformations, are not even referred to. My answer to this question is that while, in the present state of our knowledge, many results in the pure theory are arrived at most readily by dealing with properties of substitution groups, it would be difficult to find a result that could most directly be obtained by the consideration of groups of linear transformations.*

In short, he didn't see the point of representing groups on vector spaces — at least as a tool in the "pure" theory of finite groups, as opposed to their applications.

However, within months after this book was published, he discovered the work of Georg Frobenius, who used linear algebra very effectively to study groups!

So, Burnside started using linear algebra in his own work on finite groups, and by the time he wrote the second edition of his book in 1911, he'd changed his tune completely:

> *Very considerable advances in the theory of groups of finite order have been made since the appearance of the first edition of this book. In particular the theory of groups of linear substitutions has been the subject of numerous and important investigations by several writers; and the reason given in the original*

> *preface for omitting any account of it no longer holds good. In fact it is now more true to say that for further advances in the abstract theory one must look largely to the representation of a group as a group of linear transformations.*

It's interesting to see exactly how representing finite groups on vector spaces lets us understand them better. By now almost everyone agrees this is true. But how much of the detailed machinery of linear algebra is really needed? How much we could do purely combinatorially, using just spans of groupoids?

I don't really know the full answer to this question. But, it quickly leads us into the fascinating theory of "Hecke operators", which will play a big role in the Tale of Groupoidification. So, let's pursue it a bit.

Suppose we have two guys, William and Georg, who are studying a finite group G.

William says, "I'm studying how G acts on sets."

Georg replies, "I'm studying how it acts on complex vector spaces, as linear transformations. Mere sets are feeble entities. I can do anything you can do — but I have the tools of linear algebra to help me!"

William says, "But, you're paying a moral price. You're getting the complex numbers — a complicated infinite thing — involved in what should be a completely finitary and combinatorial subject: the study of a finite group. Is this really necessary?"

Georg replies, "I don't know, but it's sure nice. For example, suppose I have G acting on vector space V. Then I can always break down V into a direct sum of subspaces preserved by G, which can't themselves be broken down any further. In technical terms: every representation of G is a direct sum of irreducible representations. And, this decomposition is unique! It's very nice: it's a lot like how every natural integer has a unique prime factorization."

William says, "Yes, it's always fun when we can break things down into 'atoms' that can't be further decomposed. It's very satisfying to our reductionist instincts. But, I can do even better than you!"

Georg raises an eyebrow. "Oh?"

"Yeah," William says. "Suppose I have our group G acting on a set S. Then I can always break down S into a disjoint union of subsets preserved by G, which can't themselves be broken down any further. In technical terms: every action of G is a disjoint union of transitive actions. And, this decomposition is unique!"

Embarrassed, Georg replies, "Oh, right — we heard that back in "Week 249". I wasn't paying enough attention. But how is what you're doing better than what I'm doing? It sounds just the same."

William hesitates. "Well, first of all, a rather minor point, which I can't resist mentioning . . . when you said your decomposition of representations into irreducibles was unique, you were exaggerating a little. It's just unique up to isomorphism, and not a canonical isomorphism either.

For example, if you have an irreducible representation of G on V, there are lots of *different ways* to slice the direct sum $V \oplus V$ into two copies of the representation V. It's a sort of floppy business. On the other hand, when I have a transitive action of G on S, there's exactly *one way* to chop the disjoint union $S \sqcup S$ into two copies of the G-set S. I just look at the two orbits."

Georg says, "Hmm. This is actually a rather delicate point. There's not really a *canonical* isomorphism in your case either, since S may be isomorphic to itself in lots of ways, even as a G-set. There's something in what you say, but it's a bit hard to make precise, and it's certainly not enough to worry me."

"Okay, but here's my more serious point. Given that I can break apart any set on which G acts into 'atoms', my job is to classify those atoms: the transitive G-sets. And there's a very nice classification! Any subgroup H of G gives a transitive G-set, namely G/H, and all transitive G-sets look like this. More precisely: isomorphism classes of transitive G-sets correspond to conjugacy classes of subgroups of G.

Even better, this has a nice meaning in terms of Klein geometry. Any type of figure in Klein geometry gives a transitive G-set, with H being the stabilizer of a chosen figure of that type.

You, on the other hand, need to classify irreducible representations of G. And this is not so conceptual. What do these irreducible representations *mean* in terms of the group G?"

Georg replies, "Well, there's one irreducible representation for each conjugacy class in G..."

At this, William pounces. "You mean the *number* of isomorphism classes of irreducible representations of G equals the *number* of conjugacy classes in G! But as you know full well, there's no god-given correspondence. You can't just take a conjugacy class in G and cook up an irreducible representation, or irrep. So, you've just made my point. You've shown how mysterious these irreps actually are!"

Georg replies, "Actually in some cases there *is* a nice way to read off irreps from conjugacy classes. For example, you can do it for the symmetric groups S_n. But, I admit you can't in general... or at least, I don't know how."

William laughs, "So, I win!"

"Not at all!" replies Georg. "First, there are lots of theorems about groups that I can prove using representations, which you can't prove using actions on sets. For example, nobody knows how to prove that every group with an odd number of elements is solvable without using the tools of linear algebra."

William nods. "I admit that linear algebra is very practical. But just give us time! I proved back in 1904 that every group of size $p^a q^b$ is solvable if p and q are prime. To do it, I broke down and used linear algebra. But then, in 1972, Helmut Bender found a proof that avoids linear algebra."

Georg said, "Okay, struggle on then. So far, without using linear algebra, nobody can even prove my famous theorem on 'Frobenius groups'. The details don't matter here: the point is, this is a result on group actions, which seems to need linear algebra for its proof.

But if practicality won't sway you, maybe this conceptual argument will. My atoms are more fine-grained than yours!"

"What do you mean?" asks William.

"You can decompose any action of G into 'atoms', namely transitive G-sets. Similarly, I can decompose any representation of G into one of my 'atoms', namely irreps. But, there's an obvious way to turn G-sets into representations of G, and if we do this to one of your atoms, we don't get one of my atoms! We can usually break it down further! So, my atoms are smaller than yours."

"How does this work, exactly?"

"It's easy," says Georg, getting a bit cocky. "Say you have a group G acting on a set S. Then I can form the vector space $\mathbb{C}[S]$ whose elements are formal linear combinations of elements of S. In other words, it's the vector space having S as basis. If we're feeling sloppy we can think of guys in $\mathbb{C}[S]$ as functions on S that vanish except at finitely many points. It's better to think of them as measures on S. But anyway: since G acts on S, it acts linearly on $\mathbb{C}[S]$!

So, any G-set gives a representation of G. But, even when G acts transitively on S, its representation on $\mathbb{C}[S]$ is hardly ever irreducible."

William glowers. "Oh yeah?"

"Yeah. Suppose for example that S is finite. Then the constant functions on S form a 1-dimensional subspace of $\mathbb{C}[S]$ that's invariant under the action of G. So, at the very least, we can break $\mathbb{C}[S]$ into two pieces."

"Well," replies William defensively, "That's pretty obvious. But it's also not such a big deal. So you can break up any my transitive G-sets into two of your irreps, one being the 'trivial' irrep. So what???"

"It wouldn't be a big deal if that's *all* that ever happened," says Georg. "In fact, we can break $\mathbb{C}[S]$ into precisely two irreps whenever the action of G on S is 'doubly transitive' — meaning we can send any *pair* of distinct elements of S to any other using some element of G. But, there lots of transitive actions aren't doubly transitive! And usually, one your atoms breaks down into a *bunch* of my atoms. In fact I'd like to show you how this works, in detail, for the symmetric groups."

"Maybe next week," says William. "But, I see your point. Your atoms are more atomic than my atoms."

Georg seems to have won the argument. But, William wouldn't have conceded the point quite so fast, if he'd thought about invariant relations!

The point is this. Suppose we have two G-sets, say X and Y. Any G-set map from X to Y gives an intertwining operator from $\mathbb{C}[X]$ to $\mathbb{C}[Y]$. But, even after taking linear combinations, there are typically plenty of intertwining operators that don't arise this way. It's these extra intertwining operators that let us chop representations into smaller atoms.

But where do these extra intertwining operators come from? They come from *invariant relations* between X and Y!

And, what are these extra intertwining operators called? In some famous special cases, like in study of modular forms, they're called "Hecke operators". In some other famous special cases, like in the study of symmetric groups, they form algebras called "Hecke algebras".

A lot of people don't even know that Hecke operators and Hecke algebras are two faces of the same idea: getting intertwining operators from invariant relations. But, we'll see this is true, once we look at some examples.

I think I'll save those for future episodes. But if you've followed the Tale so far, you can probably stand a few extra hints of where we're going. Recall from "Week 250" that invariant relations between G-sets are just spans of groupoids equipped with some extra stuff. So, invariant relations between G-sets are just a warmup for the more general, and simpler, theory of spans of groupoids. I said back in "Week 248" that spans of groupoids give linear operators. What I'm trying to say now is that these linear operators are a massive generalization — but also a simplification — of what people call "Hecke operators".

Finally, for students trying to learn a little basic category theory, I'd like to cast the argument between William and Georg into the language of categories, just to help you practice your vocabulary.

A G-set is the same as a functor

$$A\colon G \to \mathsf{Set}$$

where we think of G as a 1-object category. There's a category of G-sets, namely

$$\mathrm{Hom}(G, \mathsf{Set})$$

This has functors $A\colon G \to \mathsf{Set}$ as objects, and natural transformations between these as morphisms. Usually the objects are called "G-sets", and the morphisms are called "maps of G-sets".

We can also play this whole game with the category of vector spaces replacing the category of sets. A representation of G is the same as a functor

$$A\colon G \to \mathsf{Vect}$$

As before, there's a category of such things, namely

$$\mathrm{Hom}(G, \mathsf{Vect})$$

This has functors $A\colon G \to \mathsf{Vect}$ as objects, and natural transformations between these as morphisms. Now the objects are called "representations of G" and the morphisms are called "intertwining operators".

We could let any groupoid take the place of the group G. We could also let any other category take the place of Set or Vect.

Part of what William and Georg were debating was: how similar are $\mathrm{Hom}(G, \mathsf{Set})$ and $\mathrm{Hom}(G, \mathsf{Vect})$? How are they related?

First of all, there's a functor

$$F\colon \mathsf{Set} \to \mathsf{Vect}$$

sending each set S to the vector space $\mathbb{C}[S]$ with that set as basis. So, given an action of G on a set:

$$A\colon G \to \mathsf{Set}$$

we can compose it with F and get a representation of G:

$$FA\colon G \to \mathsf{Vect}$$

This kind of representation is called a "permutation representation". And, this trick gives a functor from G-sets to representations of G:

$$\mathrm{Hom}(G, \mathsf{Set}) \to \mathrm{Hom}(G, \mathsf{Vect})$$
$$A \mapsto FA$$

If this functor were an equivalence of categories, it would have to be essentially surjective, full and faithful. But, not every representation of G is isomorphic to a permutation representation! In other words, the functor

$$\mathrm{Hom}(G, \mathsf{Set}) \to \mathrm{Hom}(G, \mathsf{Vect})$$

is not “essentially surjective”.

Moreover, not every intertwining operator between permutation representations comes from a map between their underlying G-sets! In other words, the functor

$$\mathrm{Hom}(G, \mathsf{Set}) \to \mathrm{Hom}(G, \mathsf{Vect})$$

is not “full”.

But, given two different maps from one G-set to another, they give different intertwining operators. So, at least our functor is “faithful”.

Maps of G-sets are a special case of invariant relations. So, to get a category that more closely resembles $\mathrm{Hom}(G, \mathsf{Vect})$, while remaining purely combinatorial, we can replace $\mathrm{Hom}(G, \mathsf{Set})$ by the category with G-sets as objects and invariant binary relations as morphisms. This is the basic idea of “Hecke operators”.

Or, even better, we can try a weak 2-category, with

- groupoids over G as objects
- spans of groupoids over G as morphisms
- maps between spans of groupoids over G as 2-morphisms

This is where groupoidification comes into its own.

Addendum: For more discussion, go to the n-Category Caf.

> The present treatise is intended to introduce to the reader the main outlines of the theory of groups of finite order apart from any applications. The subject is one which has hitherto attracted but little attention in this country; it will afford me much satisfaction if, by means of this book, I shall arouse interest among English mathematicians in a branch of pure mathematics which becomes the more fascinating the more it is studied
>
> — *William Burnside*

Week 253

June 27, 2007

Yay! Classes are over! Soon I'm going to Paris for three weeks, to talk with Paul-Andr Mellis about logic, games and category theory. But right now I'm in a vacation mood. So, I want to take a break from the Tale of Groupoidification, and mention some thoughts prompted by the work of Garrett Lisi:

1) Garrett Lisi, Deferential Geometry, http://deferentialgeometry.org.

Garrett is a cool dude who likes to ponder physics while living a low-budget, high-fun lifestyle: hanging out in Hawaii, surfing, and stuff like that. He recently won a Foundational Questions Institute award to think about ways to unify particle physics and gravity. That's an institute devoted precisely to risky endeavors like this.

Lately he's been visiting California. So, before giving a talk at Loops '07 — a loop quantum gravity conference taking place in Mexico this week — he stopped by Riverside to explain what he's been up to.

Briefly, he's been trying to explain the 3 generations of elementary particles using some math called "triality", which is related to the octonions and the exceptional Lie groups. In fact, he's trying to use the exceptional Lie group E_8 to describe all the particles in the Standard Model, together with gravity.

I'd like to know if these ideas hold water. So, I should try to explain them! But as usual, in this Week's Finds I'll wind up explaining not what Garrett actually did, but what it made me think about.

For a long time, people have been seeking connections between the messy pack of particles that populate the Standard Model and structures that seem beautiful and "inevitable".

A fascinating step in this direction was the $\mathrm{SU}(5)$ grand unified theory proposed in 1975 by Georgi and Glashow. So, I'll start by summarizing that… and then explain how exceptional Lie groups might get involved in this game.

What people usually call the gauge group of the Standard Model:

$$\mathrm{SU}(3) \times \mathrm{SU}(2) \times \mathrm{U}(1)$$

actually has a bit of flab in it: there's a normal subgroup that acts trivially on all known particles. This subgroup is isomorphic to $\mathbb{Z}/6$. If we mod out by this, we get the "true" gauge group of the Standard Model:

$$G = (\mathrm{SU}(3) \times \mathrm{SU}(2) \times \mathrm{U}(1))/(\mathbb{Z}/6)$$

And, this turns out to have a neat description. It's isomorphic to the subgroup of $\mathrm{SU}(5)$ consisting of matrices like this:

$$\begin{pmatrix} g & 0 \\ 0 & h \end{pmatrix}$$

where g is a 3×3 block and h is a 2×2 block. For obvious reasons, I call this group

$$\mathrm{S}(\mathrm{U}(3) \times \mathrm{U}(2))$$

If you want some intuition for this, think of the 3×3 block as related to the strong force, and the 2×2 block as related to the electroweak force. A 3×3 matrix can mix up the 3 "colors" that quarks come in — red, green, and blue — and that's what the strong force is all about. Similarly, a 2×2 matrix can mix up the 2 "isospins" that quarks and leptons come in — up and down — and that's part of what the electroweak force is about.

If this isn't enough to make you happy, go back to "Week 119", where I reviewed the Standard Model and its relation to the $\mathrm{SU}(5)$ grand unified theory. If even that isn't enough to make you happy, try this:

2) John Baez, "Elementary particles", `http://math.ucr.edu/home/baez/qg-spring2003/elementary/`

Okay — I'll assume that one way or another, you're happy with the idea of $\mathrm{S}(\mathrm{U}(3) \times \mathrm{U}(2))$ as the true gauge group of the Standard Model! Maybe you understand it, maybe you're just willing to nod your head and accept it.

Now, the fermions of the Standard Model form a very nice representation of this group. $\mathrm{SU}(5)$ has an obvious representation on $\mathbb{C}^5$, via matrix multiplication. So, it gets a representation on the exterior algebra $\wedge(\mathbb{C}^5)$. If we restrict this from $\mathrm{SU}(5)$ to $\mathrm{S}(\mathrm{U}(3) \times \mathrm{U}(2))$, we get precisely the representation of the true gauge group of the Standard Model on one generation of fermions and their antiparticles!

This really seems like a miracle when you first see it. All sorts of weird numbers need to work out exactly right for this trick to succeed. For example, it's crucial that quarks have charges $2/3$ and $-1/3$, while leptons have charges 0 and -1. One gets the feeling, pondering this stuff, that there really is some truth to the $\mathrm{SU}(5)$ grand unified theory.

To give you just a little taste of what's going on, let me show you how the exterior algebra $\wedge(\mathbb{C}^5)$ corresponds to one generation of fermions and their antiparticles. For simplicity I'll use the first generation, since the other two work just the same:

- $\wedge^0(\mathbb{C}^5) \cong \langle\text{left-handed antineutrino}\rangle$
- $\wedge^1(\mathbb{C}^5) \cong \langle\text{right-handed down quark}\rangle \oplus \langle\text{right-handed positron}, \text{right-handed antineutrino}\rangle$
- $\wedge^2(\mathbb{C}^5) \cong \langle\text{left-handed up antiquark}\rangle \oplus \langle\text{left-handed up quark}, \text{left-handed down quark}\rangle \oplus \langle\text{left-handed positron}\rangle$
- $\wedge^3(\mathbb{C}^5) \cong \langle\text{right-handed electron}\rangle \oplus \langle\text{right-handed up antiquark}, \text{right-handed down antiquark}\rangle \oplus \langle\text{right-handed up quark}\rangle$
- $\wedge^4(\mathbb{C}^5) \cong \langle\text{left-handed up antiquark}\rangle \oplus \langle\text{left-handed electron}, \text{left-handed neutrino}\rangle$
- $\wedge^5(\mathbb{C}^5) \cong \langle\text{right-handed neutrino}\rangle$

All the quarks and antiquarks come in 3 colors, which I haven't bothered to list here. Each space $\wedge^p(\mathbb{C}^5)$ is an irreducible representation of $\mathrm{SU}(5)$, but most of these break up into several different irreducible representations of $\mathrm{S}(\mathrm{U}(3) \times \mathrm{U}(2))$, which are listed as separate rows in the chart above.

If you're curious how this "breaking up" works, let me explain — it's sort of pretty. We just use the splitting

$$\mathbb{C}^5 \cong \mathbb{C}^3 \oplus \mathbb{C}^2$$

to chop the spaces $\wedge^p(\mathbb{C}^5)$ into pieces.

To see how this works, remember that $\wedge^p(\mathbb{C}^5)$ is just the vector space analogue of the binomial coefficient "$\binom{5}{p}$". A basis of $\mathbb{C}^5$ consists of 5 things, and the p-element subsets give a basis for $\wedge^p(\mathbb{C}^5)$.

In our application to physics, these 5 things consist of 3 "colors" — red, green and blue — and 2 "isospins" — up and down. This gives various possible options.

For example, suppose we want a basis of $\wedge^3(\mathbb{C}^5)$. Then we need to pick 3 things out of 5. We can do this in various ways:

- We can pick 3 colors and no isospins — there's just one way to do that.
- We can pick 2 colors and 1 isospin — there are six ways to do that.
- Or, we can pick 1 color and 2 isospins — there are three ways to do that.

So, in terms of binomial coefficients, we have

$$\begin{aligned}\binom{5}{3} &= \binom{3}{3}\binom{2}{0} + \binom{3}{2}\binom{2}{1} + \binom{3}{1}\binom{2}{2} \\ &= 1 + 6 + 3 \\ &= 10\end{aligned}$$

In terms of vector spaces we have:

$$\wedge^3(\mathbb{C}^5) \cong \wedge^3(\mathbb{C}^3) \otimes \wedge^0(\mathbb{C}^2) \oplus \wedge^2(\mathbb{C}^3) \otimes \wedge^1(\mathbb{C}^2) \oplus \wedge^1(\mathbb{C}^3) \otimes \wedge^2(\mathbb{C}^2)$$

Taking dimensions of these vector spaces, we get $10 = 1 + 6 + 3$. Finally, in terms of the $\mathrm{SU}(5)$ grand unified theory, we get this:

$$\wedge^3(\mathbb{C}^5) = \langle right-handedelectron\rangle \oplus \langle right-handedupantiquark, right-handeddownantiquark\rangle \oplus \langle right-handedu$$

If we play this game for all the spaces $\wedge^p(\mathbb{C}^5)$, here's what we get:

- $\wedge^0(\mathbb{C}^5) \cong \wedge^0(\mathbb{C}^3) \otimes \wedge^0(\mathbb{C}^2)$
- $\wedge^1(\mathbb{C}^5) \cong \wedge^1(\mathbb{C}^3) \otimes \wedge^0(\mathbb{C}^2) \oplus \wedge^0(\mathbb{C}^3) \otimes \wedge^1(\mathbb{C}^2)$
- $\wedge^2(\mathbb{C}^5) \cong \wedge^2(\mathbb{C}^3) \otimes \wedge^0(\mathbb{C}^2) \oplus \wedge^1(\mathbb{C}^3) \otimes \wedge^1(\mathbb{C}^2) \oplus \wedge^0(\mathbb{C}^3) \otimes \wedge^2(\mathbb{C}^2)$
- $\wedge^3(\mathbb{C}^5) \cong \wedge^3(\mathbb{C}^3) \otimes \wedge^0(\mathbb{C}^2) \oplus \wedge^2(\mathbb{C}^3) \otimes \wedge^1(\mathbb{C}^2) \oplus \wedge^1(\mathbb{C}^3) \otimes \wedge^2(\mathbb{C}^2)$
- $\wedge 4(\mathbb{C}^5) \cong \wedge^3(\mathbb{C}^3) \otimes \wedge^1(\mathbb{C}^2) \oplus \wedge^2(\mathbb{C}^2) \otimes \wedge^2(\mathbb{C}^2)$
- $\wedge 5(\mathbb{C}^5) \cong \wedge^3(\mathbb{C}^3) \otimes \wedge^2(\mathbb{C}^2)$

If we interpret this in terms of physics, we get back our previous chart:

- $\wedge^0(\mathbb{C}^5) \cong \langle$left-handed antineutrino$\rangle$
- $\wedge^1(\mathbb{C}^5) \cong \langle$right-handed down quark$\rangle \oplus \langle$right-handed positron, right-handed antineutrino$\rangle$

- $\wedge^2(\mathbb{C}^5) \cong \langle$left-handed up antiquark$\rangle \oplus \langle$left-handed up quark, left-handed down quark$\rangle \oplus \langle$left-handed positron$\rangle$
- $\wedge^3(\mathbb{C}^5) \cong \langle$right-handed electron$\rangle \oplus \langle$right-handed up antiquark, right-handed down antiquark$\rangle \oplus \langle$right-handed up quark$\rangle$
- $\wedge^4(\mathbb{C}^5) \cong \langle$left-handed up antiquark$\rangle \oplus \langle$left-handed electron, left-handed neutrino$\rangle$
- $\wedge^5(\mathbb{C}^5) \cong \langle$right-handed neutrino$\rangle$

Now, all this is really cool — but in fact, even before inventing the $\mathrm{SU}(5)$ theory, Georgi went a bit further, and unified all the left-handed fermions above into one irreducible representation of a somewhat bigger group: $\mathrm{Spin}(10)$. This is the double cover of the group $\mathrm{SO}(10)$, which describes rotations in 10 dimensions.

If you look at the chart above, you'll see the left-handed fermions live in the even grades of the exterior algebra of $\mathbb{C}^5$:

$$\wedge^{\mathrm{even}}(\mathbb{C}^5) = \wedge^0(\mathbb{C}^5) \oplus \wedge^2(\mathbb{C}^5) \oplus \wedge^4(\mathbb{C}^5)$$

This big space forms something called the left-handed Weyl spinor representation of $\mathrm{Spin}(10)$. It's an irreducible representation.

Similarly, the right-handed fermions live in the odd grades:

$$\wedge^{\mathrm{odd}}(\mathbb{C}^5) = \wedge^1(\mathbb{C}^5) \oplus \wedge^3(\mathbb{C}^5) \oplus \wedge^5(\mathbb{C}^5)$$

and this big space forms the right-handed Weyl spinor representation of $\mathrm{Spin}(10)$. It's also irreducible.

I can't resist mentioning that there's also a gadget called the Hodge star operator that maps $\wedge^{\mathrm{even}}(\mathbb{C}^5)$ to $\wedge^{\mathrm{odd}}(\mathbb{C}^5)$, and vice versa. In terms of physics, this sends left-handed particles into their right-handed antiparticles, and vice versa!

But if I get into digressions like these, it'll take forever to tackle the main question: how does this "Weyl spinor" stuff work?

It takes advantage of some very nice general facts. First, $\mathbb{C}^n$ is just another name for $\mathbb{R}^{2n}$ equipped with the structure of a complex vector space. This makes $\mathrm{SU}(n)$ into a subgroup of $\mathrm{SO}(2n)$. So, it makes the Lie algebra $\mathfrak{su}(n)$ into a Lie subalgebra of $\mathfrak{so}(2n)$.

The group $\mathrm{SU}(n)$ acts on the exterior algebra $\wedge(\mathbb{C}^n)$. So, its Lie algebra $\mathfrak{su}(n)$ also acts on this space. The really cool part is that this action extends to all of $\mathfrak{so}(2n)$. This is something you learn about when you study Clifford algebras, spinors and the like. I don't know how to explain it without writing down some formulas. So, for now, please take my word for it!

This business doesn't give a representation of $\mathrm{SO}(2n)$ on $\wedge(\mathbb{C}^n)$, but it gives a representation of the double cover, $\mathrm{Spin}(2n)$. This is called the "Dirac spinor" representation. It breaks up into two irreducible parts:

$$\wedge(\mathbb{C}^n) = \wedge^{\mathrm{even}}(\mathbb{C}^n) \oplus \wedge^{\mathrm{odd}}(\mathbb{C}^n)$$

and these are called the left- and right-handed "Weyl spinor" representations.

Perhaps it's time for an executive summary of what I've said so far:

> *The Dirac spinor representation of* $\mathrm{Spin}(10)$ *neatly encodes everything about how one generation of fermions interacts with the gauge bosons in the Standard Model, as long as we remember how* $\mathrm{S}(\mathrm{U}(2) \times \mathrm{U}(3))$ *sits inside* $\mathrm{SO}(10)$*, which is double covered by* $\mathrm{Spin}(10)$.

Of course, there's more to the Standard Model than this. There's also the Higgs boson, which spontaneously breaks electroweak symmetry and gives the fermions their masses. And, if we want to use this same trick to break the symmetry from $\mathrm{Spin}(10)$ down to $\mathrm{S}(\mathrm{U}(3) \times \mathrm{U}(2))$, we need to introduce *more* Higgs bosons. This is the ugly part of the story, it seems. Since I'm on vacation, I'll avoid it for now.

Next: how might exceptional Lie groups get involved in this game?

When Cartan classified compact simple Lie groups, he found 3 infinite families related to rotations in real, complex and quaternionic vector spaces: the $\mathrm{SO}(n)$'s, $\mathrm{SU}(n)$'s and $\mathrm{Sp}(n)$'s. He also found 5 exceptions, which have the charming names G_2, F_4, E_6, E_7, and E_8. These are all related to the octonions. G_2 is just the automorphism group of the octonions. The other 4 are closely related to each other — thanks to the "magic square" of Rosenfeld, Freudenthal and Tits.

I talked about the magic square a bit in "Week 106" and "Week 145", and much more here:

3) John Baez, "The magic square", `http://math.ucr.edu/home/baez/octonions/node16.html`

Instead of repeating all that, let me just summarize. The magic square gives vector space isomorphisms as follows:

$$\begin{aligned}
\mathfrak{f}_4 &\cong \mathfrak{so}(\mathbb{R} \oplus \mathbb{O}) \oplus (\mathbb{R} \otimes \mathbb{O})^2 \\
\mathfrak{e}_6 &\cong \mathfrak{so}(\mathbb{C} \oplus \mathbb{O}) \oplus (\mathbb{C} \otimes \mathbb{O})^2 \oplus \Im(\mathbb{C}) \\
\mathfrak{e}_7 &\cong \mathfrak{so}(\mathbb{H} \oplus \mathbb{O}) \oplus (\mathbb{H} \otimes \mathbb{O})^2 \oplus \Im(\mathbb{H}) \\
\mathfrak{e}_8 &\cong \mathfrak{so}(\mathbb{O} \oplus \mathbb{O}) \oplus (\mathbb{O} \otimes \mathbb{O})^2
\end{aligned}$$

Here $\mathfrak{f}_4$, $\mathfrak{e}_6$, $\mathfrak{e}_7$ and $\mathfrak{e}_8$ stand for the Lie algebras of the compact real forms of these exceptional Lie groups. $\mathbb{R}$, $\mathbb{C}$, $\mathbb{H}$, and $\mathbb{O}$ are the usual suspects — the real numbers, complex numbers, quaternions and octonions. For any real inner product space V, $\mathfrak{so}(V)$ stands for the Lie algebra of the rotation group of V. And, for each of the isomorphisms above, we must equip the vector space on the right side with a cleverly (but not perversely!) defined Lie bracket to get the Lie algebra on the left side.

Here's another way to say the same thing, which may ring more bells:

$$\begin{aligned}
\mathfrak{f}_4 &\cong \mathfrak{so}(9) \oplus S_9 \\
\mathfrak{e}_6 &\cong \mathfrak{so}(10) \oplus S_{10} \oplus \mathfrak{u}(1) \\
\mathfrak{e}_7 &\cong \mathfrak{so}(12) \oplus S_{12}^+ \oplus \mathfrak{su}(2) \\
\mathfrak{e}_8 &\cong \mathfrak{so}(16) \oplus S_{16}^+
\end{aligned}$$

Here S_9 and S_{10} are the unique irreducible real spinor representations of $\mathfrak{so}(9)$ and $\mathfrak{so}(10)$, respectively. In the other two cases, the little plus signs mean that there are two choices of irreducible real spinor representation, and we're taking the left-handed choice.

All this must seem like black magic of the foulest sort if you haven't wasted months thinking about the octonions and exceptional groups! Be grateful: I did it so you wouldn't have to.

Anyway: the case of E_6 should remind you of something! After all, we've just been talking about $\mathfrak{so}(10)$ and its left-handed spinor representation. These describe the gauge bosons and one generation of left-handed fermions in the $\mathrm{Spin}(10)$ grand unified theory. But now we're seeing this stuff neatly packed into the Lie algebra of E_6!

More precisely, the Lie algebra of E_6 can be chopped into 3 pieces in a noncanonical way:

- $\mathfrak{so}(10)$
- the unique irreducible real spinor representation of $\mathfrak{so}(10)$, which by now we've given three different names:

$$S_{10} \cong \wedge^{\mathrm{even}}(\mathbb{C}^5) \cong (\mathbb{C} \otimes \mathbb{O})^2$$

- $\mathfrak{u}(1)$

The first part contains all the gauge bosons in the $\mathrm{SO}(10)$ grand unified theory. The second contains one generation of left-handed fermions. But what about the third?

Well, S_{10} is defined to be a real representation of $\mathfrak{so}(10)$. But, it just so happens that the action of $\mathfrak{so}(10)$ preserves a complex structure on this space. This is just the obvious complex structure on $(\mathbb{C} \otimes \mathbb{O})^2$, or if you prefer, $\wedge^{\mathrm{even}}(\mathbb{C}^5)$. So, there's an action of the unit complex numbers, $\mathrm{U}(1)$, on S_{10} which commutes with the action of $\mathfrak{so}(10)$. Differentiating this, we get an action of the Lie algebra $\mathfrak{u}(1)$:

$$\mathfrak{u}(1) \otimes S_{10} \to S_{10}$$

And this map gives part of the cleverly defined Lie bracket operation in

$$\mathfrak{e}_6 \cong \mathfrak{so}(10) \oplus S_{10} \oplus \mathfrak{u}(1)$$

All this stuff is mysterious, but suggestive. It could be mere coincidence, or it could be the tip of an iceberg. It's more fun to assume the latter. So, let me say some more about it. . . .

The copy of $\mathfrak{u}(1)$ in here:

$$\mathrm{E}_6 \cong \mathfrak{so}(10) \oplus S_{10} \oplus \mathfrak{u}(1)$$

is pretty amusing from a physics viewpoint. It's if besides the gauge bosons in $\mathfrak{so}(10)$, there were one extra gauge boson whose sole role is to describe the fact that the fermions form a *complex* representation of $\mathfrak{so}(10)$. This is funny, since one of the naive ideas you sometimes hear is that you can take the obvious $\mathrm{U}(1)$ symmetry every complex Hilbert space has and "gauge" it to get electromagnetism.

That's not really the right way to understand electromagnetism! There are lots of different irreducible representations of $\mathrm{U}(1)$, corresponding to different charges, and in physics we should think about *all* of these, not just the obvious one that we automatically get from any complex Hilbert space. If we only used the obvious one, all particles would have charge 1.

But in the $\mathrm{Spin}(10)$ grand unified theory, the electromagnetic $\mathfrak{u}(1)$ Lie algebra is sitting inside $\mathfrak{so}(10)$; it's not the $\mathfrak{u}(1)$ you see above. The $\mathfrak{u}(1)$ you see above is the "obvious" one that the spinor representation S_{10} gets merely from being a complex Hilbert space.

The splitting

$$\mathfrak{e}_6 = \mathfrak{so}(10) \oplus S_{10} \oplus \mathfrak{u}(1)$$

also hints at a weird unification of bosons and fermions, something different from supersymmetry. We're seeing $\mathfrak{e}_6$ as a $\mathbb{Z}/2$-graded Lie algebra with $\mathfrak{so}(10) \oplus \mathfrak{u}(1)$ as its "bosonic" part and S_{10} as its "fermionic" part. But, this is not a Lie superalgebra, just an ordinary Lie algebra with a $\mathbb{Z}/2$ grading!

Furthermore, an ordinary Lie algebra with a $\mathbb{Z}/2$ grading is precisely what we need to build a "symmetric space". This is really cool, since it explains what I meant by saying that the split of $\mathfrak{e}_6$ into bosonic and fermionic parts is "noncanonical". We'll get a space, and each point in this space will give a different way of splitting $\mathfrak{e}_6$ as

$$\mathfrak{e}_6 = \mathfrak{so}(10) \oplus S_{10} \oplus \mathfrak{u}(1)$$

It's also cool because it gives me an excuse to talk about symmetric spaces... a topic that deserves a whole week of its own!

Symmetric spaces are the epitome of symmetry. A "homogeneous space" is a manifold with enough symmetry that any point looks like any other. A symmetric space is a homogeneous space with an extra property: the view from any point in any direction is the same as the view in the opposite direction!

Euclidean spaces and spheres are the most famous examples of symmetric spaces. If an ant decides to set up residence on a sphere, any point is just as good any other. And, if sits anywhere and looks in any direction, the view is the same as the view in the opposite direction.

The symmetric space we get from the above $\mathbb{Z}/2$-graded Lie algebra is similar, but more exotic: it's the complexified version of the octonionic projective plane!

But let's start with the basics:

Suppose someone hands you a Lie algebra $\mathfrak{g}$ with a Lie subalgebra $\mathfrak{h}$. Then you can form the simply-connected Lie group G whose Lie algebra is $\mathfrak{g}$. Sitting inside G, there's a connected Lie group H whose Lie algebra is $\mathfrak{h}$. The space

$$G/H$$

is called a "homogeneous space". Such things are studied in Klein geometry, and I've been talking about them a lot lately.

But now, suppose $\mathfrak{g}$ is a $\mathbb{Z}/2$-graded Lie algebra. Its even part will be a Lie subalgebra; call this $\mathfrak{h}$. This gives a specially nice sort of homogeneous space G/H, called a "symmetric space". This is better than your average homogeneous space.

Why? Well, first of all, for each point p in G/H there's a map from G/H to itself called "reflection through p", which fixes the point p and acts as -1 on the tangent space of p. When our point p comes from the identity element of G, this reflection map corresponds to the $\mathbb{Z}/2$ grading of the Lie algebra, which fixes the even part and acts as -1 on the odd part.

This is what I meant by saying that in a symmetric space, "the view in any direction is the same as the view in the opposite direction".

Second, these reflection maps satisfy some nice equations. Write $p > q$ for the the result of reflecting q through p. Then we have:

$$p > (p > q) = q$$

$$p > p = p$$

and

$$p > (q > r) = (p > q) > (p > r)$$

A set with an operation satisfying these equations is called an "involutory quandle". Quandles are famous in knot theory. Now we're seeing them in another role.

Let me summarize with a few theorems — I hope they're all true, because I don't know a book containing all this stuff. We can define a "symmetric space" to be an involutory quandle that's a manifold, where the operation $>$ is smooth and the reflection map

$$x \mapsto p > x$$

has derivative -1 at p. Any $\mathbb{Z}/2$-graded Lie algebra gives a symmetric space. Conversely, any symmetric space has a universal cover that's a symmetric space coming from a $\mathbb{Z}/2$-graded Lie algebra!

Using this correspondence, the Lie algebra $\mathfrak{e}_6$ with the $\mathbb{Z}/2$-grading I described gives a symmetric space, roughly:

$$\mathrm{E}_6/(\mathrm{Spin}(10) \times \mathrm{U}(1))$$

But, this guy is a lot better than your average symmetric space!

For starters, it's a "Riemannian symmetric space". This is a symmetric space with a Riemannian metric that's preserved by all the operations of reflection through points.

Compact Riemannian symmetric spaces were classified by Cartan, and you can see the classification here, in a big chart:

4) "Riemannian symmetric spaces", Wikipedia, `http://en.wikipedia.org/wiki/Riemannian_symmetric_spac`

As you'll see, there are 7 infinite families and 12 exceptional cases. The symmetric space I'm talking about now, namely $\mathrm{E}_6/(\mathrm{Spin}(10) \times \mathrm{U}(1))$, is called EIII — it's the third exceptional case. And, as you can see from the chart in this article, it's the complexified version of the octonionic projective plane! For this reason, I sometimes call it

$$(\mathbb{C} \otimes \mathbb{O})\mathbb{P}^2$$

In fact, this space is better than your average Riemannian symmetric space. It's a Khler manifold, thanks to that copy of $\mathrm{U}(1)$, which makes each tangent space complex. Moreover, the Khler structure is preserved by all the operations of reflection through points. So, it's a "hermitian symmetric space".

You're probably drowning under all this terminology unless you already know this stuff. I guess it's time for another executive summary:

> *Each point in the complexified octonionic projective plane gives a different way of splitting the Lie algebra of* E_6 *into a bosonic part and a fermionic part. The fermionic part is just what we need to describe one generation of left-handed Standard Model fermions. The bosonic part is just what we need for the gauge bosons of the* $\mathrm{Spin}(10)$ *grand unified theory, together with a copy of* $\mathfrak{u}(1)$*, which describes the* complex structure *of the left-handed Standard Model fermions.*

Another nice fact is that $(\mathbb{C} \otimes \mathbb{O})\mathbb{P}^2$ is one of the Grassmannians for E_6. I explained this general notion of "Grassmannian" back in "Week 181", and you can see this 16-dimensional one in the list near the end of that Week.

Even better, if you geometrically quantize this Grassmannian using the smallest possible symplectic structure, you get the 27-dimensional representation of E_6 on the exceptional Jordan algebra!

So, there's a lot of seriously cool math going on here... but since the basic idea of relating the Standard model to E_6 is only half-baked, all the ideas I'm mentioning now are at best quarter-baked. They're mathematically correct, but I can't tell if they're leading somewhere interesting.

In fact, I would have kept them in the oven longer had not Garrett Lisi brought E_6's big brother E_8 into the game in a tantalizing way. I'll conclude by summarizing this... and you can look at his website for more details. But first, let me emphasize that this E_8 business is the most recent and most speculative thing Garrett has done. So, if you think the following idea is nuts, please don't jump to conclusions and decide *everything* he's doing is nuts!

Briefly, his idea involves taking the description of $\mathfrak{e}_8$ I already mentioned:

$$\mathfrak{e}_8 = \mathfrak{so}(\mathbb{O} \oplus \mathbb{O}) \oplus (\mathbb{O} \otimes \mathbb{O})^2$$

and writing the linear transformations in $\mathfrak{so}(\mathbb{O} \oplus \mathbb{O})$ as two 8×8 blocks living in $\mathfrak{so}(\mathbb{O})$, together with an off-diagonal block living in $\mathbb{O} \otimes \mathbb{O}$. This gives

$$\mathfrak{e}_8 = \mathfrak{so}(\mathbb{O}) \oplus \mathfrak{so}(\mathbb{O}) \oplus (\mathbb{O} \otimes \mathbb{O})^3$$

Then, he wants to use each of the three copies of $\mathbb{O} \otimes \mathbb{O}$ to describe one of the three generations of fermions, while using the $\mathfrak{so}(\mathbb{O}) \oplus \mathfrak{so}(\mathbb{O})$ stuff to describe bosons (including gravity).

For this, he builds on some earlier work where he sought to combine gravity, the Standard Model gauge bosons, the Higgs and *one* generation of Standard Model fermions in an $\mathfrak{so}(7,1)$ version of MacDowell-Mansouri gravity.

If I were really being responsible, I would describe and assess this earlier work. But, it's summer and I just want to have fun....

In fact, the above alternate description of E_8 is the one Bertram Kostant told me about back in 1996. He said it a different way, which is equivalent:

$$\mathrm{E}_8 = \mathfrak{so}(8) \oplus \mathfrak{so}(8) \oplus \mathrm{End}(V_8) \oplus \mathrm{End}(S_8^+) \oplus \mathrm{End}(S_8^-)$$

Here V_8, S_8^+ and S_8^- are the vector, left-handed spinor, and right-handed spinor representations of $\mathrm{Spin}(8)$. All three are 8-dimensional, and all are related by outer automorphisms of $\mathrm{Spin}(8)$. That's what "triality" is all about. You can see more details in "Week 90".

The idea of relating the three generations to triality is cute. Of course, even if it worked, you'd need something to give the fermions in different generations different masses — which is what happens already in the Standard Model, thanks to the Higgs boson. It's the bane of all post-Standard Model physics: symmetry looks nice, but the more symmetry your model has, the more symmetries you need to explain away! As the White Knight said to Alice:

But I was thinking of a plan
To dye one's whiskers green,
And always use so large a fan
That they could not be seen.

Someday we may think of a way around this problem. But for now, I've got a more pressing worry. This splitting of E_6:

$$\mathrm{E}_6 = \mathfrak{so}(10) \oplus S^+_{10} \oplus \mathfrak{u}(1)$$

corresponds to a $\mathbb{Z}/2$-grading where $\mathfrak{so}(10) \oplus \mathfrak{u}(1)$ is the "bosonic" or "even" part and S^+_{10} is the "fermionic" or "odd" part. This nicely matches the way $\mathfrak{so}(10)$ describes gauge bosons and S^+_{10} describes fermions in Georgi's grand unified theory. But, this splitting of E_8:

$$\mathrm{E}_8 = \mathfrak{so}(8) \oplus \mathfrak{so}(8) \oplus \mathrm{End}(V_8) \oplus \mathrm{End}(S^+_8) \oplus \mathrm{End}(S^-_8)$$

does not correspond to any $\mathbb{Z}/2$-grading where $\mathfrak{so}(8) \oplus \mathfrak{so}(8)$ is the bosonic part and $\mathrm{End}(V) \oplus \mathrm{End}(S^+) \oplus \mathrm{End}(S^-)$ is the fermionic part. There is a closely related $\mathbb{Z}/2$-grading of E_8, but it's this:

$$\mathrm{E}_8 = \mathfrak{so}(16) \oplus S^+_{16}$$

So, right now I don't feel it's mathematically natural to use this method to combine bosons and fermions.

But, only time will tell.

Here are some more references. The $\mathrm{SU}(5)$ grand unified theory was published here:

5) Howard Georgi and Sheldon Glashow, "Unity of all elementary-particle forces", *Phys. Rev. Lett.* **32** (1974), 438.

For a great introduction to the $\mathrm{Spin}(10)$ grand unified theory — which is usually called the $\mathrm{SO}(10)$ GUT — try this:

6) Anthony Zee, *Quantum Field Theory in a Nutshell*, Chapter VII: "$\mathrm{SO}(10)$ unification", Princeton U. Press, Princeton, 2003.

Then, try these more advanced review articles:

7) Jogesh C. Pati, "Proton decay: a must for theory, a challenge for experiment", available as hep-ph/0005095.

8) Jogesh C. Pati, "Probing grand unification through neutrino oscillations, leptogenesis, and proton decay", available as hep-ph/0305221.

The last two also consider the gauge group "$G(224)$", meaning $\mathrm{SU}(2)\times\mathrm{SU}(2)\times\mathrm{SU}(4)$.

By the way, there's also a cute relation between the $\mathrm{SO}(10)$ grand unified theory and 10-dimensional Calabi-Yau manifolds, discussed here:

9) John Baez, "Calabi-Yau manifolds and the Standard Model", available as hep-th/0511086

This is an easy consequence of the stuff I've explained this week.

To see what string theorists are doing to understand the Standard Model these days, see the following papers. Amusingly, they *also* use E_8 — but in a quite different way:

10) Volker Braun, Yang-Hui He, Burt A. Ovrut and Tony Pantev, "A heterotic Standard Model", available as hep-th/0501070.

"A Standard Model from the $\mathrm{E}_8 \times \mathrm{E}_8$ heterotic superstring", hep-th/0502155.

"Vector bundle extensions, sheaf cohomology, and the heterotic Standard Model", available as hep-th/0505041.

"Heterotic Standard Model moduli", available as hep-th/0509051.

"The exact MSSM spectrum from string theory", available as hep-th/0512177.

All this stuff is really cool — but alas, they get the "minimal supersymmetric Standard Model", or MSSM, which has a lot more particles than the Standard Model, and a lot more undetermined parameters. Of course, these flaws could become advantages if the next big particle accelerator, the Large Hadron Collider, sees signs of supersymmetry.

For more on symmetric spaces, try these:

11) Sigurdur Helgason, *Differential Geometry, Lie Groups, and Symmetric Spaces*, AMS, Providence, Rhode Island, 2001.

12) Audrey Terras, *Harmonic Analysis on Symmetric Spaces and Applications I*, Springer, Berlin, 1985. *Harmonic Analysis on Symmetric Spaces and Applications II*, Springer, Berlin, 1988.

13) Arthur Besse, *Einstein Manifolds*, Springer, Berlin, 1986.

They're all classics. Helgason's book will teach you differential geometry and Lie groups before doing Cartan's classification of symmetric spaces. Terras' books are full of fun connections to other branches of math. Besse's book has lots of nice charts, and goes much deeper into the Riemannian geometry of symmetric spaces.

These dig deeper into the algebraic aspects of symmetric spaces:

14) W. Bertram, *The Geometry of Jordan and Lie structures*, Lecture Notes in Mathematics **1754**, Springer, Berlin, 2001.

15) Ottmar Loos, "Jordan triple systems, R-spaces and bounded symmetric domains", *Bull. AMS* **77** (1971), 558–561.

16) Ottmar Loos, *Symmetric Spaces I: General Theory*, W. A. Benjamin, New York, 1969. *Symmetric Spaces II: Compact Spaces and Classification*, W. A. Benjamin, New York, 1969.

Finally, an obnoxious little technical note. The complexification of the octonionic projective plane is not really $\mathrm{E}_6/(\mathrm{Spin}(10) \times \mathrm{U}(1))$; it's

$$\mathrm{E}_6/((\mathrm{Spin}(10) \times \mathrm{U}(1))/(\mathbb{Z}/4))$$

This is worked out here:

17) John Frank Adams, *Lectures on Exceptional Lie Groups*, eds. Zafer Mahmud and Mamoru Mimura, University of Chicago Press, Chicago, 1996.

Addendum: Joseph Hucks points out his paper describing the 13 different groups with Lie algebra $\mathfrak{su}(3) \oplus \mathfrak{su}(2) \oplus \mathfrak{u}(1)$, and their implications for physics:

18) Joseph Hucks, "Global structure of the standard model, anomalies, and charge quantization", *Phys. Rev. D* **43** (1991), 2709–2717.

Using $\mathrm{S}(\mathrm{U}(3) \times \mathrm{U}(2))$ and demanding anomaly cancellation, we automatically get a lot of the features of the Standard Model fermions.

Toby Bartels wisely points out that my basic examples of symmetric spaces — Euclidean spaces and sphere — are actually a bit misleading. I'd written:

> *Euclidean spaces and spheres are the most famous examples of symmetric spaces. If an ant decides to set up residence on a sphere, any point is just as good any other. And, if sits anywhere and looks in any direction, the view is the same as the view in the opposite direction.*

But in these particular examples, the view in any direction is the same as the view in *any other* direction! These spaces are more symmetrical than your average symmetric space: they're *isotropic*.

So, it's good to see some other examples, like a torus formed as the product of two circles of different radii. Any product of symmetric spaces is a symmetric space, so this is definitely a symmetric space. And, if you think about it, the ant's-eye view in any direction is just the same as the view in the opposite direction. But, this space is not isotropic: there are special directions, corresponding to "the short way around the torus" and "the long way around the torus".

The octonionic projective plane $\mathbb{OP}^2$ is not only a symmetric space: it's isotropic! But according to Tony Smith, the complexified version $(\mathbb{C} \otimes \mathbb{O})\mathbb{P}^2$ is not isotropic:

> *In* Spaces of Constant Curvature*, Fifth Edition (Publish or Perish 1984), Joseph A. Wolf says (pages 293–294):*
>
>> *. . . M is called isotropic at x if $I(M)_x$ is transitive on the unit sphere in M_x; it is isotropic if it is isotropic at every point. . . . M is isotropic if and only if it is two point homogeneous. . . . Let M be a riemannian symmetric space. Then the following conditions are equivalent. (i) M is two point homogeneous. (ii) Either M is a euclidean space or M is irreducible and of rank 1.*

> *Since* $(\mathbb{C}\otimes\mathbb{O})\mathbb{P}^2 = \mathrm{E}_6/(\mathrm{SO}(10)\times\mathrm{SO}(2))$ *is rank* 2*, it is NOT isotropic.*

In the quote by Wolf, I can only guess that $I(M)_x$ is the group of isometries of M that fix the point x, while M_x is the tangent space of M at x. Similarly, I guess that "two point homogeneous" means that for any $D \geqslant 0$, the isometry group of M acts transitively on the set of pairs of points in M whose distance from each other is D.

I also thank Tony for correcting some errors involving spinors. There's some quite subtle stuff going on here. For example, above it says that:

> *Well,* S_{10} *is defined to be a real representation of* $\mathfrak{so}(10)$*. But, it just so happens that the action of* $\mathfrak{so}(10)$ *preserves a complex structure on this space. This is just the obvious complex structure on* $(\mathbb{C}\otimes\mathbb{O})^2$*, or if you prefer,* $\wedge^{\mathrm{even}}(\mathbb{C}^5)$*. So, there's an action of the unit complex numbers,* $\mathrm{U}(1)$*, on* S_{10}^+ *which commutes with the action of* $\mathfrak{so}(10)$*.*

But in fact, $\mathfrak{so}(10)$ preserves *two* complex structures on S_{10}. This is how it always works: if some complex structure J is preserved by some group or Lie algebra action, so is $-J$. In the case at hand, one of these makes the representation of $\mathfrak{so}(10)$ into a complex representation isomorphic to its left-handed Weyl spinor representation on $\wedge^{\mathrm{even}}(\mathbb{C}^5)$. The other gives the right-handed Weyl spinor representation on $\wedge^{\mathrm{odd}}(\mathbb{C}^5)$. Neither one of these is "more correct" than the other. So, whenever I talk about $\mathfrak{e}_6$ as being related to one generation of left-handed Standard Model fermions, I could equally well say "right-handed Standard Model fermions". It just depends on which complex structure we choose!

Furthermore, when we complexify the real Lie algebra $\mathfrak{e}_6$, we get

$$\mathbb{C}\otimes\mathfrak{e}_6 \cong \mathfrak{so}(10,\mathbb{C}) \oplus \wedge(\mathbb{C}^5) \oplus \mathbb{C}$$

where $\wedge(\mathbb{C}^5)$ is the Dirac spinor representation of $\mathfrak{so}(10,\mathbb{C})$, describing both left- and right-handed fermions.

For more discussion, go to the n-Category Caf.

> *The Big Crunch* was her nickname for the mythical result that the Niah had aspired to reach: a unification of every field of mathematics that they considered significant.
>
> — *Greg Egan,* Glory

Week 254

July 13, 2007

This week I'd like to talk about exceptional Lie algebras and the Standard Model, Witten's new paper on the Monster group and black holes in 3d gravity, and Connes and Marcolli's new book! Then I want to continue the Tale of Groupoidification.

However, I don't have the energy to do this all now. And even if I did, you wouldn't have the energy to read it.

So, I'll just point you towards Connes and Marcolli's new book, which you can download for free:

1) Alain Connes and Mathilde Marcolli, *Noncommutative Geometry, Quantum Fields and Motives*, available at `http://www.alainconnes.org/downloads.html`

I hope to discuss it sometime, especially since it tackles a question I've been mulling lately: is there a good "explanation" for the Standard Model of particle physics?

For now, I'll start by discussing Witten's latest paper:

2) Edward Witten, "Three-dimensional gravity revisited", available as `arXiv:0706.3359`.

This is a bold piece of work, which seeks to relate the entropy of black holes in 3d quantum gravity to representations of the Monster group — the largest sporadic finite simple group, with about 10^{54} elements.

If the main idea is right, this gives a whole new view of "Monstrous Moonshine" — the bizarre connection between the Monster and fundamental concepts in complex analysis like the j-function. (See "Week 66" for a quick intro to Monstrous Moonshine.)

As the title hints, Witten had already tackled quantum gravity in 3 spacetime dimensions. In this earlier work, he argued it was an exactly soluble problem: a topological field theory called Chern-Simons theory. However, this theory is really an *extension* of gravity to the case of "degenerate" metrics: roughly speaking, geometries of spacetime where certain regions get squashed down to zero size. Degenerate metrics are weird. So, what happens if we try to quantize 3d gravity while insisting that the metric be nondegenerate?

It's hard to say. So, Witten takes a few clues and cleverly fits them together to make a surprising guess. He considers 3d general relativity with negative cosmological constant. This has 3d anti-DeSitter space as a solution. Anti-DeSitter space has a "boundary at infinity": a 2d cylinder with a conformal structure. The "AdS-CFT" idea, also known as "holography", suggests that in this sort of situation, 3d quantum gravity should be completely described by a field theory living on this boundary at infinity — a field theory theory with all conformal transformations as symmetries.

Which conformal field theory should correspond to 3d quantum gravity with negative cosmological constant? It depends on the value of the cosmological constant! Some topological arguments suggest that the Chern-Simons description of 3d quantum gravity is only gauge-invariant when the cosmological constant Λ takes certain special values, namely

$$\Lambda = -\frac{1}{16k^2}$$

where k is an integer, known as the "level" in Chern-Simons theory.

By the way: I'm working in Planck units here, and I'm assuming our Chern-Simons theory is left-right symmetric, just to keep things simple. I may also be making some small numerical errors.

This quantization of the cosmological constant must seem strange if you've never seen it before, but it's not really so weird. What's weird is that Witten is using Chern-Simons theory to determine the allowed values of the cosmological constant even though he wants to study what happens if gravity is *not* described by Chern-Simons theory!

Witten knows this is weird: later he says "we used the gauge theory approach to get some hints about the right values of the cosmological constant simply because it was the only tool available."

Indeed, the whole paper seems designed to refute the notion that mathematicians get less daring as they get older. He writes: "We make at each stage the most optimistic possible assumption." Perhaps he has some evidence for his guesses that he's not revealing yet. Or perhaps he's decided it takes courage verging on recklessness to track the Monster to its lair.

Anyway: next Witten relates the level k to something called the "central charge" of the conformal field theory living at the boundary at infinity.

What's the "central charge"? This is a standard concept in conformal field theory. Perhaps the simplest explanation is that in a conformal field theory, the total energy of the vacuum state is $-c/24$, where c is the central charge. So, naively you'd expect $c = 0$, but quantum effects make nonzero values of the vacuum energy possible, and even typical. A closely related cool fact is that the partition function of a conformal field theory is only a well-defined number up to multiples of

$$\exp\left(\frac{2\pi ic}{24}\right)$$

This means the partition function is a well-defined number when c is a multiple of 24. This happens in certain especially nice conformal field theories which are said to have "holomorphic factorization".

The appearance of the magic number 24 here is the first taste of Monstrous Moonshine! For more on the importance of this number in string theory, see "Week 124", "Week 125" and "Week 126".

As you can see, there are lots of subtleties here, which I really don't want to get into, but feel guilty about glossing over.

Here's another. There are really *two* conformal field theories in this game: one that describes ripples of the gravitational field moving clockwise around the boundary at infinity, and another for ripples moving counterclockwise. Our simplifying assumption about left-right symmetry lets us describe these "right-movers" and "left-movers" with the same theory. So, both have the same central charge.

In this case, the relation between central charge and level is simple:

$$c = 24k$$

Next, Witten considers the situation where k takes its smallest interesting value: $k = 1$, so $c = 24$. It just so happens that $c = 24$ conformal field theories with holomorphic factorization have been classified, at least modulo a certain conjecture:

2) A. N. Schellekens, "Meromorphic $c = 24$ conformal field theories", *Comm. Math. Phys.* **153** (1993) 159–196. Also available as hep-th/9205072.

It's believed there are 71 of them. Which one could describe 3d quantum gravity?

Of these 71, *all but one* have gauge symmetries! Now, Witten is assuming 3d quantum gravity is *not* described by Chern-Simons theory, which is a gauge theory. So, he guesses that the one exceptional theory is the right one!

And this is a very famous conformal field theory. It's a theory of a bosonic string wiggling around in a 26-dimensional spacetime curled up in clever way with the help of a 24-dimensional lattice called the Leech lattice. This theory is famous because its symmetry group is the Monster group! It is, in fact, the simplest thing we know that has the Monster group as symmetries.

For more details, try these — in rough order of increasing thoroughness:

3) Terry Gannon, "Postcards from the edge, or Snapshots of the theory of generalised Moonshine", available as arXiv:math/0109067.

Terry Gannon, "Monstrous Moonshine: the first twenty-five years", available as arXiv:math/0402345.

4) Richard Borcherds, online papers, available at http://math.berkeley.edu/~reb/papers/

5) Igor Frenkel, James Lepowsky, Arne Meurman, *Vertex operator algebras and the Monster*, Academic Press, New York, 1988.

Now, if this monstrous conformal field theory turned out to be 3d quantum gravity in disguise — viewed from infinity, so to speak — it might someday give a much better understanding of Monstrous Moonshine. However, Witten gives no explanation as to *why* this theory should be 3d gravity, except for the indirect argument I just sketched. The precise relation between 3d quantum gravity and the bosonic string wiggling around in 26 dimensions remains obscure.

However, while Witten leaves this mysterious, he does offer a tantalizing extra tidbit of evidence that the relation is real!

The partition function of the monstrous conformal field theory I just mentioned is the j-function, or more precisely:

$$J(q) = q^{-1} + 196884q + 21493760q^2 + \ldots$$

As I mentioned, this function shows up naturally in complex analysis. More precisely, it parametrizes the moduli space of elliptic curves (see "Week 125"). But, its bizarre coefficients turn out to be dimensions of interesting representations of the Monster group. For example, the smallest nontrivial representation of the Monster has dimension 196883; adding the trivial representation, we get 196884. This was one of several strange clues leading to the discovery of Monstrous Moonshine.

What Witten does is assume that the monstrous conformal field theory describes 3d quantum gravity for $k = 1$, and then use properties of the j-function to compute the entropy of black holes!

I won't attempt to explain the calculation. Suffice it to say that the lightest possible black hole turns out to have 196883 quantum states - its space of states is the smallest nontrivial representation of the Monster group. So, its entropy is:

$$\ln(196883) \approx 12.19$$

On the other hand, Hawking's semiclassical calculation gives

$$4\pi \approx 12.57$$

The match is not perfect — but it doesn't need to be, since we expect quantum corrections to Hawking's formula for small black holes.

What's more impressive is that Witten can guess the entropy of the lightest possible black hole for other values of k — meaning, other values of the cosmological constant. The space of states of these black holes are always representations of the Monster group, so we get logarithms of weird-looking integers. For example, for $k = 4$ the entropy is

$$\ln(81026609426) \approx 25.12$$

while Hawking's formula gives

$$8\pi \approx 25.13$$

Much better! And, using a formula of Petersson and Rademacher for asymptotics of coefficients of the j-function, together with some facts about Hecke operators, he shows that as $k \to \infty$, the agreement becomes perfect!

In short, there are some fascinating hints of a relation between the Monster group and black hole entropies in 3d gravity, but the details of Witten's hoped-for "AdS-CFT correspondence" between 3d gravity and the monstrous conformal field theory remain obscure. Indeed, there are lots of problems with Witten's proposal:

6) Jacques Distler, "Witten on 2+1 gravity", `http://golem.ph.utexas.edu/~distler/blog/archives/001335.html`

But, time will tell. In fact, if history is any guide, we can expect to see armies of string theorists marching into this territory any day now. So, I'll just pose one question.

There's a well-known route from 2d rational conformal field theories (or "RCFTs") to 3d topological quantum field theories (or "TQFTs"), which passes through modular tensor categories. For example, an RCFT called the Wess-Zumino-Witten model gives the TQFT called Chern-Simons theory.

But, now Witten is saying "3d quantum gravity isn't Chern-Simons theory; instead, it's something related to the monstrous CFT".

So: is the monstrous conformal field theory known to be an RCFT? If so, what 3d TQFT does it give? Could this TQFT be the 3d quantum gravity theory Witten is seeking?

Even though Witten is now claiming 3d quantum gravity *can't be* a TQFT, I think this is an interesting question. At the very least, I'd like to know more about this "Monster TQFT" — if it exists.

Now let's move from 3d quantum gravity to real-world particle physics...

Last week I described some mathematical relations between the Standard Model of particle physics, the most famous grand unified theories, and some "exceptional" structures in mathematics: the exceptional Lie group E_6, the complexified octonionic projective plane, and the exceptional Jordan algebra.

This week I want to go a bit further, and talk about the work of Kac, Larsson and others on the exceptional Lie superalgebras $\mathrm{E}(3|6)$, $\mathrm{E}(3|8)$ and $\mathrm{E}(5|10)$.

As before, my goal is to point out some curious relations between the messy pack of particles we see in nature and the "exceptional" structures we find in mathematics. By this, I mean structures that show up when you classify algebraic gadgets, but don't fit into nice systematic infinite families. Right now the Monster is the king of all exceptional structures, the biggest of the 26 sporadic finite simple groups. But, there are lots of other such structures, and they all seem to be related.

As mentioned back in "Week 66", Edward Witten once suggested that the correct theory of our universe could be an exceptional structure of some sort. There's even a fun hand-wavy argument for this idea. It goes like this: the theory of our universe *must* be incredibly special, since out of all the theories we can write down, only one describes the universe that actually exists!

In particular, lots of very simple theories do *not* describe our universe. So there must be some principle besides simplicity that picks out the theory of our universe.

Unfortunately, when we try to think about these issues seriously, we're quickly led into very deep waters. In practice, people quickly muddy these waters and create a quagmire. It's very hard to discuss this stuff without uttering nonsense. If you want to see my try, look at "Week 146".

But right now, I prefer to act like a sober, serious mathematical physicist. So, I'll tell you a bit about exceptional Lie superalgebras and how they could be related to the Standard Model.

First, some history. In 1887, Wilhelm Killing sent a letter to Friedrich Engel saying he'd classified the simple Lie algebras. Besides the "classical" ones — namely the infinite series $\mathfrak{sl}(n, \mathbb{C})$, $\mathfrak{so}(n, \mathbb{C})$ and $\mathfrak{sp}(n, \mathbb{C})$ — he found 6 exceptions: a 14-dimensional one, two 52-dimensional ones, a 78-dimensional one, a 133-dimensional one and a 248-dimensional one.

In 1894, Eli Cartan finished a doctoral thesis in which he cleaned up Engel's work. In the process, he noticed that Engel's two 52-dimensional Lie algebras were actually the same. Whoops!

So, we now have just 5 "exceptional" simple Lie algebras. In order of increasing size, they're called $\mathfrak{g}_2$, $\mathfrak{f}_4$, $\mathfrak{e}_6$, $\mathfrak{e}_7$ and $\mathfrak{e}_8$.

In 1914, Cartan realized that the smallest exceptional Lie algebra $\mathfrak{g}_2$, comes from the symmetry group of the octonions! Later it was realized that all 5 are connected to the octonions. I've written a lot about this in previous Weeks, but most of that material can be found here:

6) John Baez, "Exceptional Lie algebras", `http://math.ucr.edu/home/baez/octonions/node13.html`

Now, whenever mathematicians do something fun, they want to keep doing it, which means *generalizing* it.

One way to generalize Cartan's work is to study "symmetric spaces", which I defined last week. Briefly, a symmetric space is a manifold equipped with a geometrical structure that's very symmetrical: so much so that every point is just like every other, and the view in any direction is the same as the view in the opposite direction.

In fact, it was Cartan himself who invented the concept of symmetric space, and after he classified the simple Lie algebras he went ahead and classified these.

More precisely, I think he classified the "compact Riemannian" symmetric spaces. Every simple Lie algebra gives one of these, namely a compact simple group. But, there are others too. So, compact Riemannian symmetric spaces are a nice generalization of simple Lie algebras — and I believe Cartan succeeded in classifying them all.

Again, there are some infinite series, but also some exceptions coming from the octonions. I talked about one of these last week, namely EIII, the complexified octonionic projective plane. You can see a list here:

7) Wikipedia, "Riemannian symmetric space", `http://en.wikipedia.org/wiki/Riemannian_symmetric_space`

For a quick intro to the classification of simple Lie algebras and compact Riemannian symmetric spaces, try this great book:

8) Daniel Bump, *Lie Groups*, Springer, Berlin, 2004.

For a slower, more thorough introduction, try the book by Helgason mentioned last Week.

A second way to generalize Cartan's work is to consider simple Lie *superalgebras*.

Lie superalgebras are just like Lie algebras, except they're split into an "even" or bosonic and "odd" or fermionic part. The idea is that we stick minus signs in the usual Lie algebra formulas whenever we switch two "odd" elements.

This is very natural from a physics viewpoint, since whenever you switch two identical fermions, the wavefunction of the universe gets multiplied by -1. (Take my word for it — I've seen it happen!)

It's also very natural from a math viewpoint, since "super vector spaces" form a symmetric monoidal category with almost all the nice properties of plain old vector spaces. This lets crazed mathematicians and physicists systematically generalize pretty much all of linear algebra to the "super" world. So, why not Lie algebras?

The simple Lie superalgebras were classified by Victor Kac in 1977:

9) Victor Kac, "Lie superalgebras", *Adv. Math.* **26** (1977), 8–96.

Not counting the ordinary simple Lie algebras, there are 8 series of simple Lie superalgebras and a few exceptional ones. At least some of these exceptions come from the octonions:

10) Anthony Sudbery, "Octonionic description of exceptional Lie superalgebras", *Jour. Math. Phys.* **24** (1983), 1986–1988.

Do they all? I don't know! Someone please tell me!

A third way to generalize Cartan's work is to classify *infinite-dimensional* simple Lie algebras — or for that matter, Lie superalgebras.

So far I've implicitly assumed all our algebraic gadgets are finite-dimensional, but we can lift that restriction. If you try to classify infinite-dimensional gadgets without *any* restrictions, it can get really hairy. It turns out the nice thing is to classify "linearly compact" infinite-dimensional simple Lie algebras. I won't define the quoted phrase, since it's technical and it's explained near the beginning of this paper:

11) Victor Kac, "Classification of infinite-dimensional simple linearly compact Lie superalgebras", Erwin Schrdinger Institut preprint, 1998. Available at `http://www.esi.ac.at/Preprint-shadows/esi605.html`

Anyway, back in 1880 Lie himself made a guess about infinite-dimensional Lie algebras that would solve the problem I'm talking about now, though he didn't phrase it in the modern way. And, Cartan proved Lie's guess in 1909! Actually, there was a hole in Cartan's proof, which was only noticed much later. It was filled by Guillemin, Quillen and Sternberg in 1966.

So, here's the answer: there are 4 families of linearly compact infinite-dimensional simple Lie algebras, and no exceptions. Ignoring an important nuance I'll explain later, these are:

- The Lie algebra of all complex vector fields on $\mathbb{C}^n$.
- The Lie algebra of all complex vector fields v on $\mathbb{C}^n$ that are "divergence-free":

  $$\mathrm{div}\, v = 0$$

- The Lie algebra of all complex vector fields v on $\mathbb{C}^{2n}$ that are "symplectic":

  $$L_v \omega = 0$$

  where ω is the usual symplectic structure on $\mathbb{C}^{2n}$, and L means "Lie derivative"

- The Lie algebra of all complex vector fields v on $\mathbb{C}^{2n+1}$ that are "contact":

  $$L_v \alpha = f\alpha$$

  for some function f depending on v, where α is the usual contact structure on $\mathbb{C}^{2n+1}$.

If you don't know about symplectic structures or contact structures, don't worry — we won't need them now. The main point is that they're differential forms that show up throughout classical mechanics. So, this classification theorem is surprisingly nice.

Notice: no exceptions! That's a kind of exception in its own right.

In 1998, Victor Kac proved the "super" version of this result. In other words, he classified linearly compact infinite-dimensional Lie superalgebras! This result is Theorem 6.3 of his paper above. There turn out to be 10 families and 6 exceptions, which are called $\mathrm{E}(1|6)$, $\mathrm{E}(2|2)$, $\mathrm{E}(3|6)$, $\mathrm{E}(3|8)$, $\mathrm{E}(4|4)$ and $\mathrm{E}(5|10)$.

Many of the families are straightforward "super" generalizations of the 4 families I just showed you. Some are stranger. Most important for us today are the exceptions discovered by Irina Shchepochkina in 1983:

12) Irena Shchepochkina, "New exceptional simple Lie superalgebras", *C. R. Bul. Sci.* **36** (1983), 313–314.

The easiest to explain is $\mathrm{E}(5|10)$. And, you'll soon see that the number 5 here is related to the math of the $\mathrm{SU}(5)$ grand unified theory, which I explained last Week!

The even part of $\mathrm{E}(5|10)$ is the Lie algebra of divergence-free complex vector fields on $\mathbb{C}^5$.

The odd part of $\mathrm{E}(5|10)$ consists of closed complex 2-forms on $\mathbb{C}^5$.

The bracket of two even guys is the usual Lie bracket of vector fields.

The bracket of an even guy and an odd guy is the usual "Lie derivative" of a differential form with respect to a vector field.

The only tricky bit is the bracket of two odd guys! So, suppose μ and ν are closed complex 2-forms on $\mathbb{C}^5$. Their wedge product is a 4-form $\mu \wedge \nu$. But, we can identify this with a vector field v by demanding:

$$i_v \mathrm{vol} = \mu \wedge \nu$$

Here vol is the volume form:

$$\mathrm{vol} = dx_1 \wedge dx_2 \wedge dx_3 \wedge dx_4 \wedge dx_5$$

and $i_v\mathrm{vol}$ is the "interior product", which feeds v into vol and leaves us with a 4-form. You can check that this vector field v is divergence-free. So, we define the bracket of μ and ν to be v.

$\mathfrak{sl}(5, \mathbb{C})$ sits inside the even part of $\mathrm{E}(5|10)$ in a nice way, as the divergence-free vector fields whose coefficients are *linear* functions on $\mathbb{C}^5$. So, since $\mathfrak{su}(5)$ sits inside $\mathfrak{sl}(5, \mathbb{C})$, we get a tempting relation to $\mathrm{SU}(5)$.

(Now I'll come clean now and explain the "important nuance" I ignored earlier. For the classification theorems I mentioned earlier, we must use vector fields and differential forms with *formal power series* as coefficients. But for the purposes of mathematical physics, we should keep a more flexible attitude.)

Next, what about $\mathrm{E}(3|6)$? This is contained in $\mathrm{E}(5|10)$. To define it, we give $\mathrm{E}(5|10)$ a clever grading where x_1, x_2, x_3 are treated differently from the other two variables. Then we take the subalgebra of degree-zero guys. The details are explained in the above papers — or more simply, here:

13) Victor Kac, "Classification of infinite-dimensional simple groups of supersymmetries and quantum field theory", available as math.QA/9912235.

All this is reminiscent of how $\mathrm{SU}(5)$ contains the gauge group of the Standard Model, namely $\mathrm{S}(\mathrm{U}(3) \times \mathrm{U}(2))$. In particular, the even part of $\mathrm{E}(3|6)$ contains the Lie algebra

$$\mathfrak{sl}(3, \mathbb{C}) \oplus \mathfrak{sl}(2, \mathbb{C}) \oplus \mathfrak{gl}(1, \mathbb{C})$$

in a canonical way. So, any representation of $\mathrm{E}(3|6)$ automatically gives a representation of the Standard Model Lie algebra

$$\mathfrak{su}(3) \oplus \mathfrak{su}(2) \oplus \mathfrak{u}(1)$$

And in the above paper Kac goes even further! He defines a fairly natural class of representations of $\mathrm{E}(3|6)$, and proves something remarkable: these restrict to representations of

$$\mathfrak{su}(3) \oplus \mathfrak{su}(2) \oplus \mathfrak{u}(1)$$

that correspond precisely to the gluon, the photon and the W and Z bosons, and the quarks and leptons in one generation. . .

. . . together with one other particle, which is *not* the Higgs boson, but instead acts like a gluon with electric charge ± 1.

Darn.

One nice thing is how these Lie superalgebras get both bosons and fermions into the game in a natural way without forcing the existence of a bunch of unseen "superpartners". One unfortunate thing is that the above result gives no hint as to why there should be three generations of quarks and leptons. However, Kac and Rudakov develop some mathematics to address that question here:

14) Victor Kac and Alexi Rudakov, "Representations of the exceptional Lie superalgebra $\mathrm{E}(3,6)$: I. Degeneracy conditions". Available as math-ph/0012049.

"Representations of the exceptional Lie superalgebra $\mathrm{E}(3,6)$: II. Four series of degenerate modules". Available as math-ph/0012050.

"Representations of the exceptional Lie superalgebra $\mathrm{E}(3,6)$ III: Classification of singular vectors". Available as math-ph/0310045.

Their results are summarize at the end of this review article:

15) Victor Kac, "Classification of supersymmetries", *Proceedings of the ICM, Beijing, 2002*, vol. **1**, 319–344. Available as math-ph/0302016

Here Kac writes that "three generations of leptons occur in the stable region [whatever that means], but the situation with quarks is more complicated: this model predicts a complete fourth generation of quarks and an incomplete fifth generation (with missing down type triplets)."

So, while I don't understand "this model", it seems tantalizingly close to capturing the algebraic patterns in the Standard Model. . . without quite doing so.

Some more nice explanations and references can be found here:

16) Irina Shchepochkina, "The five exceptional simple Lie superalgebras of vector fields". Available as hep-th/9702121.

17) Pavel Grozman, Dimitry Leites and Irina Shchepochkina, "Defining relations for the exceptional Lie superalgebras of vector fields pertaining to The Standard Model", available as math-ph/0202025.

18) Pavel Grozman, Dimitry Leites and Irina Shchepochkina, "Invariant operators on supermanifolds and Standard Models", available as math.RT/0202193.

Thomas Larsson has been working on similar ideas, mainly using $\mathrm{E}(3|8)$ instead. This also contains

$$\mathfrak{sl}(3,\mathbb{C}) \oplus \mathfrak{sl}(2,\mathbb{C}) \oplus \mathfrak{gl}(1,\mathbb{C})$$

in a canonical way.

19) Thomas A. Larsson, "Symmetries of everything", available as math.RT/0202193.

"Exceptional Lie superalgebras, invariant morphisms, and a second-gauged Standard Model", available as math-ph/020202.

Thomas A. Larsson, "Maximal depth implies $\mathfrak{su}(3) + \mathfrak{su}(2) + \mathfrak{u}(1)$", available as hep-th/0208185.

Alas, $\mathrm{E}(3|8)$ gets the hypercharges of some fermions wrong. Larsson seems to say this problem also occurs for $\mathrm{E}(3|6)$, which would appear to contradict what Kac claims — but I could be misunderstanding.

I'll end with few questions. First, is there any relation between the exceptional Lie superalgebras $\mathrm{E}(5|10)$, $\mathrm{E}(3|6)$ or $\mathrm{E}(5|10)$ and the exceptional Lie algebra $\mathfrak{e}_6$? Last week I explained some relations between $\mathfrak{e}_6$ and the Standard Model; are those secretly connected to what I'm discussing this week?

Second, has anyone tried to unify all three generalizations of Cartan's classification of simple Lie algebras? Starting from simple Lie algebras, we've seen three ways to generalize:

- go to symmetric spaces,
- go the "super" version,
- go to the infinite-dimensional case.

So: has anyone tried to classify infinite-dimensional super versions of symmetric spaces? Or even finite-dimensional ones?

(Maybe the super version of a symmetric space should be called a "supersymmetric space", just for the sake of a nice pun.)

Next, the Tale of Groupoidification! I'll keep this week's episode short, since you're probably exhausted already.

I want to work my way to the concept of "Hecke operator" through a series of examples. The examples I'll use are a bit trickier than the concept I'm really interested in, since the examples involve integrals, where the Hecke operators I ultimately want to discuss involve sums. But, the examples are nice if you like to visualize stuff. . .

In these examples we'll always have a relation between two sets X and Y. We'll use this to get an operator that turns functions on X into functions on Y — a "Hecke operator".

- **The Radon transform in 2 dimensions**

  Suppose you're trying to do a CAT scan. You want to obtain a 3d image of someone's innards. Unfortunately, all you do is take lots of 2d X-ray photos of them. How can you assemble all this information into the picture you want?

  Who better to help you out than a guy named after a radioactive gas: Radon!

  In 1917, the Viennese mathematician Johann Radon tackled a related problem one dimension down. You could call it a "CAT scan for flatlanders".

Suppose you want to obtain a complete image of the insides of a 2-dimensional person, but all you can do is shine beams of X-rays through them and see how much each beam is attenuated.

So, mathematically: you have a real-valued function on the plane — roughly speaking, the density of your flatlander. You're trying to recover this function from its integrals along all possible lines. Someone hands you this function on the space of *lines*, and you're trying to figure out the original function on the space of *points*.

(Points lying on lines! If you've been following the Tale of Groupoidification, you'll know this "incidence relation" is connected to Klein's approach to geometry, and ultimately to spans of groupoids. But pretend you don't notice, yet.)

Now, it's premature to worry about this tricky "inverse problem" before we ponder what it's the inverse of: the "Radon transform". This takes our original function on the space of *points* and gives a function on the space of *lines*.

Let's call the Radon transform T. It takes a function f on the space of points and gives a function Tf on the space of lines, as follows. Given a line y, $(Tf)(y)$ is the integral of $f(x)$ over the set of all points x lying on y.

What Radon did is figure out a nice formula for the inverse of this transform. But that's not what I'm mainly interested in now. It's the Radon transform itself that's a kind of Hecke operator!

Next, look at another example.

- **The X-ray transform in n dimensions**

  This is an obvious generalization to higher dimensions of what I just described. Before we had a space

$$X = \{\text{points in the plane}\}$$

  and a space

$$Y = \{\text{lines in the plane}\}$$

  and an incidence relation

$$S = \{(x, y) \mid x \text{ is a point lying on the line } y\}$$

  If we go to n dimensions, we can replace all this with

$$\begin{aligned} X &= \{\text{points in } \mathbb{R}^n\} \\ Y &= \{\text{lines in } \mathbb{R}^n\} \\ S &= \{(x, y) \mid x \text{ is a point lying on the line } y\} \end{aligned}$$

  Again, the X-ray transform takes a function f on the space of points and gives a function Tf on the space of lines. Given a line y, $(Tf)(y)$ is the integral of $f(x)$ over the set of all x with (x, y) in S.

  Next, yet another example!

- **The Radon transform in n dimensions**

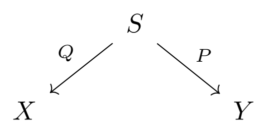
  Radon actually considered a different generalization of the 2d Radon transform, using hyperplanes instead of lines. Using hyperplanes is nicer, because it gives a very simple relationship between the Radon transform and the Fourier transform. But never mind — that's not the point here! The point is how similar everything is. Now we take:

$$X = \{\text{points in } \mathbb{R}^n\}$$
$$Y = \{\text{hyperplanes in } \mathbb{R}^n\}$$
$$S = \{(x, y) \mid x \text{ is a point lying on the hyperplane } y\}$$

And again, the Radon transform takes a function f on X and gives a function Tf on Y. Given y in Y, $(Tf)(y)$ is the integral of $f(x)$ over the set of all x with (x, y) in S.

We're always doing the same thing here. Now I'll describe the general pattern a bit more abstractly.

We've always got three spaces, and maps that look like this:



In our examples so far these maps are given by

$$P(x, y)8 = x$$
$$Q(x, y)8 = y$$

But, they don't need to be.

Now, how do we get a linear operator in this situation?

Easy! We start with a real-valued function on our space X:

$$f \colon X \to \mathbb{R}$$

Then we take f and "pull it back along P" to get a function on S. "Pulling back along P" is just impressive jargon for composing with P:

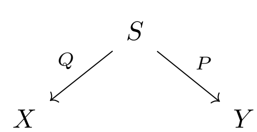
$$f \circ P \colon S \to \mathbb{R}$$

Next, we take $f \circ P$ and "push it forwards along Q" to get a function on Y. The result is our final answer, some function

$$Tf \colon Y \to \mathbb{R}$$

"Pushing forwards along Q" is just impressive jargon for integrating: $Tf(y)$ is the integral over all s in S with $Q(s) = y$. For this we need a suitable measure, and we need the integral to converge.

This is the basic idea: we define an operator T by pulling back and then pushing forward functions along a "span", meaning a diagram shaped like a bridge:



But, the reason this operator counts as a "Hecke operator" is that it gets along with a symmetry group G that's acting on everything in sight. In the examples so far, this is the group of Euclidean symmetries of $\mathbb{R}^n$. But, it could be anything.

This group G acts on all 3 spaces: X, Y, and S. This makes the space of functions on X into a representation of G! And, ditto for the space of function on Y and S.

Furthermore, the maps P and Q are "equivariant", meaning

$$P(gs) = gP(s)$$

and

$$Q(gs) = gQ(s)$$

This makes "pulling back along P" into an intertwining operator between representations of G. "Pushing forwards along Q" will also be an intertwining operator if the measure we use is G-invariant in a suitable sense. In this case, our transform T becomes an intertwining operator between group representations! Let's call an intertwining operator constructed this way a "Hecke operator".

If you're a nitpicky person, e.g. a mathematician, you may wonder what I mean by "a suitable sense". Well, each "fiber" $Q^{-1}(y)$ of the map

$$Q\colon S \to Y$$

needs a measure on it, so we can take a function on S and integrate it over these fibers to get a function on Y. We need this family of measures to be invariant under the action of G, for pushing forwards along Q be an intertwining operator.

This stuff about invariant families of measures is mildly annoying, and so is the analysis involved in making precise *which* class of functions on X we can pull back to S and then push forward to Y — we need to make sure the integrals converge, and so on. When I really get rolling on this Hecke operator business, I'll often focus on cases where X, Y, and S are *finite* sets... and then these issues go away!

Hmm. I'm getting tired, but I can't quit until I say one more thing. If you try to read about Hecke operators, you *won't* see anything about the examples I just mentioned! You're most likely to see examples where X and Y are spaces of lattices in the complex plane. This is the classic example, which we're trying to generalize. But, this example is more sophisticated than the ones I've mentioned, in that the "functions" on X and Y become "sections of vector bundles" over X and Y. The same sort of twist happens when we go from the Radon transform to the more general "Penrose transform".

Anyway, next time I'll talk about some really easy examples, where X, Y, and S are finite sets and G is a finite group. These give certain algebras of Hecke operators, called "Hecke algebras".

In the meantime, see if you can find *any* reference in the literature which admits that "Hecke algebras" are related to "Hecke operators". It ain't easy!

It's a great example of a mathematical cover-up — one we're gonna bust wide open.

Addendum: David Corfield notes that Helgason has a good textbook on the Radon transform which is *free online*. Snap it up while you can!

20) Sigurdur Helgason, *Radon Transform*, second edition, Birkhuser, New York, 1999. Also available at `http://www-math.mit.edu/~helgason/Radonbook.pdf`.

For more discussion, go to the n-Category Caf.

> "The Big Crunch" had always been a slightly mocking, irreverent term, but now she was struck anew by how little justice it did to the real trend that had fascinated the Niah. It was not a matter of everything in mathematics collapsing in on itself, with one branch turning out to have been merely a recapitulation of another under a different guise. Rather, the principle was that every sufficiently beautiful mathematical system was rich enough to mirror *in part* — and sometimes in a complex and distorted fashion — every other sufficiently beautiful system.
>
> — *Greg Egan,* Glory

Week 255

August 11, 2007

I've been roaming around Europe this summer — first Paris, then Delphi and Olympia, then Greenwich, then Oslo, and now back to Greenwich. I'm dying to tell you about the Abel Symposium in Oslo. There were lots of cool talks about topological quantum field theory, homotopy theory, and motivic cohomology.

I especially want to describe Jacob Lurie and Ulrike Tillman's talks on cobordism n-categories, Dennis Sullivan and Ralph Cohen's talks on string topology, Stephan Stolz's talk on cohomology and quantum field theory, and Fabien Morel's talk on A^1-homotopy theory. But this stuff is sort of technical, and I usually try to start each issue of This Week's Finds with something you don't need a PhD to enjoy.

So, here's a tour of the Paris Observatory:

1) John Baez, "Astronomical Paris", `http://golem.ph.utexas.edu/category/2007/07/astronomical_paris.html`

Back when England and France were battling to rule the world, each had a team of astronomers, physicists and mathematicians devoted to precise measurement of latitudes, longitudes, and times. The British team was centered at the Royal Observatory here in Greenwich. The French team was centered at the Paris Observatory, and it featured luminaries such as Cassini, Le Verrier and Laplace.

In "Week 175", written during an earlier visit to Greenwich, I mentioned a book on this battle:

2) Dava Sobel, *Longitude*, Fourth Estate Ltd., London, 1996.

It's a lot of fun, and I recommend it highly.

There's a lot more to say, though. The speed of light was first measured by Ole Romer at the Paris Observatory in 1676.

Later, Henri Poincar worked for the French Bureau of Longitude. Among other things, he was the scientific secretary for its mission to Ecuador.

To keep track of time precisely all over the world, you need to think about the finite speed of light. This may have spurred Poincar's work on relativity! Here's a good book that argues this case:

3) Peter Galison, *Einstein's Clocks, Poincar's Maps: Empires of Time*, W. W. Norton, New York, 2003. Reviewed by Robert Wald in Physics Today at `http://www.physicstoday.org/vol-57/iss-9/p57.html`

I met Galison in Delphi, and it's clear he like to think about the impact of practical stuff on math and physics.

I was in Delphi for a meeting of "Thales and Friends":

4) "Thales and Friends", `http://www.thalesandfriends.org`

This is an organization that's trying to bridge the gap between mathematics and the humanities. It's led by Apostolos Doxiadis, who is famous for this novel:

5) Apostolos Doxiadis, *Uncle Petros and Goldbach's Conjecture*, Bloomsbury, New York, 2000. Review by Keith Devlin at `http://www.maa.org/reviews/petros.html`

There's a lot I could say about this meeting, but I just want to advertise a forthcoming book by Doxiadis and a computer scientist friend of his. It's a comic book — sorry, I mean "graphic novel"! — about the history of mathematical logic from Russell to Goedel:

6) Apostolos Doxiadis and Christos Papadimitriou, *Logicomix*, to appear.

I saw a partially finished draft. I think it does a good job of explaining to nonmathematicians what the big deal was with mathematical logic around the turn of the last century. . . and how these ideas eventually led to computers. It's also a fun story.

If you're eager for summer reading and can't wait for Logicomix, you might try this other novel by Papadimitrou:

7) Christos Papadimitriou, *Turing (a Novel about Computation)*, MIT Press, Boston, 2003.

It's a history of mathematics from the viewpoint of computer science, as told by a computer program named Turing to a lovelorn archaeologist. I haven't seen it yet.

Okay — enough fun stuff. On to the Abel Symposium!

8) Abel Symposium 2007, `http://abelsymposium.no/2007`

Actually this was a lot of fun too. A bunch of bigshots were there, including a bunch who didn't even give talks, like Eric Friedlander, Ib Madsen, Jack Morava, and Graeme Segal.

(My apologies to all the bigshots I didn't list.)

Speaking of bigshots, Vladimir Voevodsky gave a special surprise lecture on symmetric powers of motives. He wowed the audience not only with his mathematical powers but also his ability to solve a technical problem that had stumped all the previous speakers! The blackboards in the lecture hall were controlled electronically, by a switch. But, the blackboards only moved a few inches before stalling out. So, people had to keep hitting the switch over and over. It was really annoying, and it became the subject of running jokes. People would ask the speakers: "Can't you talk and press buttons at the same time?"

So, what did Voevodsky do? He lifted the blackboard by hand! He laughed and said "Russian solution". But, I think it's a great example of how he gets around problems by creative new approaches.

It really pleased me how many talks mentioned n-categories, and even used them to do exciting things. This seems quite new. In the old days, bigshots might think about n-categories, but they'd be embarrassed to actually mention them, since they had a reputation for being "too abstract".

In fact, Dan Freed alluded to this in his talk on topological quantum field theory. He said that every mathematician has an "n-category number". Your n-category number is the largest n such that you can think about n-categories for a half hour without getting a splitting headache.

When Freed first invented this concept, he felt pretty self-satisfied, since his n-category number was 1, while for most mathematicians it was 0. But lately, he says, other people's n-category numbers have been increasing, while his has stayed the same.

He said this makes him suspicious. In light of the scandals plaguing the Tour de France and American baseball, he suspects mathematicians are taking "category-enhancing substances"!

Freed shouldn't feel bad: he was among the first to introduce n-categories in the subject of topological quantum field theory! He gave a nice talk on this, clear and unpretentious, leading up to a conjecture for the 3-vector space that Chern-Simons theory assigns to a point.

That would make a great followup to these papers on the 2-vector space that Chern-Simons theory assigns to a circle:

9) Daniel S. Freed, "The Verlinde algebra is twisted equivariant K-theory", available as arXiv:math/0101038.

   Daniel S. Freed, "Twisted K-theory and loop groups", available as arXiv:math/0206237.

   Daniel S. Freed, Michael J. Hopkins and Constantin Teleman, "Loop groups and twisted K-theory II", available as arXiv:math/0511232.

   Daniel S. Freed, Michael J. Hopkins and Constantin Teleman, "Twisted K-theory and loop group representations", available as arXiv:math/0312155.

In a similar vein, Jacob Lurie talked about his work with Mike Hopkins in which they proved a version of the "Baez-Dolan cobordism hypothesis" in dimensions 1 and 2. I'm calling it this because that's what Lurie called it in his title, and it makes me feel good.

You can read about this hypothesis here:

10) John Baez and James Dolan, "Higher-dimensional algebra and topological quantum field theory", *J. Math. Phys.* **36** (1995) 6073–6105. Also available as q-alg/9503002.

It was an attempt to completely describe the algebraic structure of the n-category nCob, where:

- objects are 0d manifolds,
- 1-morphisms are 1d manifolds with boundary,
- 2-morphisms are 2d manifolds with corners,
- 3-morphisms are 3d manifolds with corners,
- ...

and so on up to dimension n. Unfortunately, at the time we proposed it, little was known about n-categories above $n = 3$. For a more recent take on these ideas, see:

11) Eugenia Cheng and Nick Gurski, "Towards an n-category of cobordisms", *Theory and Applications of Categories* **18** (2007), 274–302. Available at `http://www.tac.mta.ca/tac/volumes/18/10/18-10abs.html`

Lurie and Hopkins use a new trick: they redefine $n\mathsf{Cob}$ to be a special sort of ∞-category. The idea is to use diffeomorphisms and homotopies between these as morphisms above dimension n. This gives an ∞-category version of $n\mathsf{Cob}$, where:

- objects are 0-dimensional manifolds,
- 1-morphisms are 1-dimensional manifolds with boundary,
- 2-morphisms are 2-dimensional manifolds with corners,
- 3-morphisms are 3-dimensional manifolds with corners,
- ...
- n-morphisms are n-dimensional manifolds with corners,
- $(n + 1)$-morphisms are diffeomorphisms,
- $(n + 2)$-morphisms are homotopies between diffeomorphisms,
- $(n + 3)$-morphisms are homotopies between homotopies,
- ...

and so on for ever!

Since everything here is invertible above dimension n, this is called an "(∞, n)-category".

This sounds worse than an n-category, but it's okay for small n. In particular, $(\infty, 1)$-categories are pretty well understood by now. There are a bunch of different approaches, with scary names like "topological categories", "simplicial categories", "A_∞ categories", "Segal categories", "complete Segal spaces", and "quasicategories". Luckily, all these approaches are known to be equivalent — see "Week 245" for some good introductory material by Julie Bergner and Andre Joyal. Joyal is now writing a book on this stuff.

Lurie is a real expert on $(\infty, 1)$-categories. In fact, starting as a grad student, he wrote a mammoth tome generalizing topos theory from categories to $(\infty, 1)$-categories:

12) Jacob Lurie, "Higher topos theory", available as arXiv:math/0608040.

I'm sure Freed would suspect him of taking category-enhancing substances: his category number is infinite, and this book is 619 pages long! Then he went on to apply this stuff to algebraic geometry... and the world is still reeling. I was happy to discover that he's a nice guy, enthusiastic and friendly — not the terrifying fiend I expected.

Anyway, Lurie and Hopkins have worked out the precise structure of the $(\infty, 1)$-category version of 1Cob, and also the $(\infty, 2)$-category version of 2Cob. Unfortunately this work is not yet written up. But, they use results from this paper:

13) Soren Galatius, Ib Madsen, Ulrike Tillmann, Michael Weiss, "The homotopy type of the cobordism category", available as arXiv:math/0605249.

And, Ulrike Tillmann gave a talk about this paper! It computes the "nerve" of the $(\infty, 1)$-category where:

- objects are $(n-1)$-dimensional manifolds,
- 1-morphisms are n-dimensional manifolds with boundary,
- 2-morphisms are diffeomorphisms,
- 3-morphisms are homotopies between diffeomorphisms,
- 4-morphisms are homotopies between homotopies,
- ...

The "nerve" is a trick for turning any sort of ∞-category into a space, or simplicial set. (See item J of "Week 117" for the nerve of a plain old category. This should give you the general idea.)

In her talk, she went further and computed the nerve of the (∞, k)-category where:

- objects are $(n-k)$-dimensional manifolds,
- 1-morphisms are $(n-k+1)$-dimensional manifolds with boundary,
- 2-morphisms are $(n-k+2)$-dimensional manifolds with corners,
- ...
- k-morphisms are n-dimensional manifolds with corners,
- $(k+1)$-morphisms are diffeomorphisms,
- $(k+2)$-morphisms are homotopies between diffeomorphisms,
- $(k+3)$-morphisms are homotopies between homotopies,
- ...

This is also joint work with the same coauthors, but it seems not to be written up yet, except for $k = 1$, where it's proved in the above paper. The cool thing about the new work is that it uses an idea familiar from higher category theory — a k-simplicial space — to give a rigorous description of the nerve of the above (∞, k)-category! Indeed, Tillmann told me she thinks of k-simplicial spaces as just a convenient way of dealing with higher categories.

Stephan Stolz's talk also involved cobordism n-categories, but I'll say more about that later.

Ralph Cohen and Dennis Sullivan both gave talks on string topology — a trick for studying a space by studying collections of loops in that space, and relating this to ideas from string theory.

String topology started when Chas and Sullivan took the ideas of string theory and applied them in a somewhat ethereal form to strings propagating in any manifold.

In full-fledged string theory, one of the main tools is "conformal field theory". In a CFT, if you have a state of n strings, and a Riemann surface going from n strings to m strings, you get a state of m strings.

A good way to get CFTs is to consider strings propagating on some manifold or other. Of course the manifold needs some sort of geometry, like a Riemannian metric, for your strings to know how to propagate.

But Chas and Sullivan figured out what you can do if the spacetime is a bare manifold, without any metric. Basically, you just need to stick the word "homology" in front of everything! This makes everything sufficiently floppy.

So, instead of considering actual loops in a manifold M, which form a space LM, they took the homology of LM and got a vector space or abelian group $H(LM)$. Then, for each homology class C on the moduli space of Riemann surfaces that go from n circles to m circles, they got an operation with n inputs and m outputs:

$$Z(C)\colon H(LM)^{\otimes n} \to H(LM)^{\otimes m}$$

All these operations fit together into a slight generalization of an operad, called a "PROP".

If you don't remember what an "operad" is, give yourself twenty lashes with a wet noodle and review "Week 220". Suitably punished, you can then enjoy this:

14) Ralph Cohen and Alexander Voronov, "Notes on string topology", available as arXiv:math/0503625.

Both PROPs and operads are defined near the beginning here. PROPs and operads are gadgets for describing operations with any number of inputs. Operads can only handle operations with one output. PROPS can handle operations with any number of outputs.

To see a more geometrical treatment of string topology, the way it looked before the operadchiks got ahold of it, try the original paper by Chas and Sullivan:

15) Moira Chas and Dennis Sullivan, "String topology", available as arXiv:math/9911159.

Sullivan talked about some recent refinements of string topology which deal with the fact that the moduli space of Riemann surfaces has a "boundary", so it doesn't have a closed "top-dimensional homology class".

Cohen's talk described some cool relations between string topology and symplectic geometry! In physics we use symplectic manifolds to describe the space of states — the so-called "phase space" — of a classical system. So, if you have a loop in a symplectic manifold, it can describe a periodic orbit of some classical system. In particular, if we pick a periodic time-dependent Hamiltonian for this system, a loop will be a solution of Hamilton's equations iff it's a critical point for the "action".

But, we can also imagine letting loops move in the direction of decreasing action, following the "gradient flow". They'll trace out 2d surfaces which we can think of as string world-sheets! This is just what string topology studies, but now we can get "Morse theory" into the game: this studies a space (here LM) by looking at critical points of a function on this space, and its gradient flow.

So, we get a nice interaction between periodic orbits in phase space, and the string topology of that space, and Morse theory! For more, try this:

16) Ralph Cohen, "The Floer homotopy type of the cotangent bundle", available as `arXiv:math/0702852`.

Next, let me say a bit about Stephan Stolz's talk. He spoke on his work with Peter Teichner, which is a very ambitious attempt to bring quantum field theory right into the heart of algebraic topology.

I discussed this in "Week 197". I said they were working on a wonderful analogy between quantum field theories and different flavors of cohomology. It's been published since then:

17) Stephan Stolz and Peter Teichner, "What is an elliptic object?" Available at `http://math.berkeley.edu/~teichner/papers.html`

Back then, the analogy looked like this:

1-dimensional supersymmetric QFTs	complex K-theory
2-dimensional supersymmetric conformal QFTs	elliptic cohomology

When I saw this, I tried to guess a generalization to higher dimensions.

There's an obvious guess for the right-hand column, since there's something called the "chromatic filtration", which is — very roughly - a list of cohomology theories. Complex K-theory is the 1st entry on this list, and elliptic cohomology is the 2nd! (For a lot more details, see "Week 149" and "Week 150".)

There's also an obvious guess for the left-hand column: n-dimensional supersymmetric QFTs of some sort!

The problem is the word "conformal" in the second row. In 2 dimensions, a conformal structure is a way of making spacetime look locally like the complex plane. This is great, because elliptic cohomology has a lot to do with complex analysis — or more precisely, elliptic curves and modular forms. But, it's not clear how one should generalize this to higher dimensions!

Luckily, thanks to a subsequent conversation with Witten, Stolz and Teichner realized that the partition function of a 2d supersymmetric QFT gives a modular form even if the QFT is not invariant under conformal transformations. This means we can remove the word "conformal" from the second row! For more details, try this:

18) Stephan Stolz and Peter Teichner, "Super symmetric field theories and integral

modular forms", preliminary version available at `http://math.berkeley.edu/~teichner/papers.html`

They've also gone back and added a 0th row to their chart. It's always wise to start counting at zero! Now the chart looks much nicer:

0-dimensional supersymmetric QFTs	deRham cohomology
1-dimensional supersymmetric QFTs	complex K-theory
2-dimensional supersymmetric QFTs	elliptic cohomology

Yes, good old deRham cohomology is the 0th entry in the "chromatic filtration"! It's the least scary sort of cohomology theory, at least for physicists. They get scarier as we move down the chart.

Quantum field theory also gets scarier as we move down the chart — the infinities that plague quantum field theory tend to get worse in higher dimensions of spacetime. So, while we can dream about extensions of this chart, there's already plenty to handle here.

The most audacious idea in Stolz and Teichner's work is to take a manifold X and study the set of all n-dimensional QFT's "parametrized by X".

For X a point, such a thing is just an ordinary n-dimensional QFT. Roughly speaking, this is a gadget Z that assigns:

- a Hilbert space $Z(S)$ to any $(n-1)$-dimensional Riemannian manifold S;
- a linear operator $Z(M)\colon Z(S) \to Z(S')$ to any n-dimensional Riemannian manifold M going from S to S'.

If you're a mathematician, you may know that M is really a "cobordism" from S to S', written $M\colon S \to S'$. And if you're really cool, you'll know that cobordisms form a symmetric monoidal category $n\mathsf{Cob}$, and that Z should be a symmetric monoidal functor.

If you're a physicist, you'll know that S stands for "space" and "M" stands for "spacetime". All the stuff I'm describing should remind you of the definition of a "TQFT", except now our spaces and spacetimes have Riemannian metrics, because we're doing honest QFTs, not topological ones.

Given a spacetime M, we try to compute the operator $Z(M)$ as a path integral; for example, an integral over all maps

$$f\colon M \to T$$

where f is a "field" taking values in a "target space" T.

If this seems too scary, take $n = 1$. Then we've got a 1-dimensional quantum field theory, so we can take our spacetime M to be an interval. Then f is just a path in some space T. In this case the path integral is really an integral over all paths a particle could trace out in T. So, 1-dimensional quantum field theory is just ordinary quantum mechanics!

There are a lot of subtleties I'm skipping over here, both on the math and physics sides. But never mind — the really cool part is this generalization:

Roughly speaking, an n-dimensional QFT "parametrized by X" assigns:

- a Hilbert space $Z(S)$ to any $(n-1)$-dimensional Riemannian manifold S *equipped with a map* $g\colon S \to X$;
- a linear operator $Z(M)\colon Z(S) \to Z(S')$ to any n-dimensional Riemannian cobordism $M\colon S \to S'$ *equipped with a map* $g\colon M \to X$.

If you're a mathematician, you may see we've switched to using cobordisms "over X". It's a straightforward generalization.

But what does it mean physically? Here the path integral picture is helpful. Now we're doing a path integral over all fields

$$f\colon M \to T \times X$$

where we demand that the second component of this function is

$$g\colon M \to X$$

For example, if we've got a 1d QFT, we're letting a particle roam over $T \times X$, but demanding that its X coordinates follow a specific path g.

So, we're doing a *constrained* path integral!

In heaven, everything physicists do can be made mathematically rigorous. Up there, knowing how to do these constrained path integrals would tell us how to do unconstrained path integrals: we'd just integrate over all choices of the path g. So, a QFT parametrized by X would automatically give us an ordinary QFT.

Now, an ordinary QFT is just a QFT parametrized by a point! So, if we use $\mathrm{QFT}(X)$ to mean the set of n-dimensional QFTs parametrized by X, we'd have a map

$$\mathrm{QFT}(X) \to \mathrm{QFT}(\mathrm{point})$$

This is called "pushing forward to a point".

More generally, we could hope that any map

$$F\colon X \to X'$$

gives a "pushforward" map

$$F_*\colon \mathrm{QFT}(X) \to \mathrm{QFT}(X')$$

Let's see if this makes any sense. In fact, I've been overlooking some important issues. An example will shed light on this.

Consider a 0-dimensional QFT parametrized by some manifold X. Let's call it Z. What is Z like, concretely?

For starters, notice that the only (-1)-dimensional manifold is the empty set. A 0-dimensional manifold "going from the empty set to the empty set" is just a set of points. Also, while I didn't mention it earlier, all manifolds in this game must be *compact*. So, this set of points must be finite.

If you now take the definition I wrote down and use that "symmetric monoidal functor" baloney, you'll see Z assigns a *number* to any finite set of points mapped into X.

Furthermore, this assignment must be multiplicative. So, it's enough to know a number for each point in X. In short, our QFT is just a function:

$$Z\colon X \to \mathbb{C}$$

Now suppose we map X to a point:

$$F\colon X \to \mathrm{point}$$

What should the pushforward

$$F_*\colon \mathrm{QFT}(X) \to \mathrm{QFT}(\mathrm{point})$$

do to the function Z?

There's an obvious guess: we should *integrate* this function on X to get a number — that is, a function on a point. Indeed, that's what "path integration" should reduce to in this pathetically simple case: plain old integration!

Alas, there's no good way to integrate a function over X unless this manifold comes equipped with a measure. But, if X is compact, oriented and p-dimensional, we can integrate a *p-form* over X.

More generally, if we have a bundle

$$F\colon X \to X'$$

with compact d-dimensional fibers, we can take a p-form on X and integrate it over the fibers to get a $(p-d)$-form on X'. This is how you "push forward" differential forms.

So, pushing forward is a bit subtler than I led you to believe at first. We should really talk about n-dimensional QFTs "of degree p" parametrized by X. Let's call the set of these

$$\mathrm{QFT}^p(X)$$

I won't define them, but for $n = 0$ they're just p-forms on X. Anyway: if we have a bundle

$$F\colon X \to X'$$

with compact d-dimensional fibers, we can hope there's a pushforward map

$$F_*\colon \mathrm{QFT}^p(X) \to \mathrm{QFT}^{p-d}(X')$$

There should also be a pullback map

$$F^*\colon \mathrm{QFT}^p(X') \to \mathrm{QFT}^p(X)$$

This is a lot less tricky, and I'll let you figure out how it works.

I should warn you, I've been glossing over lots of important aspects of this work — like the role played by n-categories, and the role played by supersymmetry. Supersymmetry doesn't matter much for the broad conceptual picture I've been sketching. But, we need it for this analogy to work:

0-dimensional supersymmetric QFTs	deRham cohomology
1-dimensional supersymmetric QFTs	complex K-theory
2-dimensional supersymmetric QFTs	elliptic cohomology

The idea is to impose an equivalence relation on supersymmetric QFTs, called "concordance", and try to show:

- The set of concordance classes of degree-p 0d supersymmetric QFTs parametrized by X is the pth de Rham cohomology group of X.
- The set of concordance classes of degree-p 1d supersymmetric QFTs parametrized by X is the pth K-theory group of X.
- The set of concordance classes of degree-p 2d supersymmetric QFTs parametrized by X is the pth elliptic cohomology group of X.

So far people have done this in the 0d and 1d cases. The 2d case is a major project, because it pushes the limits of what people can do with quantum field theory.

Why did I spend so much time talking about pushforwards of QFTs? Well, it's very important for defining invariants like the "fundamental class" of an oriented manifold, or the "$\hat{A}$ genus" of a spin manifold, or the "Witten index" of a string manifold.

Here's how it goes, very roughly. Suppose X is a compact Riemannian manifold. Then the simplest n-dimensional QFT parametrized by X is the one where we take the target space T (mentioned a while back) to be just a point!

This parametrized QFT is called the "nonlinear σ-model", for stupid historic reasons. All the fun happens when we push this QFT forwards to a point. Then we integrate over all the maps $g\colon M \to X$. The result — usually called the "partition function" of the nonlinear σ-model — should be an interesting invariant of X.

In the case $n = 1$, this trick gives the "$\hat{A}$ genus" of X, but it only works when X is a spin manifold: we need this to define the 1d supersymmetric nonlinear σ-model.

In the case $n = 2$, this trick gives the "Witten genus" of X, but it only works when X is a string manifold: we need this to define the 2d supersymmetric nonlinear σ-model.

For more on the $n = 1$ case, see:

19) Henning Hohnhold, Peter Teichner and Stephan Stolz, "From minimal geodesics to super symmetric field theories". In memory of Raoul Bott. Available at `http://math.berkeley.edu/~teichner/papers.html`

For the $n = 2$ case, see the papers I already listed.

(I'm confused about the case $n = 0$, for reasons having to do with the "degree" I mentioned earlier.)

Finally: the cool part, which I haven't even mentioned, is that we really need to describe n-dimensional QFTs using an *n-category* of cobordisms — not just a mere 1-category, as I sloppily said above.

This first gets exciting when we hit $n = 2$: you'll see a bunch of stuff about 2-categories (or technically, "bicategories") in the old Stolz-Teichner paper "What is an elliptic object", listed above.

In short: we're starting to see a unified picture where we study spaces by letting particles, strings, and their n-dimensional cousins roam around in these spaces. There are lots of slight variants: string topology, the Stolz-Teichner picture, and of course good old-fashioned topological quantum field theory. All of them have a lot to do with n-categories.

There's a lot more to say about all this. . . but luckily, there should be a proceedings of this conference, where you can read more. My own talk is here:

20) John Baez, "Higher gauge theory and elliptic cohomology", `http://math.ucr.edu/home/baez/abel/`

It, too, is about studying spaces by letting strings roam around inside them!

But instead of summarizing my own talk, I want to say a bit about the other side of the symposium — the motivic cohomology side!

I'll only summarize a few basic definitions. I got these from the talks by Fabian Morel and Vladimir Voevodsky, and I want to write them down before I forget! For more, try these:

21) Fabian Morel and Vladimir Voevodsky, A^1-homotopy theory of schemes, September 1998. Available at `http://citeseer.ist.psu.edu/morel98suphomotopy.html`

22) Vladimir Voevodsky (notes by Charles Weibel), "Voevodsky's Seattle Lectures: K-theory and motivic cohomology". Available at `http://citeseer.ist.psu.edu/249068.html`

Okay:

A^1-homotopy theory is an attempt to do homotopy theory for algebraic geometry. In algebraic geometry we often work over a fixed field k, and the goal here is to create a category which contains smooth algebraic varieties over k as objects, but also other more general spaces, providing a sufficiently flexible category in which to do homotopy theory.

One of the simplest smooth algebraic varieties over k is the "affine line" A^1. The algebraic functions on this line are just polynomials in one variable with coefficients in k. In A^1-homotopy theory, we want to set up a context where we can use the affine line A^1 to parametrize homotopies, much as we use the unit interval $[0,1]$ in ordinary homotopy theory.

For this, people start by looking at $\mathsf{Sm}(k)$, the category of smooth algebraic varieties over k. Then, they consider the category of "simplicial presheaves" on $\mathsf{Sm}(k)$.

A simplicial presheaf on $\mathsf{Sm}(k)$ is just a functor

$$F\colon \mathsf{Sm}(k)^{\mathrm{op}} \to \mathsf{SimpSet}$$

where SimpSet is the category of simplicial sets (see item C of "Week 115") We think of F as specifying some sort of space by telling us for each smooth algebraic variety X the simplicial set $F(X)$ of all maps into this space.

To make this kind of abstract space work nicely, $F(X)$ should depend "locally" on X. For this, we insist that given a cover of a variety X by varieties U_i, guys in $F(X)$ are the same as guys in $F(U_i)$ that agree on the intersections

$$U_i \cap U_j.$$

Here "cover" means "cover in the Nisnevich topology" — that is, an tale cover such that every point being covered is the image of a point in the cover for which the covering map induces an isomorphism of residue fields.

If you've come this far, you may not be scared to hear that the Nisnevich topology is really a "Grothendieck topology" on $\mathsf{Sm}(k)$, and I'm really demanding that F be a "sheaf" with respect to this topology.

So, the kind of "space" we're studying is a simplicial sheaf on the category of smooth varieties over k with its Nisnevich topology. We call these category of these guys $\mathsf{Space}(k)$.

Just saying this already makes me feel smart. Just think how smart I'd feel if I knew why the Nisnevich topology was better than the good old tale topology!

Anyway, to do homotopy theory with these simplicial sheaves, we need to make $\mathsf{Space}(k)$ into a "model category". I should have explained model categories in some previous Week, but I've never gotten around to it, and right now is not the time. So, I'll just say one key thing.

The *most* important thing about a model category is that it's equipped with a collection of morphisms that act like homotopy equivalences. They're called "weak equivalences".

Already in ordinary topology, these weak equivalences are a slight generalization of homotopy equivalences. They're actually the same as homotopy equivalences when the spaces involved are nice; they're designed to work better for nasty spaces.

In A^1-homotopy theory, the weak equivalences are generated by two kinds of morphisms:

- the projection maps $X \times A^1 \to X$
- the maps $\check{C}(\mathcal{U}) \to X$ coming from covers $\mathcal{U}$ of X.

Here X is any space in $\mathsf{Space}(k)$, and $\check{C}(\mathcal{U})$ is the "Cech nerve" of the cover $\mathcal{U}$.

This framework seems like a really cool blend of algebraic geometry and homotopy theory. But, to do homology theory in a good way we need to go a bit further, and introduce "motives".

However, I'm tired, and I bet you are too! Motives are a big idea, and it doesn't make sense to start talking about them now. So, some other day. . . .

Addendum: For more discussion, go to the n-Category Caf.

> And if a bird can speak, who once was a dinosaur, and a dog can dream, should it be implausible that a man might supervise the construction of light?
>
> — *King Crimson*

Week 256

August 27, 2007

My European wanderings continue. I'm in Greenwich again, just back from a mind-blowing conference in Vienna, part of a bigger program that's still going on:

1) *Poisson sigma models, Lie algebroids, deformations, and higher analogues*, Erwin Schrdinger Institute, August – September 2007, organized by Thomas Strobl, Henrique Bursztyn, and Harald Grosse. Program at `http://w3.impa.br/~henrique/esi.html`

I learned a huge amount, both from the talks and from conversations with Urs Schreiber and others. Mainly, I learned that I've really been falling behind the times when it comes to classical mechanics and quantization!

I could easily spend several Weeks trying to assimilate the half-digested information I acquired and explain it all to you. But, I want to get back to the Tale of Groupoidification! So, I'll only say a little about this wonderful conference.

You may know that in classical mechanics, the space of states of a physical system is called its "phase space". Often this is described by a "symplectic manifold" — a manifold equipped with a nondegenerate closed 2-form. Sometimes it's described by a "Poisson manifold" — a manifold equipped with a bracket operation on its smooth functions, making the smooth functions into a Lie algebra and also satisfying the product rule:

$$\{f, gh\} = \{f, g\}h + g\{f, h\}$$

Every symplectic manifold gives a Poisson manifold, but not vice versa. A good example of a Poisson manifold that's not symplectic is the phase space of a spinning point particle, which has angular momentum but no other properties.

Every mathematical physicist should know some symplectic geometry and Poisson geometry! To get started on symplectic geometry, try these, in rough order of increasing difficulty:

1) Vladimir I. Arnold, *Mathematical Methods of Classical Mechanics*, Springer, Berlin, 1997.

2) Ralph Abraham and Jerrold E. Marsden, *Foundations of Mechanics*, Benjamin-Cummings, New York, 1978.

3) Victor Guillemin and Shlomo Sternberg, *Symplectic Techniques in Physics*, Cambridge U. Press, Cambridge, 1990.

4) Ana Cannas da Silva, *Symplectic geometry*, available as `arXiv:math.SG/0505366`.

5) Sergei Tabachnikov, *Introduction to symplectic topology*, available at `http://www.math.psu.edu/tabachni/courses/symplectic.pdf`

For Poisson geometry, try the above but also:

6) Alan Weinstein, *Poisson geometry*, available at `http://galileo.stmarys-ca.edu/bdavis/poisson.pdf`

7) Darryl Holm, "Applications of Poisson geometry to physical problems", available as arXiv:0708.1585.

8) I. Vaisman, *Lectures on the Geometry of Poisson Manifolds*, Birkhaeuser, Boston, 1994.

All this stuff is great. But lately, people have started thinking about generalizations of the idea of phase space that go far beyond Poisson manifolds! In fact there seems to be an infinite sequence, which begins like this:

symplectic manifolds,
Poisson manifolds,
Courant algebroids,
...

I'd heard of Courant algebroids before, but they always seemed like a scary and arbitrary concept — until I came across this paper in Vienna:

9) Pavol Severa, 'Some title containing the words "homotopy" and "symplectic", e.g. this one', available as arXiv:math/0105080.

The title is goofy, but the paper itself contains some truly visionary speculations. Among other things, it argues that the above sequence of concepts really goes like this:

symplectic manifolds,
symplectic Lie algebroids,
symplectic Lie 2*-algebroids,*
symplectic Lie 3*-algebroids,*
...

These, in turn, are infinitesimal versions of perhaps more fundamental concepts:

symplectic manifolds,
symplectic Lie groupoids,
symplectic Lie 2*-groupoids,*
symplectic Lie 3*-groupoids,*
...

These concepts take the basic concept of classical phase space and build in symmetries, symmetries between symmetries, and so on!

So, we may be starting to see the "periodic table of n-categories" show up in classical mechanics. Back in "Week 49" I explained the most basic version of this table. Here's a tiny portion of it:

Table 4: k-tuply monoidal n-categories

	$n = 0$	$n = 1$	$n = 2$
$k = 0$	sets	categories	2-categories
$k = 1$	monoids	monoidal categories	monoidal 2-categories
$k = 2$	commutative monoids	braided monoidal categories	braided monoidal 2-categories
$k = 3$	” ”	symmetric monoidal categories	sylleptic monoidal 2-categories
$k = 4$	” ”	” ”	symmetric monoidal 2-categories
$k = 5$	” ”	” ”	” ”

An n-category has objects, 1-morphisms betwen objects, 2-morphisms between 1-morphisms, and so on up to the nth level. A “k-tuply monoidal” n-category is an $(n+k)$-category that’s trivial on the bottom k levels. It masquerades as an n-category with extra bells and whistles. As you can see, we get lots of fun structures this way.

The concept of n-category is very general: it describes things, processes that go between things, metaprocesses that go between processes and so on. But, in classical mechanics we may want to demand that all these morphisms be invertible, and that all the ways of composing them be smooth functions. Then we should get some table like this:

Table 5: k-tuply groupal Lie n-groupoids

	$n = 0$	$n = 1$	$n = 2$
$k = 0$	manifolds	Lie groupoids	Lie 2-groupoids
$k = 1$	Lie groups	Lie 2-groups	Lie 3-groups
$k = 2$	abelian Lie groups	braided Lie 2-groups	braided Lie 3-groups
$k = 3$	” ”	symmetric Lie 2-groups	sylleptic Lie 3-groups
$k = 4$	” ”	” ”	symmetric Lie 3-groups
$k = 5$	” ”	” ”	” ”

There are lots of technical issues to consider — for example, whether manifolds are a sufficiently general notion of "smooth space" to make this chart really work. But for now, the key thing is to understand what we're shooting for, so we can set up definitions that accomplish it.

For example, it would be nice if we could "differentiate" any of the gadgets on the above table, just as we differentiate a Lie group and get a Lie algebra. This should give another table, like this:

Table 6: k-tuply groupal Lie n-algebroids

	$n = 0$	$n = 1$	$n = 2$
$k = 0$	vector bundles?	Lie algebroids	Lie 2-algebroids
$k = 1$	Lie algebras	Lie 2-algebras	Lie 3-algebras
$k = 2$	abelian Lie algebras	braided Lie 2-algebras	braided Lie 3-algebras
$k = 3$	" "	symmetric Lie 2-algebras	sylleptic Lie 3-algebras
$k = 4$	" "	" "	symmetric Lie 3-algebras
$k = 5$	" "	" "	" "

The $n = k = 0$ corner is a bit puzzling — it's sort of degenerate. Everyone knows how to get Lie algebras from Lie groups. So, the real fun starts in getting Lie algebroids from Lie groupoids! If you want to see how it works, start here:

10) Alan Weinstein, "Groupoids: unifying internal and external symmetry", *AMS Notices* **43** (1996), 744–752. Also available as arXiv:math/9602220.

For more details, try this:

11) Kirill Mackenzie, *General Theory of Lie Groupoids and Lie Algebroids*, Cambridge U. Press, 2005.

There's also the question of going back. We can integrate any finite-dimensional Lie algebra to get a simply-connected Lie group — that's called Lie's 3rd theorem. But getting from Lie algebroids to Lie groupoids is harder... in fact, according to the standard definitions, it's often impossible!

That's bad enough, but the really weird part is this: you can get something like a Lie 2-*groupoid* from a Lie algebroid! This throws a serious monkey wrench into the whole periodic table.

Luckily, one of the people who really understands this stuff was at this conference in Vienna — Chenchang Zhu. And, she explained what's going on. So now I'm busily reading her papers:

12) Hsian-Hua Tseng and Chenchang Zhu, "Integrating Lie algebroids via stacks", available as arXiv:math/0405003.

13) Chenchang Zhu, "Lie n-groupoids and stacky Lie groupoids", available as arXiv:math/0609420.

14) Chenchang Zhu, "Lie II theorem for Lie algebroids via stacky Lie groupoids", available as arXiv:math/0701024.

(Lie's 2nd theorem says that all Lie algebra homomorphisms integrate to give homomorphisms between the corresponding simply-connected Lie groups.)

I'm optimistic that the patterns will be very beautiful when we fully understand them. In particular, problems also arise when trying to integrate Lie n-algebras to get Lie n-groups, but a lot of progress has been made on these problems:

15) Ezra Getzler, "Lie theory for nilpotent L_∞-algebras", available as arXiv:math/0404003.

16) Andre Henriques, "Integrating L_∞-algebras", available as arXiv:math/0603563.

The really wonderful part is that there's already a functioning theory of Lie n-algebroids, carefully disguised under the name of "NQ-manifolds of degree n". For a great introduction to these, see section 2 of this paper:

17) Dmitry Roytenberg, "On the structure of graded symplectic supermanifolds and Courant algebroids", in *Quantization, Poisson Brackets and Beyond*, ed. Theodore Voronov, Contemp. Math. 315, AMS, Providence, Rhode Island, 2002. Also available as math.SG/0203110.

Using these, people are already busy extending the ideas of classical mechanics across the top row of the periodic table!

The details are currently rather baroque. The best way to see the big picture, I think, is to simultaneously read the above papers by Pavol Severa and Dmitry Roytenberg. For example, Roytenberg's paper proves that:

- *symplectic NQ-manifolds of degree 0 = symplectic manifolds*
- *symplectic NQ-manifolds of degree 1 = Poisson manifolds*
- *symplectic NQ-manifolds of degree 2 = Courant algebroids*

If we follow his advice and define Lie n-algebroids to be NQ-manifolds of degree n, we can express this by saying:

- *symplectic Lie* 0*-algebroids = symplectic manifolds*
- *symplectic Lie* 1*-algebroids = Poisson manifolds*
- *symplectic Lie* 2*-algebroids = Courant algebroids*

And ultimately, Lie n-algebroids should be just a technical tool for studying Lie n-groupoids — modulo the tricky problems with the generalizations of Lie's 2nd theorem, mentioned above.

Though I met both Roytenberg and Severa in Vienna, I was just beginning to grasp the basics of NQ-manifolds, Courant algebroids and the like, so I couldn't take full advantage of this opportunity. I will need to pester them some other time. In fact, I was stuggling to cope with the fact that everything I just mentioned is just part of an even bigger story. . .

This bigger story involves Batalin-Vilkovisky quantization, Poisson sigma models, the proof by Kontsevich that every Poisson manifold admits a deformation quantization, its interpretation by Cattaneo and Felder in the language of 2d TQFTs, and its generalization by Hofman and Park to the quantization of Courant algebroids using 3d TQFTs. . . which should itself be the tip of a big iceberg. To quantize symplectic Lie n-algebroids, it seems we need to use $(n+1)$-dimensional TQFTs! There are some truly mind-boggling ideas afoot here, which will turn out to be quite simple when properly understood. For a taste of the underlying simplicity, try this:

18) Urs Schreiber, "That shift in dimension", `http://golem.ph.utexas.edu/category/2007/08/john_baez_and_i_spent.html`

But, I'd better learn more before trying to explain these things.

Now, let me return to the Tale of Groupoidification! When I left off, I was about to discuss an example: Hecke operators in the special case of symmetric groups. But, one reader expressed unease with what I'd done so far, saying it was too informal and hand-wavy to understand.

So, this Week I'll fill in some details about "degroupoidification" — the process that sends groupoids to vector spaces and spans of groupoids to linear operators.

How does this work? For starters, each groupoid X gives a vector space $[X]$ whose basis consists of isomorphism classes of objects of X.

Given an object x in X, let's write its isomorphism class as $[x]$. So: x in X gives $[x]$ in $[X]$.

Next, each span of groupoids

$$\begin{array}{ccc} & S & \\ \swarrow & & \searrow \\ X & & Y \end{array}$$

gives a linear operator

$$[S]\colon [X] \to [Y]$$

Note: this operator $[S]$ depends on the whole span, not just the groupoid S sitting on top. So, I'm abusing notation here.

More importantly: how do we get this operator $[S]$? The recipe is simple, but I think you'll profit much more by seeing where the recipe comes from.

To figure out how it should work, we insist that degroupoidification be something like a functor. In other words, it should get along well with composition:

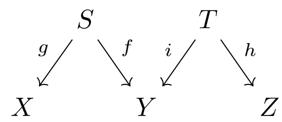
$$[TS] = [T][S]$$

and identities:

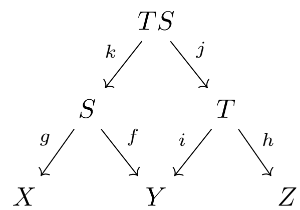
$$[1_X] = 1_{[X]}$$

(Warning: today, just to confuse you, I'll write composition in the old-fashioned backwards way, where doing S and then T is denoted TS.)

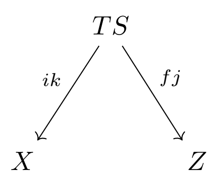
How do we compose spans of groupoids? We do it using a "weak pullback". In other words, given a composable pair of spans:



we form the weak pullback in the middle, like this:



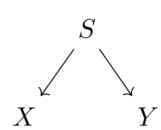
Then, we compose the arrows along the sides to get a big span from X to Z:



Never heard of "weak pullbacks"? Okay: I'll tell you what an object in the weak pullback TS is. It's an object t in T and an object s in S, together with an isomorphism between their images in Y. If we were doing the ordinary pullback, we'd demand that these images be *equal*. But that would be evil! Since t and s are living in groupoids, we should only demand that their images be *isomorphic* in a specified way.

(Exercise: figure out the morphisms in the weak pullback. Figure out and prove the universal property of the weak pullback.)

So, how should we take a span of groupoids



and turn it into a linear operator

$$[S]\colon [X] \to [Y] \text{ ?}$$

We just need to know what this operator does to a bunch of vectors in $[X]$. How do we describe vectors in $[X]$?

I already said how to get a basis vector $[x]$ in $[X]$ from any object x in X. But, that's not enough for what we're doing now, since a linear operator doesn't usually send basis vectors to *basis* vectors. So, we need to generalize this idea.

An object x in X is the same as a functor from 1 to X:

$$1 \xrightarrow{p} X$$

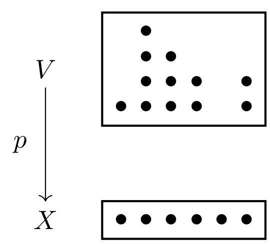
where 1 is the groupoid with one object and one morphism. So, let's generalize this and figure out how *any* functor from *any* finite groupoid V to X:

$$V \xrightarrow{p} X$$

picks out a vector in $[X]$. Again, by abuse of notation we'll call this vector $[V]$, even though it also depends on p.

First suppose V is a finite set, thought of as a groupoid with only identity morphisms. Then to define $[V]$, we just go through all the points of V, figure out what p maps them to — some bunch of objects x in X — and add up the corresponding basis vectors $[x]$ in $[X]$.

I hope you see how pathetically simple this idea is! It's especially familiar when V and X are both sets. Here's what it looks like then:



I've drawn the elements of V and X as little circles, and shown how each element in X has a bunch of elements of V sitting over it. When degroupoidify this to get a vector in the vector space $[X]$, we get:

$$[V] = (1, 4, 3, 2, 0, 2)$$

This vector is just a list of numbers saying how many points of V are sitting over each point of X!

Now we just need to generalize a bit further, to cover the case where V is a groupoid:

$$V \xrightarrow{p} X$$

Sitting over each object x in X we have its "essential preimage", which is a groupoid. To get the vector $[V]$, we add up basis vectors $[x]$ in $[X]$, one for each isomorphism class of objects in X, multiplied by the "cardinalities" of their essential preimages.

Now you probably have two questions:

A) Given a functor $p\colon V \to X$ between groupoids and an object x in X, what's the "essential preimage" of x?

and

B) what's the "cardinality" of a groupoid?

Here are the answers:

A) An object in the essential preimage of x is an object v in V equipped with an isomorphism from $p(v)$ to x.

(Exercise: define the morphisms in the essential preimage. Figure out and prove the universal property of the essential preimage. Hint: the essential preimage is a special case of a weak pullback!)

B) To compute the cardinality of a groupoid, we pick one object from each isomorphism class, count its automorphisms, take the *reciprocal* of this number, and add these numbers up.

(Exercise: check that the cardinality of the groupoid of finite sets is $e = 2.718281828\ldots$ If you get stuck, read "Week 147".)

Also: define the morphisms in the essential preimage. Figure out and prove the universal property of the essential preimage. Hint: the essential preimage is a special case of a weak pullback!)

Okay. Now in principle you know how any groupoid over X, say

$$V \to X$$

determines a vector $[V]$ in $[X]$. You have to work some examples to get a feel for it, but I want to get to the punchline. We're unpeeling an onion here, and we're almost down to the core, where you see there's nothing inside and wonder why you were crying so much.

So, let's finally figure out how a span of groupoids

$$\begin{array}{ccc} & S & \\ \swarrow & & \searrow \\ X & & Y \end{array}$$

gives a linear operator

$$[S]\colon [X] \to [Y]$$

It's enough to know what this operator does to vectors coming from groupoids over X:

$$V \to X$$

And, the trick is to notice that such a diagram is the same as a silly span like this:

$$\begin{array}{ccc} & V & \\ \swarrow & & \searrow \\ 1 & & X \end{array}$$

1 is the groupoid with one object and one morphism, so there's only one choice of the left leg here!

So here's what we do. To apply the operator $[S]$ coming from the span

$$\begin{array}{ccc} & S & \\ \swarrow & & \searrow \\ X & & Y \end{array}$$

to the vector $[V]$ corresponding to the silly span

$$\begin{array}{ccc} & V & \\ \swarrow & & \searrow \\ 1 & & X \end{array}$$

we just compose these spans, and get a silly span

$$\begin{array}{ccc} & SV & \\ \swarrow & & \searrow \\ 1 & & Y \end{array}$$

which picks out a vector $[SV]$ in $[Y]$. Then, we define

$$[S][V] = [SV]$$

Slick, eh? Of course you need to check that $[S]$ is well-defined.

Given that, it's trivial to prove that $[-]$ gets along with composition of spans:

$$[TS] = [T][S]$$

At least, it's trivial once you know that composition of spans is associative up to equivalence, and equivalent spans give the same operator! But your friendly neighborhood category theorist can check such facts in a jiffy, so let's just take them for granted. Then the proof goes like this. We have:

$$\begin{array}{lll} [TS][V] & = [(TS)V] & \text{by definition} \\ & = [T(SV)] & \text{by those facts I just mentioned} \\ & = [T][SV] & \text{by definition} \\ & = [T][S][V] & \text{by definition} \end{array}$$

Since this is true for all $[V]$, we conclude

$$[TS] = [T][S]$$

Voil!

By the way, if "Week 47" doesn't satisfy your hunger for information on groupoid cardinality, try this:

19) John Baez and James Dolan, "From finite sets to Feynman diagrams", in *Mathematics Unlimited — 2001 and Beyond*, vol. **1**, eds. Bjorn Engquist and Wilfried Schmid, Springer, Berlin, 2001, pp. 29-50. Also available as `math.QA/0004133`.

For more on turning spans of groupoids into linear operators, and composing spans via weak pullback, try these:

20) Jeffrey Morton, "Categorified algebra and quantum mechanics", *TAC* **16** (2006), 785–854. Available at `http://www.emis.de/journals/TAC/volumes/16/29/16-29abs.html`; also available as `math.QA/0601458`.

21) Simon Byrne, *On Groupoids and Stuff*, honors thesis, Macquarie University, 2005, available at http://www.maths.mq.edu.au/~street/ByrneHons.pdf and http://math.ucr.edu/home/baez/qg-spring2004/ByrneHons.pdf

For a more leisurely exposition, with a big emphasis on applications to combinatorics and the quantum mechanics of the harmonic oscillator, try:

22) John Baez and Derek Wise, “Quantization and Categorification”, lecture notes available at:
http://math.ucr.edu/home/baez/qg-fall2003/
http://math.ucr.edu/home/baez/qg-winter2004/
http://math.ucr.edu/home/baez/qg-spring2004/

Finally, a technical note. Why did I say the degroupoidification process was “something like” a functor? It’s because spans of groupoids don’t want to be a category!

Already spans of sets don’t naturally form a category. They form a weak 2-category! Since pullbacks are only defined up to canonical isomorphism, composition of spans of sets is only associative up to isomomorphism... but luckily, this “associator” isomorphism satisfies the “pentagon identity” and all that jazz, so we get a weak 2-category, or bicategory.

Similarly, spans of groupoids form a weak 3-category. Weak pullbacks are only defined up to canonical equivalence, so composition of spans of groupoids are associative up to equivalence... but luckily this “associator” equivalence satisfies the pentagon identity up to an isomorphism, and this “pentagonator” isomomorphism satisfies a coherence law of its own, governed by the 3d Stasheff polytope.

So, we’re fairly high in the ladder of n-categories. But, if we want a mere category, we can take groupoids and *equivalence classes* of spans. Then, degroupoidification gives a functor

$$[-]\colon [\text{finite groupoids}, \text{spans}] \to [\text{vector spaces}, \text{linear maps}]$$

That’s the fact whose proof I tried to sketch here.

While I’m talking about annoying technicalities, note we need some sort of finiteness assumption on our spans of groupoids to be sure all the necessary sums converge. If we go all-out and restrict to spans where all groupoids involved are finite, we’ll be very safe. The cardinality of a finite groupoid is a nonnegative rational number, so we can take our vector spaces to be defined over the rational numbers.

But, it’s also fun to consider “tame” groupoids, as defined that paper I wrote with Jim Dolan. These have cardinalities that can be irrational numbers, like e. So, in this case we should use vector spaces over the real numbers — or complex numbers, but that’s overkill.

Finding a class of groupoids or other entities whose cardinalities are complex would be very nice, to push the whole groupoidification program further into the complex world. In the above paper by Jeff Morton, he uses sets over $\mathrm{U}(1)$, but that’s probably not the last word.

> Viewed superficially, mathematics is the result of centuries of effort by thousands of largely unconnected individuals scattered across continents, centuries and millennia. However the internal logic of its development much

more closely resembles the work of a single intellect developing its thought in a continuous and systematics way — much as in an orchestra playing a symphony written by some composer the theme moves from one instrument to another, so that as soon as one performer is forced to cut short his part, it is taken up by another player, who continues with due attention to the score.

— *I. R. Shavarevich*

Week 257

October 14, 2007

Time flies! This week I'll finally finish saying what I did on my summer vacation. After my trip to Oslo I stayed in London, or more precisely Greenwich. While there, I talked with some good mathematicians and physicists: in particular, Minhyong Kim, Ray Streater, Andreas Dring and Chris Isham. I also went to a topology conference in Sheffield... and Eugenia Cheng explained some cool stuff on the train ride there. I want to tell you about all this before I forget.

Also, the Tale of Groupoidification has taken a shocking new turn: it's now becoming available as a series of *videos*.

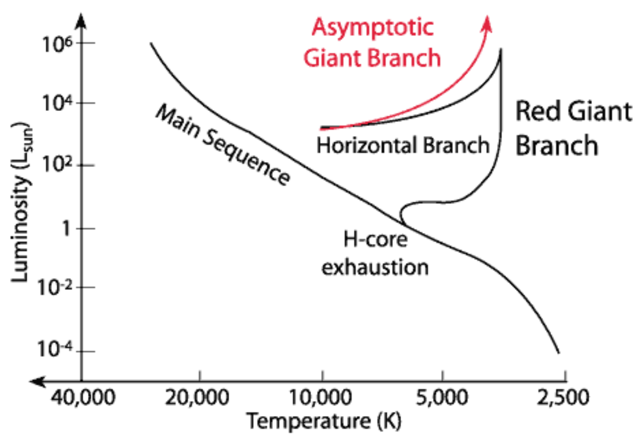
But first, some miscellaneous fun stuff on math and astronomy.

Math: if you haven't seen a sphere turn inside out, you've got to watch this classic movie, now available for free online:

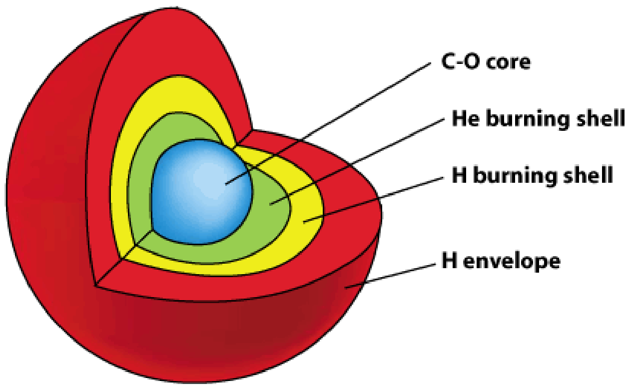
1) The Geometry Center, "Outside in", `http://video.google.com/videoplay?docid=-6626464599825291409`

Astronomy: did you ever wonder where dust comes from? I'm not talking about dust bunnies under your bed — I'm talking about the dust cluttering our galaxy, which eventually clumps together to form planets and... you and me!

These days most dust comes from aging stars called asymptotic giant branch stars:



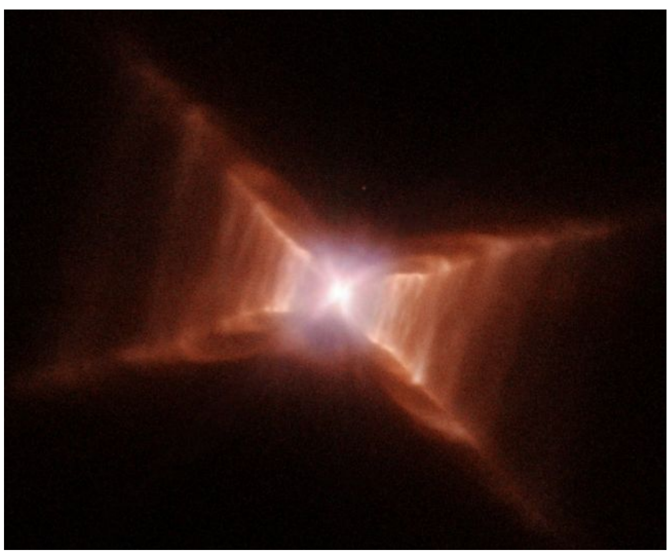
The sun will eventually become one of these. The story goes like this: first it'll keep burning until the hydrogen in its core is exhausted. Then it'll cool and become a red giant. Eventually helium at the core will ignite, and the Sun will heat up and shrink again... but its core will then become cluttered with even heavier elements, so it'll cool and expand once more, moving onto the "asymptotic giant branch". At this point it'll have a layered structure: heavier elements near the bottom, then a layer of helium, then hydrogen on the top:



(A similar fate awaits any star between 0.6 and 10 solar masses, though the details depend on the mass. For the more dramatic fate of heavier stars, see "Week 204".)

Anyway: this layered structure is unstable, so asymptotic giant branch stars pulse every 10 to 100 thousand years or so. And, they puff out dust! Stellar wind then blows this dust out into space.

A great example is the Red Rectangle:



2) "Rungs of the Red Rectangle", Astronomy picture of the day, May 13, 2004, `http://apod.nasa.gov/apod/ap040513.html`

Here two stars 2300 light years from us are spinning around each other while pumping out a huge torus of icy dust grains and hydrocarbon molecules. It's not really shaped like a rectangle or X — it just looks that way. The scene is about 1/3 of a light year across.

Ciska Markwick-Kemper is an expert on dust. She's an astrophysicist at the University of Manchester. Together with some coauthors, she wrote a paper about the Red Rectangle:

3) F. Markwick-Kemper, J. D. Green, E. Peeters, "Spitzer detections of new dust components in the outflow of the Red Rectangle", *Astrophys. J.* **628** (2005) L119–L122. Also available as astro-ph/0506473.

They used the Spitzer Space Telescope — an infrared telescope on a satellite in earth orbit — to find evidence of magnesium and iron oxides in this dust cloud.

But, what made dust in the early Universe? It took about a billion years after the Big Bang for asymptotic giant branch stars to form. But we know there was a lot of dust even before then! We can see it in distant galaxies lit up by enormous black holes called "quasars", which pump out vast amounts of radiation as stuff falls into them.

Markwick-Kemper and coauthors have also tackled that question:

4) F. Markwick-Kemper, S. C. Gallagher, D. C. Hines and J. Bouwman, "Dust in the wind: crystalline silicates, corundum and periclase in PG 2112+059", *Astrophys. J.* **668** (2007), L107–L110. Also available as arXiv:0710.2225.

They used spectroscopy to identify various kinds of dust in a distant galaxy: a magnesium silicate that geologists call "forsterite", a magnesium oxide called "periclase", and aluminum oxide, otherwise known as "corundum" — you may have seen it on sandpaper.

And, they hypothesize that these dust grains were formed in the hot wind emanating from the quasar at this galaxy's core!

So, besides being made of star dust, as in the Joni Mitchell song, you also may contain a bit of black hole dust.

Okay — now that we've got that settled, on to London!

Minhyong Kim is a friend I met back in 1986 when he was a grad student at Yale. After dabbling in conformal field theory, he became a student of Serge Lang and went into number theory. He recently moved to England and started teaching at University College, London. I met him there this summer, in front of the philosopher Jeremy Bentham, who had himself mummified and stuck in a wooden cabinet near the school's entrance.

If you're not into number theory, maybe you should read this:

5) Minhyong Kim, "Why everyone should know number theory", available at `http://www.ucl.ac.uk/~ucahmki/numbers.pdf`

Personally I never liked the subject until I realized it was a form of *geometry*. For example, when we take an equation like this

$$x^2 + y^3 = 1$$

and look at the real solutions, we get a curve in the plane — a "real curve". If we look at the complex solutions, we get something bigger. People call it a "complex curve", because it's analogous to a real curve. But topologically, it's 2-dimensional.

If we use polynomial equations with more variables, we get higher-dimensional shapes called "algebraic varieties" — either real or complex. Either way, we can study these shapes using geometry and topology.

But in number theory, we might study the solutions of these equations in some other number system — for example in $\mathbb{Z}/p$, meaning the integers modulo some prime p. At first glance there's no geometry involved anymore. After all, there's just a *finite set* of solutions! However, algebraic geometers have figured out how to apply ideas from geometry and topology, mimicking tricks that work for the real and complex numbers.

All this is very fun and mind-blowing — especially when we reach Grothendieck's idea of tale topology, developed around 1958. This is a way of studying "holes" in things like algebraic varieties over finite fields. Amazingly, it gives results that nicely match the results we get for the corresponding complex algebraic varieties! That's part of what the Weil conjectures say.

You can learn the details here:

6) J. S. Milne, "Lectures on tale Cohomology", available at `http://www.jmilne.org/math/CourseNotes/math732.html`

Anyway, I quizzed about Minhyong about one of the big mysteries that's been puzzling me lately. I want to know why the integers resemble a 3-dimensional space — and how prime numbers give something like "knots" in this space!

I made a small step toward explaining this back in "Week 205". There I sketched one of the basic ideas of algebraic geometry: every commutative ring, for example the integers or the integers modulo p, has a kind of space associated to it, called its "spectrum". We can think of elements of the commutative ring as functions on this space. I also explained why the process turning a commutative ring into a space is "contravariant". This implies that the obvious map from the integers to the integers modulo p

$$\mathbb{Z} \to \mathbb{Z}/p$$

gives rise to a map going *the other way* between spectra:

$$\mathrm{Spec}(\mathbb{Z}/p) \to \mathrm{Spec}(\mathbb{Z})$$

In "Week 218" I reviewed an old argument saying that $\mathrm{Spec}(\mathbb{Z})$ is analogous to the complex plane, and that $\mathrm{Spec}(\mathbb{Z}/p)$ is analogous to a point. From this viewpoint, primes gives something like points in a plane.

However, from a different viewpoint, primes give something like circles in a 3d space!

The easy thing to see is how $\mathrm{Spec}(\mathbb{Z}/p)$ acts more like a circle than a point. In particular, its "tale topology" resembles the topology of a circle. Oversimplifying a bit, the reason is that just as the circle has one n-fold cover for each integer $n > 0$, so too does $\mathrm{Spec}(\mathbb{Z}/p)$. To get the n-fold cover of the circle, you just wrap it around itself n times. To get the n-fold cover of $\mathrm{Spec}(\mathbb{Z}/p)$, we take the spectrum of the field with p^n elements, which is called $\mathbb{F}_{p^n}$. $\mathbb{Z}/p$ sits inside this larger field:

$$\mathbb{Z}/p \to \mathbb{F}_{p^n}$$

so by the contravariance I mentioned, we get a map going the other way:

$$\mathrm{Spec}(\mathbb{F}_{p^n}) \to \mathrm{Spec}(\mathbb{Z}/p)$$

which is our n-fold cover.

I should explain this in much more detail someday — it involves the relation between tale cohomology, Galois theory and covering spaces. I began tackling this in "Week 213", but I have a long way to go.

Anyway, the basic idea here is that each prime p gives a "circle" $\mathrm{Spec}(\mathbb{Z}/p)$ sitting inside $\mathrm{Spec}(\mathbb{Z})$. But the really nonobvious part is that according to tale cohomology, $\mathrm{Spec}(\mathbb{Z})$ is 3-*dimensional* — and the different circles corresponding to different primes are *linked!*

I've been fascinated by this ever since I heard about it, but I got even more interested when I saw a draft of a paper by Kapranov and Smirnov. I got it from Thomas Riepe, who got it from Yuri Manin. There's a version on the web:

7) M. Kapranov and A. Smirnov, "Cohomology determinants and reciprocity laws: number field case", available at `http://wwwhomes.uni-bielefeld.de/triepe/F1.html`

It begins:

> *The analogies between number fields and function fields have been a long-time source of inspiration in arithmetic. However, one of the most intriguing problems in this approach, namely the problem of the absolute point, is still far from being satisfactorialy understood. The scheme* $\mathrm{Spec}(\mathbb{Z})$*, the final object in the category of schemes, has dimension 1 with respect to the Zariski topology and at least 3 with respect to the etale topology. This has generated a long-standing desire to introduce a more mythical object P, the "absolute point", with a natural morphism* $X \to P$ *given for any arithmetic scheme* X *[. . .]*

Even though I don't fully understand this, I can tell something big is afoot here. I think they're saying that because $\mathrm{Spec}(\mathbb{Z})$ is so big and fancy from the viewpoint of tale topology, there should be some mysterious kind of "point" that's much smaller than $\mathrm{Spec}(\mathbb{Z})$ — the "absolute point".

Anyway, in this paper the authors explain how the Legendre symbol of primes is analogous to the linking number of knots.

The Legendre symbol depends on two primes: it's 1 or -1 depending on whether or not the first is a square modulo the second. The linking number depends on two knots: it says how many times the first winds around the second.

The linking number stays the same when you switch the two knots. The Legendre symbol has a subtler symmetry when you switch the two primes: this symmetry is called quadratic reciprocity, and it has lots of proofs, starting with a bunch by Gauss — all a bit tricky.

I'd feel very happy if I truly understood why quadratic reciprocity reduces to the symmetry of the linking number when we think of primes as analogous to knots. Unfortunately, I'll need to think a lot more before I really get the idea. I got into number theory late in life, so I'm pretty slow at it.

This paper studies subtler ways in which primes can be "linked":

8) Masanori Morishita, "Milnor invariants and Massey products for prime numbers", *Compositio Mathematica* **140** (2004), 69–83.

You may know the Borromean rings, a design where no two rings are linked in isolation, but all three are when taken together. Here the linking numbers are zero, but the linking can be detected by something called the "Massey triple product". Morishita generalizes this to primes!

But I want to understand the basics. . .

The secret 3-dimensional nature of the integers and certain other "rings of algebraic integers" seems to go back at least to the work of Artin and Verdier:

9) Michael Artin and Jean-Louis Verdier, *Seminar on tale cohomology of number fields*, Woods Hole, 1964.

You can see it clearly here, starting in section 2:

10) Barry Mazur, "Notes on the tale cohomology of number fields", *Annales Scientifiques de l'Ecole Normale Superieure Ser. 4* **6** (1973), 521–552. Also available at `http://www.numdam.org/numdam-bin/fitem?id=ASENS_1973_4_6_4_521_0`

By now, a big "dictionary" relating knots to primes has been developed by Kapranov, Mazur, Morishita, and Reznikov. This seems like a readable introduction:

11) Adam S. Sikora, "Analogies between group actions on 3-manifolds and number fields", available as `arXiv:math/0107210`.

I need to study it. These might also be good — I haven't looked at them yet:

12) Masanori Morishita, "On certain analogies between knots and primes", *J. Reine Angew. Math.* **550** (2002), 141–167.

    Masanori Morishita, "On analogies between knots and primes", *Sugaku* **58** (2006), 40–63.

After giving a talk on 2-Hilbert spaces at University College, I went to dinner with Minhyong and some folks including Ray Streater. Ray Streater and Arthur Wightman wrote the book "PCT, Spin, Statistics and All That". Like almost every mathematician who has seriously tried to understand quantum field theory, I've learned a lot from this book. So, it was fun meeting Streater, talking with him — and finding out he'd once been made an honorary colonel of the US Army to get a free plane trip to the Rochester Conference! This was a big important particle physics conference, back in the good old days.

He also described Geoffrey Chew's Rochester conference talk on the analytic S-matrix, given at the height of the bootstrap model fad. Wightman asked Chew: why assume from the start that the S-matrix was analytic? Why not try to derive it from simpler principles? Chew replied that "everything in physics is smooth". Wightman asked about smooth functions that aren't analytic. Chew thought a moment and replied that there weren't any.

Ha-ha-ha. . .

What's the joke? Well, first of all, Wightman had already succeeded in deriving the analyticity of the S-matrix from simpler principles. Second, any good mathematician — but not necessarily every physicist, like Chew — will know examples of smooth functions that aren't analytic.

Anyway, Streater has just finished an interesting book on "lost causes" in physics: ideas that sounded good, but never panned out. Of course it's hard to know when a cause is truly lost. But a good pragmatic definition of a lost cause in physics is a topic that shouldn't be given as a thesis problem.

So, if you're a physics grad student and some professor wants you to work on hidden variable theories, or octonionic quantum mechanics, or deriving laws of physics from Fisher information, you'd better read this:

13) Ray F. Streater, *Lost Causes in and Beyond Physics*, Springer Verlag, Berlin, 2007.

(I like octonions — but I agree with Streater about not inflicting them on physics grad students! Even though all my students are in the math department, I still wouldn't want them working *mainly* on something like that. There's a lot of more general, clearly useful stuff that students should learn.)

I also spoke to Andreas Dring and Chris Isham about their work on topos theory and quantum physics. Andreas Dring lives near Greenwich, while Isham lives across the Thames in London proper. So, I talked to Dring a couple times, and once we visited Isham at his house.

I mainly mention this because Isham is one of the gurus of quantum gravity, profoundly interested in philosophy. . . so I was surprised, at the end of our talk, when he showed me into a room with a huge rack of computers hooked up to a bank of about 8 video monitors, and controls reminiscent of an airplane cockpit.

It turned out to be his homemade flight simulator! He's been a hobbyist electrical engineer for years — the kind of guy who loves nothing more than a soldering iron in his hand. He'd just gotten a big 750-watt power supply, since he'd blown out his previous one.

Anyway, he and Dring have just come out with a series of papers:

14) Andreas Dring and Christopher Isham, "A topos foundation for theories of physics: I. Formal languages for physics", available as quant-ph/0703060.

"II. Daseinisation and the liberation of quantum theory", available as quant-ph/0703062.

"III. The representation of physical quantities with arrows", available as quant-ph/0703064.

"IV. Categories of systems", available as quant-ph/0703066.

Though they probably don't think of it this way, you can think of their work as making precise Bohr's ideas on seeing the quantum world through classical eyes. Instead of talking about all observables at once, they consider collections of observables that you can measure simultaneously without the uncertainty principle kicking in. These collections are called "commutative subalgebras".

You can think of a commutative subalgebra as a classical snapshot of the full quantum reality. Each snapshot only shows part of the reality. One might show an electron's position; another might show it's momentum.

Some commutative subalgebras contain others, just like some open sets of a topological space contain others. The analogy is a good one, except there's no one commutative subalgebra that contains *all* the others.

Topos theory is a kind of "local" version of logic, but where the concept of locality goes way beyond the ordinary notion from topology. In topology, we say a property makes sense "locally" if it makes sense for points in some particular open set. In the Dring-Isham setup, a property makes sense "locally" if it makes sense "within a particular classical snapshot of reality" — that is, relative to a particular commutative subalgebra.

(Speaking of topology and its generalizations, this work on topoi and physics is related to the "tale topology" idea I mentioned a while back — but technically it's much simpler. The tale topology lets you define a topos of sheaves on a certain category. The Dring-Isham work just uses the topos of presheaves on the poset of commutative subalgebras. Trust me — while this may sound scary, it's much easier.)

Dring and Isham set up a whole program for doing physics "within a topos", based on existing ideas on how to do math in a topos. You can do vast amounts of math inside any topos just as if you were in the ordinary world of set theory — but using intuitionistic logic instead of classical logic. Intuitionistic logic denies the principle of excluded middle, namely:

> *"For any statement P, either P is true or* $\mathrm{not}(P)$ *is true."*

In Dring and Isham's setup, if you pick a commutative subalgebra that contains the position of an electron as one of its observables, it can't contain the electron's momentum. That's because these observables don't commute: you can't measure them both simultaneously. So, working "locally" — that is, relative to this particular subalgebra — the statement

$$P = \text{"the momentum of the electron is zero"}$$

is neither true nor false! It's just not defined.

Their work has inspired this very nice paper:

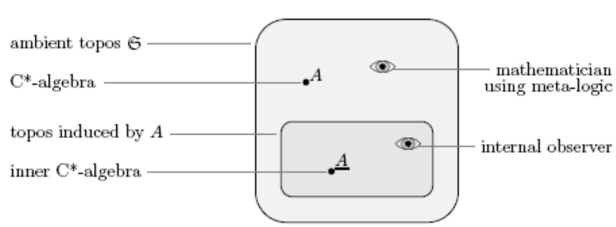
15) Chris Heunen and Bas Spitters, "A topos for algebraic quantum theory", available as `arXiv:0709.4364.`

so let me explain that too.

I said you can do a lot of math inside a topos. In particular, you can define an algebra of observables — or technically, a "C^*-algebra".

By the Isham-Dring work I just sketched, any C^*-algebra of observables gives a topos. Heunen and Spitters show that the original C^*-algebra gives rise to a C^*-algebra *in this topos*, which is *commutative* even if the original one was noncommutative! That actually makes sense, since in this setup each "local view" of the full quantum reality is classical.

So, they get this sort of picture:



I've been taking the "ambient topos" to be the familiar category of sets, but it could be something else.

What's really neat is that the Gelfand-Naimark theorem, saying commutative C^*-algebras are always algebras of continuous functions on compact Hausdorff spaces, can be generalized to work within any topos. So, we get a space *in our topos* such that observables of the C^*-algebra *in the topos* are just functions on this space.

I know this sounds technical if you're not into this stuff. But it's really quite wonderful. It basically means this: using topos logic, we can talk about a classical space of states for a quantum system! However, this space typically has "no global points" — that's called the "Kochen-Specker theorem". In other words, there's no overall classical reality that matches all the classical snapshots.

As you can probably tell, category theory is gradually seeping into this post, though I've been doing my best to keep it hidden. Now I want to say what Eugenia Cheng explained on that train to Sheffield. But at this point, I'll break down and assume you know some category theory — for example, monads.

If you don't know about monads, never fear! I defined them in "Week 89", and studied them using string diagrams in "Week 92". Even better, Eugenia Cheng and Simon Willerton have formed a little group called the Catsters — and under this name, they've put some videos about monads and string diagrams onto YouTube! This is a really great new use of technology. So, you should also watch these:

16) The Catsters, "Monads", `http://youtube.com/view_play_list?p=0E91279846EC843E`

The Catsters, "Adjunctions", `http://youtube.com/view_play_list?p=54B49729E5102248`

The Catsters, "String diagrams, monads and adjunctions", `http://youtube.com/view_play_list?p=50ABC4792BD0A086`

A very famous monad is the "free abelian group" monad

$$F\colon \mathsf{Set} \to \mathsf{Set}$$

which eats any set X and spits out the free abelian group on X, say $F(X)$4. A guy in $F(X)$ is just a formal linear combination of guys in X, with integer coefficients.

Another famous monad is the "free monoid" monad

$$G\colon \mathsf{Set} \to \mathsf{Set}$$

This eats any set X and spits out the free monoid on X, namely $G(X)$. A guy in $G(X)$ is just a formal product of guys in X.

Now, there's yet another famous monad, called the "free ring" monad, which eats any set X and spits out the free ring on this set. But, it's easy to see that this is just $F(G(X))$! After all, $F(G(X))$ consists of formal linear combinations of formal products of guys in X. But that's precisely what you find in the free ring on X.

But why is FG a monad? There's more to a monad than just a functor. A monad is really a kind of *monoid* in the world of functors from our category (here Set) to itself. In particular, since F is a monad, it comes with a natural transformation called a "multiplication":

$$m\colon FF \Rightarrow F$$

which sends formal linear combinations of formal linear combinations to formal linear combinations, in the obvious way. Similarly, since G is a monad, it comes with a natural transformation

$$n\colon GG \Rightarrow G$$

sending formal products of formal products to formal products. But how does FG get to be a monad? For this, we need some natural transformation from $FGFG$ to FG!

There's an obvious thing to try, namely

$$FGFG \Rightarrow FFGG \xRightarrow{mn} FG$$

where in the first step we switch G and F somehow, and in the second step we use m and n. But, how do we do the first step?

We need a natural transformation

$$d\colon GF \Rightarrow FG$$

which sends formal products of formal linear combinations to formal linear combinations of formal products. Such a thing obviously exists; for example, it sends

$$(x + 2y)(x - 3z)$$

to

$$xx + 2yx - 3xz - 6yz$$

It's just the distributive law!

Quite generally, to make the composite of monads F and G into a new monad FG, we need something that people call a "distributive law", which is a natural transformation

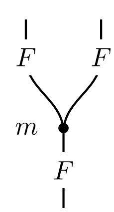
$$d\colon GF \Rightarrow FG$$

This must satisfy some equations — but you can work out those yourself. For example, you can demand that

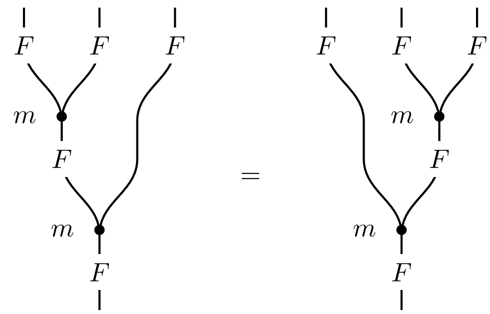
$$FGFG \xRightarrow{FdG} FFGG \xRightarrow{mn} FG$$

make FG into a monad, and see what that requires. (Besides the "multiplication" in our monad, we also need the "unit", so you should also think about that — I'm ignoring it here because it's less sexy than the multiplication, but it's equally essential.)

However: all this becomes more fun with string diagrams! As the Catsters explain, and I explained in "Week 89", the multiplication $m\colon FF \Rightarrow F$ can be drawn like this:

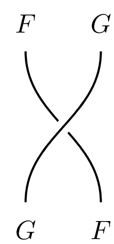


And, it has to satisfy the associative law, which says we get the same answer either way when we multiply three things:



The multiplication $n\colon GG \Rightarrow G$ looks similar to m, and it too has to satisfy the associative law.

How do we draw the distributive law $d\colon FG \Rightarrow GF$? Since it's a process of switching two things, we draw it as a *braiding*:

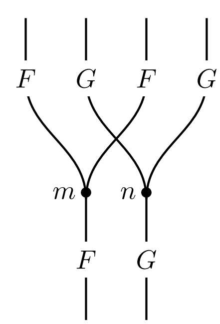


I hope you see how incredibly cool this is: the good old distributive law is now a *braiding*, which pushes our diagrams into the third dimension!

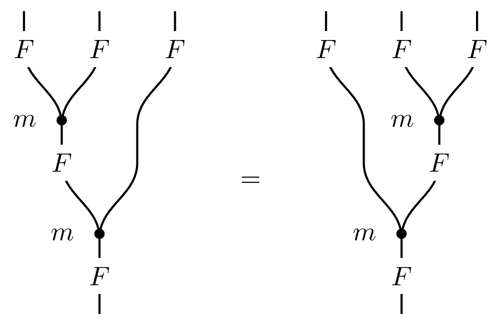
Given this, let's draw the multiplication for our would-be monad FG, namely

$$FGFG \xRightarrow{FdG} FFGG \xRightarrow{mn} FG$$

It looks like this:



Now, we want *this* multiplication to be associative! So, we need to draw an equation like this:

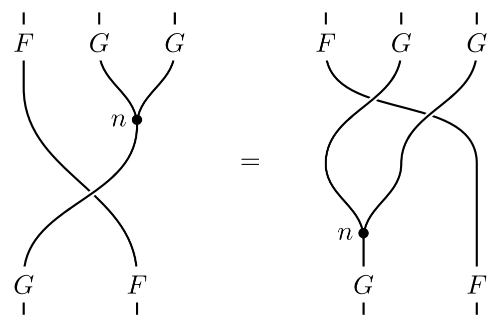


but with the strands *doubled*, as above — I'm too lazy to draw this here. And then we need to find some nice conditions that make this associative law true. Clearly we should use the associative laws for m and n, but the "braiding" — the distributive law $d\colon FG \Rightarrow GF$ — also gets into the act.

I'll leave this as a pleasant exercise in string diagram manipulation. If you get stuck, you can peek in the back of the book:

17) Wikipedia, "Distibutive law between monads", `http://en.wikipedia.org/wiki/Distributive_law_between_monads`

The two scary commutative rectangles on this page are the "nice conditions" you need. They look nicer as string diagrams. One looks like this:




```
F\    G\   /G             F\    G/    /G
  \     \ /                 \   /    /
   \     |n                  \ /    /
    \   /                     /    /
     \ /             =       / \  /
      /                     /    /
     / \                   /    /
    /   \                  \   /  \
   /     \                  \ /    \
 G/       \F                  |n     \F
 /         \                G|       \
```

In words:

> *"multiply two G's and slide the result over an F" =*
> *"slide both the G's over the F and then multiply them"*

If the pictures were made of actual string, this would be obvious!

The other condition is very similar. I'm too lazy to draw it, but it says

> *"multiply two F's and slide the result under a G" =*
> *"slide both the F's under a G and then multiply them"*

All this is very nice, and it goes back to a paper by Beck:

18) Jon Beck, *Distributive laws*, Lecture Notes in Mathematics **80**, Springer, Berlin, 1969, pp. 119–140.

This isn't what Eugenia explained to me, though — I already knew this stuff. She started out by explaining something in a paper by Street:

19) Ross Street, "The formal theory of monads", *J. Pure Appl. Alg.* **2** (1972), 149–168.

which is reviewed at the beginning here:

20) Steve Lack and Ross Street, "The formal theory of monads II", *J. Pure Appl. Alg.* **175** (2002), 243–265. Also available at `http://www.maths.usyd.edu.au/u/stevel/papers/ftm2.html`

(Check out the cool string diagrams near the end!)

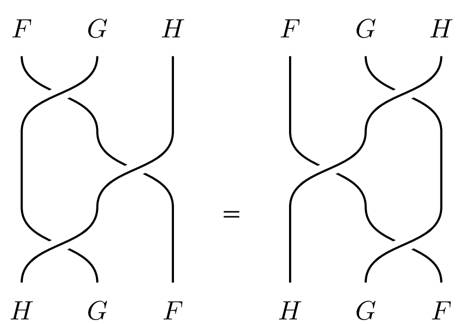
Street noted that we can talk about monads, not just in the 2-category of categories, but in any 2-category. I actually explained monads at this level of generality back in "Week 89". Indeed, for any 2-category $\mathcal{C}$, there's a 2-category $\mathsf{Mnd}(\mathcal{C})$ of monads in $\mathcal{C}$.

And, he noted that a monad in $\mathsf{Mnd}(\mathcal{C})$ is a pair of monads in $\mathcal{C}$ related by a distributive law!

That's already mindbogglingly beautiful. According to Eugenia, it's practically the last sentence in Street's paper. But in her new work:

21) Eugenia Cheng, "Iterated distributive laws", available as `arXiv:0710.1120`.

she goes a bit further: she considers monads in $\mathsf{Mnd}(\mathsf{Mnd}(C))$, and so on. Here's the punchline, at least for today: she shows that a monad in $\mathsf{Mnd}(\mathsf{Mnd}(C))$ is a triple of monads F, G, H related by distributive laws satisfying the Yang-Baxter equation:



This is also just what you need to make the composite FGH into a monad!

By the way, the pathetic piece of ASCII art above is lifted from "Week 1", where I first explained the Yang-Baxter equation. That was back in 1993. So, it's only taken me 14 years to learn that you can derive this equation from considering monads in the category of monads in the category of monads in a 2-category.

You may wonder if this counts as progress — but Eugenia studies lots of *examples* of this sort of thing, so it's far from pointless.

Okay... finally, the Tale of Groupoidification. I'm a bit tired now, so instead of telling you more of the tale, let me just say the big news.

Starting this fall, James Dolan and I are running a seminar on geometric representation theory, which will discuss:

- Actions and representations of groups, especially symmetric groups
- Hecke algebras and Hecke operators
- Young diagrams
- Schubert cells for flag varieties
- q-deformation
- Spans of groupoids and groupoidification

This is the Tale of Groupoidification in another guise.

Moreover, the Catsters have inspired me to make videos of this seminar! You can already find some here, along with course notes and blog entries where you can ask questions and talk about the material:

22) John Baez and James Dolan, "Geometric representation theory seminar", `http://math.ucr.edu/home/baez/qg-fall2007/`

More will show up in due course. I hope you join the fun.

Addenda: I thank Eugenia Cheng for some corrections. Thomas Larsson points out that you can find some of Streater's "lost causes in physics" online:

23) Ray F. Streater, "Various lost causes in physics and elsewhere", `http://www.mth.kcl.ac.uk/~streater/causes.html`

For the proof of the Gelfand-Naimark theorem inside a topos, see:

24) Bernhard Banachewski and Christopher J. Mulvey, "A globalisation of the Gelfand duality theorem", *Ann. Pure Appl. Logic* **137** (2006), 62–103. Also available at `http://www.maths.sussex.ac.uk/Staff/CJM/research/pdf/globgelf.pdf`

They show that any commutative C^*-algebra A in a Grothendieck topos is canonically isomorphic to the C^*-algebra of continuous complex functions on the compact, completely regular locale that is its maximal spectrum (that is, the space of homomorphisms $f\colon A \to \mathbb{C}$). Conversely, they show any compact completely regular locale X gives a commutative C^*-algebra consisting of continuous complex functions on X. Even better, they explain what all this stuff means.

Jordan Ellenberg sent me the following comments about knots and primes:

1. *In the viewpoint of Deninger, very badly oversimplified,* $\mathrm{Spec}(\mathbb{Z})$ *is to be thought of not just as a* 3*-manifold but as a* 3*-manifold with a flow, in which the primes are not just knots, but are precisely the closed orbits of the flow!*
2. *One thing to keep in mind about the analogy is that "the complement of a knot or link in a* 3*-manifold" and "the complement of a prime or composite integer in* $\mathrm{Spec}(\mathbb{Z})$*" (which is to say* $\mathrm{Spec}(\mathbb{Z}[1/N])$ *are both "things which have fundamental groups," thanks to Grothendieck in the latter case. And much of the concrete part of the analogy (like the stuff about linking numbers) follows from this fact.*
3. *On a similar note, a recent paper of Dunfield and Thurston which I like a lot, "Finite covers of random* 3*-manifolds," develops a model of "random* 3*-manifold" and shows that the behavior of the first homology of a random* 3*-manifold* $\bmod p$ *is exactly the same as the* predicted *behavior of the* $\bmod p$ *class group of a random number field under the Cohen–Lenstra heuristics. In other words, you should not think of* $\mathrm{Spec}(\mathbb{Z})$ *or* $\mathrm{Spec}(\mathbb{Z}[1/N])$ *as being anything like a* particular 3*-manifold – better to think of the class of* 3*-manifolds as being like the class of number fields.*

Here's one of Deninger's papers:

25) Christopher Deninger, "Number theory and dynamical systems on foliated spaces", available as `arXiv:math/0204110`.

And here's the paper by Dunfield and Thurston:

26) Nathan M. Dunfield and William P. Thurston, "Finite covers of random 3-manifolds", available as `arXiv:math/0502567`.

On the n-Category Caf, a number theorist named James corrected some serious mistakes in the original version of this Week's Finds. Here are his remarks on why $\mathrm{Spec}(\mathbb{Z})$ is 3-dimensional:

> *So then why should there be the two dimensions of primes needed to make* $\mathrm{Spec}(\mathbb{Z})$ *three-dimensional? I don't think there is a pure-thought answer to this question. As you wrote, there is a scientific answer in terms of Artin-Verdier duality, which is pretty much the same as class field theory. There is also a pure-thought answer to an analogous question. Let me try to explain that.*
>
> *Instead of considering* $\mathbb{Z}$*, let's consider* $\mathbb{F}[x]$*, where* $\mathbb{F}$ *is a finite field. They are both principal ideal domains with finite residue fields, and this makes them*

behave very similarly, even on a deep level. I'll explain why $\mathbb{F}[x]$ is three-dimensional, and then by analogy we can hope $\mathbb{Z}$ is, too. Now $\mathbb{F}[x]$ is an $\mathbb{F}$-algebra. In other words, $X = \mathrm{Spec}(\mathbb{F}[x])$ is a space mapping to $S = \mathrm{Spec}(\mathbb{F})$. I already explained why S is a circle from the point of view of the tale topology. So, if X is supposed to be three-dimensional, the fibers of this map better be two-dimensional. What are the fibers of this map? Well, what are the points of S? A point in the tale topology is Spec of some field with a trivial absolute Galois group, or in other words, an algebraically closed field (even better, a separably closed one). Therefore a tale point of S is the same thing as Spec of an algebraic closure $\overline{\mathbb{F}}$ of F. What then is the fiber of X over this point? It's Spec of the ring $\overline{\mathbb{F}}[x]$. Now, this *is just the affine line over an algebraically closed field, so we can figure out its cohomological dimension. The affine line over the complex numbers, another algebraically closed field, is a plane and therefore has cohomological dimension 2. Since tale cohomology is kind of the same as usual singular cohomology, the tale cohomological dimension of $\mathrm{Spec}(\overline{\mathbb{F}}[x])$ ought to be 2.*

Therefore X looks like a 3-manifold fibered in 2-manifolds over $\mathrm{Spec}(\mathbb{F})$, which looks like a circle. Back to $\mathrm{Spec}(\mathbb{Z})$, we analogously expect it to look like a 3-manifold, but absent a (non-formal) theory of the field with one element, $\mathbb{Z}$ is not an algebra over anything. Therefore we expect $\mathrm{Spec}(\mathbb{Z})$ to be a 3-manifold, but not fibered over anything.

For more discussion, go to the n-Category Caf.

It is a glorious feeling to discover the unity of a set of phenomena that at first seem completely separate.

— *Albert Einstein*

Week 258

November 25, 2007

Happy Thanksgiving! Today I'll talk about a conjecture by Deligne on Hochschild cohomology and the little 2-cubes operad.

But first I'll talk about… dust!

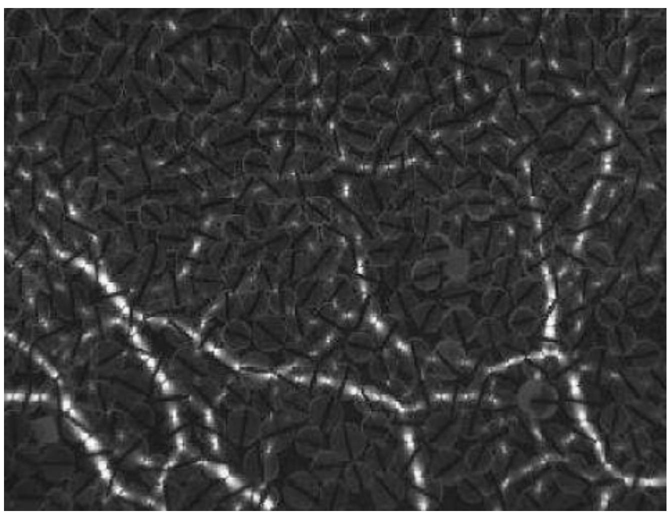
I began "Week 257" with some chat about about dust in a binary star system called the Red Rectangle. So, it was a happy coincidence when shortly thereafter, I met an expert on interstellar dust.

I was giving some talks at James Madison University in Harrisonburg, Virginia. They have a lively undergraduate physics and astronomy program, and I got a nice tour of some labs — like Brian Utter's granular physics lab.

It turns out nobody knows the equations that describe the flow of grainy materials, like sand flowing through an hourglass. It's a poorly understood state of matter! Luckily, this is a subject where experiments don't cost a million bucks.

Brian Utter has a nice apparatus consisting of two clear plastic sheets with a bunch of clear plastic disks between them — big "grains". And, he can make these grains "flow". Since they're made of a material that changes its optical properties under stress, you can see "force chains" flicker in and out of existence as lines of grains get momentarily stuck and then come unstuck!

These force chains look like bolts of lightning:

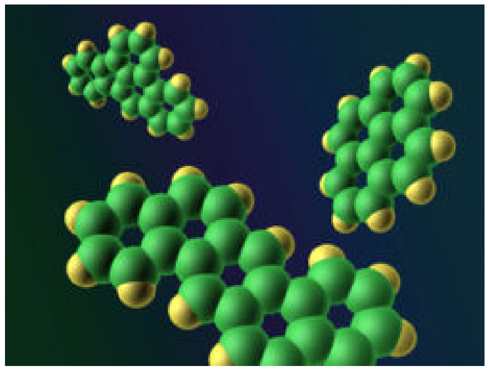


1) Brian Utter and R. P. Behringer, "Self-diffusion in dense granular shear flows", *Physical Review E* **69**, 031308 (2004). Also available as arXiv:cond-mat/0402669.

I wonder if conformal field theory could help us understand these simplified 2-dimensional models of granular flow, at least near some critical point between "stuck" and "unstuck" flow. Conformal field theory tends to be good at studying critical points in 2d physics.

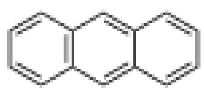
Anyway, I'm digressing. After looking at a chaotic double pendulum in another lab, I talked to Harold Butner about his work using radio astronomy to study interstellar dust.

He told me that the dust in the Red Rectangle contains a lot of PAHs — "polycyclic aromatic hydrocarbons". These are compounds made of hexagonal rings of carbon atoms, with some hydrogens along the edges.

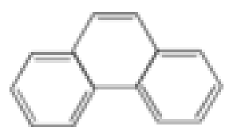


On Earth you can find PAHs in soot, or the tarry stuff that forms in a barbecue grill. Wherever carbon-containing materials suffer incomplete combustion, you'll find PAHs.

Benzene has a single hexagonal ring, with 6 carbons and 6 hydrogens. You've probably heard about naphthalene, which is used for mothballs. This consists of two hexagonal rings stuck together. True PAHs have more. Anthracene and phenanthrene consist of three rings:

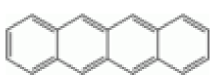


Anthracene

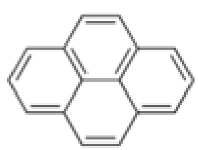


Phenanthrene

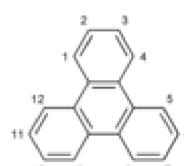
Napthacene, pyrene, triphenylene and chrysene consist of four:



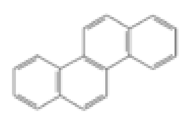
Napthacen (aka Tetracene)



Pyrene



Triphenylene



Chrysene

and so on:

2) Wikipedia, "Polycyclic aromatic hydrocarbon", `http://en.wikipedia.org/wiki/Polycyclic_aromatic_hydrocarbon`

In 2004, a team of scientists discovered anthracene and pyrene in the Red Rectangle! This was first time such complex molecules had been found in space:

3) Uma P. Vijh, Adolf N. Witt, and Karl D. Gordon, "Small polycyclic aromatic hydrocarbons in the Red Rectangle", *The Astrophysical Journal* **619** (2005) 368–378. Also available as astro-ph/0410130.

By now, lots of organic molecules have been found in interstellar or circumstellar space. There's a whole "ecology" of organic chemicals out there, engaged in complex reactions. Life on planets might someday be seen as just an aspect of this larger ecology.

I've read that about 10% of the interstellar carbon is in the form of PAHs — big ones, with about 50 carbons per molecule. They're common because they're incredibly stable. They've even been found riding the shock wave of a supernova explosion!

PAHs are also found in meteorites called "carbonaceous chondrites". These space rocks contain just a little carbon — about 3% by weight. But, 80% of this carbon is in the form of PAHs.

Here's an interview with a scientist who thinks PAHs were important precursors of life on Earth:

5) "Aromatic world", interview with Pascale Ehrenfreund, *Astrobiology Magazine*, available at http://www.astrobio.net/news/modules.php?op=modload&name=News&file=article&sid=1992

And here's a book she wrote, with a chapter on organic molecules in space:

6) Pascale Ehrenfreud, editor, Astrobiology: Future Perspectives, Springer Verlag, 2004.

Harold Butner also told me about dust disks that have been seen around the nearby stars Vega and Epsilon Eridani. By examining these disks, we may learn about planets and comets orbiting these stars. Comets emit a lot of dust, and planets affect its motion.

Mathematicians will be happy to know that *symplectic geometry* is required to simulate the motion of this dust:

7) A. T. Deller and S. T. Maddison, "Numerical modelling of dusty debris disks", *Astrophys. J.* **625** (2005), 398–413. Also available as astro-ph/0502135

Okay... now for a bit about Hochschild cohomology. I want to outline a conceptual proof of Deligne's conjecture that the cochain complex for Hochschild cohomology is an algebra for the little 2-cubes operad. There are a bunch of proofs of this by now. Here's a great introduction to the story:

8) Maxim Kontsevich, "Operads and motives in deformation quantization", available as arXiv:math/9904055.

I was inspired to seek a more conceptual proof by some conversations I had with Simon Willerton in Sheffield this summer, and this paper of his:

9) Andrei Caldararu and Simon Willerton, "The Mukai pairing, I: a categorical approach", available as arXiv:0707.2052.

But, while trying to write up a sketch of this more conceptual proof, I discovered that it had already been worked out:

10) Po Hu, Igor Kriz and Alexander A. Voronov, "On Kontsevich's Hochschild cohomology conjecture", available at arXiv:math.AT/0309369.

This was a bit of a disappointment — but also a relief. It means I don't need to worry about the technical details: you can just look them up! Instead, I can focus on sketching the picture I had in mind.

If you don't know anything about Hochschild cohomology, don't worry! It only comes in at the very end. In fact, the conjecture follows from something simpler and more general. So, what you really need is a high tolerance for category theory, homological algebra and operads.

First, suppose we have any monoidal category. Such a category has a tensor product and a unit object, which we'll call I. Let $\mathrm{end}(I)$ be the set of all endomorphisms of this unit object.

Given two such endomorphisms, say

$$f\colon I \to I$$

and

$$g\colon I \to I$$

we can compose them, getting

$$f \circ g\colon I \to I$$

This makes $\mathrm{end}(I)$ into a monoid. But we can also tensor f and g, and since $I \otimes I$ is isomorphic to I in a specified way, we can write the result simply as

$$f \otimes g\colon I \to I$$

This makes $\mathrm{end}(I)$ into a monoid in another, seemingly different way.

Luckily, there's a thing called the Eckmann-Hilton argument which says these two ways are equal. It also says that $\mathrm{end}(I)$ is a *commutative* monoid! It's easiest to understand this argument if we write $f \circ g$ vertically, like this:

$$\begin{matrix} f \\ g \end{matrix}$$

and $f \otimes g$ horizontally, like this:

$$f \quad g$$

Then the Eckmann-Hilton argument goes as follows:

$$\begin{matrix} f \\ g \end{matrix} = \begin{matrix} 1 & f \\ g & 1 \end{matrix} = g \quad f = \begin{matrix} g & 1 \\ 1 & f \end{matrix} = \begin{matrix} g \\ f \end{matrix}$$

Here 1 means the identity morphism $1\colon I \to I$. Each step in the argument follows from standard stuff about monoidal categories. In particular, an expression like

$$\begin{matrix} f & g \\ h & k \end{matrix}$$

is well-defined, thanks to the interchange law

$$(f \otimes g) \circ (h \otimes k) = (f \circ h) \otimes (g \circ k)$$

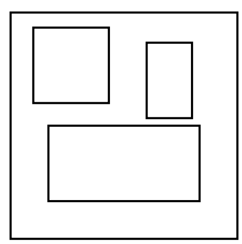
If we want to show off, we can say the interchange law says we've got a "monoid in the category of monoids" — and the Eckmann-Hilton argument shows this is just a monoid. See "Week 100" for more.

But the cool part about the Eckmann-Hilton argument is that we're just moving f and g around each other. So, this argument has a topological flavor! Indeed, it was first presented as an argument for why the second homotopy group is commutative. It's all about sliding around little rectangles... or as we'll soon call them, "little 2-cubes".

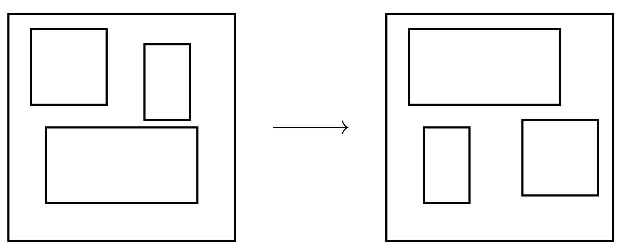
Next, let's consider a version of this argument that holds only "up to homotopy". This will apply when we have not a *set* of morphisms from any object X to any object Y, but a *chain complex* of morphisms.

Instead of getting a set $\mathrm{end}(I)$ that's a commutative monoid, we'll get a cochain complex $\mathrm{END}(I)$ that's a commutative monoid "up to coherent homotopy". This means that the associative and commutative laws hold up to homotopies, which satisfy their own laws up to homotopy, ad infinitum.

More precisely, $\mathrm{END}(I)$ will be an "algebra of the little 2-cubes operad". This implies that for every configuration of n little rectangles in a square:



we get an n-ary operation on $\mathrm{END}(I)$. For every homotopy between such configurations:



we get a chain homotopy between n-ary operations on $\mathrm{END}(I)$. And so on, ad infinitum.

For more on the little 2-cubes operad, see "Week 220". In fact, what I'm trying to do now is understand some mysteries I described in that article: weird relationships between the little 2-cubes operad and Poisson algebras.

But never mind that stuff now. For now, let's see how easy it is to find situations where there's a cochain complex of morphisms between objects. It happens throughout homological algebra!

If that sounds scary, you should refer to a book like this as you read on:

10) Charles Weibel, *An Introduction to Homological Algebra*, Cambridge U. Press, Cambridge, 1994.

Okay. First, suppose we have an abelian category. This provides a context in which we can reason about chain complexes and cochain complexes of objects. A great example is the category of R-modules for some ring R.

Next, suppose every object X in our abelian category has an "projective resolution" — that is, a chain complex

$$X^0 \xleftarrow{d^0} X^1 \xleftarrow{d^1} X^2 \xleftarrow{d^2} \dots$$

where each object X^i is projective, and the homology groups

$$H^i = \frac{\ker(d^i)}{\operatorname{im}(d^{i-1})}$$

are zero except for H^0, which equals X. You should think of a projective resolution as a "puffed-up" version of X that's better for mapping out of than X itself.

Given this, besides the usual set $\mathrm{Hom}(X, Y)$ of morphisms from the object X to the object Y, we also get a cochain complex which I'll call the "puffed-up hom":

$$\mathrm{HOM}(X, Y)$$

How does this work? Simple: replace X by a chosen projective resolution

$$X^0 \leftarrow X^1 \leftarrow X^2 \leftarrow \dots$$

and then map this whole thing to Y, getting a cochain complex

$$\mathrm{Hom}(X^0, Y) \to \mathrm{Hom}(X^1, Y) \to \mathrm{Hom}(X^2, Y) \to \dots$$

This cochain complex is the puffed-up hom, $\mathrm{HOM}(X, Y)$.

Now, you might hope that the puffed-up hom gives us a new category where the hom-sets are actually cochain complexes. This is morally true, but the composition

$$\circ \colon \mathrm{HOM}(X, Y) \times \mathrm{HOM}(Y, Z) \to \mathrm{HOM}(X, Z)$$

probably isn't associative "on the nose". However, I think it should be associative up to homotopy! This homotopy probably won't satisfy the law you'd hope for — the pentagon identity. But, it should satisfy the pentagon identity up to homotopy! In fact, this should go on forever, which is what we mean by "up to coherent homotopy". This kind of situation is described by an infinite sequence of shapes called "associahedra", discovered by Stasheff (see "Week 144").

If this is the case, instead of a category we get an "A_∞-category": a gadget where the hom-sets are cochain complexes and the associative law holds up to coherent homotopy. I'm not sure the puffed-up hom gives an A_∞-category, but let's assume so and march on.

Suppose we take any object X in our abelian category. Then we get a cochain complex

$$\mathrm{END}(X) = \mathrm{HOM}(X, X)$$

equipped with a product that's associative up to coherent homotopy. Such a thing is known as an "A_∞-algebra". It's just an A_∞-category with a single object, namely X.

Next suppose our abelian category is monoidal. (To get the tensor product to play nice with the hom, assume tensoring with any object is right exact.) Let's see what happens to the Eckmann-Hilton argument. We should get a version that holds "up to coherent homotopy".

Let I be the unit object, as before. In addition to composition:

$$\circ\colon \mathrm{END}(I) \times \mathrm{END}(I) \to \mathrm{END}(I)$$

tensoring should give us another product:

$$\otimes\colon \mathrm{END}(I) \times \mathrm{END}(I) \to \mathrm{END}(I)$$

which is also associative up to coherent homotopy. So, $\mathrm{END}(I)$ should be an A_∞-algebra in two ways. But, since composition and tensoring in our original category get along nicely:

$$(f \otimes g) \circ (h \otimes k) = (f \circ h) \otimes (g \circ k)$$

$\mathrm{END}(I)$ should really be an A_∞-algebra in the category of A_∞-algebras!

Given this, we're almost done. A monoid in the category of monoids is a commutative monoid — that's another way of stating what the Eckmann-Hilton argument proves. Similarly, an A_∞-algebra in the category of A_∞-algebras is an algebra of the little 2-cubes operad. So, $\mathrm{END}(I)$ is an algebra of the little 2-cubes operad.

Now look at an example. Fix some algebra A, and take our monoidal abelian category to have:

- (A, A)-bimodules as objects
- (A, A)-bimodule homomorphisms as morphisms

Here the tensor product is the usual tensor product of bimodules, and the unit object I is A itself. And, as Simon Willerton pointed out to me, $\mathrm{END}(I)$ is a cochain complex whose cohomology is familiar: it's the "Hochschild cohomology" of A.

So, the cochain complex for Hochschild cohomology is an algebra of the little 2-cubes operad! But, we've seen this as a consequence of a much more general fact.

To wrap up, here are a few of the many technical details I glossed over above.

First, I said an projective resolution of X is a puffed-up version of X that's better for mapping out of. This idea is made precise in the theory of model categories. But, instead of calling it a "puffed-up version" of X, they call it a "cofibrant replacement" for X. Similarly, a puffed-up version of X that's better for mapping into is called a "fibrant replacement".

For a good introduction to this, try:

11) Mark Hovey, *Model Categories*, American Mathematical Society, Providence, Rhode Island, 1999.

Second, I guessed that for any abelian category where every object has a projective resolution, we can create an A_∞-category using the puffed-up hom, $\mathrm{HOM}(X, Y)$. Alas, I'm not really sure this is true.

Hu, Kriz and Voronov consider a more general situation, but what I'm calling the "puffed-up hom" should be a special case of their "derived function complex". However, they don't seem to say what weakened sort of category you get using this derived function complex — maybe an A_∞-category, or something equivalent like a quasicategory or Segal category? They somehow sidestep this issue, but to me it's interesting in its own right.

At this point I should mention something well-known that's similar to what I've been talking about. I've been talking about the "puffed-up hom" for an abelian category with enough projectives. But most people talk about "Ext", which is the cohomology of the puffed-up hom:

$$\mathrm{Ext}^i(X,Y) = H^i(\mathrm{HOM}(X,Y))$$

And, while I want

$$\mathrm{END}(X) = \mathrm{HOM}(X,X)$$

to be an A_∞-algebra, most people seem happy to have

$$\mathrm{Ext}(X) = H(\mathrm{HOM}(X,X))$$

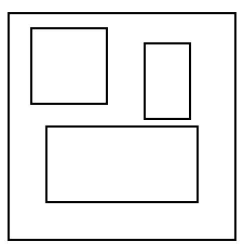
be an A_∞-algebra. Here's a reference:

12) D.-M. Lu, J. H. Palmieri, Q.-S. Wu and J. J. Zhang, "A_∞-structure on Ext-algebras", available as arXiv:math.KT/0606144.

I hope they're secretly getting this A_∞-structure on $H(\mathrm{HOM}(X,X))$ from an A_∞-structure on $\mathrm{HOM}(X,X)$. They don't come out and say this is what they're doing, but one promising sign is that they use a theorem of Kadeishvili, which says that the cohomology of an A_∞-algebra is an A_∞-algebra.

Finally, the really interesting part: how do we make an A_∞-algebra in the category of A_∞-algebras into an algebra of the little 2-cubes operad? This is the heart of the "homotopy Eckmann-Hilton argument".

I explained operads, and especially the little k-cubes operad, back in "Week 220". The little k-cubes operad is an operad in the world of topological spaces. It has one abstract n-ary operation for each way of sticking n little k-dimensional cubes in a big one, like this:



typical 3-ary operation
in the little 2-cubes operad

A space is called an "algebra" of this operad if these abstract n-ary operations are realized as actual n-ary operations on the space in a consistent way. But, when we study the homology of topological spaces, we learn that any space gives a chain complex. This lets us convert any operad in the world of topological spaces into an operad in the world of chain complexes. Using this, it also makes sense to speak of a *chain complex* being an algebra of the little k-cubes operad. Or, for that matter, a cochain complex.

Let's use "$E(k)$" to mean the chain complex version of the little k-cubes operad.

An "A_∞-algebra" is an algebra of a certain operad called A-infinity. This isn't quite the same as the operad $E(1)$, but it's so close that we can safely ignore the difference here: it's "weakly equivalent".

Say we have an A_∞-algebra in the category of A_∞-algebras. How do we get an algebra of the little 2-cubes operad, $E(2)$?

Well, there's a way to tensor operads, such that an algebra of $P \otimes Q$ is the same as a P-algebra in the category of Q-algebras. So, an A_∞-algebra in the category of A_∞-algebras is the same as an algebra of

$$A_\infty \otimes A_\infty$$

Since A_∞ and $E(1)$ are weakly equivalent, we can turn this algebra into an algebra of

$$E(1) \otimes E(1)$$

But there's also an obvious operad map

$$E(1) \otimes E(1) \to E(2)$$

since the product of two little 1-cubes is a little 2-cube. This too is a weak equivalence, so we can turn our algebra of $E(1) \otimes E(1)$ into an algebra of $E(2)$.

The hard part in all this is showing that the operad map

$$E(1) \otimes E(1) \to E(2)$$

is a weak equivalence. In fact, quite generally, the map

$$E(k) \otimes E(k') \to E(k + k')$$

is a weak equivalence. This is Proposition 2 in the paper by Hu, Kriz and Voronov, based on an argument by Gerald Dunn:

13) Gerald Dunn, "Tensor products of operads and iterated loop spaces", *Jour. Pure Appl. Alg* **50** (1988), 237–258.

Using this, they do much more than what I've sketched: they prove a conjecture of Kontsevich which says that the Hochschild complex of an algebra of the little k-cubes operad is an algebra of the little $(k + 1)$-cubes operad!

That's all for now. Sometime I should tell you how this is related to Poisson algebras, 2d TQFTs, and much more. But for now, you'll have to read that in Kontsevich's very nice paper.

Addenda: Over at the n-Category Caf, Michael Batanin made some comments on the difficulties in making my proposed argument rigorous, his own work in doing just this (long before I came along), and the history of Deligne's conjecture (which I deliberately didn't go into, since it's such a long story). Mikael Vejdemo Johansson explained more about the A_∞-structure on Ext.

Modulo some typographical changes, Michael Batanin wrote:

Hi, John.

Just a few remarks about your stuff on Deligne's conjecture. Unfortunately, technical details are important in this business.

First, we have to be careful about tensor product of operads. A very long standing question is: Let A be a E_1-operad and B be a cofibrant E_1-operad. Is it true that their tensor product $A \otimes B$ is an E_2-operad? The answer is unknown, even though Dunn's argument is correct and the tensor product of two little 1-cube operads is equivalent to the little 2-cube operad. Unfortunately, the theorem from Hu, Kriz and Voronov is based implicitly on an affirmative answer to the above question.

I think the history of Deligne's conjecture is quite remarkable and complicated and still developing. The most conceptual and correct proof I know is provided by Tamarkin in

14) *Dmitry Tamarkin, "What do DG categories form?", available as* `math.CT/0606553`.

And it uses my up to homotopy Eckmann–Hilton argument. This argument is based on a techniques of compactification of configuration spaces and first was proposed by Getzler and Jones. I think I already wrote about it in a post to n-category cafe where Dolgushev's work was discussed. Here is the reference to my lecture about Deligne's conjecture:

15) *Michael A. Batanin, "Deligne's conjecture: an interplay between algebra, geometry and higher category theory", talk at ANU Canberra, November 3 2006, available at* `http://www.math.mq.edu.au/~street/BataninMPW.pdf`

Concerning your idea to construct an A-infinity category using $\mathrm{Hom}(PX, Y)$, where PX is a projective resolution: it's been done by me many years ago and in a more general situation. It is long story to tell but more or less I prove that your Hom functor is equivalent as a simplicially coherent bimodule to the homotopy coherent left Kan extension of the inclusion functor

Projective bounded chain complex $\to$ Bounded chain complex

along itself. Then the Kleisli category of this distributor has a canonical A-infinity structure and this Kleisli category is equivalent in an appropriate sense to your 'puffed' category. In fact, the situation I consider in my paper is much more general and includes simplicial Quillen categories as a very special example. The paper is:

16) *Michael A. Batanin, "Categorical strong shape theory",* Cahiers de Topologie et Geom. Diff. ***V.XXXVIII-1*** *(1997), 3–67.*

and its companion

> 17) *Michael A. Batanin, "Homotopy coherent category theory and A_∞ structures in monoidal categories",* Jour. Pure Appl. Alg. ***123** (1998), 67–103.*
>
> *Regards,*
> *Michael*

Batanin's talk has a very nice introduction to his "derived Eckmann-Hilton argument", which is a precise version of what I was attempting to sketch in this Week's Finds. Here's the paper by Getzler and Jones:

18) Ezra Getzler and J. D. S. Jones, "Operads, homotopy algebra and iterated integrals for double loop spaces", available as hep-th/9403055.

It's very interesting, but it was never published, perhaps because of some subtle flaws caught by Tamarkin.

Modulo some typographical changes and extra references, Mikael Vejdemo Johansson wrote:

> *I could try to claim that I'm starting to become an expert on things A_∞, but given that Jim Stasheff is an avid commenter here, I don't quite dare to. :)*
>
> *However, I have read the Lu-Palmieri-Wu-Zhang [LPWZh] paper mentioned in the exposition backwards and forwards. On the face, what LPWZh try to do is to take the survey articles by Bernhard Keller:*
>
> 19) *Bernhard Keller, Introduction to A_∞-algebras and modules, available as* arXiv:math/9910179.
>
> *"A brief introduction to A_∞-algebras", notes from a talk at the workshop on* Derived Categories, Quivers and Strings*, Edinburgh, August 2004. Available at* http://www.institut.math.jussieu.fr/~keller/publ/index.html
>
> *"A_∞-algebras in representation theory", contribution to the* Proceedings of ICRA IX, Beijing 2000. *Available at* http://www.institut.math.jussieu.fr/~keller/publ/index.html
>
> *"A_∞-algebras, modules and functors", available as* arXiv:math/0510508.
>
> *outlining the use of A_∞-algebras in representation theory, and widening the scope of their proven usability while actually proving the many unproven and interesting statements that Keller makes.*
>
> *At the core of this lies two different theorems. One is the Kadeishvili theorem (which in various guises has been proven by everyone involved with A_∞-algebras, and a few more, in my impression ;) that says that you can carry A_∞-algebras across taking homology. Kadeishvili's argument specializes to the case where you start with an A_∞-algebra with only m_1 and m_2 are non-trivial — i.e. a plain old dg-algebra. For higher generality, you'd probably want to turn to the Homology Perturbation Theory crowd with Stasheff, Gugenheim and Huebschmann among the more famous names. . .*

> *Hence, if we take graded endomorphism algebra of a resolution of M and introduce the "homotopy differential":*
>
> $$\partial f = df + fd$$
>
> *then cycles are chain maps and the homology picks out exactly the algebra cohomology over the appropriate module category. Thus, we get* Ext *as the homology of a dg-algebra, and thus,* Ext *has an A_∞-algebra structure.*
>
> *The second cornerstone of these papers is the Keller higher multiplication theorem: if the ring R is sufficiently nice, then the A_∞-algebra structure on* $\mathrm{Ext}^*_R(M, M)$ *for some appropriate module M will allow you to recover a presentation of R explicitly.*
>
> *I hope this answers your question about the origin of their A_∞-algebra structure.*

Note the great technical simplification of working with what I called $\mathrm{Hom}(PX, PX)$ instead of $\mathrm{Hom}(PX, X)$ — composition becomes strictly associative!

For more discussion, go to the n-Category Caf.

> We need a really short and convincing argument for this very fundamental fact about the Hochschild complex.
>
> — *Maxim Kontsevich*

> Higher category theory provides us with the argument Kontsevich was looking for.
>
> — *Michael Batanin*

Week 259

December 9, 2007

This week I'll talk about the "field with one element" — even though it doesn't exist. It's a mathematical phantom.

But first: the Egg Nebula.

In "Week 257" and "Week 258" I talked about interstellar dust. As I mentioned, lots of it comes from "asymptotic giant branch" stars — stars like our Sun, but later in their life, when they're big, red, pulsing, and puffing out elements like hydrogen, helium, carbon, nitrogen, and oxygen.

The pulsations grow wilder and wilder until the star blows off its entire outer atmosphere, forming a big cloud of gas and dust misleadingly called a "planetary nebula". It leaves behind its dense inner core as a hot white dwarf. Intense radiation from this core eventually heats the gas and dust until they glow.

Back in "Week 223" I showed my favorite example of a planetary nebula: the Cat's Eye.

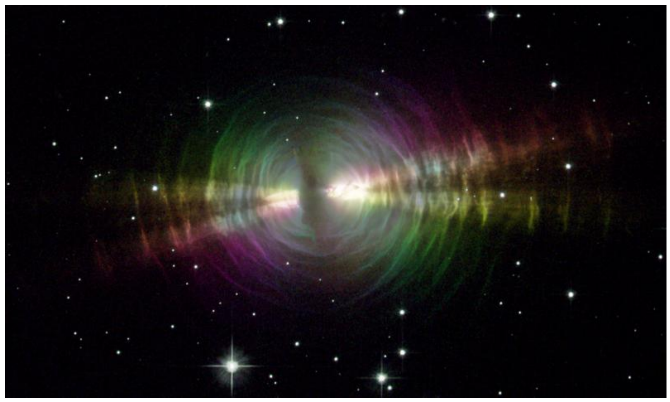
And I quoted the astronomer Bruce Balick on what will happen here when our Sun becomes a planetary nebula 6.9 billion years from now:

> *Here on Earth, we'll feel the wind of the ejected gases sweeping past, slowly at first (a mere 5 miles per second!), and then picking up speed as the spasms continue — eventually to reach 1000 miles per second!! The remnant Sun will rise as a dot of intense light, no larger than Venus, more brilliant than 100 present Suns, and an intensely hot blue-white color hotter than any welder's*

> *torch. Light from the fiendish blue "pinprick" will braise the Earth and tear apart its surface molecules and atoms. A new but very thin "atmosphere" of free electrons will form as the Earth's surface turns to dust.*

Eerie!

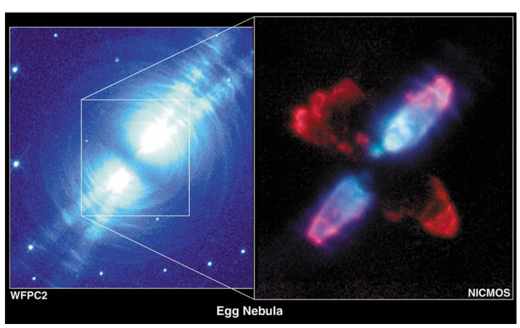
Here's a "protoplanetary nebula" — that is, a planetary nebula that's just getting started:



1) "Rainbow image of a dusty star", NASA, `http://hubblesite.org/newscenter/archive/releases/nebula/planetary/2003/09/`

It's called the "Egg Nebula". You can see layers of dust coming out puff after puff, shooting outwards at about 20 kilometers per second, stretching out for about a third of a light year. The colors — red, green and blue — aren't anything you'd actually see. They're just an easy way to depict three different polarizations of light. I don't know why the light is polarized that way.

You can see a dark disk of thicker dust running around the star. It could be an "accretion disk" spiralling into the star. The "beams" shining out left and right are still poorly understood. Maybe they're jets of matter ejected from the north and south poles of the disk? This idea seem more plausible when you look at this photo taken by NICMOS, which is Hubble's "Near Infrared Camera and Multi-Object Spectrometer":



2) Raghvendra Sahai, "Egg Nebula in polarized light", Hubble Heritage Project, `http://heritage.stsci.edu/2003/09/supplemental.html`

This near-infrared image also shows a bright spot called "Peak A" about 500 AU from the central star. An "AU", or astronomical unit, is the distance from the Sun to the Earth.

Nobody knows what this bright spot is. Some argue that it's just a clump of dust reflecting light from the main star. Others advocate a more exciting theory: it's a white dwarf orbiting the main star, which exploded in a "thermonuclear burst" after accreting a bunch of dust.

3) Joel H. Kastner and Noam Soker, "The Egg Nebula (AFGL 2688): deepening enigma", to appear in *Asymmetrical Planetary Nebulae III*, eds. M. Meixner, J. Kastner, N. Soker, and B. Balick, ASP Conference Series. Available as `astro-ph/0309677`.

I hope to say more about planetary nebulae in future Weeks, mainly because they're so beautiful.

But now: the field with one element!

A field is a mathematical structure where you can add, multiply, subtract and divide in ways that satisfy these familiar rules:

- $x + y = y + x$
- $(x + y) + z = x + (y + z)$
- $x + 0 = x$
- $xy = yx$
- $(xy)z = x(yz)$
- $1x = x$
- $x(y + z) = xy + xz$
- every element x has an element $-x$ with $x + (-x) = 0$
- every element x that's not 0 has an element $1/x$ with $x(1/x) = 1$

You'll note that the last clause is the odd man out. Addition, subtraction and multiplication can all be described as everywhere defined operations. Division cannot, since we can't divide by 0. This is the funny thing about fields, which is what causes the problem we'll run into.

Everyone who has studied math knows three examples of fields: the rational numbers $\mathbb{Q}$, the real numbers $\mathbb{R}$ and the complex numbers $\mathbb{C}$. There are a lot more, too — for example, function fields, number fields, and finite fields.

Let me say a tiny bit about these three kinds of fields.

The simplest sort of "function field" consists of rational functions of one variable — that is, ratios of polynomials, like this:

$$\frac{2z^3 + z + 1}{z^2 - 7}$$

Here the coefficients of your polynomials should lie in some field $\mathbb{F}$ you already know about. The resulting field is called $\mathbb{F}(z)$. If $\mathbb{F}$ is the complex numbers, we can think of $\mathbb{F}(z) = \mathbb{C}(z)$ as consisting of functions on the Riemann sphere. In "Week 201", I explained how the symmetries of this field form a group important in special relativity: the Lorentz group!

It's also very interesting to study the field of functions on a surface fancier than the sphere, but still defined by algebraic equations, like the surface of a doughnut or n-holed doughnut. Number theorists and algebraic geometers spend a lot of time thinking about these fields, which are called "function fields of complex curves".

For example, different ways of describing the surface of a doughnut by algebraic equations give different "elliptic curves". This is terminology is bound to puzzle beginners! They're called "curves" even though it's 2-dimensional, because it takes one *complex* number to say where you are on a little patch of a surface, just as it takes one *real* number to say where you are on an ordinary curve like a circle. That's the origin of the term "complex curve". And, they're called "elliptic" because they first showed up when people were studying elliptic integrals, which are generalizations of trig functions from circles to ellipses.

I explained more about elliptic curves in "Week 13" and "Week 125". Lurking behind this, there's a lot of fascinating stuff about function fields of elliptic curves.

The simplest sort of "number field" comes from taking the rational numbers and throwing in the solutions of a polynomial equation. For example, in "Week 20" I talked about the "golden field", which consists of all numbers of the form

$$a + b\sqrt{5}$$

where a and b are rational.

One of the most beautiful ideas in math is the analogy between number fields and function fields — the idea that numbers are like functions on some sort of "space". I began explaining this in "Week 205", "Week 216" and "Week 218", but there's much more to say about what's known... and also many things that remain mysterious.

In particular, it's pretty well understood how number fields resemble function fields of complex curves, and how this relates number theory to 2-*dimensional* topology. But, there are also many analogies between number theory and 3-*dimensional* topology, which I began listing in "Week 257". It seems these analogies are doomed to remain mysterious until we get a handle on the field with one element. But more on that later.

The simplest sort of "finite field" comes from choosing a prime number p and taking the integers modulo p. The result is sometimes called $\mathbb{Z}/p$, especially when you're just concerned with addition. But when you think of it as a field, it's better to call it $\mathbb{F}_p$.

The reason is that there's a finite field of size q whenever q is a *power* of a prime, and this field is unique — so it's called $\mathbb{F}_q$. You build $\mathbb{F}_q$ sort of like how you build the complex numbers starting from the real numbers, or number fields starting from the rational numbers. Namely, to construct $\mathbb{F}_{p^n}$, you take $\mathbb{F}_p$ and throw in the roots of a well-chosen polynomial of degree n: one that doesn't have any roots in $\mathbb{F}_p$, but "wants" to have n different roots.

Okay: that was a tiny bit about function fields, number fields and finite fields. But now I need to point out some slight lies I told!

I said there was a finite field with q elements whenever q was a prime power. You might think this should include $q = 1$, since 1 is the *zeroth* power of *any* prime.

So, is there a field with one element?

If so, it must have $1 = 0$. That doesn't violate the definition of a field that I gave you... does it? The definition said any element that's not 0 has a reciprocal. In this particular example, 0 also has a reciprocal, since we can set $1/0 = 1$ and not get into any contradictions. But that's not a problem: in usual math practice, saying "we can divide by anything that's not zero" doesn't deny the possibility that we can divide by 0.

Unfortunately, allowing a field with $1 = 0$ causes nothing but grief. For example, we can define vector spaces using any field (people say "over" any field), and there's a nice theorem saying two vector spaces are isomorphic if and only if they have the same dimension. And normally, there's one vector space of each dimension. But the last part isn't true for a field with $1 = 0$. In a vector space over such a field, every vector v has

$$v = 1v = 0v = 0$$

So, every vector space is 0-dimensional!

To prevent such problems, people add one extra clause to the definition of a field:

- 1 is not equal to 0

This clause looks even more tacked-on and silly than the clause saying everything *nonzero* has a reciprocal... but it works fairly well.

However, the field with one element still wants to exist! Not the silly field with $1 = 0$, but something else, something more mysterious... something that Gavin Wraith calls a "mathematical phantom":

4) Gavin Wraith, "Mathematical phantoms", `http://www.wra1th.plus.com/gcw/rants/math/MathPhant.html`

What's a mathematical phantom? According to Wraith, it's an object that doesn't exist within a given mathematical framework, but nonetheless "obtrudes its effects so convincingly that one is forced to concede a broader notion of existence".

Like a genie that talks its way out of a bottle, a sufficiently powerful mathematical phantom can talk us into letting it exist by promising to work wonders for us. Great examples include the number zero, irrational numbers, negative numbers, imaginary numbers, and quaternions. At one point all these were considered highly dubious entities. Now they're widely accepted. They "exist". Someday the field with one element will exist too!

Why?

I gave a lot of reasons in "Week 183", "Week 184", "Week 185", "Week 186" and "Week 187", but let me rapidly summarize.

It's all about "q-deformation". In physics, people talk about q-deformation when they're taking groups and turning them into "quantum groups". But it has a closely related aspect that's in some ways more fundamental. When we count things involving n-dimensional vector spaces over the finite field $\mathbb{F}_q$, we often get answers that are polynomials in q. If we then set $q = 1$, the resulting formulas count analogous things involving n-element sets!

So, finite sets want to be finite-dimensional vector spaces over the (nonexistent) field with one element... or something like that. We can be more precise after looking at some examples.

Here's the simplest example. Say we count lines through the origin in an n-dimensional vector space over $\mathbb{F}_q$. We get the "q-integer"

$$\frac{q^n-1}{q-1} = 1 + q + q^2 + \ldots + q^{n-1}$$

which I'll write as $[n]$ for short.

Setting $q = 1$, we get n. This is the number of points in an n-element set. Sure, that sounds silly. But, I'm trying to make a point here! At $q = 1$, stuff about n-dimensional vector spaces over $\mathbb{F}_q$ reduces to stuff about n-element sets, and the q-integer $[n]$ reduces to the ordinary integer n.

This may not be the best way to understand the pattern, though. Lines through the origin in an n-dimensional vector space are the same as points in an $(n-1)$-dimensional projective space. So, the real analogy may be between "points in a projective space" and "points in a set".

Here's a more impressive example. Pick any uncombed Young diagram D with n boxes. Here's one with 8 boxes:

□
□ □
□ □ □
□ □

This has

- 1 box in the first row,
- 3 boxes in the first two rows,
- 6 boxes in the first three rows,
- 8 boxes in the first four rows.

Then, count the "D-flags on an n-dimensional vector space over $\mathbb{F}_q$". In our example, such a D-flag is:

a 1-*dimensional subspace*
of a 3-*dimensional subspace*
of a 6-*dimensional subspace*
of a 8-*dimensional vector space over* $\mathbb{F}_q$

If you actually count these D-flags you'll get some formula, which is a polynomial in q. And when you set $q = 1$, you'll get the number of "D-flags on an n-element set". In our example, such a D-flag is:

a 1-*element subset*
of a 3-*element subset*
of a 6-*element subset*
of a 8-*element set*

For details, and a proof that this really works, try:

5) John Baez, "Lecture 4" in the *Geometric Representation Theory Seminar*, October 9, 2007. Available at `http://math.ucr.edu/home/baez/qg-fall2007/qg-fall2007.html#f07_4`

These examples can be generalized. In "Week 187" I showed how to get one example for each subset of the dots in any Dynkin diagram! This idea goes back to Jacques Tits, who was the first to suggest that there should be a field with one element. Dynkin diagrams give algebraic groups over $\mathbb{F}_q$... but he noticed that these groups reduce to "Coxeter groups" as $q \to 1$. And, if you mark some dots on a Dynkin diagram you get a "flag variety" on which your algebraic group acts... but as $q \to 1$, this reduces to a finite set on which your Coxeter group acts.

If you don't understand the previous paragraph, don't worry — it's over now. It's great stuff, but my main point is that there seems to be an analogy like this:

$q = 1$	$q = p^n$ for p prime
n-element set	$(n-1)$-dimensional projective space over $\mathbb{F}_q$
integer n	q-integer $[n]$
permutation groups S_n	projective special linear group $\mathrm{PSL}(n, \mathbb{F}_q)$
factorial $n!$	q-factorial $[n]!$

This opens up lots of questions. For example, if projective spaces over $\mathbb{F}_1$ are just finite sets, what should *vector spaces* over $\mathbb{F}_1$ be?

People have thought about this, and the answer seems to be "pointed sets" — sets with a distinguished point, which you can think of as the "origin". A pointed set with $n+1$ elements seems to act like an n-dimensional vector space over $\mathbb{F}_q$.

For more clues, and an attempt to do algebraic geometry using the field with one element, try this:

6) Christophe Soul, "On the field with one element", Talk given at the *Arbeitstagung*, Bonn, June 1999, IHES preprint available at `http://www.ihes.fr/\~soule/f1-soule.pdf`

Soul tries to define "algebraic varieties" over $\mathbb{F}_1$, namely curves and their higher-dimensional generalization. And, he talks a lot about zeta functions for such varieties. He goes into more detail here:

7) Christophe Soul, "Les varietes sur le corps a un element", *Moscow Math. Jour.* **4** (2004), 217–244, 312.

The theme of zeta functions — see "Week 216" — is deeply involved in this business. For more, try these papers:

8) N. Kurokawa, $Zeta functions over \mathbb{F}_1$, *Proc. Japan Acad. Ser. A Math. Sci.* **81** (2006), 180–184.

9) Anton Deitmar, "Remarks on zeta functions and K-theory over $\mathbb{F}_1$", available as `arXiv:math/0605429`.

But instead of talking about zeta functions, I'd like to talk about two approaches to giving a formal definition of the field with one element. Both of them involve taking the concept of field and modifying it so it doesn't necessary involve the operation of addition. The first one, due to Deitmar, simply throws out addition! The second, due to Nikolai Durov, allows for a wide choice of operations — and thus a wide supply of "exotic fields".

For Deitmar's approach, try these:

10) Anton Deitmar, "Schemes over $\mathbb{F}_1$", available as arXiv:math/0404185.

$\mathbb{F}_1$-schemes and toric varieties, available as arXiv:math/0608179.

The usual approach to fields treats fields as specially nice commutative rings. A "commutative ring" is a gadget where you can add and multiply, and these rules hold:

- $x + y = y + x$
- $(x + y) + z = x + (y + z)$
- $x + 0 = x$
- $xy = yx$
- $(xy)z = x(yz)$
- $1x = x$
- $x(y + z) = xy + xz$
- every element x has an element $-x$ with $x + (-x) = 0$

Deitmar throws out addition and treats fields as specially nice commutative monoids. A commutative "monoid" is a gadget where you can multiply, and these rules hold:

- $xy = yx$
- $(xy)z = x(yz)$
- $1x = x$

For Deitmar, the field with one element, $\mathbb{F}_1$, is just the commutative monoid with one element, namely 1. A "vector space over $\mathbb{F}_1$" is just a set on which this monoid acts via multiplication... but that amounts to just a plain old set. The "dimension" of such a "vector space" is just its cardinality.

All this so far is quite trivial, but Deitmar makes a nice attempt at redoing algebraic geometry to include this field with one element. One reason to do this is to understand the mysterious 3-dimensional aspect of number theory.

To explain this, I need to say a bit about "schemes". In ordinary algebraic geometry, we turn commutative rings into spaces to think about them geometrically. I explained this back in "Week 199" and "Week 205", but let me review quickly, and go further:

We can think of elements of a commutative ring R as functions on certain space called the "spectrum" of R, $\mathrm{Spec}(R)$. This space has a topology, so we can also talk about functions that are defined, not on all of $\mathrm{Spec}(R)$, but just *part* of $\mathrm{Spec}(R)$ — namely some open set. Indeed, for each open set U in $\mathrm{Spec}(R)$, there's a commutative ring $\mathcal{O}(U)$ consisting of those functions defined on U. These commutative rings are related in nice ways:

1. If the open set V is smaller than U, we can restrict functions from U to V, getting a ring homomorphism $\mathcal{O}(U) \to \mathrm{O}(V)$
2. If U is covered by a bunch of open sets U_i, and we have a function f_i in each $\mathcal{O}(U_i)$, such that f_i and f_j agree when restricted to the set $U_i \cap U_j$, then there's a unique function f in $\mathcal{O}(U)$ that restricts to each of these functions f_i.

Something satisfying condition 1 is called a "presheaf" of commutative rings; something also satisfying condition 2 is called a "sheaf" of commutative rings.

So, $\mathrm{Spec}(R)$ is not just a topological space, it's equipped with a sheaf of commutative rings. People call this a "ringed space".

Whenever we have a ringed space, we can ask if it comes from a commutative ring R in the way I just sketched. If so, we call it an "affine scheme". Affine schemes are just a fancy geometrical way of talking about commutative rings!

More interestingly, whenever we have a ringed space, we can ask if it's *locally* isomorphic to one coming from a commutative ring. In other words: does every point have a neighborhood that, as a ringed space, looks like $\mathrm{Spec}(R)$ for some commutative ring R? Or in other words: is our ringed space *locally* isomorphic to an affine scheme? If so, we call it a "scheme".

A classic example of a scheme that's not an affine scheme is the Riemann sphere. There aren't any rational functions defined on the whole Riemann sphere, except for constants — the rest all blow up somewhere. So, it's hopeless trying to think of the Riemann sphere as an affine scheme.

But, for any open set U in the Riemann sphere there's a commutative ring $\mathcal{O}(U)$ consisting of rational functions that are defined on U. So, the Riemann sphere becomes a ringed space. And, it's *locally* isomorphic to the complex plane, which is the affine scheme corresponding to the commutative ring of complex polynomials in one variable. So, the Riemann sphere is a scheme!

For more on schemes, try this nice introduction, which actually has lots of pictures:

11) David Eisenbud and Joe Harris, *The Geometry of Schemes*, Springer, Berlin, 2000.

Now, we can talk about schemes "over a field $\mathbb{F}$", meaning that each commutative ring $\mathcal{O}(U)$ is also a vector space over $\mathbb{F}$, in a well-behaved way, giving us a "sheaf of commutative rings over $\mathbb{F}$". For example, the Riemann sphere is a scheme over $\mathbb{C}$.

There's a secret 3-dimensional aspect to the affine scheme $\mathrm{Spec}(\mathbb{Z})$, where $\mathbb{Z}$ is the commutative ring of integers. As explained in the Addenda to "Week 257", we might understand this if we could see $\mathrm{Spec}(\mathbb{Z})$ as a scheme over the field with one element! For more, see this:

12) M. Kapranov and A. Smirnov, "Cohomology determinants and reciprocity laws: number field case", available at `http://wwwhomes.uni-bielefeld.de/triepe/F1.html`

So, we really need a theory of schemes over the field with one element. The problem is, $\mathbb{F}_1$ isn't really a field. In Deitmar's approach, it's just a commutative monoid.

So, let me sketch how Deitmar gets around this. In a nutshell, he takes advantage of the fact that a lot of basic algebraic geometry only requires multiplication, not addition!

He starts by defining a "commutative ring over $\mathbb{F}_1$" to be simply a commutative monoid. The simplest example is $\mathbb{F}_1$ itself.

Now, watch how he gets away with never using addition:

He defines an "ideal" in a commutative monoid R to be a subset I for which the product of something in I with anything in R again lies in I. He says an ideal P is "prime" if whenever a product of two elements in R is in P, at least one of them is in P.

He defines the "spectrum" $\mathrm{Spec}(R)$ of a commutative monoid R to be the set of its prime ideals. He gives this the "Zariski topology". That's the topology where the closed sets are the whole space, or any set of prime ideals that contain a given ideal.

He then shows how to get a sheaf of commutative monoids on $\mathrm{Spec}(R)$. He defines a "scheme" to be a space equipped with a sheaf of commutative monoids that's *locally* isomorphic to one of this sort.

If you know algebraic geometry, these definitions should seem very familiar. And if you don't, you can just replace the word "monoid" by "ring" everywhere in the previous three paragraphs, and you'll get the standard definitions in algebraic geometry!

Deitmar shows how to build a scheme over $\mathbb{F}_1$ called the "projective line". The projective line over $\mathbb{C}$ is just the Riemann sphere. The projective line over $\mathbb{F}_1$ has just two points (or more precisely, two closed points). This is good, because the projective line over the field with q elements has

$$[2] = 1 + q$$

points, and we're doing the $q = 1$ case.

Deitmar's construction seems like a lot of work to get ahold of the 2-element set, if that's all it secretly is. But, I need to think about this more. After all, he doesn't just get a space; he gets a sheaf of commutative monoids on this space! And what's that like? I should work it out.

Deitmar also shows how to relate schemes over $\mathbb{F}_1$ to the usual sort of schemes.

From a commutative ring, we can always get a commutative monoid just by forgetting the addition. This process has a kind of reverse, too. Namely, from a commutative monoid, we can get a commutative ring simply by taking formal integral linear combinations of elements. Using this, Deitmar shows how we can turn ordinary schemes into schemes over $\mathbb{F}_1$... and conversely. He says an ordinary scheme is "defined over $\mathbb{F}_1$" if it arises in this way from a scheme over $\mathbb{F}_1$.

Okay, that's a taste of Deitmar's approach. For Durov's approach, try this mammoth 568-page paper:

13) Nikolai Durov, "New approach to Arakelov geometry", available as `arXiv:0704.2030`.

or read our discussions of it at the n-Category Caf, starting here:

14) David Corfield, "The field with one element", `http://golem.ph.utexas.edu/category/2007/04/the_field_with_one_element.html`

Durov defines a "generalized ring" to be what Lawvere much earlier called an "algebraic theory". What is it? Nothing scary! It's just a gadget with a bunch of abstract n-ary operations closed under composition, permutation, duplication and deletion of arguments, and equipped with an identity operation.

So, for example, if our gadget has a binary operation

$$(x, y) \mapsto f(x, y)$$

we can compose this with itself to get the ternary operation

$$(x, y, z) \mapsto f(f(x, y), z)$$

and the 4-ary operation

$$(w, x, y, z) \mapsto f(w, f(x, f(y, z)))$$

and so on. We can then permute arguments in our 4-ary operation to get one like this:

$$(w, x, y, z) \mapsto f(z, f(x, f(w, y)))$$

or duplicate some arguments to get a binary operation like this

$$(x, y) \mapsto f(x, f(x, f(y, y)))$$

From this we can then form a 3-ary operation by deleting an argument, for example like this:

$$(x, y, z) \mapsto f(x, f(x, f(y, y)))$$

If you know about "operads", this kind of gadget is just a specially nice operad where we can duplicate and delete operations.

Now, a generalized ring is said to be "commutative" if all the operations commute in a certain sense. (I'll let you guess what it means for an n-ary operation to commute with an m-ary operation.) We get an example of a commutative generalized ring from a commutative ring R if we let the n-ary operations be "n-ary R-linear combinations", like this:

$$(x_1, \ldots, x_n) \mapsto r_1 x_1 + \ldots + r_n x_n$$

We also get a very similar example from any commutative "rig", which is a gizmo satisfying rules like those of a commutative ring, but without negatives:

- $x + y = y + x$
- $(x + y) + z = x + (y + z)$
- $x + 0 = x$
- $xy = yx$
- $(xy)z = x(yz)$
- $1x = x$
- $x(y + z) = xy + xz$

And, we get an example from any commutative monoid, where we only have 1-ary operations, coming from multiplication by elements of our monoid:

$$(x_1) \mapsto rx_1$$

So, Durov's framework generalizes Deitmar's! But, it includes a lot more examples: exotic hothouse flowers like the "tropical rig", the "real integers", and more. He develops a theory of schemes for all these generalized rings, and builds it "up to construction of algebraic K-theory, intersection theory and Chern classes" — fancy things that algebraic geometers like.

What I don't yet see is how either Deitmar's or Durov's approach helps us understand the secret 3-dimensional nature of $\mathrm{Spec}(\mathbb{Z})$. I may just need to read their papers more carefully and think about them more.

Finally, here's yet another approach to the field with one element:

15) Bertrand Toen and M. Vaquie, "Under $\mathrm{Spec}(\mathbb{Z})$", available as `arXiv:math/0509684`.

16) Shai Haran, "Non-additive geometry", *Composito Mathematica* **143** (2007), 613–638.

Toen describes interesting relations between algebra over $\mathbb{F}_1$ and stable homotopy theory. Haran even suggests that the Riemann Hypothesis could be proved if we understood enough about the geometry of schemes over $\mathbb{F}_1$! This is fascinating... I don't understand it, but I want to.

In short, a mathematical phantom is gradually taking solid form before our very eyes! In the process, a grand generalization of algebraic geometry is emerging, which enriches it to include some previously scorned entities: rigs, monoids and the like. And, this enrichment holds the promise of shedding light on some otherwise impenetrable mysteries: for example, the deep inner meaning of q-deformation, and the 3-dimensional nature of $\mathrm{Spec}(\mathbb{Z})$.

Addenda: I thank Thomas Riepe and David Corfield for drawing my attention to the paper by Shai Haran. Thomas Riepe also recommends the following online introduction to schemes:

17) Marc Levine, "Summer course in motivic homotopy theory", available at `http://www.math.neu.edu/~levine/publ/SummerSchoolAG.pdf`

Kevin Buzzard has a word of advice about the "generic point":

> > *We can think of elements of a commutative ring R as functions on certain space called the "spectrum" of R, $\mathrm{Spec}(R)$.*
>
> *So this is the set of all* prime *ideals of R, right? Not just the maximal ones? So...*
>
> > *So, the Riemann sphere is a scheme!*

> *Well, you have to throw in a mystical extra "generic point" if you really want to make it a scheme :-) Corresponding to the zero ideal. My impression is that most non-algebraic geometers think that the generic point is either confusing or just plain daft. But believe me, it's a* really good *idea! For decades in the literature in algebraic geometry people were using the word "generic" to mean "something that was true most of the time" — in fact a "generic point" is probably another really good example of a phantom! For example a meromorphic function on the Riemann sphere that wasn't zero would be "generically non-zero" to people like Borel and Weil, and if you asked them for a definition they would say that it just meant something like "the zero locus in the space had a smaller dimension than the whole space" or "the zero locus was nowhere dense in any component of the space" or something, and of course people could even make rigorous definitions that worked in particular cases and so on, but then Grothendieck came along with his "generic point", corresponding to the zero ideal [note to sub: check to see whether the idea was in the literature pre-Grothendieck!] and suddenly a function that was "generically non-zero" was just a function which was non-zero on the generic point! Such a cool way of doing it :-)*
>
> *Kevin*

If you get stuck on my puzzle "what does it mean for an n-ary operation to commute with an m-ary operation?", let me just show you what it means for a binary operation f to commute with a ternary operation g. It means:

$$g(f(x_1,x_2),f(x_3,x_4),f(x_5,x_6)) = f(g(x_1,x_2,x_3),g(x_4,x_5,x_6))$$

I hope this example gives away the general pattern.

If this is confusing, look at the case where we start with a ring R and take as our n-ary operations the "n-ary R-linear combinations"

$$(x_1,\ldots,x_n) \mapsto r_1x_1 + \ldots + r_nx_n$$

with r_i in R. Here an example of a binary operation is addition:

$$(x_1,x_2) \mapsto x_1 + x_2$$

while every unary operation is multiplication by some element of R:

$$x_1 \mapsto rx_1$$

To say "addition commutes with multiplication by an element of R" means that

$$r(x_1 + x_2) = rx_1 + rx_2$$

This is just the distributive law so it holds for any ring R.

But, for the unary operations to commute with each other, we need R to be commutative, since this says:

$$r(sx_1) = s(rx_1)$$

(In the calculations I just did, we can either think of the x_i as elements of a specific R-module, or more abstractly as "dummy variables" used to describe the ring R as a generalized ring in Durov's sense — what Lawvere calls an algebraic theory.)

For more discussion, go to the n-Category Caf.

The analogy between number fields and function fields finds a basic limitation with the lack of a ground field. One says that $\mathrm{Spec}(\mathbb{Z})$ (with a point at infinity added, as is familiar in Arakelov geometry) is like a (complete) curve, but over which field?

— *Christophe Soul*

Week 260

December 24, 2007

Since it's Christmas Eve, I thought I'd list some free books you can download. I'm a big fan of giving the world presents... and I'm not the only one.

But first, this week's nebulae! Here's one called the Retina:

1) "Retina Nebula", Hubble Heritage Project, `http://heritage.stsci.edu/2002/14/`

This is actually a tube of ionized gas about a quarter of a light-year across and one light-year long. It's a planetary nebula produced by a dying star. If you zoom in and look closely, you can see this star lurking in the middle, now a mere white dwarf.

The blue light is the most energetic, so it's really hot where you see blue. This blue light comes from singly ionized helium — helium where one electron has been knocked off. The green light is a bit less energetic: that's from doubly ionized oxygen. The red light comes from even cooler regions: that's from singly ionized nitrogen.

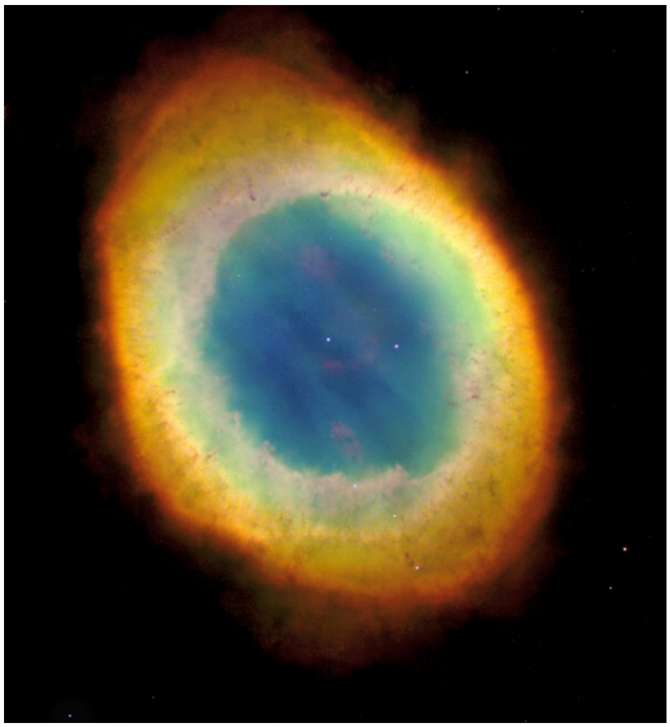
You can also see a lot of "dust lanes" in this photo. They're beautiful. And they're big! The width of each one is about 160 times the distance between the Sun and the Earth. The gas and dust in these lanes is about 1000 times higher than elsewhere. But what creates them?

Apparently, when the fast-moving glowing hot gas from the star crashes into the invisible gas in the surrounding interstellar space, the boundary gets sort of crumpled, and these dust lanes form. It's vaguely similar to the puffy surface of a cumulus cloud. But here the mechanism is different, because it involves a "shock wave": the hot gas is moving faster than the speed of sound as it hits the cold gas!

This effect is called a "Vishniac instability", since in 1983, the astrophysicist Ethan Vishniac showed that a shock wave moving in a sufficiently compressible medium would be subject to an instability of this sort, growing as the square root of time. I've never seen how Vishniac's calculations work, so the mathematics underlying this beautiful phenomenon will have to wait for another day.

Note that this planetary nebula, like the others I've shown you, is far from spherically symmetric. Astrophysicists used to pretend stars were spherically symmetric. But, that's a bad approximation whenever anything really exciting happens... just like in the old joke where the punchline is "consider a spherical cow".

As I said, the Retina Nebula is actually shaped like a tube. Viewed from either end, this tube would look very different — probably like the Ring Nebula:



2) "Ring Nebula", Hubble Heritage Project, `http://heritage.stsci.edu/1999/01/`

This is one light-year across. Again we see He II blue light with a wavelength of 4686 angstroms, then O III green light at 5007 angstroms, then N II red light at 6584 angstroms. You can also see the white dwarf as a tiny dot in the center; it's about 100,000 kelvin in temperature.

(In case you're wondering, an "angstrom" is an obsolete but popular unit of distance, equal to 10^{-10} meters. Just like the "parsec", it's a sign that astronomy is an old science. Anders Jonas ngstrm was one of the founders of spectroscopy, back around 1860. Archaic conventions may also explain why singly ionized helium is called "He II", and so on. Maybe the number zero hadn't fully caught on.)

Next: free books!

At least around here, Christmas seems to be all about buying stuff and giving it away. Giving is good. But I think gifts have more soul if you make them yourself. This is one of the great things about the internet: it lets us create things and give them to *everyone in the world* — or more precisely: everybody who wants them, and nobody who doesn't.

In this spirit, here's a roundup of free books on math and physics: gifts from their authors to you. There are lots out there. I'll only list a few. For more, try these sites:

3) George Cain, "Online Mathematics Textbooks", `http://www.math.gatech.edu/~cain/textbooks/onlinebooks.html`

4) "Free Online Mathematics Books", `http://www.pspxworld.com/book/mathematics/`

5) Alex Stefanov, "Textbooks in Mathematics", `http://users.ictp.it/~stefanov/mylist.html` or (with annoying ads, but more permanent) `http://us.geocities.com/alex_stef/mylist.html`

Despite its title, Stefanov's excellent site includes a lot of books on physics. I can't find lists *specifically* devoted to free physics books, but there are a lot out there — including a lot on the arXiv.

Anyway, let's dive in!

What if you're dying to learn physics, but don't know where to start? Start here:

6) "Physics Books Online", `http://www.sciencebooksonline.info/physics.html`.

You'll find plenty of free online books, starting from the basics and working up to advanced topics. But to dig deeper into these mysteries, you'll eventually need to learn a bunch of math. Do you remember what Victor Weisskopf said when a student asked how much math a physicist needs to know?

"More."

This can be scary when you're just getting started. What if you don't know calculus, for example?

Simple: learn calculus! This book is a classic — and it's free:

7) Gilbert Strang, *Calculus*, Wellesley-Cambridge Press, Cambridge, 1991. Also available at `http://ocw.mit.edu/ans7870/resources/Strang/strangtext.htm`

It really explains things clearly. I may use it the next time I teach calculus. We professors need to quit making our students buy expensive textbooks, and switch to free online books! We could join forces and make wiki textbooks that are a lot better and more flexible than the budget-busting, back-breaking mammoths we currently inflict on our kids. But there are already a lot of good texts available free online.

Or: what if you know calculus, but you're still swimming through the undergraduate sea of differential equations, Fourier transforms, matrices, vectors and tensors? Then this should be really helpful:

8) James Nearing, *Mathematical Tools for Physics*, available at `http://www.physics.miami.edu/~nearing/mathmethods/`

Unlike the usual dry and formal textbook, it reads like a friendly uncle explaining things in plain English, trying to cut through the red tape and tell you how to actually think about this stuff.

For example, on page 3 he introduces the hyperbolic trig functions:

> *Where do hyperbolic functions come from? If you have a mass in equilibrium, the total force on it is zero. If it's in* stable *equilibrium then if you push it a little to one side and release it, the force will push it back to the center. If it is* unstable *then when it's a bit to one side it will be pushed farther away from the equilibrium point. In the first case, it will oscillate about the equilibrium position and the function of time will be a circular trigonometric function — the common sines or cosines of time,* $A\cos(\omega t)$*. If the point is unstable, the motion will be described by hyperbolic functions of time,* $\sinh(\omega t)$ *instead of* $\sin(\omega t)$*. An ordinary ruler held at one end will swing back and forth, but if you try to balance it at the other end it will fall over. That's the difference between* $\cos$ *and* $\cosh$.

He goes into more detail later, after introducing the complex numbers. This book also features some great animations of Taylor series and Fourier series, like this movie of the Taylor series of the sine function.

There are free online books at all levels. . . so let's soar a bit higher. How about if you're a more advanced student trying to learn general relativity? Here you go:

9) Sean M. Carroll, *Lecture Notes on General Relativity*, available as `gr-qc/9712019`

How about quantum field theory? Then you're in luck — there are *two* detailed books available online:

10) Warren Siegel, *Fields*, available as `hep-th/9912205`

11) Mark Srednicki, *Quantum Field Theory*, Cambridge U. Press, Cambridge, 2007. Also available at `http://www.physics.ucsb.edu/~mark/qft.html`

Or what about algebraic topology? Again you're in luck, since you can read both Allen Hatcher's gentle introduction and Peter May's high-powered "concise course":

11) Allen Hatcher, *Algebraic Topology*, Cambridge U. Press, Cambridge, 2002. Also available at `http://www.math.cornell.edu/~hatcher/AT/ATpage.html`

12) Peter May, *A Concise Course in Algebraic Topology*, U. of Chicago Press, Chicago, 1999. Also available at `http://www.math.uchicago.edu/~may/CONCISE/ConciseRevised.pdf`

May has a lot of more advanced topology books available at his website, too — like this classic, where he used operads to solve important problems involving loop spaces:

13) Peter May, *The Geometry of Iterated Loop Spaces*, Lecture Notes in Mathematics **271**, Springer, Berlin, 1972. Also available at `http://www.math.uchicago.edu/~may/BOOKS/gils.pdf`

Or say you want to learn about vector bundles and how they show up in physics, from the basics all the way to fancy stuff like D-branes and K-theory? Try this — it's a great sequel to Husemoller's classic intro to fiber bundles:

14) Dale Husemoller, Michael Joachim, Branislav Jurco and Martin Schottenloher, *Basic Bundle Theory and K-Cohomology Invariants*, Lecture Notes in Physics **726**, Springer, Berlin, 2008. Also available at http://www.mathematik.uni-muenchen.de/~schotten Texte/978-3-540-74955-4_Book_LNP726.pdf

The list goes on and on! The American Mathematical Society will give you books for free if you prove that you're not a robot by solving a little puzzle:

15) American Mathematical Society, "Books Online By Subject", http://www.ams.org/online_bks/online_subject.html

Apparently they don't want robots learning advanced math and putting us professors out of business by teaching with more charisma and flair. (By the way: make sure to let them put cookies on your web browser, or they'll send you an endless succession of these puzzles, without explaining why!)

Since James Dolan and I plan to explain symmetric groups and their Hecke algebras in our online seminar, this particular book from the AMS caught my eye:

16) David M. Goldschmidt, *Group Characters, Symmetric Functions, and the Hecke Algebra*, AMS, Providence, Rhode Island, 1993. Also available as http://www.ams.org/online_bks/ulect4/

Since we're also struggling to understand the Langlands program, this looks good too:

17) Armand Borel, *Automorphic Forms, Representations, and L-functions*, AMS, 2 volumes, Providence, Rhode Island, 1979. Also available at http://www.ams.org/online_bks/pspum331/ and http://www.ams.org/online_bks/pspum332/

It's a serious collection of expository papers by bigshots like Borel, Cartier, Deligne, Jacquet, Knapp, Langlands, Lusztig, Tate, Tits, Zuckerman, and many more.

"Motives" are the mysterious virtual building blocks that algebraic varieties are built from. If you're ready to learn about motives — I'm not sure I am — try this:

18) Marc Levine, *Mixed Motives*, AMS, Providence, Rhode Island, 1998. Also available at http://www.ams.org/online_bks/surv57/

Or, if you're interested in using category theory to make analysis clearer and more beautiful, try this:

19) Andreas Kriegl and Peter W. Michor, *The Convenient Setting of Global Analysis*, AMS, Providence, Rhode Island, 1997. Also available at http://www.ams.org/online_bks/surv53/

The focus is on getting and working with a "convenient category" of infinite-dimensional manifolds. The idea of a "convenient category" goes back to topology: at some point, people realized they wanted this property to hold:

$$\mathcal{C}(X \times Y, Z) \cong \mathcal{C}(X, \mathcal{C}(Y, Z))$$

Here $\mathcal{C}(X, Y)$ is the space of maps from X to Y. So, the isomorphism above says that a map from $X \times YtoZ$ should correspond to a map from X to $\mathcal{C}(Y, Z)$. A category with this property is called "cartesian closed". While it may not be obvious why, this property is so wonderful that people threw out the category of topological spaces and continuous maps and replaced it with a slightly different one, just to get this to hold.

Another sort of "convenient category" for differential geometry uses infinitesimals. Again, you can learn about this in a free book:

20) Anders Kock, *Synthetic Differential Geometry*, Cambridge U. Press, Cambridge, 2006. Also available at `http://home.imf.au.dk/kock/`

This category is not just cartesian closed — it's a topos!

If you don't know what a topos is, never fear — more free books are coming to your rescue:

21) Robert Goldblatt, *Topoi, the Categorial Analysis of Logic*, Dover, 1983. Also available at `http://historical.library.cornell.edu/cgi-bin/cul.math/docviewer?did=Gold010`

22) Michael Barr and Charles Wells, *Toposes, Triples and Theories*, Springer, Berlin, 1983. Also available at `http://www.case.edu/artsci/math/wells/pub/ttt.html`

The first one is so gentle it makes a good introduction to category theory as a whole. The second scared the bejeezus out of me for a decade, but now I like it.

I like Jordan algebras, so I was also pleased to see this classic offered for free at the AMS website:

23) Nathan Jacobson, *Structure and Representations of Jordan Algebras*, AMS, Providence, Rhode Island, 1968. Also available at `http://www.ams.org/online_bks/coll39/`

Fans of exceptional Lie algebras will like the last two chapters, on "connections with Lie algebras" and "exceptional Jordan algebras".

Speaking of Lie algebras, I'd never seen this textbook before:

24) Shlomo Sternberg, *Lie Algebras*, `http://www.math.harvard.edu/~shlomo/docs/lie_algebras.pdf`

It's a somewhat quirky introduction, not for beginners I think, but it features some nice special topics: character formulas, the Kostant Dirac operator, and a detailed study of the center of the universal enveloping algebra.

This intro to Lie groups is also a bit quirky, but if you like Feynman diagrams or spin networks, it's irreplaceable:

25) Predrag Cvitanovic, *Birdtracks, Lie's, and Exceptional Groups*, available at `http://www.nbi.dk/GroupTheory/`

One of the great things about this book is that it classifies simple Lie groups according to their "skein relations" — properties of their representations, written out diagrammatically. In so doing, Cvitanovic realized that there's a "magic triangle" containing all the exceptional Lie groups. This subsumes the "magic square" of Freudenthal and Tits, which I discussed in "Week 145" and my octonion webpages.

This idea of Cvitanovic is closely related to the "exceptional series" of Lie groups — a pattern whose existence was conjectured by Deligne. I love the term "exceptional series". It's an oxymoron, since the exceptional groups were defined as those that don't fit into any series. But, it makes sense!

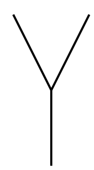
To see the exceptional series, it helps to do a mental backflip called "Tannaka-Krein duality", where you focus on the category of representations of the Lie group, instead of the group itself. Then, draw the morphisms in that category as diagrams, like Feynman diagrams! Then see what identities they satisfy. New patterns leap out: new series unify what had been "exceptions".

Very briefly, the idea goes like this. Suppose we have a Lie group G with Lie algebra L. The Lie bracket takes two elements x and y and spits out one element $[x, y]$, and it's linear in each variable, so it gives a linear operator

$$L \otimes L \to L$$

which is actually a morphism in the category of representations of G.

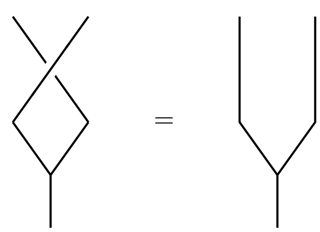
So, following the philosophy of Feynman diagrams, we can draw the bracket operation like this:



We can even use this to state the definition of a Lie algebra using diagrams! To say the bracket is antisymmetric:

$$[y, x] = -[x, y]$$

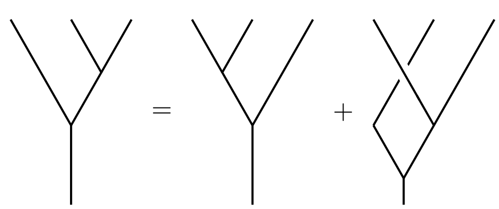
we just draw this:



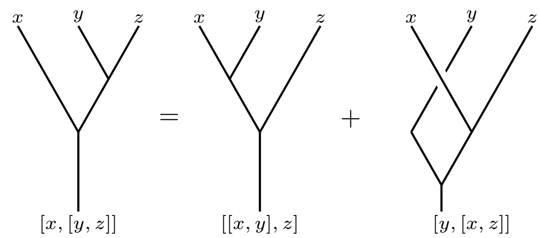
To say the Jacobi identity:

$$[x, [y, z]] = [[x, y], z] + [y, [x, z]]$$

we just draw this:



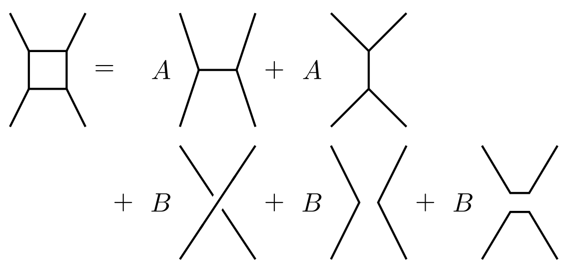
If that's too cryptic, maybe this will explain what I'm doing:



But in fact, people usually massage this picture to make it even more cryptic, and call it the "IHX" identity — since the three terms look like the letters I, H, and X by the time they're done twisting them around. For a good explanation, with pretty pictures, see:

26) Greg Muller, "Chord diagrams and Lie algebras", `http://cornellmath.wordpress.com/2007/12/25/chord-diagrams-and-lie-algebras/`

It then turns out that the exceptional Lie algebras F_4, E_6, E_7 and E_8 satisfy *yet another* identity:



for various choices of the constants A and B. So, they fit into a "series"!

I believe the main point of this identity, going back to Vogel's paper "Algebraic structures on modules of diagrams", is that for these Lie algebras, the square of the quadratic Casimir is the only degree-4 Casimir.

I think there's a lot more to be discovered here, in part by taking the gnarly computations people have done so far and making them more beautiful and conceptual. So, I urge all fans of exceptional mathematics, diagrams, and categories to look at these:

27) Pierre Deligne, "La serie exceptionnelle des groupes de Lie", *C. R. Acad. Sci. Paris Ser. I Math* **322** (1996), 321–326.

Pierre Deligne and R. de Man, "The exceptional series of Lie groups II", *C. R. Acad. Sci. Paris Ser. I Math* **323** (1996), 577–582.

Pierre Deligne and Benedict Gross, "On the exceptional series, and its descendants", *C. R. Acad. Sci. Paris Ser. I Math* **335** (2002), 877–881. Also available as `http://www.math.ias.edu/~phares/deligne/ExcepSeries.ps`

28) Pierre Vogel, "Algebraic structures on modules of diagrams", 1995. Available at `http://www.institut.math.jussieu.fr/~vogel/` or `http://citeseer.ist.psu.edu/469395.html`

"The universal Lie algebra", 1999. Available at http://www.institut.math.jussieu.fr/~vogel/

"Vassiliev theory and the universal Lie algebra", 2000. Available at http://www.institut.math.jussieu.fr/~vogel/

For a good overview, try this:

28) J. M. Landsberg and L. Manivel, "Representation theory and projective geometry", 2002. Available at arXiv:math/0203260.

Alas, they avoid drawing Feynman diagrams, though they talk about them in section 4. They prefer to use ideas from algebraic geometry:

29) J. M. Landsberg and L. Manivel, "The projective geometry of Freudenthal's magic square", *J. Algebra* **239** (2001), 477–512. Also available as arXiv:math/9908039.

J. M. Landsberg and L. Manivel, "Triality, exceptional Lie algebras and Deligne dimension formulas", *Adv. Math.* **171** (2002), 59–85. Also available as arXiv:math/0107032.

J. M. Landsberg and L. Manivel, "Series of Lie groups", available as arXiv:math/0203241.

Bruce Westbury, whom longtime readers of This Week's Finds will remember as John Barrett's collaborator, has also worked on this subject. He has pointed out that both the magic square and the magic triangle can be given an extra row and column if we introduce a 6-dimensional algebra halfway between the quaternions and the octonions:

30) Bruce Westbury, "Sextonions and the magic square", available as arXiv:math/0411428.

For even more references, try this:

31) Bruce Westbury, "References on series of Lie groups", http://www.mpim-bonn.mpg.de/digitalAssets/2763_references.pdf

This stuff has been on my mind recently, since I've been working on exceptional groups and grand unified theories with my student John Huerta. Also, my friend Tevian Dray has a student who just finished a thesis on a related topic:

32) Aaron Wangberg, "The structure of E_6", available as arXiv:0711.3447.

In a nutshell: E_6 is secretly $\mathrm{SL}(3, \mathbb{O})$. Octonions rock!
Happy holidays. Keep learning cool stuff.

Addenda: Thomas Riepe listed some more free online math books. Tony Smith pointed out something I already knew, but didn't make clear above: the idea that E_6 is secretly $\mathrm{SL}(3, \mathbb{O})$ is far from new.

Thomas wrote:

Some more links:

- *Milne's great collection (incl. the famous LNM 900), leading the reader from basic algebra through algebraic number theory, class fields, modular forms, arithmetic groups,... up to etale cohomology, Shimura varieties etc.*
- *Friedhelm Waldhausen's lectures on algebraic topology and K-theory.*
- *DML: Digital Mathematics Library*
- *G. Harder's math-links*
- *MSRI online books*

Finally:

> *"Nearly three and a half centuries of scientific study and achievement is now available online in the Royal Society Journals Digital Archive. This is the longest-running and arguably most influential journal archive in Science, including all the back articles of both Philosophical Transactions and Proceedings."*

Tony Smith wrote:

Thanks for an interesting list of stuff in week 260, but I have some questions about this:

> *32) Aaron Wangberg, "The structure of E_6", available as* `arXiv:0711.3447`.
>
> *In a nutshell: E_6 is secretly $\mathrm{SL}(3, \mathbb{O})$. Octonions rock!*

Not only from your brief list descrption, but also from reading the paper at pages 96 ff I get the impression that Wangberg is claiming the result $\mathrm{E}_6 = \mathrm{SL}(3, \mathbb{O})$. Do you get the same impression? I hope not, and I hope that my impression is somehow mistaken, because the result $\mathrm{E}_6 = \mathrm{SL}(3, \mathbb{O})$ is (and has been for some time) well known and in the literature. For example, in `hep-th/9309030` *Martin Cederwall and Christian R. Preitschopf said:*

> *... It should be possible to realize $\mathrm{E}_6 = \mathrm{SL}(3; \mathbb{O})$ [18,24] on them in a "spinor-like" manner, much like $\mathrm{SO}(10) = \mathrm{SL}(2; \mathbb{O})$ acts on its 16-dimensional spinor representations that play the role of homogeneous coordinates for $\mathbb{OP}^1$ ...*
>
> ...
>
> *18. H. Freudenthal,* Adv. Math. ***1*** *(1964) 145.*
>
> ...
>
> *24. A. Sudbery,* J. Phys. ***A17*** *(1984) 939. ....*

> *Although that Freudenthal Adv. Math. is listed as a reference in Wangberg's paper (as reference 5), I did not see the Sudbery paper listed, and I did not see the Freudenthal reference on page 96.*
>
> *Please don't misunderstand this message. I think that Wangberg's thesis is very interesting. I am just trying to get a correct historical record.*
>
> *Tony*
>
> *PS — In Sudbery's 1984 paper, he not only says (at page 950) "...* $\mathfrak{sl}(3, \mathbb{K})$ *... When* $\mathbb{K} = \mathbb{O}$*, this Lie algebra is a non-compact form of the exceptional Lie algebra* E_6*, the maximal compact subalgebra being* F_4 *..." but he goes on to say "...* $\mathfrak{sp}(6, \mathbb{K})$ *... when* $\mathbb{K} = \mathbb{O}$ *it is a non-compact form of* E_7*, the maximal compact subalgebra being* $\mathrm{E}_6 \oplus \mathfrak{so}(2)$*. ...".*

For more discussion, go to the n-Category Caf.

> If nature has made any one thing less susceptible than all others of exclusive property, it is the action of the thinking power called an idea, which an individual may exclusively possess as long as he keeps it to himself; but the moment it is divulged, it forces itself into the possession of every one, and the receiver cannot dispossess himself of it. Its peculiar character, too, is that no one possesses the less, because every other possesses the whole of it.
>
> — *Thomas Jefferson*

Week 261

March 19, 2008

Sorry for the long pause! I've been busy writing. For example: a gentle introduction to category theory, focusing on its role as a "Rosetta Stone" that helps us translate between four languages:

1) John Baez and Mike Stay, "Physics, topology, logic and computation: a Rosetta Stone", to appear in *New Structures in Physics*, ed. Bob Coecke. Available at `http://math.ucr.edu/home/baez/rosetta.pdf`

The idea is to take this chart and make it really precise:

Physics	**Topology**	**Logic**	**Computation**
Hilbert space	manifold	proposition	data type
operator	cobordism	proof	program

In each case we have a kind of "thing" and a kind of "process" going between things. But it turns out we can make the analogies much sharper and more detailed than that.

The hard work has already been done by many researchers. People working on topological quantum field theory have seen how cobordisms — spacetimes going from one slice of space to another — are analogous to operators between Hilbert spaces. The "Curry-Howard correspondence" makes the analogy between proofs and programs precise. Girard's work on "linear logic" sets up an analogy between operators and proofs. And so on. . . .

We're just trying to present these analogies in an easy-to-read form, all in one place. I hope that pondering them will help us break down some walls separating disciplines. In more optimistic moments, I even thnk they represent the first steps toward a general theory of systems and processes! Then I remember that scientists are trained to distrust such grand visions, and for good reasons. Time will tell.

But enough of that. This Week will be an ode to the number 3.

First, though… here's the nebula of the week!

2) "Hubble finds an hourglass nebula around a dying star", `http://hubblesite.org/newscenter/archive/releases/nebula/planetary/1996/07/`

It looks like the eye of Sauron in Tolkien's *Lord of the Rings* trilogy. It's not. It's a planetary nebula 8000 light years away, called MyCn 18 — or, more romantically, the Engraved Hourglass Nebula.

The colors look unreal. They are.

- Hα light is shown as as green, but it's actually red. This is the light hydrogen emits when its one electron jumps from its $n = 3$ state to its $n = 2$ state.
- N II light is shown as red, and it actually is. This is light from singly ionized nitrogen.
- O III light is shown as blue, but it's actually green. This is light from doubly ionized oxygen.
- Furthermore, the colors have been adjusted so that regions where Hα and O II overlap are orange.

Okay, so the colors are fake. But how did this weird nebula form? You can see a clue if you pay attention: the bright white dwarf star isn't located exactly at the center. It's a bit to the left! This paper, written by the folks who took the photograph, argues that it has an unseen companion:

3) Raghvendra Sahai et al, "The Etched Hourglass Nebula MyCn 18. I: Hubble space telescope observations", *The Astronomical Journal* **118** (1999), 468–476. Also

available at `http://www.iop.org/EJ/article/1538-3881/118/1/468/990080.text.html`

This paper tackles the difficult problem of modelling the nebula:

4) Raghvendra Sahai et al, "The Etched Hourglass Nebula MyCn 18. II: A spatio-kinematic model", *The Astronomical Journal* **110** (2000), 315–322. Also available at `http://www.iop.org/EJ/article/1538-3881/119/1/315/990248.text.html`

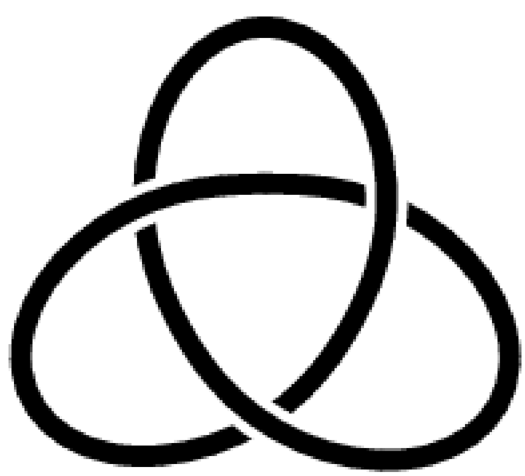
It doesn't seem that the white dwarf alone could have produced all the glowing gas we see here. A red giant companion could help. But, there are lots of mysteries.

That shouldn't be surprising. Even the simplest things can be quite rich in complexity if you look at them hard enough. I'll illustrate this with a little ode to the number 3. I'll start off slow, and ramp up to a discussion of how all these mathematical entities are locked in a tight embrace:

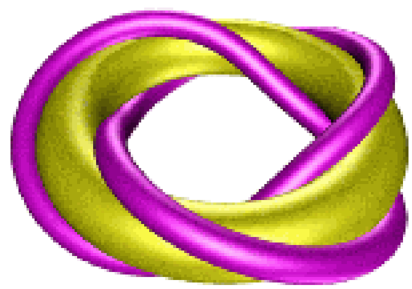
- the trefoil knot
- cubic polynomials
- the group of permutations of 3 things
- the three-strand braid group
- modular forms and cusp forms

As a kind of intermezzo, I'll talk about how to solve the cubic equation. We all learn about quadratic equations in school: they're the bread and butter of algebra, right after linear equations. Cubics are trickier, but studying them can give you a lifetime's worth of fun.

Let's start with the trefoil knot. This is the simplest of knots:



You can even draw it on the surface of a doughnut! Just take a pen and draw a curve that winds around your doughnut three time in one direction as it winds twice in the other direction:



5) Center for the Popularisation of Mathematics, "Torus knots", `http://www.popmath.org.uk/sculpmath/pagesm/torus2.html`

Mathematically, the surface of a doughnut is called a "torus". We can describe a point on the torus by two angles running from 0 to 2π — the "latitude" and "longitude". But another name for such an angle is a "point on the unit circle". If we think of the unit circle in the complex plane, this gives us a nice equation for the trefoil:

$$u^2 = v^3$$

Here u and v are complex numbers with absolute value 1. The equation says that as u moves around the unit circle, v moves around $2/3$ as fast. So, the set of solutions is a curve on the torus that winds around thrice in one direction while it winds around twice in the other direction — a trefoil knot!

We can also drop the restriction that u and v have absolute value 1. Then the equation $u^2 = v^3$ is famous for other reasons — it's related to cubic equations!

As you've probably heard, there's a formula for solving cubic equations, sort of like the quadratic formula, but bigger and badder. It goes back to some Italians in the 1500s who liked to challenge each other with equations and make bets on who could solve them: Scipione del Ferro, Niccolo Tartaglia and Gerolamo Cardano.

Imagine we're trying to solve a cubic equation. We can always divide by the coefficient of the cubic term, so it's enough to consider equations like this:

$$z^3 + Az^2 + Bz + C = 0$$

If we could solve this and find the roots a, b, and c, we could write it as:

$$(z - a)(z - b)(z - c) = 0$$

But this means

$$A = -(a + b + c)$$
$$B = ab + bc + ca$$
$$C = -abc$$

Note that A, B, and C don't change when we permute a, b, and c. So, they're called "symmetric polynomials" in the variables a, b, and c.

You see this directly, but there's also a better explanation: the coefficients of a polynomial depend on its roots, but they don't change when we permute the roots.

I can't resist mentioning a cool fact, which is deeply related to the trefoil: *every* symmetric polynomial of a, b, and c can be written as a polynomial in A, B, and C — and in a unique way!

In fact, this sort of thing works not just for cubics, but for polynomials of any degree. Take a general polynomial of degree n and write the coefficients as functions of the roots. Then these functions are symmetric polynomials, and *every* symmetric polynomial in n variables can be written as a polynomial of these — and in a unique way.

But, back to our cubic. Note that $-A/3$ is the average of the three roots. So, if we slide z over like this:

$$x = z + \frac{A}{3}$$

we get a new cubic equation for which the average of the three roots is zero. This new cubic equation will be of this form:

$$x^3 + Bx + C = 0$$

for some new numbers B and C. In other words, the "A" in this new cubic is zero, since we translated the roots to make their average zero.

So, to solve cubic equations, it's enough to solve cubics like $x^3 + Bx + C = 0$. This is a great simplification. When you first see it, it's really exciting. But then you realize you have no idea what to do next! This must be why it's called a "depressed cubic".

In fact, Scipione del Ferro figured out how to solve the "depressed cubic" shortly after 1500. So, you might think he could solve any cubic. But, *negative numbers hadn't been invented yet*. This prevented him from reducing any cubic to a depressed one!

It's sort of hilarious that Ferro was solving cubic equations before negative numbers were worked out. It should serve as a lesson: we mathematicians often work on fancy stuff before understanding the basics. Often that's why math seemss hard! But often it's impossible to discover the basics except by working on fancy stuff and getting stuck.

Here's one trick for solving the depressed cubic $x^3 + Bx + C = 0$. Write

$$x = y - \frac{B}{3y}$$

Plugging this in the cubic, you'll get a quadratic equation in y^3, which you can solve. From this you can figure out y, and then x.

Alas, I have no idea what this trick means. Does anyone know? Ferro and Tartaglia used a more long-winded method that seems just as sneaky. Later Lagrange solved the cubic yet another way. I like his way because it contains strong hints of Galois theory.

You can see all these methods here:

6) Wikipedia, "Cubic function", `http://en.wikipedia.org/wiki/Cubic_equation`.

So, I won't say more about solving the cubic now. Instead, I want to explain the "discriminant". This is a trick for telling when two roots of our cubic are equal. It turns out to be related to the trefoil knot.

For a quadratic equation $ax^2 + bx + c = 0$, the two roots are equal precisely when $b^2 - 4ac = 0$. That's why $b^2 - 4ac$ is called the "discriminant" of the quadratic. The same idea works for other equations; let's see how it goes for the cubic.

Suppose we were smart enough to find the roots of our cubic

$$x^3 + Bx + C = 0$$

and write it as

$$(x - a)(x - b)(x - c) = 0$$

Then two roots are equal precisely when

$$(a - b)(b - c)(c - a) = 0$$

The left side isn't a symmetric polynomial in a, b, and c; it changes sign whenever we switch two of these variables. But if we square it, we get a symmetric polynomial that does the same job:

$$D = (a - b)^2(b - c)^2(c - a)^2$$

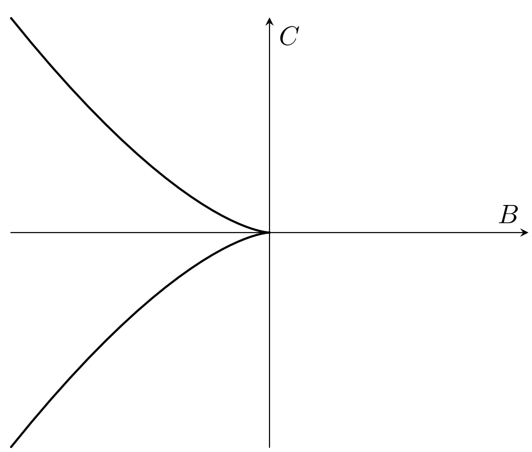
This is the discriminant of the cubic! By what I said about symmetric polynomials, it has to be a polynomial in B and C (since $A = 0$). If you sweat a while, you'll see

$$D = -4B^3 - 27C^2$$

So, here's the grand picture: we've got a 2-dimensional space of cubics with coordinates B and C. Sitting inside this 2d space is a curve consisting of "degenerate" cubics — cubics with two roots the same. This curve is called the "discriminant locus", since it's where the discriminant vanishes:

$$4B^3 + 27C^2 = 0$$

If we only consider the case where B and C are real, the discriminant locus looks like this:



It's smooth except at the origin, where it has a sharp point called a "cusp".

Now here's where the trefoil knot comes in. The equation for the discriminant locus:

$$4B^3 + 27C^2 = 0$$

should remind you of the equation for the trefoil:

$$u^2 = v^3$$

Indeed, after a linear change of variables they're the same! But, for the trefoil we need u and v to be *complex* numbers. We took them to be unit complex numbers, in fact.

So, the story is this: we've got a 2-dimensional *complex* space of complex cubics. Sitting inside it is a *complex* curve, the discriminant locus. In our new variables, it's this:

$$u^2 = v^3$$

If we intersect this discriminant locus with the torus

$$|u| = |v| = 1$$

we get a trefoil knot. But that's not all!

Normal folks think of knots as living in ordinary 3d space, but topologists often think of them as living in a 3-sphere: a sphere in 4d space. That's good for us. We can take this 4d space to be our 2d complex space of complex cubics! We can pick out spheres in this space by equations like this:

$$|u|^2 + |v|^3 = c \qquad (c > 0)$$

These are not round 3-spheres, thanks to that annoying third power. But, they're topologically 3-spheres. If we take any one of them and intersect it with our discriminant locus, we get a trefoil knot! This is clear when $c = 2$, since then we have

$$|u|^2 + |v|^3 = 2$$

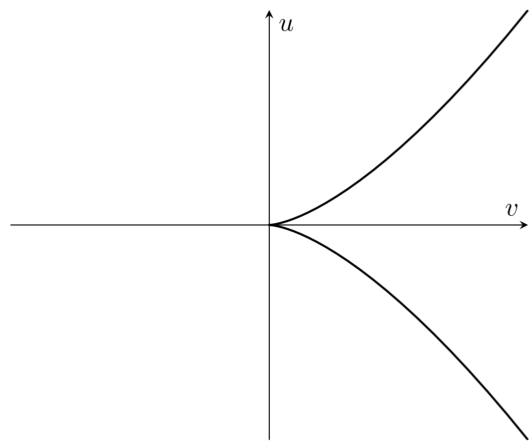
and

$$u^2 = v^3$$

which together imply

$$|u| = |v| = 1$$

But if you think about it, we also get a trefoil knot for any other $c > 0$. This trefoil shrinks as $c \to 0$, and at $c = 0$ it reduces to a single point, which is also the cusp here:



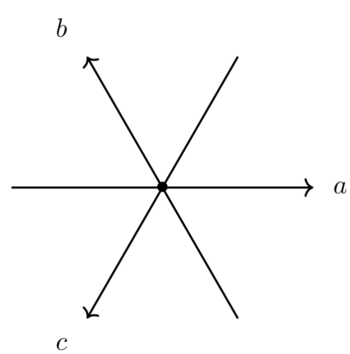
We don't see trefoil knots in this picture because it's just a real 2d slice of the complex 2d picture. But, they're lurking in the background!

Now let me say how the group of permutations of three things gets into the game. We've already seen the three things: they're the roots a, b, and c of our depressed cubic! So, they're three points on the complex plane that add to zero. Being a physicist at heart, I sometimes imagine them as three equal-mass planets, whose center of mass is at the origin.

The space of possible positions of these planets is a 2d complex vector space, since we can use any two of their positions as coordinates and define the third using the relation

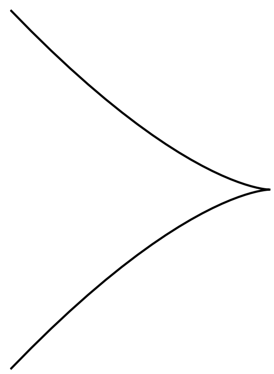
$$a + b + c = 0$$

So, there are three coordinate systems we can use: the (a, b) system, the (b, c) system and the (c, a) system. We can draw all three coordinate systems at once like this:



The group of permutations of 3 things acts on this picture by permuting the three axes. Beware: I've only drawn a 2-dimensional *real* vector space here, just a slice of the full 2d complex space.

Now suppose we take this 2d complex space and mod out by the permutation symmetries. What do we get? It turns out we get *another* 2d complex vector space! In this new space, the three coordinate axes shown above become just one thing. . . but this thing is a curve, like this:



Look familiar? Sure! It's just the discriminant locus we've seen before.

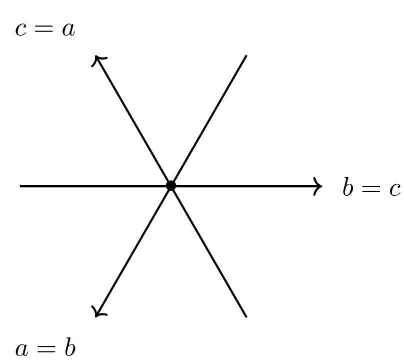
Why does it work this way? The explanation is sitting before us. We've got two 2d complex vector spaces: the space of possible *ordered triples of roots* of a depressed cubic, and the space of possible *coefficients*. There's a map from the first space to the second, since the coefficients are functions of the roots:

$$B = ab + bc + ca$$
$$C = -abc$$

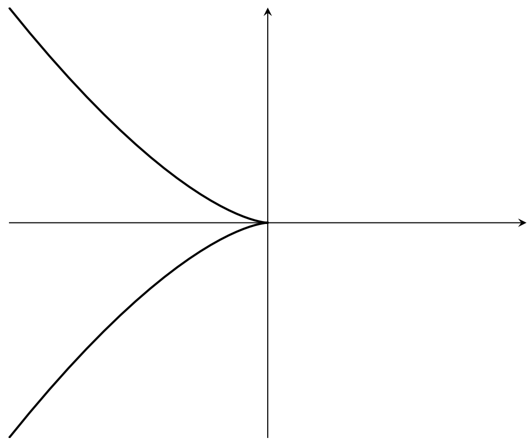
These functions are symmetric polynomials: they don't change when we permute a, b, and c. And, it follows from what I said earlier that we can get *any* symmetric polynomial as a function of these — under the assumption that $a + b + c = 0$, that is.

So, the map where we mod out by permutation symmetries of the roots is exactly the map from roots to coefficients.

The lines in this picture are places where two roots are equal:



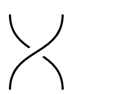
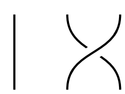
So, when we apply the map from roots to coefficients, these lines get mapped to the discriminant locus:



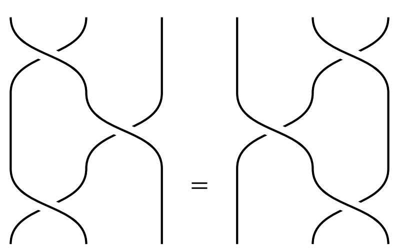
You should now feel happy and quit reading... unless you know a bit of topology. If you *do* know a little topology, here's a nice spinoff of what we've done. Though I didn't say it using so much jargon, we've already seen that space of nondegenerate depressed cubics is $\mathbb{C}^2$ minus a cone on the trefoil knot. So, the fundamental group of this space is the same as the fundamental group of S^3 minus a trefoil knot. This is a famous group: it has three generators x, y, z, and three relations saying that:

- x conjugated by y is z
- y conjugated by z is x
- z conjugated by x is y

On the other hand, we've seen this space is the space of triples of distinct points in the plane, centered at the origin, mod permutations. The condition "centered at the origin" doesn't affect the fundamental group. So, this fundamental group is another famous group: the "braid group on 3 strands". This has two generators:

$X =$  $Y =$ 

and one relation, called the "Yang-Baxter equation" or "third Reidemeister move":



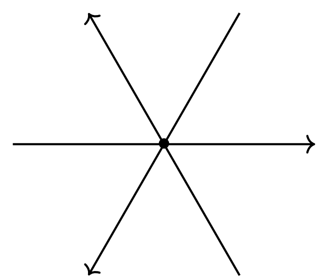
XYX $\quad$ YXY

So: the 3-strand braid group is *isomorphic* to the fundamental group of the complement of the trefoil! You may enjoy checking this algebraically, using generators and relations, and then figuring out how this algebraic proof relates to the geometrical proof.

I find all this stuff very pretty. . .

. . . but what's really *magnificent* is that most of it generalizes to any Dynkin diagram, or even any Coxeter diagram! (See "Week 62" for those.)

Yes, we've secretly been studying the Coxeter diagram A_2, whose "Coxeter group" is the group of permutations of 3 things, and whose "Weyl chambers" look like this:



Let me just sketch how we can generalize this to A_{n-1}. Here the Coxeter group is the group of permutations of n things, which I'll call $n!$.

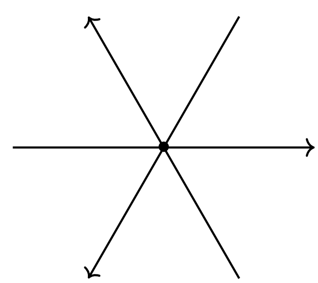
Let X be the space of n-tuples of complex numbers summing to 0. X is a complex vector space of dimension $n-1$. We can think of any point in X as the ordered n-tuple of roots of some depressed polynomial of degree n. Here "depressed" means that the leading coefficient is 1 and the sum of the roots is zero. This condition makes polynomials sad.

The permutation group $n!$ acts on X in an obvious way. The quotient $X/n!$ is isomorphic (as a variety) to another complex vector space of dimension $n-1$: namely, the space of depressed polynomials of degree n. The quotient map

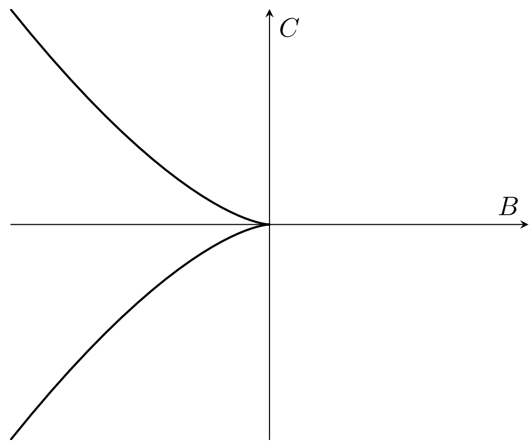
$$X \to X/n!$$

is just the map from roots to coefficients!

Sitting inside X is the set D consisting of n-tuples of roots where two or more roots are equal. D is the union of a bunch of hyperplanes, as we saw in our example:



Sitting inside $X/n!$ is the "discriminant locus" $D/n!$, consisting of *degenerate* depressed polynomials of degree n — that is, those with two or more roots equal. This is a variety that's smooth except for some sort of "cusp" at the origin:



The fundamental group of the complement of the discriminant locus is the braid group on n strands. The reason is that this group describes homotopy classes of ways that n points in the plane can move around and come back to where they were (but possibly permuted). These points are the roots of our polynomial.

On the other hand, the discriminant locus is topologically the cone on some higher-dimensional knot sitting inside the unit sphere in $\mathbb{C}^{n-1}$. So, the fundamental group of the complement of this knot is the braid group on n strands.

This relation between higher-dimensional knots and singularities was investigated by Milnor, not just for the A_n series of Coxeter diagrams but more generally:

7) John W. Milnor, *Singular Points of Complex Hypersurfaces*, Princeton U. Press, 1969.

The other Coxeter diagrams give generalizations of braid groups called Artin-Brieskorn groups. Algebraically you get them by taking the usual presentations of the Coxeter groups and dropping the relations saying the generators (reflections) square to 1.

If you like braid groups and Dynkin diagrams, Artin-Brieskorn groups are irresistible! For a fun modern account, try:

8) Daniel Allcock, "Braid pictures for Artin groups", available as arXiv:math.GT/9907194.

But I'm digressing! I must return and finish my ode to the number 3. I need to say how modular forms get into the game!

I'll pick up the pace a bit now — if you're tired, quit here.

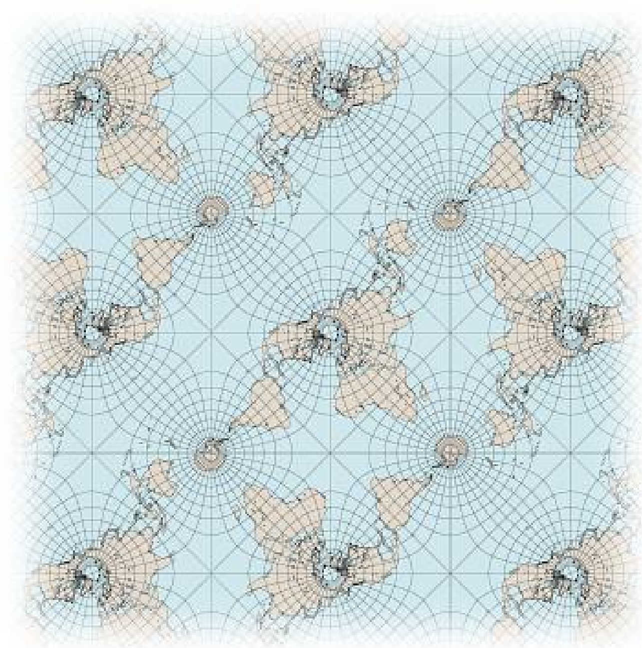
Any cubic polynomial $P(x)$ gives something called an "elliptic curve". This consists of all the complex solutions of

$$y^2 = P(x)$$

together with the point (∞, ∞), which we include to make things nicer.

Clearly this elliptic curve has two points (x, y) for each value of x *except* for $x = \infty$ and the roots of $P(x)$, where it just has one. So, it's a "branched double cover" of the Riemann sphere, with branch points at the roots of our cubic and the point at infinity.

In fact, this elliptic curve has the topology of a torus, at least when all the roots of our cubic are different. If you have trouble imagining a torus that's a branched double cover of a sphere, ponder this:



9) Carlos Furuti, "Peirce's quincuncial map", `http://www.progonos.com/furuti/MapProj/Normal/ProjConf/projConf.html`

This square map of the Earth is an unwrapped torus; each point of the Earth shows up lots of times. If we wrap it up just right, we get a branched double cover of the sphere! Can you spot the branch points? For a lot more explanation, read "Week 229".

Now, way back in "Week 13", I turned this story around. I started with a torus formed as the quotient of the complex plane by a lattice — and showed how to get an elliptic curve out of it. I wrote the equation for this elliptic curve in "Weierstrass form":

$$y^2 = 4x^3 - g_2x - g_3$$

By a simple change of variables, this is equivalent to a depressed cubic:

$$y^2 = x^3 + Bx + C$$

So, we can think of g_2 and g_3 as coordinates on our 2d space of depressed cubics! They're just rescaled versions of our coordinate functions B and C.

What's the big deal? Well, g_2 and g_3 are famous examples of "modular forms" — whatever those are. In fact, it's a famous fact that every modular form is a polynomial in g_2 and g_3.

I defined modular forms back in "Week 142", where I summarized the Taniyama-Shimura-Weil theorem: the big theorem about modular forms that implies Fermat's Last

Theorem. So, you can reread the definition there if you're curious. But if you've never seen it before, it's a bit intimidating. A modular form of weight w is a function on the space of lattices that transforms in a certain bizarre way, satisfying a certain growth condition. . . blah blah blah.

It's important stuff, and incredibly cool once you get a feel for it. But suppose we're trying to explain modular forms more simply. Then we can avoid a lot of technicalities if we just say a modular form is a polynomial on the space of depressed cubics! In other words, a polynomial in our friends B and C.

Then we can make some definitions. The "weight" of the modular form

$$B^i C^j$$

is $4i + 6j$. Okay, I admit this sounds arbitrary and weird without a lot more explanation. But better: a "cusp form" is a modular form that vanishes on the discriminant locus. Then we can see every cusp form is the product of the discriminant $4B^3 + 27C^2$ and some other modular form. . . and we can use this to work out lots of basic stuff about modular forms.

So, I hope you now see how tightly entwined all these ideas are:

- the trefoil knot
- cubic polynomials
- the group of permutations of 3 things
- the three-strand braid group
- modular forms and cusp forms

At this point I should give credit where credit is due. As usual, I've been talking to Jim Dolan, and many of these ideas come from him. But also, you can think of this Week as an expansion of the remarks by Joe Christy and Swiatowslaw Gal in the Addenda to "Week 233". And, it was Chris Hillman who first told Jim and me that $\mathrm{SL}(2,\mathbb{R})/\mathrm{SL}(2,\mathbb{Z})$ looks like S^3 minus a trefoil knot.

Finally, I should say that my low-budget approach to modular forms mostly just handles so-called "level 0" modular forms — the basic kind, defined using the group

$$\Gamma = \mathrm{PSL}(2,\mathbb{Z})$$

More exciting are modular forms that transform nicely only for a *subgroup* of Γ. Jim and I are just beginning to understand these. But the modular forms for $\Gamma(2)$ fit nicely into today's ode! Here $\Gamma(2)$ is the subgroup of Γ consisting of matrices congruent to the identity matrix $\mod 2$. What does this have to do with my ode to the number 3? Well,

$$\Gamma/\Gamma(2) \cong \mathrm{PSL}(2,\mathbb{F}_2)$$

and this is isomorphic to the group of permutations of 3 things!

So, as a final flourish, I claim that:

Modular forms for $\Gamma(2)$ are polynomials on the space X consisting of roots of depressed cubics:

$$X = \{(a,b,c) \mid a,b,c \text{ complex with } a+b+c=0\}$$

Modular forms for Γ are polynomials on the space $X/3!$ consisting of coefficients of depressed cubics:

$$X/3! = \{(B, C) \mid B, C \text{ complex}\}$$

The obvious quotient map $X \to X/3!$ sends roots to coefficients:

$$(a, b, c) \mapsto (B, C) = (ab + bc + ca, abc)$$

and this induces the inclusion of modular forms for Γ into modular forms for $\Gamma(2)$:

$$B \mapsto ab + bc + ca$$
$$C \mapsto abc$$

I hope this is all true!

Modular forms for $\Gamma(2)$ are particularly nice. A good example is the *cross-ratio*, much beloved in complex analysis. If you want to learn more about this stuff, try:

10) Igor V. Dolgachev, "Lectures on modular forms", Fall 1997/8, available at `http://www.math.lsa.umich.edu/~idolga/modular.pdf`

especially chapter 9 for level 2 modular forms. Also:

11) Henry McKean and Victor Moll, *Elliptic Curves: Function Theory, Geometry, Arithmetic*, Cambridge U. Press, 1999.

especially chapter 4.

Addendum: For more discussion, go to the n-Category Caf.

> It is difficult to give an idea of the vast extent of modern mathematics. The word "extent" is not the right one: I mean extent crowded with beautiful detail — not an extent of mere uniformity such as an objectless plain, but a tract of beautiful country to be rambled through and studied to every detail of hillside and valley, stream, rock, wood and flower.
>
> — *Arthur Cayley*

Week 262

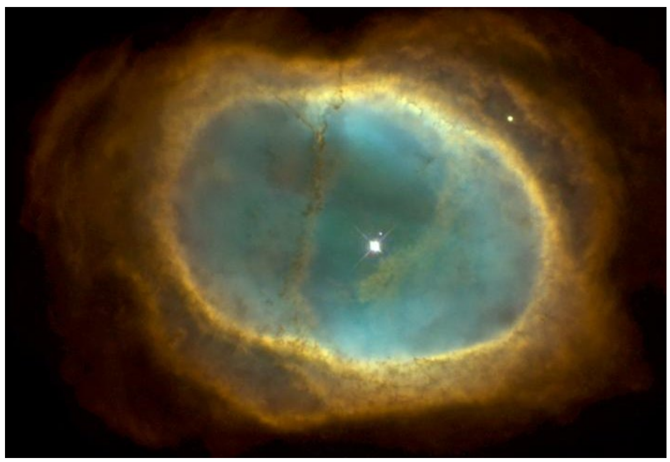
March 29, 2008

I'm done with teaching until fall, and now I'll be travelling a lot. I just got back from Singapore. It's an incredibly diverse place. I actually had to buy a book to understand all the foods! I'm now acquainted with the charms of appam, kaya toast, and babi buah keluak. But I didn't get around to trying a chendol, a bandung, or a Milo dinosaur, even though they're all available in every hawker center.

Today I'll talk about quantum technology in Singapore, atom chips, graphene transistors, nitrogen-vacancy pairs in diamonds, a new construction of $\mathfrak{e}_8$, and a categorification of quantum $\mathfrak{sl}(2)$.

But first — the astronomy pictures of the week!

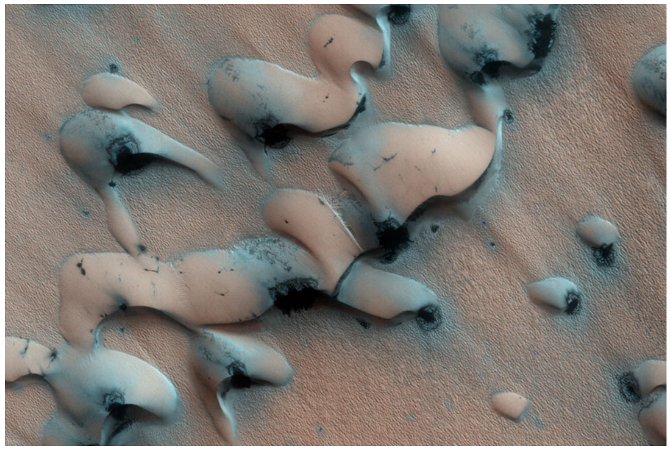
First another planetary nebula — the "Southern Ring Nebula":



1) Hubble Heritage Project, "Planetary Nebula NGC 3132", `http://heritage.stsci.edu/1998/39/index.html`

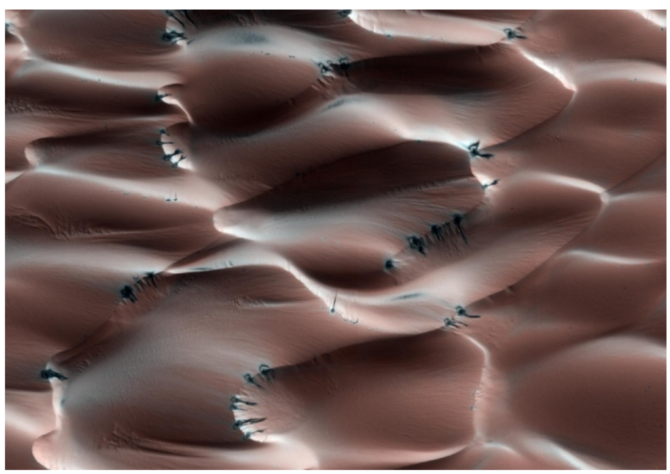
This bubble of hot gas is .4 light years in diameter. You can see *two* stars near its center. The faint one is the white dwarf remnant of the star that actually threw off the gas forming this nebula. The gas is expanding outwards at about 20 kilometers per second. The intense ultraviolet radiation from the white dwarf is ionizing this gas and making it glow.

The Southern Ring Nebula is 2000 light years from us. Much closer to home, here's a new shot of the frosty dunes of Mars:



2) HiRISE (High Resolution Imaging Science Experiment), "Defrosting polar sand dunes", http://hirise.lpl.arizona.edu/PSP_007043_2650

These horn-shaped dunes are called "barchans"; you can read more about them at "Week 228". The frost is carbon dioxide, evaporating as the springtime sun warms the north polar region. Here's another photo, taken in February:



3) HiRISE (High Resolution Imaging Science Experiment), "Defrosting northern dunes", http://hirise.lpl.arizona.edu/PSP_007193_2640

The dark stuff pouring down the steep slopes reminds me of water, but they say it's dust!

(If you click on these Mars photos, you'll get some amazing larger views.)

Meanwhile, down here on Earth, I had some good conversations with mathematicians and physicists at the National University of Singapore (NUS), and also with Artur Ekert and Valerio Scarani, who work here:

4) Centre for Quantum Technologies, `http://www.quantumlah.org/`

I like the name "quantumlah". "Lah" is perhaps the most famous word in Singlish: you put it at the end of a sentence for emphasis, to convey "acceptance, understanding, lightness, jest, and a medley of other positive feelings". Unfortunately I didn't get to hear much Singlish during my visit.

The Centre for Quantum Technologies is hosted by NUS but is somewhat independent. It reminds me a bit of the Institute for Quantum Computing — see "Week 235" — but it's smaller, and still getting started. They hope to take advantage of the nearby semiconductor fabrication plants, or "fabs", to build stuff.

They've got theorists and experimentalists. Being overly theoretical myself, I asked: what are the most interesting real-life working devices we're likely to see soon? Ekert mentioned "quantum repeaters" — gadgets that boost the power of a beam of entangled photons while still maintaining quantum coherence, as needed for long-distance quantum cryptography. He also mentioned "atom chips", which use tiny wires embedded in a silicon chip to trap and manipulate cold atoms on the chip's surface:

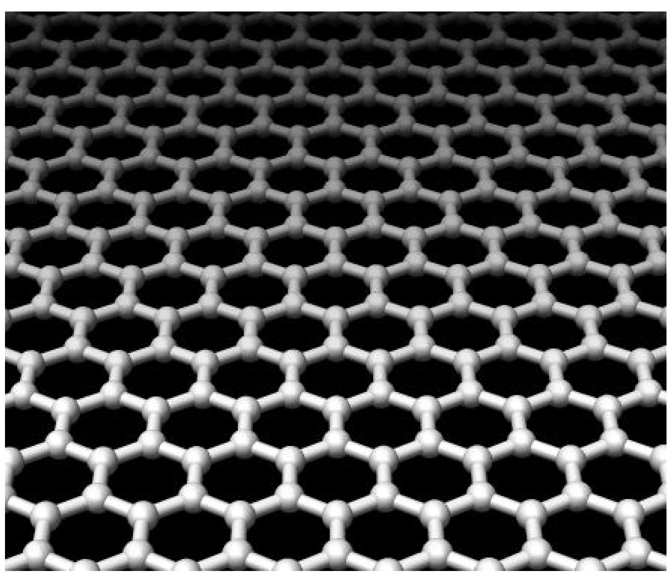
5) Atomchip Group, `http://www.atomchip.org/`

6) Atom Optics Group, Laboratoire Charles Fabry, "Atom-chip experiment", `http://atomoptic.iota.u-psud.fr/research/chip/chip.html`

There's also a nanotech group at NUS:

7) Nanoscience and Nanotechnology Initiative, National University of Singapore, `http://www.nusnni.nus.edu.sg/`

who are doing cool stuff with "graphene" — hexagonal sheets of carbon atoms, like individual layers of a graphite crystal:



Graphene is closely related to buckyballs (see "Week 79") and polycyclic aromatic hydrocarbons (see "Week 258").

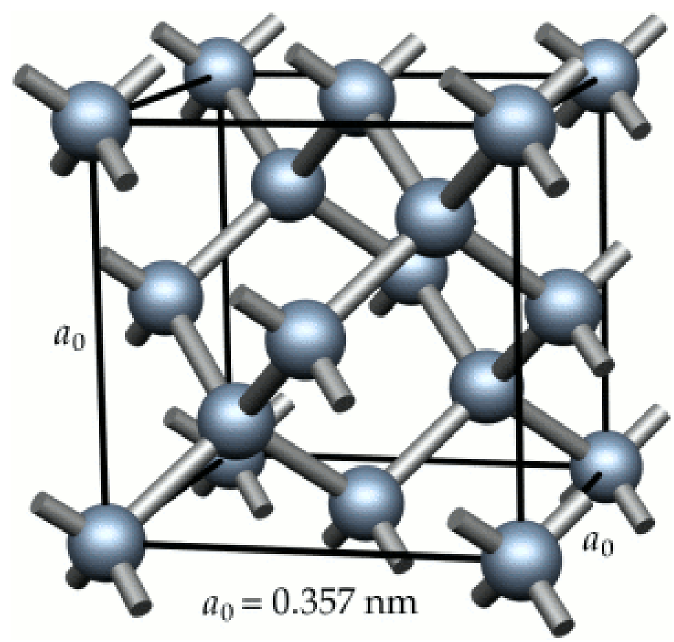
Some researchers believe that graphene transistors could operate in the terahertz range, about 1000 times faster than conventional silicon ones. The reason is that electrons move much faster through graphene. Unfortunately the difference in conductivity between the "on" and "off" states is less for graphene. This makes it harder to work with. People think they can solve this problem, though:

8) Kevin Bullis, "Graphene transistors", *Technology Review*, January 28, 2008, `http://www.technologyreview.com/Nanotech/20119/`

Duncan Graham-Rowe, "Better graphene transistors", *Technology Review*, March 17, 2008, `http://www.technologyreview.com/Nanotech/20424/`

Ekert also told me about another idea for carbon-based computers: "nitrogen-vacancy centers". These are very elegant entities. To understand them, it helps to know a bit about diamonds. You really just need to know that diamonds are crystals made of carbon. But I can't resist saying more, because the geometry of these crystals is fascinating.

A diamond is made of carbon atoms arranged in tetrahedra, which then form a cubical structure, like this:



9) Steve Sque, "Structure of diamond", `http://newton.ex.ac.uk/research/qsystems/people/sque/diamond/structure/`

Here you see 4 tetrahedra of carbon atoms inside a cube. Note that there's one carbon at each corner of the cube, and also one in the middle of each face. If that was all, we'd have a "face-centered cubic". But there are also 4 more carbons inside the cube — one at the center of each tetrahedron!

If you look really carefully, you can see that the full pattern consists of two interpenetrating face-centered cubic lattices, one offset relative to the other along the cube's main diagonal!

While the math of the diamond crystal is perfectly beautiful, nature doesn't always get it quite right. Sometimes a carbon atom will be missing. In fact, sometimes a cosmic ray will knock a carbon out of the lattice! You can also do it yourself with a beam of neutrons or electrons. The resulting hole is called a "vacancy". If you heat a diamond to about 900 kelvin, these vacancies start to move around like particles.

Diamonds also have impurities. The most common is nitrogen, which can form up 1% of a diamond. Nitrogen atoms can take the place of carbon atoms in the crystal. Sometimes these nitrogen atoms are isolated, sometimes they come in pairs.

When a lone nitrogen encounters a vacancy, they stick together! We then have a "nitrogen-vacancy center". It's also common for 4 nitrogens to surround a vacancy. Many other combinations are also possible — and when we get enough of these nitrogen-vacancy combinations around, they form larger structures called "platelets".

10) R. Jones and J. P. Goss, "Theory of aggregation of nitrogen in diamond", in *Properties, Growth and Application of Diamond*, eds. Maria Helena Nazare and A. J. Neves, EMIS Datareviews Series, 2001, 127–130.

A nice thing about nitrogen-vacancy centers is that they act like spin-1 particles. In fact, these spins interact very little with their environment, thanks to the remarkable properties of diamond. So, they might be a good way to store quantum information: they can last 50 microseconds before losing coherence, even at room temperature. If we could couple them to each other in interesting ways, maybe we could do some "spintronics", or even quantum computation:

11) Sankar das Sarma, "Spintronics", *American Scientist* **89** (2001), 516–523. Also available at `http://www.physics.umd.edu/cmtc/earlier_papers/AmSci.pdf`

Lone nitrogens are even more robust carriers of quantum information: their time to decoherence can be as much as a millisecond! The reason is that, unlike nitrogen-vacancy centers, lone nitrogens have "dark spins" — their spin doesn't interact much with light. But this can also makes them harder to manipulate. So, it may be easier to use nitrogen-vacancy centers. People are busy studying the options:

12) R. J. Epstein, F. M. Mendoza, Y. K. Kato and D. D. Awschalom, "Anisotropic interactions of a single spin and dark-spin spectroscopy in diamond", *Nature Physics* **1** (2005), 94–98. Also available as `arXiv:cond-mat/0507706`.

13) Ph. Tamarat et al, "The excited state structure of the nitrogen-vacancy center in diamond", available as `arXiv:cond-mat/0610357`.

14) R. Hanson, O. Gywat and D. D. Awschalom, "Room-temperature manipulation and decoherence of a single spin in diamond", *Phys. Rev.* **B74** (2006) 161203. Also available as `quant-ph/0608233`

But regardless of whether anyone can coax them into quantum computation, I like diamonds. Not to own — just to contemplate! I told you about the diamond rain on

Neptune back in "Week 160". And in "Week 193", I explained how diamonds are the closest thing to the E_8 lattice you're likely to see in this 3-dimensional world.

The reason is that in any dimension you can define a checkerboard lattice called D_n, consisting of all n-tuples of integers that sum to an even integer. Then you can define a set called D_n^+ by taking two copies of the D_n lattice: the original and another shifted by the vector $(1/2, \ldots, 1/2)$. D_8^+ is the E_8 lattice, but D_3 is the face-centered cubic, and D_3^+ is the pattern formed by carbons in a diamond!

In case you're wondering: in math, unlike crystallography, we reserve the term "lattice" for a discrete subgroup of $\mathbb{R}^n$ that's isomorphic to $\mathbb{Z}^n$. The set D_n^+ is only closed under addition when n is even. So, the carbons in a diamond don't form a lattice in the strict mathematical sense. On the other hand, the face-centered cubic really is a lattice, the D_3 lattice — and this is secretly the same as the A_3 lattice, familiar from stacking oranges. It's one of the densest ways to pack spheres, with a density of

$$\frac{\pi}{3\sqrt{2}} \approx 0.74.$$

The D_3^+ pattern, on the other hand, has a density of just

$$\frac{\pi\sqrt{3}}{16} \approx 0.34.$$

This is why ice becomes denser when it melts: it's packed in a close relative of the D_3^+ pattern, with an equally low density.

(Do diamonds become denser when they melt? Or do they always turn into graphite when they get hot enough, regardless of the pressure? Inquiring minds want to know. These days inquiring minds use search engines to answer questions like this... but right now I'd rather talk about E_8.)

As you probably noticed, Garrett Lisi stirred up quite a media sensation with his attempt to pack all known forces and particles into a theory based on the exceptional Lie group E_8:

15) Garrett Lisi, "An exceptionally simple theory of everything", available as `arXiv:0711.0770`

Part of his idea was to use Kostant's triality-based description of E_8 to explain the three generations of leptons — see "Week 253" for more. Unfortunately this part of the idea doesn't work, for purely group-theoretical reasons:

16) Jacques Distler, "A little group theory", `http://golem.ph.utexas.edu/~distler/blog/archives/001505.html`, "A little more group theory", `http://golem.ph.utexas.edu/~distler/blog/archives/001532.html`

There would also be vast problems trying get all the dimensionless constants in the Standard Model to pop out of such a scheme — or to stick them in somehow.

Meanwhile, Kostant has been doing new things with E_8. He's mainly been using the complex form of E_8, while Lisi needs a noncompact real form to get gravity into the game. So, the connection between their work is somewhat limited. Nonetheless, Kostant enjoys the idea of a theory of everything based on E_8.

He recently gave a talk here at UCR:

17) Bertram Kostant, 'On some mathematics in Garrett Lisi's "E_8 theory of everything"', February 12, 2008, UCR. Video and lecture notes at `http://math.ucr.edu/home/baez/kostant/`

He did some amazing things, like chop the 248-dimensional Lie algebra of E_8 into 31 Cartan subalgebras in a nice way, thus categorifying the factorization

$$248 = 8 \times 31$$

To do this, he used a copy of the 32-element group $(\mathbb{Z}/2)^5$ sitting in E_8, and the 31 nontrivial characters of this group.

Even more remarkably, this copy of $(\mathbb{Z}/2)^5$ sits inside a copy of $\mathrm{SL}(2, \mathbb{F}_{32})$ inside E_8, and the centralizer of a certain element of $\mathrm{SL}(2, \mathbb{F}_{32})$ is a product of two copies of the gauge group of the Standard Model! What this means — if anything — remains a mystery.

Indeed, pretty much everything about E_8 seems mysterious to me, since nobody has exhibited it as the symmetry group of anything more comprehensible than E_8 itself. This paper sheds some new light this puzzle:

17) Jos Miguel Figueroa-O'Farrill, "A geometric construction of the exceptional Lie algebras F_4 and E_8", available as `arXiv:0706.2829`.

The idea here is to build the Lie algebra of E_8 using Killing spinors on the unit sphere in 16 dimensions.

Okay — what's a Killing spinor?

Well, first I need to remind you about Killing vectors. Given a Riemannian manifold, a "Killing vector" is a vector field that generates a flow that preserves the metric! A transformation that preserves the metric is called an "isometry", and these form a Lie group. Killing vector fields form a Lie algebra if we use the ordinary Lie bracket of vector fields, and this is the Lie algebra of the group of isometries.

Now, if our manifold has a spin structure, a "Killing spinor" is a spinor field ψ such that

$$D_v \psi = k v \psi$$

for some constant k for every vector field v. Here $D_v \psi$ is the covariant derivative of ψ in the v direction, while $v\psi$ is defined using the action of vectors on spinors. Only the sign of the constant k really matters, since rescaling the metric rescales this constant.

It's a cute equation, but what's the point of it? Part of the point is this: the action of vectors on spinors

$$V \otimes S \to S$$

has a kind of adjoint

$$S \otimes S \to V$$

This lets us take a pair of spinor fields and form a vector field. This is what people mean when they say spinors are like the "square root" of vectors. And, if we do this to two *Killing* spinors, we get a *Killing* vector! You can prove this using that cute equation — and that's the main point of that equation, as far as I'm concerned.

Under good conditions, this fact lets us define a "Killing superalgebra" which has the Lie algebra of Killing vectors as its even part, and the Killing spinors as its odd part.

In this superalgebra, the bracket of two Killing vectors is just their ordinary Lie bracket. The bracket of a Killing vector and a Killing spinor is defined using a fairly obvious notion of the "Lie derivative of a spinor field". And, the bracket of two Killing spinors is defined using the map

$$S \otimes S \to V$$

which, as explained, gives a Killing vector.

Now, you might think our "Killing superalgebra" should be a Lie superalgebra. But in some dimensions, the map

$$S \otimes S \to V$$

is skew-symmetric. Then our Killing superalgebra has a chance at being a plain old Lie algebra! We still need to check the Jacobi identity. And this only works in certain special cases:

If you take S^7 with its usual round metric, the isometry group is $\mathrm{SO}(8)$, so the Lie algebra of Killing vectors is $\mathfrak{so}(8)$. There's an 8-dimensional space of Killing spinors, and the action of $\mathfrak{so}(8)$ on this gives the real left-handed spinor representation S_8^+. The Jacobi identity holds, and you get a Lie algebra structure on

$$\mathfrak{so}(8) \oplus S_8^+$$

But then, thanks to triality, you knock yourself on the head and say "I could have had a V_8!" After all, up to an outer automorphism of $\mathfrak{so}(8)$, the spinor representation S_8^+ is the same as the 8-dimensional vector representation V_8. So, your Lie algebra is the same as

$$\mathfrak{so}(8) \oplus V_8$$

with a certain obvious Lie algebra structure. This is just $\mathfrak{so}(9)$. So, it's nothing exceptional, though you arrived at it by a devious route.

If you take S^8 with its usual round metric, the Lie algebra of Killing vector fields is $\mathfrak{so}(9)$. Now there's a 16-dimensional space of Killing spinor fields, and the action of $\mathfrak{so}(9)$ on this gives the real (non-chiral) spinor representation S_9. The Jacobi identity holds, and you get a Lie algebra structure on

$$\mathfrak{so}(9) \oplus S_9$$

This gives the exceptional Lie algebra $\mathfrak{f}_4$!

Finally, if you take S^{15} with its usual round metric, the Lie algebra of Killing vector fields is $\mathfrak{so}(16)$. Now there's a 128-dimensional space of Killing spinor fields, and the action of $\mathfrak{so}(16)$ on this gives the left-handed real spinor representation S_{16}^+. The Jacobi identity holds, and you get a Lie algebra structure on

$$\mathfrak{so}(16) \oplus S_{16}^+$$

This gives the exceptional Lie algebra $\mathfrak{e}_8$!

In short, what Figueroa-O'Farrill has done is found a nice geometrical interpretation for some previously known algebraic constructions of $\mathfrak{f}_4$ and $\mathfrak{e}_8$. Unfortunately, he still needs to verify the Jacobi identity in the same brute-force way. It would be nice to find a slicker proof. But his new interpretation is suggestive: it raises a lot of new questions. He lists some of these at the end of the paper, and mentions a really big one at the beginning.

Namely: the spheres S^7, S^8 and S^{15} all show up in the Hopf fibration associated to the octonionic projective line:

$$S^7 \to S^{15} \to S^8$$

Does this give a nice relation between $\mathfrak{so}(9)$, $\mathfrak{f}_4$ and $\mathfrak{e}_8$? Can someone guess what this relation should be? Maybe $\mathfrak{e}_8$ is built from $\mathfrak{so}(9)$ and $\mathfrak{f}_4$ somehow.

I also wonder if there's a Killing superalgebra interpretation of the Lie algebra constructions

$$\mathfrak{e}_6 = \mathfrak{so}(10) \oplus S_{10} \oplus \mathfrak{u}(1)$$

and

$$\mathfrak{e}_7 = \mathfrak{so}(12) \oplus S_{12}^+ \oplus \mathfrak{su}(2)$$

These would need to be trickier, with the $\mathfrak{u}(1)$ showing up from the fact that S_{10} is a complex representation, and the $\mathfrak{su}(2)$ showing up from the fact that S_{12}^+ is a quaternionic representation. The algebra is explained here:

18) John Baez, *The octonions*, section 4.3: "the magic square", available at `http://math.ucr.edu/home/baez/octonions/node16.html`

A geometrical interepretation would be nice!

Finally — my former student Aaron Lauda has been working with Khovanov on categorifying quantum groups, and their work is starting to really take off. I'm just beginning to read his new papers, but I can't resist bringing them to your attention:

19) Aaron Lauda, "A categorification of quantum $\mathfrak{sl}(2)$", available as `arXiv:0803.3652`.

Aaron Lauda, "Categorified quantum $\mathfrak{sl}(2)$ and equivariant cohomology of iterated flag varieties", available as `arXiv:0803.3848`.

He's got a 2-*category* that decategorifies to give the quantized universal enveloping algebra of $\mathfrak{sl}(2)$! And similarly for all the irreps of this algebra!

There's more to come, too. . . .

Addenda: Starting this Week, you can see more discussion and also *questions I'm dying to know the answers to* over at the n-Category Caf. Whenever I write This Week's Finds, I come up with lots of questions. If you can help me with some of these, I'll be really grateful.

Jos Figueroa-O'Farrill sent an email saying:

> *About the geometric constructions of exceptional Lie algebras, you are totally spot on in that what is missing is a more conceptual understanding of the construction which would render the odd-odd-odd component of the Jacobi identity 'trivial', as is the case for the remaining three components. One satisfactory way to achieve this would be to understand of what in, say, the* 15*-sphere is* E_8 *the automorphisms. I'm afraid I don't have an answer.*
>
> *As for* E_6 *and* E_7*, there is a similar geometric construction for* E_6 *and one for* E_7 *is in the works as part of a paper with Hannu Rajaniemi, who was a*

student of mine. The construction is analogous, but for one thing. One has to construct more than just the Killing vectors out of the Killing spinors: in the case of E_6*, it is enough to construct a Killing* 0*-form (i.e., a constant) which then acts on the Killing spinors via a multiple of the Dirac operator. (This is consistent with the action of 'special Killing forms' a.k.a. 'Killing-Yano tensors' on spinors.) The odd-odd-odd Jacobi identity here is even more mysterious: it does not simply follow from representation theory (i.e., absence of invariants in the relevant representation where the 'jacobator' lives), but follows from an explicit calculation. The case of* E_7 *should work in a similar way, but we still have not finished the construction. (Hannu has a real job now and I've been busy with other projects of a less 'recreational' nature.) In*

20) *Jos Figueroa-O'Farrill, "A geometrical construction of exceptional Lie algebras", talk at Leeds, February 13, 2008, available at* `http://www.maths.ed.ac.uk/~jmf/CV/Seminars/Leeds.pdf`

you'll find the PDF version of a Keynote file I used for a geometry seminar I gave recently on this topic in Leeds. This geometric construction has its origin, as does the notion of Killing spinor itself, in the early supergravity literature. Much of the early literature on supergravity backgrounds was concerned with the so-called Freund-Rubin backgrounds: product geometries $L \times R$*, with* L *a lorentzian constant curvature spacetime and* R *a riemannian homogeneous space and the only nonzero components of the flux were proportional to the volume forms of* L *and/or* R*. For such backgrounds, supergravity Killing spinors, which are in bijective correspondence with the supersymmetries of a (bosonic) background, reduce to geometric Killing spinors.*

To any supersymmetric supergravity background one can associate a Lie superalgebra, called the Killing superalgebra. This is the superalgebra generated by the Killing spinors; that is, if we let $K = K_0 \oplus K_1$ *denote the Killing superalgebra, then*

$$K_1 = \{\textit{Killing spinors}\}$$

and

$$K_0 = [K_1, K_1]$$

This is a Lie superalgebra, due to the odd-odd Lie bracket being symmetric, as is typical in lorentzian signature in the physically interesting dimensions.

I gave a triangular seminar in London about this topic and you can find slides here:

21) *Jos Figueroa-O'Farrill, "Killing superalgebras in supergravity", talk at University of London, February 27, 2008, available at* `http://www.maths.ed.ac.uk/~jmf/CV/Seminars/KSA.pdf`

There is some overlap with the one in Leeds, but not too much.

Cheers, Jos

These comments by Thomas Fischbacher should also fit into the big picture somehow:

As you know, there is a nice triality symmetric construction of E_8 *that starts from* $\mathrm{SO}(8) \times \mathrm{SO}(8)$. *But, considering the maximally split real form* $\mathrm{E}_8(8)$, *did you also know that this* $\mathrm{SO}(8) \times \mathrm{SO}(8)$ *is best regarded as* $\mathrm{SO}(8, \mathbb{C}^+)$, *with* $\mathbb{C}^+$ *being the split-complex numbers with* $i^2 = +1$? *There also are* 56*-dimensional real subgroups such as* $\mathrm{SO}(8, \mathbb{C})$ *(2 different embeddings — "IIA" and "IIB") — and there also is* $\mathrm{SO}(8, \mathbb{C}0)$.

Basically, the way this works is that you can extend $\mathrm{SO}(8)\times\mathrm{SO}(8)$ *to $mathrm{SO}(16)$ or* $\mathrm{SO}(8, 8)$ *— depending on whether you add the* $V \times V$ *or* $S \times S$ 8×8*-block. But if you take diagonal* $\mathrm{SO}(8)$ *subgroups, then the* 8×8 *all split into* $28+35+1$, *and you can play nice games with these 28's. . .*

See:

22) T. Fischbacher, H. Nicolai and H. Samtleben, "Non-semisimple and complex gaugings of $N = 16$ *supergravity", available as* `hep-th/0306276`.

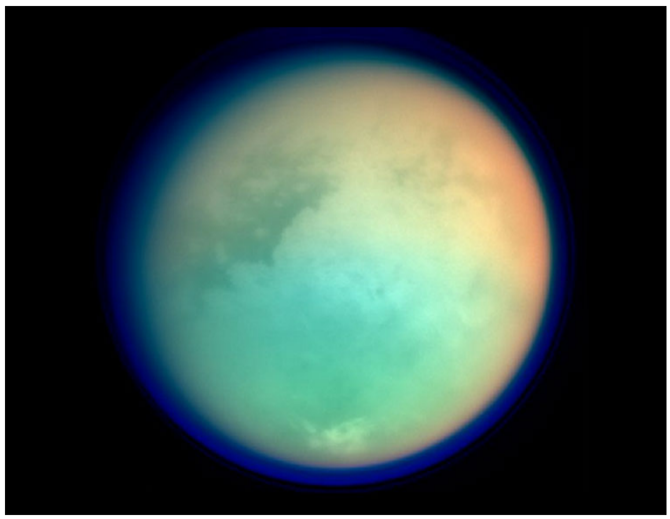
A knowledge of the existence of something we cannot penetrate, of the manifestations of the profoundest reason and the most radiant beauty, which are only accessible to our reason in their most elementary forms. It is this knowledge and this emotion that constitute the truly religious attitude; in this sense, and in this alone, I am a deeply religious man.

— *Albert Einstein*

Week 263

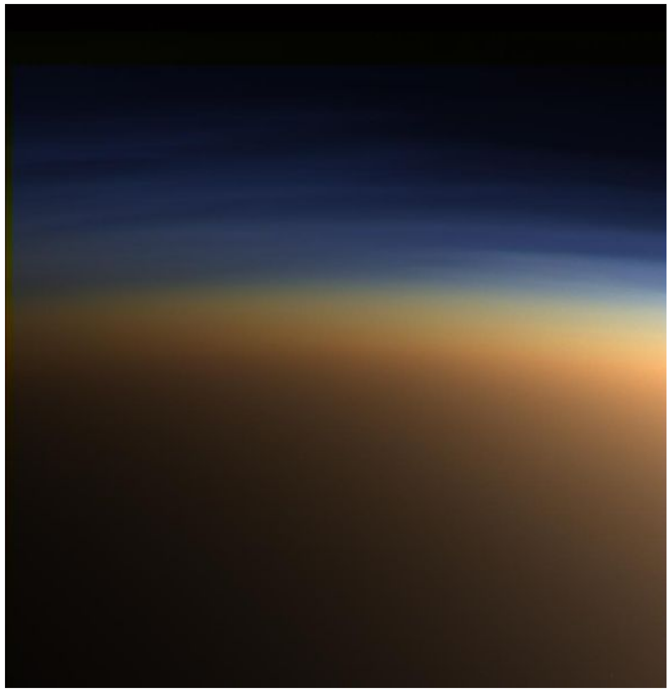
April 5, 2008

Enough nebulae! Today's astronomy picture is Saturn's moon Titan, photographed by the Cassini probe. Red and green represent methane absorption bands, while blue represents ultraviolet. Note the incredibly deep atmosphere — hundreds of kilometers deep. That's because Titan has a dense atmosphere but not much gravity. The pale feature in the center here is called Xanadu.



1) Astronomy Picture of the Day, "Tantalizing Titan", `http://apod.nasa.gov/apod/ap041028.html`

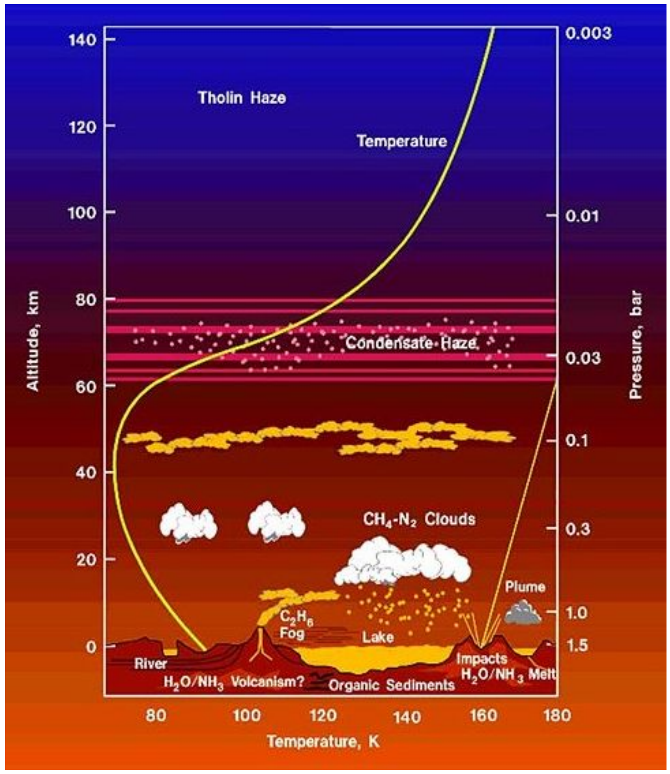
If you fell into Titan's atmosphere, here's what you'd see. Unlike the previous picture, this is in natural colors, taken by the Cassini probe on March 31st, 2005 from a distance of just 9,500 kilometers:



2) Wikipedia, "Titan's atmosphere", `http://en.wikipedia.org/wiki/Titan_(moon)#Atmosphere`

The orange stuff is hydrocarbon "smog", perhaps made of chemicals called tholins which I don't really understand. When you got further down the atmosphere would be so thick, and the gravity so low, that you could fly through it by flapping wings attached to your arms. Unfortunately the atmosphere would be very cold, and unbreathable: mostly nitrogen, with a little methane and ethane. (I wrote about the hydrocarbon rain on Titan back in "Week 160", and showed you the first pictures of its lakes in "Week

210".)



Astronomy is great, but today I want to talk about group theory. As you may have heard, John Thompson and Jacques Tits won the 2008 Abel prize for their work on groups:

3) Abel Prize, "2008 Laureates", `http://www.abelprisen.no/en/prisvinnere/2008/`

If you want a fun, nontechnical book that gives a good taste of the sort of things Thompson thought about, try this:

4) Marcus du Sautoy, *Symmetry: a Journey into the Patterns of Nature*, HarperCollins, 2008.

Mathematicians will enjoy this book for its many anecdotes about the heroes of symmetry, from Pythagoras to Thompson and other modern group theorists. Nonmathematicians will learn a lot about group theory in a fun easy way.

As a PhD student working under Saunders Mac Lane, Thompson began his career with a bang, by solving a 60-year-old conjecture posed by the famous group theorist Frobenius.

5) Mactutor History of Mathematics Archive, "John Griggs Thompson", `http://www-history.mcs.st-andrews.ac.uk/Biographies/Thompson_John.html`

But, he's mainly famous for helping prove an even harder theorem that's even simpler to state — one of those precious nuggets of knowledge that mathematicians fight so hard to establish:

"Every finite group with an odd number of elements is solvable."

We say a group is "solvable" if it can be built out of abelian groups in finitely many stages: the group at each stage mod the group at the previous stage must be abelian. The term "solvable" comes from Galois theory, since we can solve a polynomial equation using radicals iff its Galois group is solvable.

Way back in 1911, Burnside conjectured that every finite group with an odd number of elements is solvable. John Thompson and Walter Feit proved this in 1963. Their proof took all 255 pages of an issue of the Pacific Journal of Mathematics!

The proof has been simplified a bit since then, but not much. Versions can be found in two different books, and there is a project underway to verify it by computer:

6) Wikipedia, "Feit-Thompson Theorem", `http://en.wikipedia.org/wiki/Feit-Thompson_theorem`

This theorem, also called the "odd order theorem", marked a trend toward really long proofs in finite group theory, as part of a quest to classify finite "simple" groups. A simple group is one that has no nontrivial normal subgroups. In other words: there's no way to find a smaller group inside it, mod out by that, and get another smaller group. So, more loosely speaking, we can't build it up in several stages: it's a single-stage affair, a basic building block.

One reason finite simple groups are important is that *every* finite group can be built up in stages, where the group at each stage mod the group at the previous stage is a finite simple group. So, the finite simple groups are like the "prime numbers" or "atoms" of finite group theory.

The first analogy is nice because *abelian* finite simple groups practically *are* prime numbers. More precisely, every abelian finite simple group is $\mathbb{Z}/p$, the group of integers $\bmod p$, for some prime p. So, building a finite group from simple groups is a grand generalization of factoring a natural number into primes.

However, the second analogy is nice because just as you can build different molecules with the same collection of atoms, you can build different finite groups from the same finite simple groups.

I actually find a third analogy helpful. As I hinted, for any finite group we can find an increasing sequence of subgroups, starting with the trivial group and working on up to the whole group, such that each subgroup mod the previous one is a finite simple group. So, we're building our group as a "layer-cake" with these finite simple groups as "layers".

But: knowing the layers is not enough: each time we put on the next layer, we also need some "frosting" or "jam" to stick it on! Depending on what kind of frosting we use, we can get different cakes!

To complicate the analogy, stacking the layers in different orders can sometimes give the same cake. This is reminiscent of how multiplying prime numbers in different orders gives the same answer. But, unlike multiplying primes, we can't *always* build our layer cake in any order we like.

Apart from the order, though, the layers are uniquely determined — just as every natural number has a unique prime factorization. This fact is called the "Jordan-Hlder theorem", and these layer cakes are usually called "composition series". For more, try this:

7) Wikipedia, "Composition series", `http://en.wikipedia.org/wiki/Composition_series`

But let's see some examples!

Suppose we want to build a group out of just two layers, where each layer is the group of integers $\mod 3$, otherwise known as $\mathbb{Z}/3$. There are two ways to do this. One gives $\mathbb{Z}/3 \oplus \mathbb{Z}/3$, the group of pairs of integers $\mod 3$. The other gives $\mathbb{Z}/9$, the group of integers $\mod 9$.

We can think of $\mathbb{Z}/3 \oplus \mathbb{Z}/3$ as consisting of pairs of digits $0, 1, 2$ where we add each digit separately $\mod 3$. For example:

$$01 + 02 = 00$$
$$12 + 11 = 20$$
$$11 + 20 = 01$$

We can think of $\mathbb{Z}/9$ as consisting of pairs of digits $0, 1, 2$ where we add each digit $\mod 3$, but then carry a 1 from the 1's place to the 10's place when the sum of the digits in the 1's place exceeds 2 — just like you'd do when adding in base 3. I hope you remember your early math teachers saying "don't forget to carry a 1!" It's like that. For example:

$$01 + 02 = 10$$
$$12 + 11 = 00$$
$$11 + 20 = 01$$

So, the "frosting" or "jam" that we use to stick our two copies of $\mathbb{Z}/3$ together is the way we carry some information from one to the other when adding! If we do it trivially, not carrying at all, we get $\mathbb{Z}/3 \oplus \mathbb{Z}/3$. If we do it in a more interesting way we get $\mathbb{Z}/9$.

In fact, this how it always works when we build a layer cake of groups. The frosting at each stage tells us how to "carry" when we add. Suppose at some stage we've got some group G. Then we want to stick on another layer, some group H. An element of the resulting bigger group is just a pair (g, h). But we add these pairs like this:

$$(g, h) + (g', h') = (g + g' + c(h, h'), h + h')$$

where

$$c \colon H \times H \to G$$

tells us how to "carry" from the "H place" to the "G place" when we add. So, information percolates down when we add two guys in the new top layer of our group.

Of course, not any function c will give us a group: we need the group laws to hold, like the associative law. To make these hold, the function c needs to satisfy some equations. If it does, we call it a "2-cocycle".

These cocycles are studied in a subject called "group cohomology". Usually people focus on the simplest case, when our original group G is abelian, and its elements commute with everything in the big new group we're building. If this isn't true, we need something more general: *nonabelian* group cohomology, often called "Schreier theory" (see "Week 223").

I like this layer cake business because it's charming and it generalizes in two nice ways. First of all, it works for lots of algebraic gadgets besides groups. Second of all, it works for *categorified* versions of these gadgets.

For example, a group is a category with one object, all of whose morphisms are invertible. Similarly, an "n-group" is an n-category with one object, all of whose 1-morphisms, 2-morphisms and so on are invertible. We can build up n-groups as layer cakes where the layers are groups. It's a more elaborate version of what I just described — and it uses not just "2-cocycles" but also "3-cocycles" and so on. I never really understand group cohomology until I learned to see it this way.

But what's *really* cool is that n-groups can also be thought of as topological spaces. This lets us build every space as a "layer cake" where the layers are groups! These groups are called the "homotopy groups" of the space. The nth homotopy group keeps track of how many n-dimensional holes the space has — see "Week 102" for details.

But of course, they don't call the process of sticking these groups together a "layer cake": that would be too undignified. They call it a "Postnikov tower". And instead of "frosting", they speak of "Postnikov invariants". Every space is the union of a bunch of connected pieces, each of which is determined by its homotopy groups and its Postnikov invariants.

(At least this is true if you count spaces as the same when they're "weakly homotopy equivalent". This is a fairly sloppy equivalence relation beloved by homotopy theorists. You've probably heard how a topologist is someone who can't tell the difference between a doughnut and a coffee cup. Actually they can tell: they just don't care! A homotopy theorist is a more relaxed sort of guy who doesn't even care about the difference between a doughnut and a Moebius strip. They're both just fattened up versions of a circle.)

Mike Shulman and I tried to explain this layer cake business here:

8) John Baez and Michael Shulman, "Lectures on n-categories and cohomology", to appear in "n-Categories: Foundations and Applications", eds. John Baez and Peter May. Also available as `arXiv:math/0608420`

Whoops! I see I've drifted from my supposed topic — the work of John Thompson — to something I actually understand. It was a digression, but not a completely pointless one. From what I've told you, it follows that every space with finite homotopy groups can be built as a fancy "layer cake" made of finite simple groups.

And even better, the finite simple groups have now been classified! — we think. There are 18 infinite series of these groups, and also 26 exceptions called "sporadic" groups, ranging in size from the five Mathieu groups (see "Week 234") on up to the Monster (see "Week 20" and "Week 66").

9) Wikipedia, List of finite simple groups, `http://en.wikipedia.org/wiki/List_of_finite_simple_groups`

Proving that these are all the possibilities took mathematicians about 10,000 pages of work! The Feit-Thompson theorem is a small but crucial piece in this enormous pyramid of proofs. There could still be some mistakes here and there, but experts are busy working through the details more carefully.

Among the 26 sporadic groups, one is called the Thompson group. It was discovered by Thompson, and it's a subgroup of a version of the group E_8 defined over $\mathbb{F}_3$, the field with 3 elements. It has about 9×10^{16} elements, and it has a 248-dimensional representation over $\mathbb{F}_3$. I don't know much about it. I mention it just to show what crazy possibilities had to be considered to classify all finite simple groups — and how deeply Thompson was involved in this work.

But what about Jacques Tits?

10) Mactutor History of Mathematics Archive, "Jacques Tits", http://www-history.mcs.st-andrews.ac.uk/Biographies/Tits.html

He's not mentioned in du Sautoy's book "Symmetry", which is a pity, but not surprising, since too many mathematicians have studied group theory to fit comfortably in one story. He has a sporadic finite simple group named after him, but his work leaned in a different direction, more focused on the role of groups in geometry. He was an honorary member of Bourbaki, and in that role he helped awaken interest in the work of Coxeter.

I've mentioned his work on the "magic square" of exceptional Lie groups in "Week 145" and "Week 253"... but he's more famous for his work on "buildings", sometimes called "Bruhat-Tits buildings".

The subject of buildings has a reputation for being intimidating, perhaps because the *definition* of a building looks scary and unmotivated. You can read these and decide for yourself:

11) Wikipedia, "Building (mathematics)", http://en.wikipedia.org/wiki/Building_%28mathematics%29

12) Kenneth S. Brown, "What is a building?", *Notices AMS* **49** (2002), 1244–1245. Also available at http://www.ams.org/notices/200210/what-is.pdf

13) Paul Garrett, *Buildings and Classical Groups*, CRC Press, 1997. Preliminary version available at http://www.math.umn.edu/~garrett/m/buildings/

14) Kenneth S. Brown, *Buildings*, Springer, 1989.

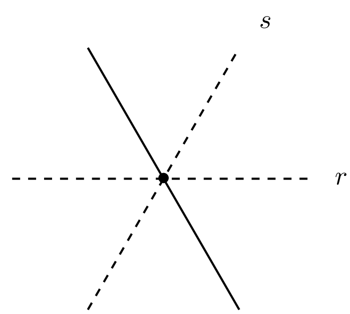
15) Mark Ronan, *Lectures on Buildings*, Academic Press, 1989.

Personally I found it a lot easier to start with *examples*.

So, start with any "finite reflection group" — a finite group of transformations of $\mathbb{R}^n$ that's generated by reflections. The possibilities have been completely worked out, and I listed them back in "Week 62". But let's do an easy one: the symmetry group of an equilateral triangle.

I can't resist mentioning that this group is also S_3, the group of all permutations of the three vertices of the triangle. In fact, this group was the star of "Week 261", where it showed up as the Galois group of the cubic equation! We can solve a cubic using radicals since this group is solvable. In other words, we can build this group as a "layer cake" from the abelian groups $\mathbb{Z}/3$ and $\mathbb{Z}/2$. The bottom layer is $\mathbb{Z}/3$, the subgroup of even permutations. The top layer is S_3 modulo the even permutations, namely $\mathbb{Z}/2$. Galois theory says you can solve a cubic by messing around a bit, then taking a square root, and then taking a cube root. Why a square root *first?* Because you build this sort of layer cake from the bottom up, but you eat it from the top down, slicing off one layer at a time.

But now we want to think about how this group is generated by reflections. You can use just two, for example the reflections across the mirrors labelled r and s here:



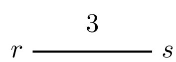
Let's call these reflections r and s. They clearly satisfy

$$r^2 = s^2 = 1$$

but since the mirrors are at an angle of $\pi/3$ from each other, they also satisfy

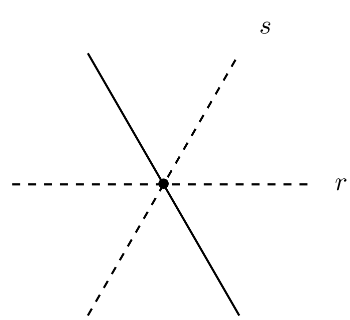
$$(rs)^3 = 1$$

This gives a presesentation of our group S_3. We can summarize this presentation with a little "Coxeter diagram":

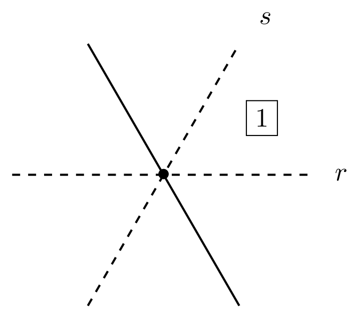


where the dots r and s are the generators, and the edge labelled "3" is the interesting relation $(rs)^3 = 1$. I explained these diagrams more carefully back in "Week 62". If you know about Dynkin diagrams, these are pretty similar — see "Week 63" and "Week 64" for details.

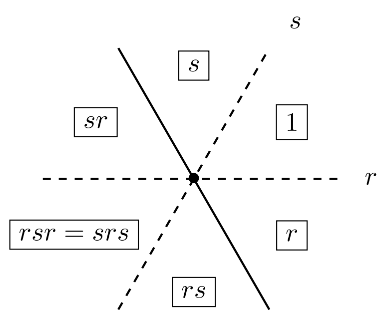
Note that the mirrors in this picture:



chop the plane into 6 "chambers", and the group S_3 has 6 elements. This is no coincidence — it works like this for any finite reflection group! We can pick any chamber as our favorite and label it “1”:



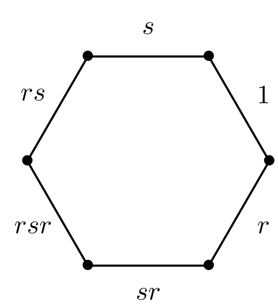
Then, we can label any other chamber by the unique element of our group that carries our favorite chamber to that one:



If we start with chamber 1 and keep reflecting across mirrors, we keep getting products of more and more generators until we reach the diametrically opposite chamber, which corresponds to the so-called “long word” in our finite reflection group. In this case, the long word is $rsr = srs$.

(Fanatical devotees will also note that this equation is the “Yang-Baxter equation” mentioned in “Week 261”.)

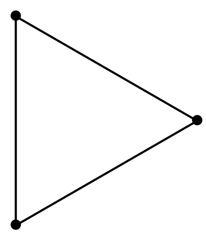
Now, Coxeter thought about all this stuff, and he realized that it was nice to introduce a polytope with one face for each chamber — in this case, just a hexagon:



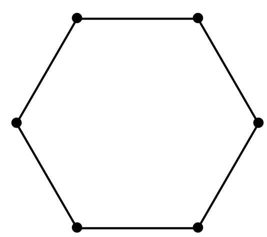
This is called the “Coxeter complex” of our finite reflection group. Our finite reflection group acts on it, and it acts on the faces in a free and transitive way.

But, you'll note we started with the symmetry group of an equilateral triangle, and wound up with a hexagon! What happened?

The quick way to say it is this: combinatorially speaking, the hexagon is the "barycentric subdivision" of our original triangle. Not the inside of the triangle — just its surface, or boundary! The boundary of the triangle is a simplicial complex made of 3 vertices and 3 edges:



so if we barycentrically subdivide it, we get 6 vertices and 6 edges:

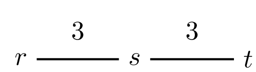


and that's our hexagon — drawn puffed out a bit, just for the sake of prettiness.

If this seems bizarre — and it probably does, given how lousy these pictures are — I urge you to try the next example on your own. Take the symmetry group of the regular tetrahedron, also known as S_4, the group of permutations of 4 things. Show it's generated by three reflections r, s, t with relations

$$
\begin{aligned}
r^2 = s^2 = t^2 &= 1 \\
(rs)^3 = (st)^3 &= 1 \\
rt &= tr
\end{aligned}
$$

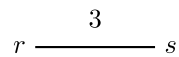
We can summarize these with the following Coxeter diagram:



Draw all mirrors corresponding to reflections in S_4, and show they chop 3d space into 24 chambers, one for each element of S_4. Then, barycentrically subdivide the boundary of the tetrahedron and check that the resulting "Coxeter complex" has 24 faces, one inside each chamber.

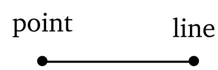
Anyway, one thing Tits did is realize how these Coxeter complexes show up in the geometry of the *Lie groups*, or more generally *algebraic groups*, associated to Dynkin diagrams.

For example, if I take this guy:

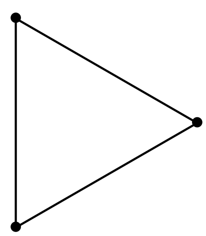


and remove some of the labels, I get the so-called A_2 Dynkin diagram:

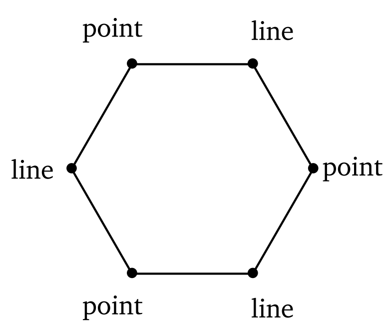
which corresponds to the Lie group $\mathrm{PSL}(3)$. And, this is the group of symmetries of projective plane geometry! Each dot in the Dynkin corresponds to a "type of figure":



and the edge corresponds to an "incidence relation": in projective plane geometry, a point can lie on a line. This shape, which we've seen before:



is then revealed to stand for a configuration of 3 points and 3 lines, satisfying incidence relations obvious from the picture. To put points and lines on an equal footing, we switch to the the Coxeter complex:



where now the vertices represent "figures" and the edges represent "incidence relations". It turns out that inside any projective plane, we can find lots of configurations like this: 3 points and 3 lines, each pair of points lying on one of the lines, and each pair of lines intersecting in a point. Such a configuration is called an "apartment".

If we take all the apartments coming from a projective plane, they form a simplicial complex called a "building". And this generalizes to any geometry corresponding to any sort of Dynkin diagram. The building knows everything about the geometry: all the figures, all the incidence relations.

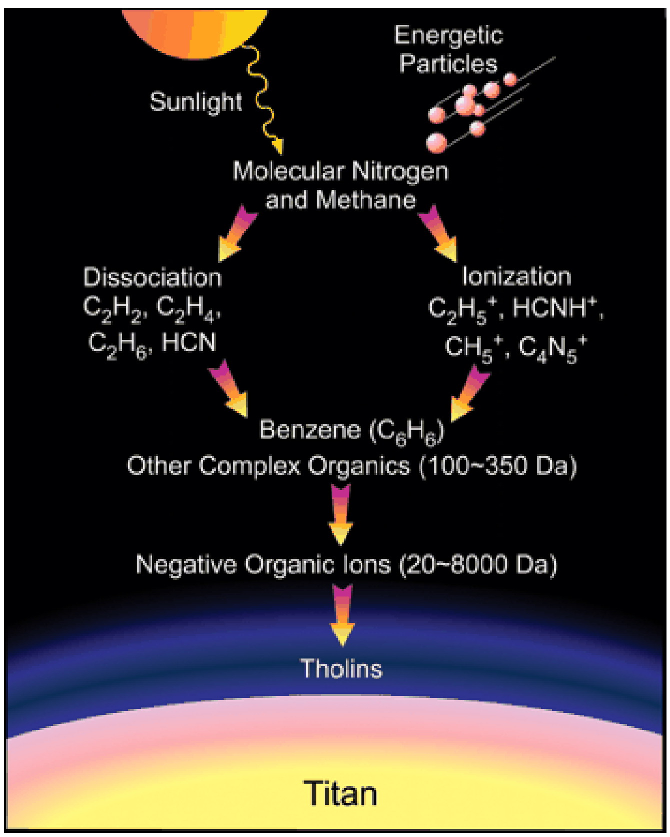
And that's all I have time for now, but it's just the beginning of the marvelous theory Jacques Tits worked out.

Addenda: At least on Titan, tholins seem to be a complex brew of compounds made by irradiation of molecular nitrogen and methane in the upper atmosphere. The same sort of compounds could be an early chemical step in the formation of life on Earth — that's one reason I'm interested. They're related to PAHs, or "polycyclic aromatic hydrocarbons", which are ubiquitous in outer space — I wrote about those back in "Week 258". I guess the main difference is that tholins contain nitrogen!

I found some more information on tholins here:

16) J. H. Waite, Jr., et al, "The process of tholin formation in Titan's upper atmosphere", *Science* **316** (2007), 870–875.

Here's a picture of how tholins get made, from this paper:



You can see more discussion and also *questions I'm dying to know the answers to* over at the n-Category Caf. Whenever I write This Week's Finds, I come up with lots of questions. If you can help me with some of these, I'll be really grateful.

It was technical — there was no way to avoid it. But it was a wonderful thing. We'd finally busted it. But then, just before we were about to submit the paper, Walter noticed a mistake.

— *John Thompson*

Week 264

May 18, 2008

Here's a puzzle. Guess the next term of this sequence:

$$1, 1, 2, 3, 4, 5, 6, \ldots$$

and then guess the *meaning* of this sequence! I'll give away the answer after telling you about Coleman's videos on quantum field theory and an amazing result on the homotopy groups of spheres.

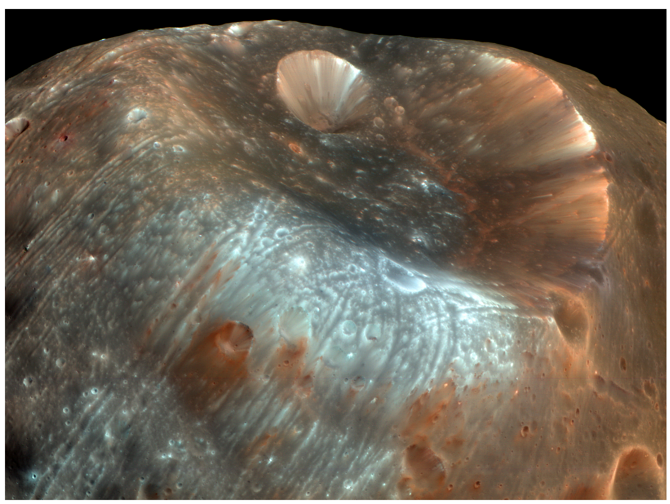
But first... the astronomy picture of the day.

The Eaton Collection at UC Riverside may be the world's best library of science fiction:

1) The Eaton Collection of Science Fiction, Fantasy, Horror and Utopian Literature, `http://eaton-collection.ucr.edu/`.

Right now my wife Lisa Raphals is attending a conference there on the role of Mars in SF, called "Chronicling Mars". Gregory Benford, Frederik Pohl, Greg Bear, David Brin, Kim Stanley Robinson and even Ray Bradbury are all there! But for some reason I'm staying home working on This Week's Finds. I'd say that shows true devotion — or maybe just stupidity.

Anyway, in honor of the occasion, here's an incredible closeup of a crater on Mars' moon Phobos:



2) Astronomy Picture of the Day, "Stickney Crater", `http://apod.nasa.gov/apod/ap080410.html`

It's another great example of how machines in space now deliver many more thrills per buck than the old-fashioned approach using canned primates. This photo was taken by HiRISE, the High Resolution Imaging Science Experiment — the same satellite that took the stunning photos of Martian dunes which graced "Week 262".

Mars has two moons, Phobos and the even tinier Deimos. Their names mean "fear" and "dread" in Greek, since in Greek mythology they were sons of Mars (really Ares), the god of war.

Interestingly, Kepler predicted that Mars had two moons before they were seen. This sounds impressive, but it was simple interpolation, since Earth has 1 moon and Jupiter has 4. Or at least Galileo saw 4 — now we know there are a lot more.

Phobos is only 21 kilometers across, and the big crater you see here - Stickney Crater — is about 9 kilometers across. That's almost half the size of the whole moon! The collision that created it must have almost shattered Phobos.

Phobos is so light — just twice the density of water — that people once thought it might be hollow. This now seems unlikely, though it's been the premise of a few SF stories. It's more likely that Phobos is a loosely packed pile of carbonaceous chondrites captured from the asteroid belt.

Phobos orbits so close to Mars that it zips around once every 8 hours, faster than Mars itself rotates! Oddly, in 1726 Jonathan Swift wrote about two moons of Mars in his novel "Gulliver's Travels" — and he guessed that the inner one orbited Mars every 10 hours.

Gravitational tidal forces are dragging Phobos down, so in only 10 million years it'll either crash or — more likely — be shattered by tidal forces and form a ring of debris.

So, enjoy it while it lasts.

Anyone who's seriously struggled to master quantum field theory is likely to have profited from this book:

3) Sidney Coleman, *Aspects of Symmetry: Selected Erice Lectures*, Cambridge U. Press, Cambridge, 1988.

It's brimming with wisdom and humor. You should have already encountered quantum field theory before trying it: what you'll get are deeper insights.

But what if you're just getting started?

Sidney Coleman, recently deceased, was one of the best quantum field theorists from the heyday of particle physics. As a grad student I took a course on quantum field theory from Eddie Farhi, who said he based his class on the notes from Coleman's class at Harvard. So, I've always been curious about these notes. Now they're available online in handwritten form:

4) Sidney Coleman, "lecture notes on quantum field theory", transcribed by Brian Hill, `http://www.damtp.cam.ac.uk/user/dt281/qft/col1.pdf` and `http://www.damtp.cam.ac.uk/user/dt281/qft/col2.pdf`

Someone should LaTeX them up!

Even more fun, you can now see *videos* of Coleman teaching quantum field theory:

5) Sidney Coleman, *Physics 253: Quantum Field Theory*, 50 lectures recorded 1975–1976, `http://www.physics.harvard.edu/about/Phys253.html`

This is a younger, hipper Coleman than I'd ever seen: long-haired, sometimes puffing on a cigarette between sentences. He begins by saying "Umm... this is Physics 253, a course in relativistic quantum mechanics. My name is Sidney Coleman. The apparatus you see around you is part of a CIA surveillance project."

I wish I'd had access to these when I was a kid!

Now for some miraculous math. Daniel Moskovich kindly pointed out a paper that describes all the homotopy groups of the 2-sphere, and I want to summarize the main result.

I explained the idea of homotopy groups back in "Week 102". Very roughly, the nth homotopy group of a space X, usually denoted $\pi_n(X)$, is the set of ways you can map an n-sphere into that space, where we count two ways as the same if you can continuously deform one to the other. If a space has holes, homotopy groups are one way to detect those holes.

Homotopy groups are notoriously hard to compute — so even for so humble a space as the 2-sphere, S^2, there's a sense in which "nobody knows" all its homotopy groups. People know the first 64, though. Here are a few:

$$\begin{aligned}
\pi_1(S^2) &= 0 \\
\pi_2(S^2) &= \mathbb{Z} \\
\pi_3(S^2) &= \mathbb{Z} \\
\pi_4(S^2) &= \mathbb{Z}/2 \\
\pi_5(S^2) &= \mathbb{Z}/2 \\
\pi_6(S^2) &= \mathbb{Z}/4 \times \mathbb{Z}/3 \\
\pi_7(S^2) &= \mathbb{Z}/2 \\
\pi_8(S^2) &= \mathbb{Z}/2 \\
\pi_9(S^2) &= \mathbb{Z}/3 \\
\pi_{10}(S^2) &= \mathbb{Z}/3 \times \mathbb{Z}/5 \\
\pi_{11}(S^2) &= \mathbb{Z}/2 \\
\pi_{12}(S^2) &= \mathbb{Z}/2 \times \mathbb{Z}/2 \\
\pi_{13}(S^2) &= \mathbb{Z}/2 \times \mathbb{Z}/2 \times \mathbb{Z}/3 \\
\pi_{14}(S^2) &= \mathbb{Z}/2 \times \mathbb{Z}/2 \times \mathbb{Z}/4 \times \mathbb{Z}/3 \times \mathbb{Z}/7 \\
\pi_{15}(S^2) &= \mathbb{Z}/2 \times \mathbb{Z}/2
\end{aligned}$$

Apart from the fact that they're all abelian groups, all finite except for the first two, it's hard to spot any pattern!

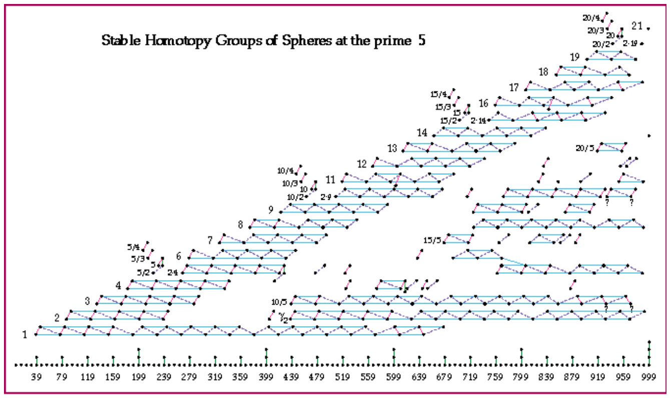
In fact there's a majestic symphony of patterns in the homotopy groups of spheres, starting from ones that are easy to explain and working on up to those that push the frontiers of mathematics, like elliptic cohomology. But, many of these patterns are too complex for present-day mathematics until we use some tricks to "water down" or simplify the homotopy groups.

So, what people often do first is take the limit of $\pi_{n+k}(S^n)$ as $n \to \infty$, getting what's called the kth "stable" homotopy group of spheres. It's a wonderful but well-understood fact that these limits really exist. But so far, even these are too complicated to understand until we work "at a prime p". This means that we take the kth stable homotopy group of spheres and see which groups of the form $\mathbb{Z}/p^n$ show up in it. For example,

$$\pi_{14}(S^2) = \mathbb{Z}/2 \times \mathbb{Z}/2 \times \mathbb{Z}/4 \times \mathbb{Z}/3 \times \mathbb{Z}/7$$

but if we work "at the prime 2" we just see the $\mathbb{Z}/2 \times \mathbb{Z}/2 \times \mathbb{Z}/4$.

After all this data processing, we get some astounding pictures:



This picture summarizes the first 999 stable homotopy groups of spheres at the prime 5. To understand exactly what it means, read this:

6) Allen Hatcher, "Stable homotopy groups of spheres", `http://www.math.cornell.edu/~hatcher/stemfigs/stems.html`

Order teetering on the brink of chaos! If you're brave, you can learn more about this stuff here:

7) Douglas C. Ravenel, *Complex Cobordism and Stable Homotopy Groups of Spheres*, AMS, Providence, Rhode Island, 2003.

If you're less brave, I strongly suggest starting here:

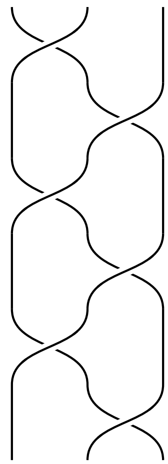
8) Wikipedia, "Homotopy groups of spheres", `http://en.wikipedia.org/wiki/Homotopy_groups_of_spheres`

But now, I want to talk about an amazing paper that pursues a very different line of attack. It gives a beautiful description of *all* the homotopy groups of S^2, in terms of braids:

9) A. Berrick, F. R. Cohen, Y. L. Wong and J. Wu, "Configurations, braids and homotopy groups", *J. Amer. Math. Soc.* **19** (2006), 265–326. Also available at `http://www.math.nus.edu.sg/~matwujie/BCWWfinal.pdf`

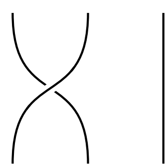
For this you need to realize that for any n, there's a group B_n whose elements are

n-strand braids. For example, here's an element of B_3:

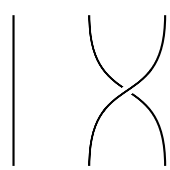


I actually talked about this specific braid back in "Week 233". But anyway, we count two braids as the same if you can wiggle one around until it looks like the other without moving the ends at the top and bottom — which you can think of as nailed to the ceiling and floor.

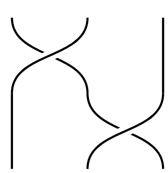
How do braids become a group? Easy: we multiply them by putting one on top of the other. For example, this braid:

$A =$ 

times this one:

$B =$ 

equals this:

$AB =$ 

and in fact the big one I showed you earlier is $(AB)^3$.

As you let your eye slide from the top to the bottom of a braid, the strands move around. We can visualize their motion as a bunch of points running around the plane, never bumping into each other. This gives an interesting way to generalize the concept of a braid! Instead of points running around the plane, we can have points running around

S^2, or some other surface X. So, for any surface X and any number n of strands, we get a "surface braid group", called $B_n(X)$.

As I hinted in "Week 261", these surface braid groups have cool relationships to Dynkin diagrams. I urged you to read this paper, and I'll urge you again:

10) Daniel Allcock, "Braid pictures for Artin groups", available as arXiv:math.GT/9907194.

But for now, we just need the "spherical braid group" $B_n(S^2)$ together with the usual braid group B_n.

Let's say a braid is "Brunnian" if when you remove any one strand, the remaining braid becomes the identity: you can straighten out all the remaining strands to make them vertical. It's a fun little exercise to check that Brunnian braids form a subgroup of all braids. So, we have an n-strand Brunnian braid group BB_n.

The same idea works for braids on other surface, like the 2-sphere. So, we also have an n-strand *spherical* Brunnian braid group $BB_n(S^2)$.

Now, there's obvious map

$$B_n \to B_n(S^2)$$

Why? An element of B_n describes the motion of a bunch of points running around the plane, but the plane sits inside the 2-sphere: the 2-sphere is just the plane with an extra point tacked on. So, an ordinary braid gives a spherical braid.

This map clearly sends Brunnian braids to spherical Brunnian braids, so we get a map

$$f\colon BB_n \to BB_n(S^2)$$

And now we're ready for the shocking theorem of Berrick, Cohen, Wong and Wu:

> ***Theorem:*** *For $n > 3$, $\pi_n(S^2)$ is $BB_n(S^2)$ modulo the image of f.*

In something more like plain English: when n is big enough, the nth homotopy group of the 2-sphere consists of spherical Brunnian braids modulo ordinary Brunnian braids!

Zounds! What do the homotopy groups of S^2 have to do with braids? It's not supposed to be obvious! The proof of this result is long and deep, making use of flows on metric spaces, and also the fact that all the Brunnian braid groups BB_n fit together into a "simplicial group" whose nth homology is the nth homotopy group of S^2. I'd love to understand all this stuff, but I don't yet.

This result doesn't instantly help us "compute" the homotopy groups of S^2 — at least not in the sense of writing them down as a product of groups like $\mathbb{Z}/p^n$. But, it gives a new view of these homotopy groups, and there's no telling where this might lead.

When I was first getting ready to write this article, I was also going to tell you about some amazing descriptions of the homotopy groups of the *3-sphere*, due to Wu.

However, I later realized — first to my shock, and then my embarrassment for not having known it already — that the nth homotopy group of S^3 is *the same* as the nth homotopy group of S^2, at least for $n > 2$. Do you see why?

Given this, it turns out that Wu's results are predecessors of the theorem just stated, a bit more combinatorial and less "geometric". Wu's results appeared here:

12) Jie Wu, "On combinatorial descriptions of the homotopy groups of certain spaces", *Math. Proc. Camb. Phil. Soc.* **130** (2001), 489–513. Also available at `http://www.math.nus.edu.sg/~matwujie/newnewpis_3.pdf`

    Jie Wu, "A braided simplicial group", *Proc. London Math. Soc.* **84** (2002), 645–662. Also available at `http://www.math.nus.edu.sg/~matwujie/wgroup05-19-01.pdf`

and there's a nice summary of these results on his webpage:

13) Jie Wu, "2.1 Homotopy groups and braids", halfway down the page at `http://www.math.nus.edu.sg/~matwujie/Research2.html`

See also this expository paper:

14) Fred R. Cohen and Jie Wu, "On braid groups and homotopy groups", *Geometry & Topology Monographs* **13** (2008), 169–193. Also available at `http://www.math.nus.edu.sg/~matwujie/cohen.wu.GT.revised.29.august.2007.pdf`

Next I want to talk about puzzle mentioned at the start of this Week's Finds... but first I should answer the puzzle I just raised. Why do the homotopy groups of S^2 match those of S^3 after a while? Because of the Hopf fibration! This is a fiber bundle with S^3 as total space, S^2 as base space and S^1 as fiber:

$$S^1 \to S^3 \to S^2$$

Like any fiber bundle, it gives a long exact sequence of homotopy groups as explained in "Week 151":

$$\ldots \to \pi_n(S^1) \to \pi_n(S^3) \to \pi_n(S^2) \to \pi_{n-1}(S^1) \to \ldots$$

but the homotopy groups of S^1 vanishes after the first, so we get

$$\ldots \to 0 \to \pi_n(S^3) \to \pi_n(S^2) \to 0 \to \ldots$$

for $n > 2$, which says that

$$\pi_n(S^3) \cong \pi_n(S^2)$$

Okay, now for this mysterious sequence:

$$1, 1, 2, 3, 4, 5, 6, \ldots$$

The next term is obviously 7. If you guessed anything else, you were over-analyzing. So the real question is: why the funny "hiccup" at the beginning? Why the repeated 1?

You'll find two explanations of this sequence in Sloane's Online Encyclopedia of Integer Sequences, but neither of them is the reason James Dolan and I ran into it. We were learning about theta functions...

Say you have a torus. Then the complex line bundles over it are classified by an integer called their "first Chern number". In some sense, this integer this measures how "twisted" the bundle is. For example, you can put any connection on the bundle, compute its curvature 2-form, and integrate it over the torus: up to some constant factor, you'll get the first Chern number.

A torus is a 2-dimensional manifold, but we can also make it into a 1-dimensional *complex* manifold, often called an "elliptic curve". In fact we can do this in infinitely many fundamentally different ways, one for each point in the "moduli space of elliptic curves". I've explained this repeatedly here — try "Week 125" for a good starting-point — so I won't do so again. The details don't really matter here.

Back to line bundles. If we pick an elliptic curve, we can try to classify the *holomorphic* complex line bundles over it — that is, those where the transition functions are holomorphic (or in other words, complex-analytic). Here the classification is subtler. It turns out you need, not just the first Chern number, which is discrete, but another parameter which can vary in a *continuous* way.

Interestingly, this other parameter can be thought of as just a point on your elliptic curve! So, an elliptic curve is a space that classifies holomorphic line bundles over itself — at least, those with fixed first Chern number. Curiously circular, eh? This is just one of several curiously circular classification theorems that happen in this game...

But I'm actually digressing a bit — I'm having trouble resisting the temptation to explain everything I've just been learning, since it's so simple and beautiful. Don't worry — all you need to know is that holomorphic line bundles over an elliptic curve are classified by an integer and some other continuous parameter.

The puzzle then arises: how many holomorphic sections do these line bundles have? More precisely: what's the *dimension* of the space of holomorphic sections?

Before I answer this, I can't resist adding that these holomorphic sections have a long and illustrious history — they're called "theta functions", and you can learn about them here:

15) Jun-ichi Igusa, *Theta Functions*, Springer, Berlin, 1972.

16) David Mumford, *Tata Lectures on Theta*, 3 volumes, Birkhauser, Boston, 1983–1991.

They're important in geometric quantization, where holomorphic sections of line bundles describe states of quantum systems, and the reciprocal of the first Chern number is proportional to Planck's constant. In fact, I first ran into theta functions years ago, when trying to quantize a black hole — see the end of "Week 112" for more details.

But anyway, here's the answer to the puzzle. The dimension turns out not to depend on the continuous parameter labelling our line bundle, but only on its first Chern number. If that number is negative, the dimension is 0. But if it's $0, 1, 2, 3, 4, 5, 6$ and so on, the dimension goes like this:

$$1, 1, 2, 3, 4, 5, 6, \ldots$$

Now, this sequence is fairly weird, because of the extra "1" at the beginning. I hadn't noticed this back when I was quantizing black holes, because the extra "1" happens for first Chern number zero, which would correspond to Planck's constant being *infinite*. But now that I'm just thinking about math, it sticks out like a sore thumb!

It's got to be right, since the line bundle with first Chern number zero is the trivial bundle, its sections are just functions, and the only holomorphic functions on a compact complex manifold are constants - so there's a 1-dimensional space of them. But, it's weird.

Luckily, Jim figured out the explanation for this sequence. First of all, we can encode it into a power series:

$$1 + x + 2x^2 + 3x^3 + 4x^4 + \ldots$$

which we can rewrite as a rational function:

$$1 + x + 2x^2 + 3x^3 + 4x^4 + \ldots = \frac{1 - x^6}{(1 - x)(1 - x^2)(1 - x^3)}$$

Now, the reason for doing this is that we can pick a line bundle of first Chern number 1, say L, and get a line bundle of any Chern number n by taking the nth tensor power of L — let's call that $L^{\otimes n}$. We can multiply a section of $L^{\otimes n}$ and a section of $L^{\otimes m}$ to get a section of $L^{\otimes (n+m)}$. So, all these spaces of sections we're studying fit together to form a commutative graded ring! And, whenever you have a graded ring, it's a good idea to write down a power series that encodes the dimensions of each grade, just as we've done above. This is called a "Poincare series".

And, when you have a commutative graded ring with one generator of degree 1, one generator of degree 2, one generator of degree 3, one relation of degree 6, and no "relations between relations" (or "syzygies"), its Poincare series will be

$$\frac{1 - x^6}{(1 - x)(1 - x^2)(1 - x^3)}$$

That's how it always works — think about it.

So, it's natural to hope that our ring built from holomorphic sections of all the line bundles $L^{\otimes n}$ will have one generator of degree 1, one of degree 2, one of degree 3, and one relation of degree 6.

And, this seems to be true!

As I mentioned, people usually call these holomorphic sections "theta functions". So, what we're getting is a description of the ring of theta functions in terms of generators and relations.

How does it work, exactly? Well, I must admit I'm not quite sure. Jim has some ideas, but it seems I need to do something a bit different to get his story to work for me. Maybe it goes something like this. We can write any elliptic curve as the solutions of this equation:

$$y^2 = x^3 + Bx + C$$

for certain constants B and C that depend on the elliptic curve. (See "Week 13" and "Week 261" for details.) Now, this equation is not homogeneous in the variables y and x, but we can think of it as homogeneous in a sneaky sense if we throw in an extra variable like this:

$$y^2 = x^3 + Bxz^4 + Cz^6$$

and decree that:

- y has grade 3
- x has grade 2
- z has grade 1

Then all the terms in the equation have grade 6. So, we're getting a commutative graded ring with generators of degree 1, 2, and 3 and a relation of grade 6. And, I'm hoping this ring consists of algebraic functions on the total space of some line bundle L^* over our elliptic curve. z should be a function that's linear in the fiber directions, hence a section of L. x should be quadratic in the fiber directions, hence a section of $L^{\otimes 2}$. And y should be cubic, hence a section of $L^{\otimes 3}$. If L has first Chern number 1, I think we're in business.

If anybody knows about this stuff, I'd appreciate corrections or references.

There's a *lot* more to say about this business... because it's all part of a big story about elliptic curves, theta functions and modular forms. But, I want to quit here for now.

Addenda: I thank David Corfield for pointing out how to get ahold of Wu's papers free online — and earlier, for telling me Wu's combinatorial description of $\pi_3(S^2)$.

Martin Ouwehand told me that some of Coleman's lecture notes on quantum field theory are available in TeX here:

17) Sidney Coleman, *Quantum Field Theory*, first 11 lectures notes TeXed by Bryan Ginge Chen, available at `http://www.physics.upenn.edu/~chb/phys253a/coleman/`

James Dolan pointed out that this article:

18) Wikipedia, "Riemann-Roch theorem", `http://en.wikipedia.org/wiki/Riemann-Roch`

has some very relevant information on the sequence

$$1, 1, 2, 3, 4, 5, 6, \ldots$$

though it's phrased not in terms of "sections of line bundles", but instead in terms of "divisors" (secretly another way of talking about the same thing). Let me quote a portion, just to whet your interest:

> *We start with a connected compact Riemann surface of genus g, and a fixed point P on it. We may look at functions having a pole only at P. There is an increasing sequence of vector spaces: functions with no poles (i.e., constant functions), functions allowed at most a simple pole at P, functions allowed at most a double pole at P, a triple pole, ... These spaces are all finite dimensional. In case $g = 0$ we can see that the sequence of dimensions starts*
>
> $$1, 2, 3, \ldots$$
>
> *This can be read off from the theory of partial fractions. Conversely if this sequence starts*
>
> $$1, 2, \ldots$$
>
> *then g must be zero (the so-called Riemann sphere).*

> *In the theory of elliptic functions it is shown that for $g = 1$ this sequence is*
>
> $$1, 1, 2, 3, 4, 5, \ldots$$
>
> *and this characterises the case $g = 1$. For $g > 2$ there is no set initial segment; but we can say what the tail of the sequence must be. We can also see why $g = 2$ is somewhat special.*
>
> *The reason that the results take the form they do goes back to the formulation (Roch's part) of the [Riemann-Roch] theorem: as a difference of two such dimensions. When one of those can be set to zero, we get an exact formula, which is linear in the genus and the degree (i.e. number of degrees of freedom). Already the examples given allow a reconstruction in the shape*
>
> $$\textit{dimension} - \textit{correction} = \textit{degree} - g + 1.$$
>
> *For $g = 1$ the correction is 1 for degree 0; and otherwise 0. The full theorem explains the correction as the dimension associated to a further, 'complementary' space of functions.*

You can see more discussion of this Week's Finds at the n-Category Caf.

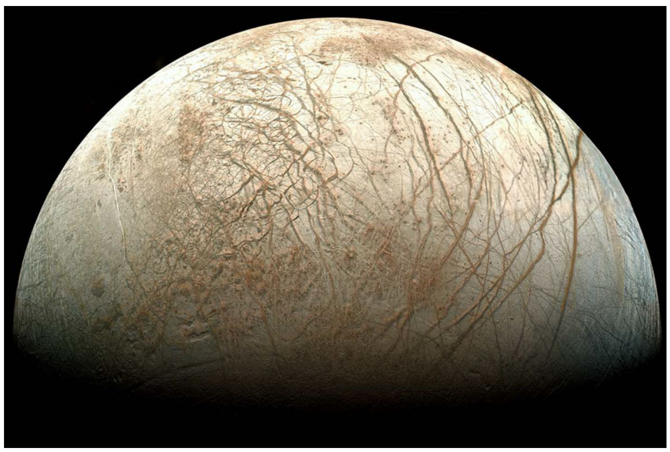
> The career of a young theoretical physicist consists of treating the harmonic oscillator in ever-increasing levels of abstraction.
>
> — *Sidney Coleman*

Week 265

May 25, 2008

Today I'd like to talk about the Pythagorean pentagram, Bill Schmitt's work on Hopf algebras in combinatorics, the magnum opus of Aguiar and Mahajan, and quaternionic analysis. But first, the astronomy picture of the week.

I seem to be into moons these days: first Saturn's moon Titan in "Week 263", and then Mars' moon Phobos in "Week 264". On the cosmic scale, our Solar System is like our back yard. It may not be important in the grand scheme of things, but we should get to know it and learn to take care of it. It's got lots of cool moons. So this week, let's talk about Europa:

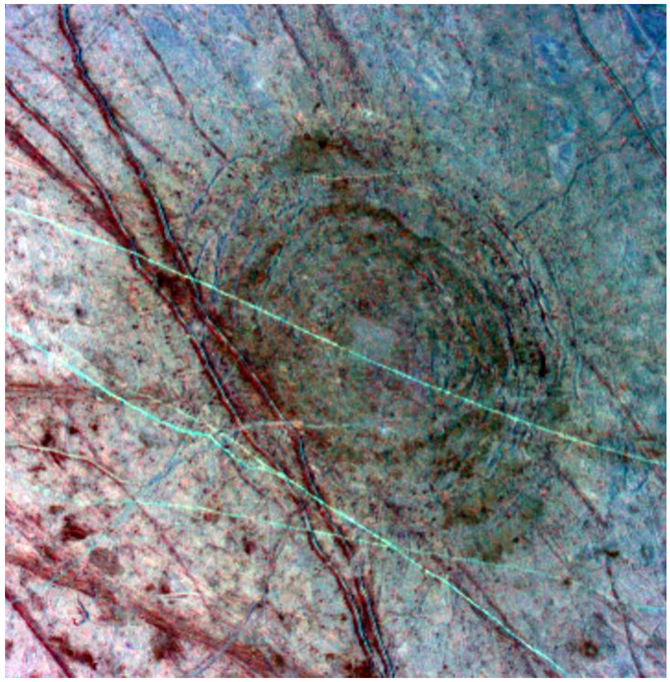


1) Astronomy Picture of the Day, "Gibbous Europa", `http://antwrp.gsfc.nasa.gov/apod/ap071202.html`

Europa is the fourth biggest moon of Jupiter, the smallest of the four seen by Galileo. It's 3000 kilometers in diameter, slightly smaller than our moon, and it zips around Jupiter once every 3.5 of our days, though it's almost twice as far from Jupiter as our moon is from us.

It looks like a cracked ball of ice, and that's what it is — at least near the surface.

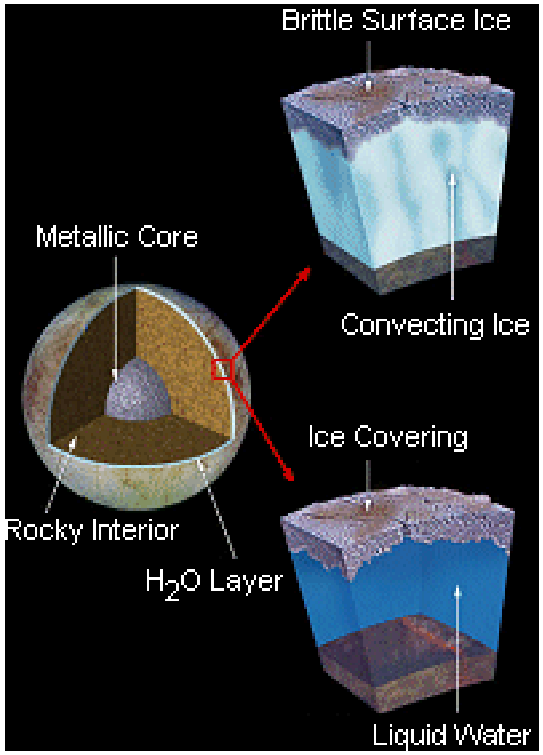
Indeed, this ancient impact crater looks like a smashed windshield, or a frozen lake that's been hit with a sledgehammer:



2) NASA Photojournal, "Ancient impact basin on Europa", `http://photojournal.jpl.nasa.gov/catalog/PIA00702`

But this crater, called Tyre, is huge: about as big as the island of Hawaii, 145 kilometers across! (Beware: this picture is a composite of three photos taken by the Galileo spacecraft in 1997. It's in false color designed to show off various structures: the original crater, the later red cracks, and the blue-green ridges.)

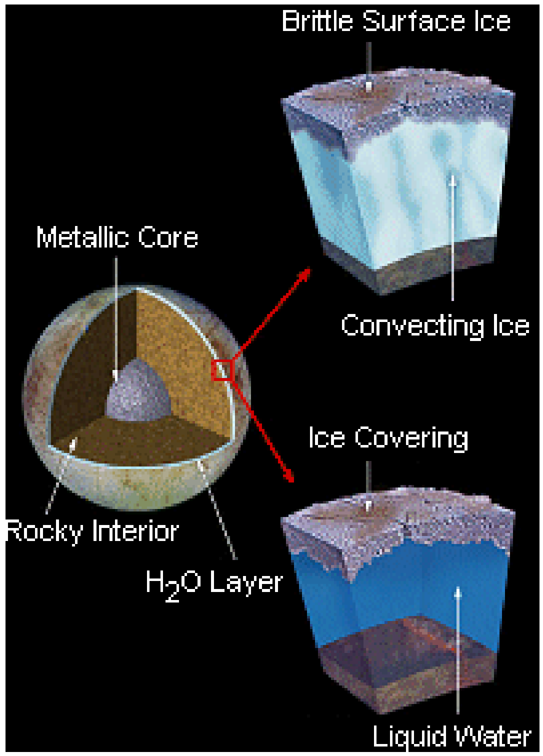
The big question is whether there's liquid water beneath the icy surface… and if so, maybe life? One model of this moon posits a solid ice crust. Another says there's liquid water too:



3) NASA Photojournal, Model of Europa's subsurface structure, `http://photojournal.jpl.nasa.gov/catalog/PIA01669`

How can we tell? Europa is the *smoothest* of all solid planets and moons, with lots of cracks and ridges but few remaining craters. This suggests either an ocean beneath the surface, or at least ice warm enough to keep convection going. The region called

Conamara Chaos looks like pack ice here on Earth, hinting at liquid water beneath:



4) NASA Photojournal, "Europa: ice rafting view", `http://photojournal.jpl.nasa.gov/catalog/PIA01127`

The bluish white areas have been blanketed with ice dust ejected from far away when an impact formed a crater called Pwyll. The reddish brown regions could contain salts or sulfuric acid — it's hard to find out using spectroscopy, since there's too much ice.

Another very nice piece of evidence for *salty* liquid water inside Europa is that the magnetic field of Jupiter induces electric currents in this moon, which in turn create their own magnetic fields! These fields were detected when the Galileo probe swooped closest to Europa back in 2000:

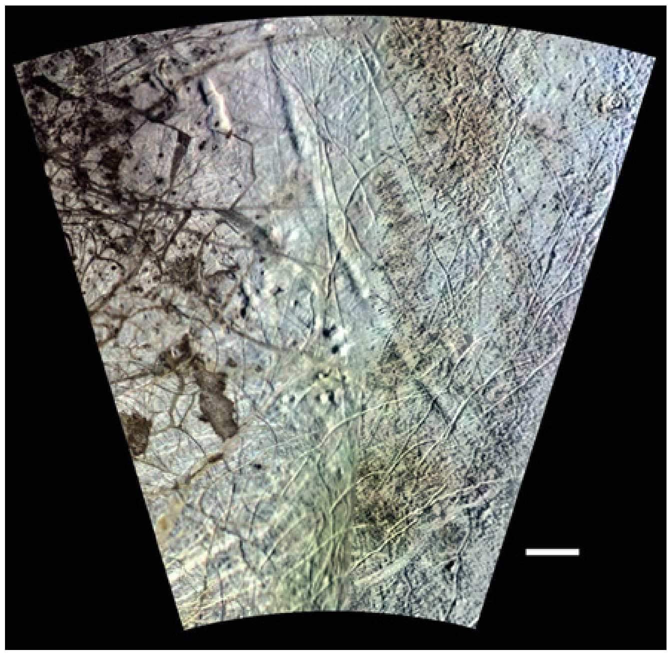
5) M. G. Kivelson, K. K. Khurana, C. T. Russell, M. Volwerk, R.J. Walker, and C. Zimmer, "Galileo magnetometer measurements: a stronger case for a subsurface ocean at Europa", *Science* **289** (2000), 1340–1343.

At the time, Margaret Kivelson, head of the magnetometer project, said:

> *I think these findings tell us that there is indeed a layer of liquid water beneath Europa's surface. I'm cautious by nature, but this new evidence certainly makes the argument for the presence of an ocean far more persuasive. Jupiter's magnetic field at Europa's position changes direction every* $5\frac{1}{2}$ *hours. This changing magnetic field can drive electrical currents in a conductor, such as an ocean.*

> *Those currents produce a field similar to Earth's magnetic field, but with its magnetic north pole — the location toward which a compass on Europa would point — near Europa's equator and constantly moving. In fact, it is actually reversing direction entirely every* $5\frac{1}{2}$ *hours.*

A couple weeks ago, another nice piece of evidence was announced:



Scale bar is 100 kilometers long.

Picture by Paul Schenk, Lunar and Planetary Institute.

6) Paul Schenk, Isamu Matsuyama and Francis Nimmo, "True polar wander on Europa from global-scale small-circle depressions", *Nature* **453** (2008), 368–371.

Paul Schenk, "Scars from Europa's polar wandering betray ocean beneath", `http://www.lpi.usra.edu/science/schenk/europaCropCircles/`

There are two arc-shaped depressions exactly opposite each other on Europa, each hundreds of kilometers long and between .3 and 1.5 kilometers deep. According to the above paper, these scars have just the right shape to be caused the moon's icy shell rotating a quarter turn relative to the interior! The authors believe this could happen most easily if it were floating on an ocean.

If Europa has an ocean under its ice, other questions immediately arise. How thick is the ice and how deep is the ocean? Some guess 15–30 kilometers of ice atop 100 kilometers of liquid. What keeps it warm? Heating produced by tidal forces may be the best bet — radioactivity from the core contributes just about 100 billion watts, not nearly enough:

7) M. N. Ross and G. Schubert, "Tidal heating in an internal ocean model of Europa", *Nature* **325** (1987), 133–144.

And then for the really big question: could there be *life* on Europa? Antarctica has an enormous lake called Lake Vostok buried under 4 kilometers of ice, and when people drilled into it they found all sorts of bizarre life forms that had never been seen before. So, especially if Europa had been warmer once, it's conceivable that life might have formed there and survives to this day. Of course, the surface of Europa makes Antarctica look downright balmy: it's -160 Celsius at the equator. And liquid water below could be mixed with sulfuric acid, or lots of nasty salts...

Nonetheless, some dream of sending a satellite to Europa, perhaps to impact it at high velocity and see what's inside, or perhaps to land and melt down through the ice:

8) Leslie Mullen, "Hitting Europa hard (interview of Karl Hibbits)", *Astrobiology Magazine*, May 1, 2006, `http://www.astrobio.net/news/article1944.html`

But these dreams may not come true anytime soon. In 2005, NASA cancelled its ambitious plans for the Jupiter Icy Moons Orbiter:

10) Wikipedia, "Jupiter Icy Moons Orbiter", `http://en.wikipedia.org/wiki/Jupiter_Icy_Moons_Orbiter`

The U.S. Congress, the National Academy of Sciences, and the NASA Advisory Committee have all supported a mission to Europa, but NASA has still not funded this project:

11) Leonard David, "Europa mission: lost in NASA budget", *SPACE.com*, February 7, 2006, `http://www.space.com/news/060207_europa_budget.html`

Unfortunately, NASA still spends most of its money on expensive manned missions — the Buck Rogers approach to space. They think the public wants the "glamor" of manned missions. So, while they just safely landed the Phoenix spacecraft on Mars, they're also busy struggling to fix a toilet in near earth orbit, on the International Space Station.

To study the underground ocean of Europa, our best hope may lie with the European Space Agency's "Jovian Europa Orbiter", part of a project called the Jovian Minisat Explorer:

12) ESA Science and Technology, "Jovian Minisat Explorer", `http://sci.esa.int/science-e/www/object/index.cfm?fobjectid=35982`

This hasn't been funded yet, and there's no telling if it ever will. But people are already working to make sure Europa doesn't get contaminated by bacteria from Earth:

13) National Research Council, "Preventing the Forward Contamination of Europa", The National Academies Press, Washington, DC, 2000. Also available at `http://www.nap.edu/catalog.php?record_id=9895`

In fact the US and many other countries are obligated to do this, since they signed a United Nations treaty that requires it.

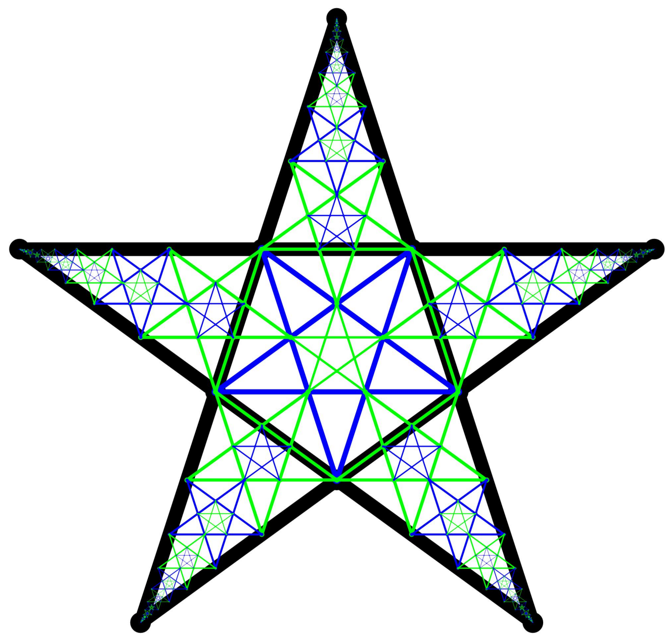
The Galileo probe had not been sterilized in a way that would kill extremophiles — organisms that survive extreme conditions. So, the National Research Council recommended that NASA crash Galileo into Jupiter when its mission was over, to avoid an accidental collision with Europa. So, that's what they did! After 14 years of collecting data about Jupiter and its moons, Galileo crashed into Jupiter and burned up in its atmosphere on September 21, 2004.

Maybe I'll talk about other moons of Jupiter next week. . . the most interesting ones besides Europa are volcanic, sulfurous Io and icy Ganymede, biggest of all.

But now let me turn to the Pythagorean pentagram.

The Pythagoreans — that strange Greek cult of vegetarian mathematicians — were apparently fascinated by the pentagram. Why? I don't think there's any textual evidence to help us answer this question, but luckily there's another way to settle it: unsubstantiated wild guesses!

If you take a pentagram and keep on drawing lines through points that are already present, you can generate this picture:



14) James Dolan, "Pythagorean pentagram", `http://math.ucr.edu/home/baez/pythagorean_pentagram.jpg`

This is just the beginning of an infinite picture packed with pentagrams. The sizes of these pentagrams are related by various powers of the golden ratio:

$$\Phi = \frac{1+\sqrt{5}}{2} = 1.6180339\ldots$$

In particular, if you run up any arm of the big pentagram you'll see little pentagrams, alternating blue and green in the above picture, each $1/\Phi$ times as big as the one before.

And if you contemplate these, you can see that:

$$\Phi = 1 + 1/\Phi$$

I could explain how, but I prefer to leave it as a fun little puzzle. If you get stuck, I'll give you a clue later.

This might have interested the Pythagoreans, since it quickly implies that

$$\Phi = 1 + \frac{1}{\Phi} = 1 + \frac{1}{1 + \frac{1}{\Phi}} = 1 + \frac{1}{1 + \frac{1}{1 + \frac{1}{\Phi}}}$$

and so on. This means that the continued fraction expansion of Φ never ends, so it must be irrational! There's some evidence that early Greeks were interested in continued fraction expansions... you can read about that in this marvelous speculative book:

15) David Fowler, *The Mathematics Of Plato's Academy: A New Reconstruction*, 2nd edition, Clarendon Press, Oxford, 1999. Review by Fernando Q. Gouvła for MAA Online available at https://www.maa.org/press/maa-reviews/the-mathematics-of-platos-academy-a-new

If so, we can imagine that early Greek mathematicians discovered the irrationality of the golden ratio by contemplating the Pythagorean pentagram.

I recently gave a talk about this and other fun aspects of the number 5 at George Washington University and Google.

I was invited to Google by my student Mike Stay — more about that some other day, perhaps. But I'd been invited to George Washington University by Bill Schmitt. We went to grad school together. While I was studying quantum field theory with Irving Segal, he was studying combinatorics with Gian-Carlo Rota. Later he taught me about Joyal's "especes de structures", also known as "species" or "structure types". Later still, these turned out to be deeply related to the quantum harmonic oscillator and Feynman diagrams! For more on that, see "Week 185" and "Week 202".

Bill has always been interested in getting Hopf algebras from structure types. The idea is implicit in some work of Rota:

16) Saj-Nicole Joni and Gian-Carlo Rota, "Coalgebras and bialgebras in combinatorics", *Studies in Applied Mathematics* **61** (1979), 93–139.

    Gian-Carlo Rota, "Hopf algebras in combinatorics", in *Gian-Carlo Rota on Combinatorics: Introductory Papers and Commentaries*, ed. J. P. S. Kung, Birkhauser, Boston, 1995.

but my favorite explanation is here:

17) William R. Schmitt, "Hopf algebras of combinatorial structures", *Canadian Journal of Mathematics* **45** (1993), 412–428. Also available at http://home.gwu.edu/~wschmitt/papers/hacs.pdf

Let me sketch the simplest result in this paper! For starters, recall that a structure type is any sort of structure you can put on finite sets. In other words, it's a functor

$$F\colon \mathsf{FinSet}_0 \to \mathsf{Set}$$

where FinSet_0 is the groupoid of finite sets and bijections. The idea is that for any finite set X, $F(X)$ is the set all of structures of the given type that we can put on X. A good example is $F(X) = 2^X$, the set of 2-colorings of X.

Starting from this, we can form a groupoid of F-structured finite sets and structure-preserving bijections. For example, the groupoid of 2-colored finite sets and color-preserving bijections. The idea should be obvious, but it's good to make it precise. For category hotshots it's just the groupoid of "elements" of F, called $\mathsf{elt}(F)$. But if you're not a hotshot yet, I should explain this.

An object of $\mathsf{elt}(F)$ is a finite set X together with an element a in $F(X)$. A morphism of $\mathsf{elt}(F)$, say

$$f\colon (X, a) \to (X', a')$$

is a bijection

$$f\colon X \to X'$$

such that

$$F(f)(a) = a'$$

In other words: f is a bijection that carries the F-structure on X to the F-structure on X'.

Anyway: given a structure type F, we can form a vector space B_F whose basis consists of isomorphism classes of $\mathsf{elt}(F)$. And in the paper above, Bill describes various ways to make B_F into various kinds of coalgebra or Hopf algebra.

I'll only explain the simplest one. There are lots of structure types where you can "restrict" a structure on a big set to a structure on a smaller set. For example, a 2-coloring of a set restricts to a 2-coloring of any subset. Let's call such a thing a "structure type with restriction".

Technically, a structure type with restriction is a functor

$$F\colon \mathsf{Inj}^{\mathrm{op}} \to \mathsf{Set}$$

where Inj is the category of finite sets and injections. When we have such a thing, the inclusion

$$i\colon X \to X'$$

of a little set X in a bigger set X' gives a map

$$F(i)\colon F(X') \to F(X)$$

that says how to restrict F-structures on X' to F-structures on X.

In this situation, Bill shows that the vector space B_F becomes a cocommutative coalgebra. In particular, it gets a comultiplication

$$\Delta\colon B_F \to B_F \otimes B_F$$

which satisfies laws just like the commutative and associative laws for ordinary multiplication, only "backwards".

The idea is simple: we comultiply a finite set with an F-structure on it by chopping the set in two parts in all possible ways and using our ability to restrict the F-structure to each part. I could write down the formula, but it's better to guess it and then check your guess in Bill's paper! See his Proposition 3.1.

After Bill came up with this stuff, the connection between Hopf algebras and combinatorics became a big business — largely due to Kreimer's work on Hopf algebras and Feynman diagrams. I talked about this back in "Week 122" — but here's a more recent review, with a hundred references for further study:

18) Kurusch Ebrahimi-Fard and Dirk Kreimer, "Hopf algebra approach to Feynman diagram calculations", available as hep-th/0510202.

This yields lots of applications of Bill's ideas to quantum physics. I have no idea how this huge industry is related to my work with James Dolan and Jeffrey Morton on structure types, more general "stuff types", quantum field theory and Feynman diagrams. But, maybe you can figure it out if you read these:

19) John Baez and Derek Wise, *Quantization and Categorification*.
Fall 2003 notes: `http://math.ucr.edu/home/baez/qg-fall2003`
Winter 2004 notes: `http://math.ucr.edu/home/baez/qg-winter2004/`
Spring 2004 notes: `http://math.ucr.edu/home/baez/qg-spring2004/`

20) Jeffrey Morton, "Categorified algebra and quantum mechanics", *Theory and Applications of Categories* **16** (2006), 785–854. Available at `http://www.emis.de/journals/TAC/volumes/16/29/16-29abs.html` and as math/0601458.

While you're mulling over these ideas, it might pay to ponder this paper Bill told me about:

21) Marcelo Aguiar and Swapneel Mahajan, "Monoidal functors, species and Hopf algebras", available at `http://www.math.tamu.edu/~maguiar/a.pdf`

It's 588 pages long! It's a bunch of very sophisticated combinatorics touching on ideas dear to my heart: q-deformation, species, Fock space, and higher categories. I can't summarize it, but here are some immediately gripping portions:

- Chapter 5, "Higher monoidal categories". Here they discuss "n-monoidal categories", which are categories equipped with a list of tensor products with lax interchange laws relating each tensor product to all the later ones on the list:

$$(A \otimes_i B) \otimes_j (A' \otimes_i B') \to (A \otimes_j A') \otimes_i (B \otimes_j B')$$

  for $i < j$. These gadgets generalize the "iterated monoidal categories" of Balteanu, Fiedorowicz, Schwaenzel, Vogt and also Forcey — I gave some references on these back in "Week 209". The big difference seems to be that the Fiederowicz gang has all the tensor products share the same unit. That's great for what they want to do — namely, get a kind of category whose nerve is an n-fold loop space. But, Aguiar and Mahajan study a bunch of examples coming from combinatorics where different products have different units! It's really these examples that are interesting to me, though the abstract concepts are cool too.

- Chapter 7, "Hopf monoids in species". Here they use "species" to mean what I'd call "linear structure types", that is, functors

$$F\colon \mathsf{FinSet}_0 \to \mathsf{Vect}$$

  where Vect is the category of vector spaces. In Section 7.9 they take Bill Schmitt's trick for getting cocommutative coalgebras from structure types with restriction, and use it to get cococommutative comonoids in the category of linear structure types! In Section 7.10 they take another trick to get coalgebras from structure types:

22) William R. Schmitt, "Incidence Hopf algebras", *Journal of Pure and Applied Algebra* **96** (1994), 299–330. Also available at http://home.gwu.edu/~wschmitt/papers/iha.pdf

and do something similar with that.

- Chapter 9, "From species to graded vector spaces: Fock functors". This studies what happens when you turn a Hopf monoid in the category of linear structure types into a graded Hopf algebra — a kind of generalized Fock space.
- Chapter 11, "Hopf monoids from geometry". Here they get Hopf monoids from the A_n Coxeter complexes, using a lot of ideas related to Jacques Tits' theory of buildings. There's a lot of q-deformation going on here! All these ideas are close to my heart.

You can get more of a sense of what Aguiar is up to by looking at his homepage. I'll just list a *few* of the cool papers there:

23) Marcelo Aguiar's homepage, http://www.math.tamu.edu/~maguiar/

Marcelo Aguiar, *Internal categories and quantum groups*, Ph.D. thesis, Cornell University, August 1997. Available at http://www.math.tamu.edu/~maguiar/thesis2.pdf

Marcelo Aguiar, "Braids, q-binomials and quantum groups", *Advances in Applied Mathematics* **20** (1998) 323–365. Also available at http://www.math.tamu.edu/~maguiar/braids.ps.gz

Marcelo Aguiar and Swapneel Mahajan, *Coxeter groups and Hopf algebras*, Fields Institute Monographs, Volume **23**, AMS, Providence, RI, 2006. Also available at http://www.math.tamu.edu/~maguiar/monograph.pdf

Check out the mysterious table of "generalized binomial coefficients" in the second of these papers — it suggests many links between different subjects of mathematics!

I was going to say a bit about quaternionic analysis, but now I'm worn out. So, I'll just say that anyone interested in generalizing complex analysis to the quaternions must read two papers. The first I had managed to lose for a long time... but now I've found it again:

24) Anthony Sudbery, "Quaternionic analysis", *Math. Proc. Camb. Phil. Soc.* **85** (1979), 199–225. Available at http://citeseer.ist.psu.edu/10590.html and (slightly different version) http://theworld.com/~sweetser/quaternions/ps/Quaternionic-analysis.pdf

The second was brought to my attention by David Corfield:

25) Igor Frenkel and Matvei Libine, "Quaternionic analysis, representation theory and physics", available as arXiv:0711.2699

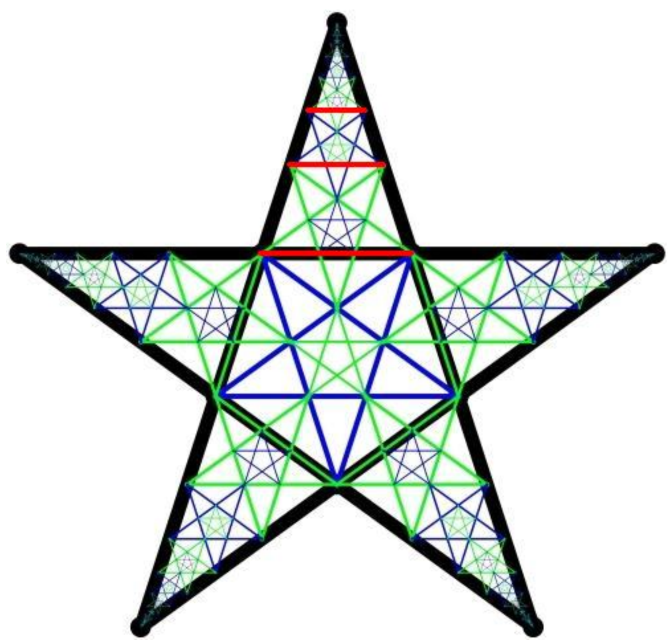
Since Igor Frenkel is a bigshot, this paper may finally bring this neglected subject some of the attention it deserves! Like Corfield, I'll just quote the abstract, to make your mouth water:

We develop quaternionic analysis using as a guiding principle representation theory of various real forms of the conformal group. We first review the Cauchy-Fueter and Poisson formulas and explain their representation theoretic meaning. The requirement of unitarity of representations leads us to the extensions of these formulas in Minkowski space, which can be viewed as another real form of quaternions. Representation theory also suggests a quaternionic version of the Cauchy formula for the second order pole. Remarkably, the derivative appearing in the complex case is replaced by the Maxwell equations in the quaternionic counterpart. We also uncover the connection between quaternionic analysis and various structures in quantum mechanics and quantum field theory, such as the spectrum of the hydrogen atom, polarization of vacuum, and one-loop Feynman integrals. We also make some further conjectures. The main goal of this and our subsequent paper is to revive quaternionic analysis and to show profound relations between quaternionic analysis, representation theory and four-dimensional physics.

Finally, here's a clue for the Pythagorean pentagram puzzle. To prove that

$$\Phi = 1 + 1/\Phi,$$

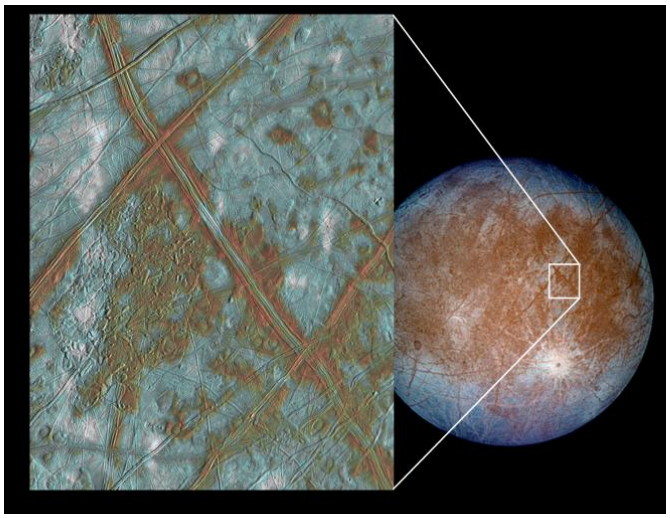
show the length of the longest red interval here is the sum of the lengths of the two shorter ones:



26) James Dolan and John Baez, "annotated picture of Pythagorean pentagram", http://math.ucr.edu/home/baez/golden_ratio_pentagram.jpg

For more on the golden ratio, try "Week 203". For more on its relation to the dodecahedron, see "Week 241".

Addenda: Here's another stunning picture of the ridges and cracks on Europa:



27) NASA Photojournal, "Blocks in the Europan crust provide more evidence of subterranean ocean", `http://photojournal.jpl.nasa.gov/catalog/PIA03002`

You can see more discussion of this Week's Finds at the n-Category Caf. You can also see a list of questions I'd like your help with!

> There is geometry in the humming of the strings, there is music in the spacing of the spheres. — *Pythagoras*

Week 266

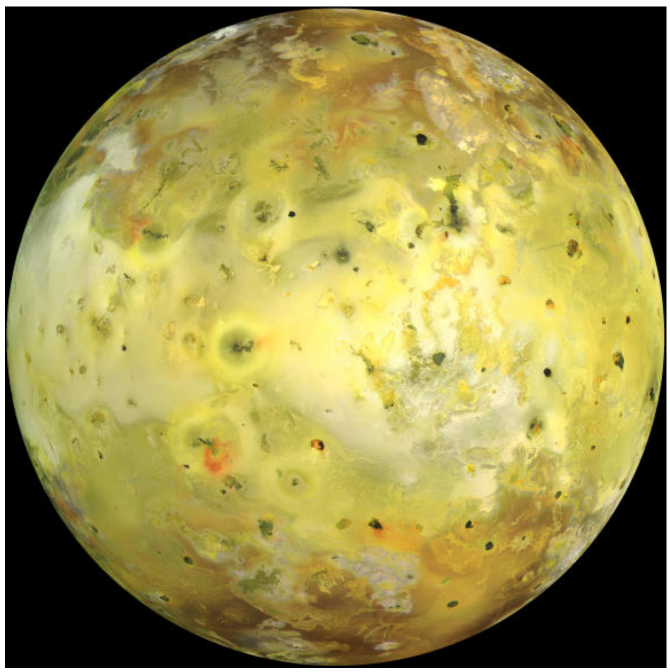
June 20, 2008

I'm at this workshop now, and I want to talk about it:

1) *Workshop on Categorical Groups*, June 16–20, 2008, Universitat de Barcelona, organized by Pilar Carrasco, Josep Elgueta, Joachim Kock and Antonio Rodrguez Garzn, `http://mat.uab.cat/~kock/crm/hocat/cat-groups/`

But first, the moon of the week — and a bit about that mysterious fellow Pythagoras, and the Pythagorean tuning system.

Here's a picture of Jupiter's moon Io:



1) "Io in True Color", Astronomy Picture of the Day, `http://antwrp.gsfc.nasa.gov/apod/ap040502.html`

It's yellow! — a world covered with sulfur spewed from volcanos, burning hot inside from intense tidal interactions with Jupiter's mighty gravitational field... but frigid at the surface.

Last week I talked about something called the "Pythagorean pentagram". That's a cool name — but it's far from clear who first discovered this entity, so I started feeling a bit guilty for using it, and I started wondering what we actually know about Pythagoras or the mathematical vegetarian cult he supposedly launched. Tim Silverman pointed me to a scholarly book on the subject:

2) Walter Burkert, *Lore and Science in Ancient Pythagoreanism*, Harvard U. Press, Cambridge, Massachusetts, 1972.

It turns out we know very little about Pythagoras: a few grains of solid fact, surrounded by a huge cloud of stories that grows larger and larger as we move further and further away from the 6th century BC, when he lived. This is especially true when it comes to his contributions to mathematics. The infamous pseudohistorian Eric Temple Bell begins his book "The Magic of Numbers" as follows:

> *The hero of our story is Pythagoras. Born to immortality five hundred years before the Christian era began, this titanic spirit overshadows western civilization. In some respects he is more vividly alive today than he was in his mortal prime twenty-five centuries ago, when he deflected the momentum of prescientific history toward our own unimagined scientific and technological culture. Mystic, philosopher, experimental physicist, and mathematician of the first rank, Pythagoras dominated the thought of his age and foreshadowed the scientifi mysticisms of our own.*

But, there's no solid evidence for any of this, except perhaps his interest in mysticism and numerology and the incredible growth of his legend as the centuries pass. We're not even sure he proved the "Pythagorean theorem", much less all the other feats that have been attributed to him. As Burkert explains:

> *No other branch of history offers such temptations to conjectural reconstruction as does the history of mathematics. In mathematics, every detail has its fixed and unalterable place in a nexus of relations, so that it is often possible, on the basis of a brief and casual remark, to reconstruct a complicated theory. It is not surprising, then, that gap in the history of mathematics that was opened up by a critical study of the evidence about Pythagoras has been filled by a whole succession of conjectural supplements.*

There's a new book out on Pythagoras:

3) Kitty Ferguson, *The Music of Pythagoras: How an Ancient Brotherhood Cracked the Code of the Universe and Lit the Path from Antiquity to Outer Space*, Walker and Company, 2008.

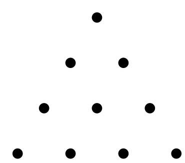
The subtitle is sensationalistic, exactly the sort of thing that would make Burkert cringe. But the book is pretty good, and Ferguson is honest about this: after asking "What do we know about Pythagoras?", she lists everything we know in one short paragraph, and then emphasizes: that's *all*.

He was born on the island of Samos sometime around 575 BC. He went to Croton, a city in what is now southern Italy. He died around 495 BC. We know a bit more — but not much.

It's much easier to learn about the Renaissance "neo-Pythagoreans". This book is a lot of fun, though too romantic to be truly scholarly:

4) S. K. Heninger, Jr., *Touches of Sweet Harmony: Pythagorean Cosmology and Renaissance Poetics*, The Huntington Library, San Marino, California, 1974.

It seems clear that the Renaissance neo-Pythagoreans, and even the Greek Pythagoreans, and perhaps even old Pythagoras himself were much taken with something called the tetractys:

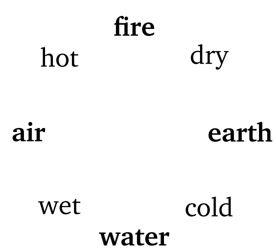


To appreciate the tetractys, you have to temporarily throw out modern scientific thinking and get yourself in the mood of magical thinking — or "correlative cosmology", which tries to understand the universe by setting up elaborate correspondences between this, that, and the other thing. To the Pythagoreans, the four rows of the tetractys represented the point, line, triangle and tetrahedron. But the "fourness" of the tetractys also represented the four classical elements: earth, air, water and fire. It's fun to compare these early groping attempts to impose order on the universe to later, less intuitive but far more predictively powerful schemes like the Periodic Table or the Standard Model. So, let's take a look!

The Renaissance thinkers liked to organize the four elements using a chain of analogies running from light to heavy:

fire : air :: air : water :: water : earth

Them also organized them in a diamond, like this:



Sometimes they even put a fifth element in the middle: the "quintessence", or "aether", from which heavenly bodies were made. And following Plato's Timaeus dialog, they set up an analogy like this:

fire	tetrahedron
air	octahedron
water	icosahedron
earth	cube
quintessence	dodecahedron

This is cute! Fire feels pointy and sharp like tetrahedra, while water rolls like round icosahedra, and earth packs solidly like cubes. Dodecahedra are different than all the rest, made of pentagons, just as you might expect of "quintessence". And air. . . well, I've never figured out what air has to do with octahedra. You win some, you lose some — and in correlative cosmology, a discrepancy here and there doesn't falsify your ideas.

The tetractys also took the Pythagoreans in other strange directions. For example, who said this?

> *"What you suppose is four is really ten. . . "*

A modern-day string theorist talking to Lee Smolin about the dimension of spacetime? No! Around 150 AD, the rhetorician Lucian of Samosata attributed this quote to Pythagoras, referring to the tetratkys and the fact that it has $1 + 2 + 3 + 4 = 10$ dots. This somehow led the Pythagoreans to think the number 10 represented "perfection". If there turn out to be 4 visible dimensions of spacetime together with 6 curled-up ones explaining the gauge group $\mathrm{U}(1) \times \mathrm{SU}(2) \times \mathrm{SU}(3)$, maybe they were right.

Pythagorean music theory is a bit more comprehensible: along with astronomy, music is one of the first places where mathematical physics made serious progress. The Greeks, and the Babylonians before them, knew that nice-sounding intervals in music correspond to simple rational numbers. For example, they knew that the octave corresponds to a ratio of $2:1$. We'd now call this a ratio of *frequencies*; one can get into some interesting scholarly arguments about when and how well the Greeks knew that sound was a *vibration*, but never mind — read Burkert's book if you're interested.

Whatever these ratios meant, the Greeks also knew that a fifth corresponds to a ratio of $3:2$, and a fourth to $4:3$.

By the way, if you don't know about musical intervals like "fourths" and "fifths", don't feel bad. I won't explain them now, but you can learn about them and hear them here:

5) Brian Capleton, "Musical intervals", `http://www.amarilli.co.uk/music/intervs.htm`

and then practice recognizing them:

6) Ricci Adams, "Interval ear trainer", `http://www.musictheory.net/trainers/html/id90_en.html`

If you nose around Capleton's website, you'll see he's quite a Pythagorean mystic himself!

Anyway, at some moment, lost in history by now, people figured out that the octave could be divided into a fourth and a fifth:

$$\frac{2}{1} = \frac{4}{3} \times \frac{3}{2}$$

And later, I suppose, they defined a whole tone to be the difference, or really ratio, between a fifth and a fourth:

$$\frac{3/2}{4/3} = \frac{9}{8}$$

So, when you go up one whole tone in the Pythagorean tuning system, the higher note should vibrate $9/8$ as fast as the lower one. If you try this on a modern keyboard, it looks like after going up 6 whole tones you've gone up an octave. But in fact if you buy the Pythagorean definition of whole tone, 6 whole tones equals

$$(\frac{9}{8})^6 = \frac{531441}{262144} \approx 2.027286530\ldots$$

which is, umm, not quite 2!

Another way to put it is that if you go up 12 fifths, you've *almost* gone up 7 octaves, but not quite: the so-called circle of fifths doesn't quite close, since

$$\frac{(3/2)^{12}}{2^7} = \frac{531441}{524288} \approx 1.01264326\ldots$$

This annoying little discrepancy is called the "Pythagorean comma".

This sort of discrepancy is an unavoidable fact of mathematics. Our ear likes to hear frequency ratios that are nice simple rational numbers, and we'd also like a scale where the notes are evenly spaced — but we can't have both. Why? Because you can't divide an octave into equal parts that are rational ratios of frequencies. Why? Because a nontrivial nth root of 2 can never be rational.

So, irrational numbers are lurking in any attempt to create an equally spaced (or as they say, "equal-tempered") tuning system.

You might imagine this pushed the Pythagoreans to confront irrational numbers. This case has been made by the classicist Tannery, but Burkert doesn't believe it: there's no written evidence suggesting it.

You could say the existence of irrational numbers is the root of all evil in music. Indeed, the diminished fifth in an equal tempered scale is called the "diabolus in musica", or "devil in music", and it has a frequency ratio equal to the square root of 2.

Or, you could say that this built-in conflict is the spice of life! It makes it impossible for harmony to be perfect and therefore dull.

Anyway, Pythagorean tuning is not equal-tempered: it's based on making lots of fifths equal to exactly $3/2$. So, all the frequency ratios are fractions built from the numbers 2 and 3. But, some of them are nicer than others:

- first = $1/1$
- second = $9/8$
- third = $81/64$
- fourth = $4/3$
- fifth = $3/2$
- sixth = $27/16$
- seventh = $243/128$
- octave = $2/1$

As you can see, the third, sixth and seventh are not very nice: they're complicated fractions, so they don't sound great. They're all a bit sharp compared to the following tuning system, which is a form of "just intonation":

- first = $1/1$
- second = $9/8$
- third = $5/4$
- fourth = $4/3$
- fifth = $3/2$
- sixth = $5/3$
- seventh = $15/8$
- octave = $2/1$

Just intonation brings in fractions involving the number 5, which we might call the "quintessence" of music: we need it to get a nice-sounding third. A long and interesting tale could be told about this tuning system — but not now. Instead, let's just see how the third, sixth and seventh differ:

- In just intonation the third is $5/4 = 1.25$, but in Pythagorean tuning it's $81/64 = 1.265625$. The Pythagorean system is about 1.25% sharp.
- In just intonation the sixth is $5/3 = 1.6666\ldots$, but in Pythagorean tuning it's $81/64 = 1.6875$. The Pythagorean system is about 0.7% sharp.
- In just intonation the seventh is $15/8 = 1.875$, but in Pythagorean tuning it's $243/128 = 1.8984375$. The Pythagorean system is about 1.25% sharp.

Here you can learn more about Pythagorean tuning, and hear it in action:

7) Margo Schulter, "Pythagorean tuning and medieval polyphony", `http://www.medieval.org/emfaq/harmony/pyth.html`

8) Reginald Bain, "A Pythagorean tuning of the diatonic scale", `http://www.music.sc.edu/fs/bain/atmi02/pst/index.html`

There's also a murky relation between Pythagorean tuning and something called the "Platonic Lambda". This is a certain way of labelling the edges of the tetractys by powers of 2 on one side, and powers of 3 on the other:

$$\begin{array}{ccccccc} & & & 1 & & & \\ & & 2 & & 3 & & \\ & 4 & & & & 9 & \\ 8 & & & & & & 27 \end{array}$$

I can't help wanting to flesh it out like this, so going down and to the left is multiplication by 2, while going down and to the right is multiplication by 3:

$$\begin{array}{ccccccc} & & & 1 & & & \\ & & 2 & & 3 & & \\ & 4 & & 6 & & 9 & \\ 8 & & 12 & & 18 & & 27 \end{array}$$

So, I was pleased when in Heninger's book I saw the numbers on the bottom row in a plate from a 1563 edition of "De Natura Rerum", a commentary on Plato's Timaeus written by the Venerable Bede sometime around 700 AD!

In this plate, the elements fire, air, water and earth are labelled by the numbers 8, 12, 18 and 27. This makes the aforementioned analogies:

fire : air :: air : water :: water : earth

into strict mathematical proportions:

8 : 12 :: 12 : 18 :: 18 : 27

Cute! Of course it doesn't do much to help us understand fire, air, earth and water. But, it goes to show how people have been struggling a long time to find mathematical patterns in nature. Most of these attempts don't work. Occasionally we get lucky… and over the millennia, these scraps of luck added up to the impressive theories we have today.

Next: the categorical groups workshop here in Barcelona!

A "categorical group", also called a "2-group", is a category that's been equipped with structures mimicking those of a group: a product, identity, and inverses, satisfying the usual laws either "strictly" as equations or "weakly" as natural isomorphisms. Pretty much anything people do with groups can also be done with 2-groups. That's a lot of stuff — so there's a lot of scope for exploration! There's a powerful group of algebraists in Spain engaged in this exploration, so it makes sense to have this workshop here.

Let me say a little about some of the talks we've had so far. I'll mainly give links, instead of explaining stuff in detail.

On Monday, I kicked off the proceedings with this talk:

9) John Baez, "Classifying spaces for topological 2-groups", `http://math.ucr.edu/home/baez/barcelona/`

Just as we can try to classify principal bundles over some space with any fixed group as gauge group, we can try to classify "principal 2-bundles" with a given "gauge 2-group". It's a famous old theorem that for any topological group G, we can find a space BG such that principal G-bundles over any mildly nice space X are classified by maps from X to BG. (Homotopic maps correspond to isomorphic bundles.) A similar result holds for topological 2-groups!

Indeed, Baas Bkstedt and Kro did something much more general for topological 2-*categories*:

10) Nils Baas, Marcel Bkstedt and Tore Kro, "2-Categorical K-theories", available as math/0612549.

Just as a group is a category with one object and with all morphisms being invertible, a 2-group is a 2-category with one object and all morphisms and 2-morphisms invertible. But the 2-group case is worthy of some special extra attention, so Danny Stevenson studied that with a little help from me:

11) John Baez and Danny Stevenson, "The classifying space of a topological 2-group", available as arXiv/0801.3843

and that's what I talked about. If you're also interested in classifying spaces of 2-categories that aren't topological, just "discrete", you should try these:

12) John Duskin, "Simplicial matrices and the nerves of weak n-categories I: nerves of bicategories", available at http://www.tac.mta.ca/tac/volumes/9/n10/9-10abs.html

13) Manuel Bullejos and A. Cegarra, "On the geometry of 2-categories and their classifying spaces", available at http://www.ugr.es/%7Ebullejos/geometryampl.pdf

14) Manuel Bullejos, Emilio Faro and Victor Blanco, "A full and faithful nerve for 2-categories", *Applied Categorical Structures* **13** (2005), 223–233. Also available as arXiv:math/0406615.

On Monday afternoon, Bruce Bartlett spoke on a geometric way to understand representations and "2-representations" of ordinary finite groups. You can see his talk here, and also a version which has less material, explained in a more elementary way:

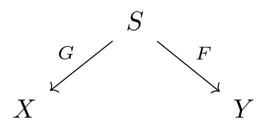
15) Bruce Bartlett, "The geometry of unitary 2-representations of finite groups and their 2-characters", talk at the *Categorical Groups workshop in Barcelona*, June 16, 2008, available at http://brucebartlett.postgrad.shef.ac.uk/research/Barcelona.pdf

Bruce Bartlett, "The geometry of 2-representations of finite groups", talk at the Max Kelly Conference, Cape Town, 2008, available at http://brucebartlett.postgrad.shef.ac.uk/research/MaxKellyTalk.pdf

Both talks are based on this paper:

16) Bruce Bartlett, "The geometry of unitary 2-representations of finite groups and their 2-characters", draft available at http://brucebartlett.postgrad.shef.ac.uk/research/Max%20Kelly%20Proceedings.pdf

The first big idea here is that the category of representations of a finite group G is equivalent to some category where an object X is a complex manifold on which G acts, equipped with an invariant hermitian metric and an equivariant $\mathrm{U}(1)$ bundle. A morphism from X to Y in this category is not just the obvious sort of map; instead, it's diagram of maps shaped like this:



This is called a "span". So, we're seeing a very nice extension of the Tale of Groupoidification, which began in "Week 247" and continued up to "Week 257", when it jumped over to my seminar.

But Bruce doesn't stop here! He then *categorifies* this whole story, replacing representations of G on Hilbert spaces by representations on 2-Hilbert spaces, and replacing $\mathrm{U}(1)$ bundles by $\mathrm{U}(1)$ gerbes. This is quite impressive, with nice applications to a topological quantum field theory called the Dijkgraaf-Witten model.

Next, to handle the TQFT called Chern-Simons theory, Bruce plans to replace the finite group G by a compact Lie group. Another, stranger direction he could go is to replace G by a finite 2-group. Then he'd make contact with the categorified Dijkgraaf-Witten TQFT studied in these papers:

17) David Yetter, "TQFT's from homotopy 2-types", *Journal of Knot Theory and its Ramifications* **2** (1993), 113–123.

18) Timothy Porter and Vladimir Turaev, "Formal homotopy quantum field theories, I: Formal maps and crossed C-algebras", available as `arXiv:math/0512032`.

Timothy Porter and Vladimir Turaev, "Formal homotopy quantum field theories, II: Simplicial formal maps", in *Categories in Algebra, Geometry and Mathematical Physics*, eds. A. Davydov et al, Contemp. Math **431**, AMS, Providence Rhode Island, 2007, 375–403. Also available as `arXiv:math/0512034`.

19) Joo Faria Martins and Timothy Porter, "On Yetter's invariant and an extension of the Dijkgraaf-Witten invariant to categorical groups", available as `arXiv:math/0608484`.

As the last paper explains, we can also think of this TQFT as a field theory where the "field" on a spacetime X is a map

$$f\colon X \to BG$$

where BG is the classifying space of the 2-group G.

Given all this, it's natural to contemplate a further generalization of Bruce's work where G is a Lie 2-group. Unfortunately, Lie 2-groups don't have many representations on 2-Hilbert space of the sort I've secretly been talking about so far: that is, finite-dimensional ones.

So we may, perhaps, need to ponder representations of Lie 2-groups on infinite-dimensional 2-Hilbert spaces.

Luckily, that's just what Derek Wise spoke about on Wednesday morning! His talk also included some pictures with intriguing relations to the pictures in Bruce's talk. You can see the slides here:

19) Derek Wise, "Representations of 2-groups on higher Hilbert spaces", `http://math.ucdavis.edu/\~derek/talks/barcelona2008.pdf`

They make a nice introduction to a paper he's writing with Aristide Baratin, Laurent Freidel and myself. Our work uses ideas like measurable fields of Hilbert spaces, which are already important for understanding infinite-dimensional unitary group representations. But if you're less fond of analysis, jump straight to pages 20, 23 and 25, where he gives a geometrical interpretation of these infinite-dimensional representations, along with the intertwining operators between them... and the "2-intertwining operators" between *those*.

This work relies heavily on the work of Crane, Sheppeard and Yetter, cited in "Week 210" — so check out that, too!

There's much more to say, but I'm running out of steam, so I'll just mention a few more talks: Enrico Vitale's talk on categorified homological algebra, and the talks by David Roberts and Aurora del Ro on the fundamental 2-group of a topological space.

To set these in their proper perspective, it's good to recall the periodic table of n-categories, mentioned in "Week 49":

Table 10: k-tuply monoidal n-categories

	$n = 0$	$n = 1$	$n = 2$
$k = 0$	sets	categories	2-categories
$k = 1$	monoids	monoidal categories	monoidal 2-categories
$k = 2$	commutative monoids	braided monoidal categories	braided monoidal 2-categories
$k = 3$	" "	symmetric monoidal categories	sylleptic monoidal 2-categories
$k = 4$	" "	" "	symmetric monoidal 2-categories
$k = 5$	" "	" "	" "

The idea here is that an $(n+k)$-category with only one j-morphism for $j < k$ acts like an n-category with extra bells and whistles: a "k-tuply monoidal n-category". This idea has not been fully established, and there are some problems with naive formulations of it, but it's bound to be right when properly understood, and it's useful for anyone trying to understand the big picture of mathematics.

Now, an n-category with everything invertible is called an "n-groupoid". Such a thing is believed to be essentially the same as a "homotopy n-type", meaning a nice space, like a CW complex, with vanishing homotopy groups above the nth — where we count homotopy equivalent spaces as the same. If we accept this, the n-groupoid version of the Periodic Table can be understood using homotopy theory. It looks like this:

Table 11: k-tuply groupal n-groupoids

	$n = 0$	$n = 1$	$n = 2$
$k = 0$	sets	groupoids	2-groupoids
$k = 1$	groups	2-groups	3-groups
$k = 2$	abelian groups	braided 2-groups	braided 3-groups
$k = 3$	" "	symmetric 2-groups	sylleptic 3-groups
$k = 4$	" "	" "	symmetric 3-groups
$k = 5$	" "	" "	" "

Most of this workshop has focused on 2-groups. But abelian groups are especially interesting and nice, and there's a huge branch of math called "homological algebra" that studies categories similar to the category of abelian groups. These are called "abelian categories". In an abelian category, you've got direct sums, kernels, cokernels, exact sequences, chain complexes and so on — all things you're used to in the category of abelian groups!

Can we categorify all this stuff? Yes — and that's what Enrico Vitale is busy doing! He started by telling us how all these ideas generalize from abelian groups to symmetric 2-groups, and how they change.

For example, besides the "kernel" and "cokernel", we also need extra concepts. The reason is that the kernel of a homomorphism says if the homomorphism is one-to-one, while its cokernel says if it's onto. Functions can be nice in two basic ways: they can be one-to-one, or onto. But because categories have an extra level, functors between them can be nice in *three* ways, called "faithful", "full" and "essentially surjective". So, we need more than just the kernel and cokernel to say what's going on. We also need the "pip" and "copip".

The concepts of exact sequence and chain complex get subtler, too. You can read about these things here:

20) Aurora del Ro, Martnez-Moreno and Enrico Vitale, "Chain complexes of symmetric categorical groups", *JPAA* **196** (2005), 279–312. Also available at `http://www.math.ucl.ac.be/membres/vitale/SCG-compl3.pdf`

21) Pilar Carrasco, Antonio Garzn and Enrico Vitale, "On categorical crossed modules", *TAC* **16** (2006), 85–618, available as `http://tac.mta.ca/tac/volumes/16/22/16-22abs.html`

By generalizing properties of the category of abelian groups, people invented the concept of "abelian category". Similarly, Vitale told us a definition of "2-abelian 2-category", obtained by generalizing properties of the 2-category of symmetric 2-groups. I believe this is discussed here:

22) Mathieu Dupont: *Catgories abliennes en dimension 2*, Ph.D. Thesis, Universit Catholique de Louvain, 2008. Available in English as arXiv:0809.1760. Original available at http://hdl.handle.net/2078.1/12735

Mathieu Dupont is defending his dissertation on June 30th. I hope he puts it on the arXiv after that. (He did!)

All this stuff gets even more elaborate as we move to n-groups for higher n. To some extent this is the subject of homotopy theory, but one also wants a more explicitly algebraic approach. See for example:

23) Giuseppe Metere: *The ziqqurath of exact sequences of n-groupoids*, Ph.D. Thesis, Universit di Milano, 2008. Also available at arXiv:0802.0800.

The relation between 2-groups and topology is made explicit using the concept of "fundamental 2-group". Just as every space equipped with a basepoint has a fundamental group, it has a fundamental 2-group. And for a homotopy 2-type, this 2-group captures *everything* about the space - at least if we count homotopy equivalent spaces as the same.

David Roberts prepared an excellent talk about the fundamental 2-group of a space for this workshop. Unfortunately, he was unable to come. Luckily, you can still see his talk:

24) David Roberts, Fundamental 2-groups and 2-covering spaces, http://golem.ph.utexas.edu category/2008/06/fundamental_2groups_and_2cover.html

The basic principle of Galois theory says that covering spaces of a connected space are classified by subgroups of its fundamental group. Here Roberts explains how "2-covering spaces" of a connected space are classified by "sub-2-groups" of its fundamental 2-group!

Aurora del Ro spoke on fundamental 2-groups and their application to K-theory. Whenever we have a fibration of pointed spaces

$$F \to E \to B$$

we get a long exact sequence of homotopy groups

$$\ldots \to \pi_n(F) \to \pi_n(E) \to \pi_n(B) \to \pi_{n-1}(F) \to \ldots$$

This is a standard tool in algebraic topology; I sketched how it works in "Week 151".

Now, the nth homotopy group of a space X, written $\pi_n(X)$, is just the fundamental group of the $(n-1)$-fold loop space of X. So, the Spanish categorical group experts define the nth "homotopy 2-group" of a space X to be the fundamental 2-group of an iterated loop space of X. And, it turns out that any fibration of spaces gives a long exact sequence of homotopy 2-groups!

I was surprised by this, but in retrospect I shouldn't have been. Any fibration gives a "long exact sequence of iterated loop spaces":

$$\ldots \to L^n F \to L^n E \to L^n B \to L^n - 1^F \to \ldots$$

So, as soon as we have a definition of "fundamental n-groupoids" and long exact sequences of n-groupoids, and can show that taking the fundamental n-groupoid preserves exactness, we can get a long exact sequence of fundamental n-groupoids. If we simply define a fundamental n-groupoid to *be* a homotopy n-type, this should not be hard.

But this was just the warmup for Aurora's talk, which was about K-theory. Quillen set up modern algebraic K-theory by defining the K-groups of a ring R to be the homotopy groups of a certain space called $B\mathrm{GL}(R)^+$. In here talk, Aurora defined the K-2-groups of a ring in the same way, but using homotopy 2-groups! And then she went ahead and studied them. . .

The slides for Aurora's talk are — as for many of the talks — available from the workshop's website:

25) Aurora del Ro, "Algebraic K-theory for categorical groups", `http://mat.uab.cat/~kock/crm/hocat/cat-groups/slides/delRio.pdf`

See also the paper she and Antonio Garzn wrote on this topic:

26) Antonio Garzn and Aurora del Ro, "On algebraic K-theory categorical groups", `http://www.ugr.es/~agarzon/K-thCG.pdf`

Also try these other papers:

26) Antonio Garzn and Aurora del Ro, "Low-dimensional cohomology of categorical groups", *Cahiers de Topologie et Gomtrie Diffrentielle Catgoriques* **44** (2003), 247–280. Available at `http://www.numdam.org/numdam-bin/fitem?id=CTGDC_2003__44_4_247_0`

This one gets into K-theory:

27) Antonio Garzn and Aurora del Ro, "The Whitehead categorical group of derivations", *Georgian Mathematical Journal* **09** (2002), 709–721. Available at `http://www.heldermann.de/GMJ/GMJ09/GMJ094/gmj09053.htm`

Addenda: Writing the above stuff caused me to miss Behrang Noohi's talk on using diagrams called "butterflies" to efficiently describe weak homomorphisms between strict 2-groups (in the guise of crossed modules). Luckily Tim Porter summarized it at the n-Category Caf:

27) Timothy Porter, "Behrang Noohi on butterflies and weak morphisms between 2-groups", available at `http://golem.ph.utexas.edu/category/2008/06/behrang_noohi_on_butterflies_a html`

For more details, you can't beat the original paper:

28) Behrang Noohi, "On weak maps between 2-groups", available as arXiv:math/0506313.

Also at the n-Category Caf, Bruce Bartlett discussed Tim Porter's talk at the categorical groups workshop:

29) Bruce Bartlett, "Tim Porter on formal homotopy quantum field theories and 2-groups", available at `http://golem.ph.utexas.edu/category/2008/06/tim_porter_on_formal_homotopy.html`

Actually Porter gave two talks. The first was an introduction to simplicial methods and crossed complexes, but Bartlett didn't summarize that, and no slides are available. So for that, you should get ahold of the following free book:

30) Timothy Porter, "The Crossed Menagerie: an Introduction to Crossed Gadgetry and Cohomology in Algebra and Topology", available at `http://www.informatics.bangor.ac.uk/~tporter/menagerie.pdf`

and (harder) this review article highly recommended by Porter:

31) E. Curtis, "Simplicial homotopy theory", *Adv. Math.* **6** (1971), 107–209.

The second talk by Porter, the one Bartlett blogged about, can be found at the workshop's website:

32) Timothy Porter, "Formal homotopy quantum field theories and 2-groups", available at `http://mat.uab.cat/~kock/crm/hocat/cat-groups/slides/Porter.pdf`

This talk covered the papers by Martins, Porter and Turaev mentioned above.

I apologize to everyone whose talks I have not mentioned!

You can see more discussion of this Week's Finds at the n-Category Caf.

Virtue is harmony.

— *attributed to Pythagoras*

Week 267

July 23, 2008

After the workshop on categorical groups in Barcelona, I went to Granada — the world capital of categorical groups! Pilar Carrasco, an expert on this subject, had kindly invited me to spend a week there and give some talks. Even more kindly, she put me in a hotel right next to the Alhambra.

So, this week I'll tell you about some categorical groups I saw in the Alhambra.

I've long been fascinated by that melting-pot of cultures in southern Spain called Andalusia. I wrote about it back in "Week 221". It was invaded by Muslims in 711 AD, and became a center for mathematics and astronomy from around 930 AD to around the 1200s, when the city of Toledo, recaptured by Catholic Spaniards, became the center of a big translation industry — translating Arabic and Hebrew texts into Latin. This was important for the transmission of ancient Greek writings into Western Europe.

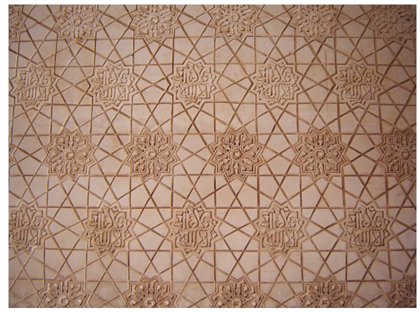
The Alhambra was built after the true heyday of Andalusia, in the era when Muslims had almost been pushed out by the Catholics. Its construction was begun by Muhammed ibn Nasr, founder of the Nasrid Dynasty — the last Muslim dynasty in Spain.

In 1236, Ferdinand III of Castile captured the marvelous city of Cordoba, "ornament of the world". Ibn Nasr saw which way the wind was blowing, and arranged to pay tribute to Ferdinand and even help him take the city of Seville in return for leaving his city — Granada — alone. He started building the Alhambra in 1238. It was completed in the late 1300s.

For the mathematician, one striking thing about the Alhambra is the marvelous tile patterns. On my visit, I took photos of all the tiles I could see:

1) John Baez, Alhambra tiles, `http://math.ucr.edu/home/baez/alhambra`

Here are a few of my favorites:

Some people say that tilings with all 17 possible "wallpaper groups" as symmetries can be found in the Alhambra. This article rebuts that claim with all the vehemence such an academic issue deserves, saying that only 13 wallpaper groups are visible:

2) Branko Grnbaum, "The emperor's new clothes: full regalia, G-string, or nothing?", with comments by Peter Hilton and Jean Pedersen, *Math. Intelligencer* **6** (1984), 47–56.

As mentioned in "Week 221", this page shows 13 of the 17:

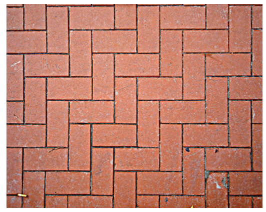
3) Steve Edwards, "Tilings from the Alhambra", `http://www2.spsu.edu/math/tile/grammar/moor.htm`

Of the remaining four, two seem completely absent in Islamic art — the groups called "pgg" and "pg". Both are fairly low on symmetries, so they might have been avoided for lack of visual interest.

Let me describe them, just for fun. You can learn the definition of wallpaper groups, and a lot more about them, from this rather wonderful article:

4) Wikipedia, "Wallpaper group", `http://en.wikipedia.org/wiki/Wallpaper_group`

The group *pgg* is the symmetry group of this popular zig-zag method of laying bricks:



The only rotations in this group are 180-degree rotations: you can rotate any brick 180 degrees around its center, and the pattern comes back looking the same. There are no reflections in this group. But, there are "glide reflections" in two diagonal directions: a "glide reflection" is a combination of a translation along some line and a reflection across that line.

The group *pg* is a subgroup of pgg. If we take our zig-zag pattern of bricks and break the 180-degree rotation symmetry somehow, the remaining symmetry group is pg:

This group contains no reflections and no rotations. It contains translations along one diagonal axis and "glide reflections" along another.

For more on tilings, try this book. Among other things, it points out that there's a lot more beauty and mathematical structure in tilings than is captured by their symmetry groups!

5) Branko Grnbaum and G. C. Shephard, *Tilings and Patterns*, New York, Freeman, 1987.

The mathemagician John Horton Conway has come up with a very nice proof that there are only 17 wallpaper groups. This is nicely sketched in the Wikipedia article above, but detailed here:

6) John H. Conway, "The orbifold notation for surface groups", in *Groups, Combinatorics and Geometry*, London Math. Soc. Lecture Notes Series **165**, Cambridge U. Press, Cambridge, 1990, pp. 438–447

Here's the basic idea. Take a wallpaper pattern and count two points as "the same" if they're related by a symmetry. In other words — in math jargon — take the plane and mod out by the wallpaper group. The result is a 2-dimensional "orbifold".

In a 2d manifold, every point has a little neighborhood that looks like the plane. In a 2d orbifold, every point has a little neighborhood that looks either like the plane, or the plane mod a finite group of rotations and/or reflections.

Let's see how this works for a few simple wallpaper groups.

I'll start with the most boring wallpaper group in the world, $p1$. If you thought pg was dull, wait until you see $p1$. It's the symmetry group of this wallpaper pattern:

```
  R       R       R
R R R R R R R R R R R
  R       R       R
  R       R       R
  R       R       R
R R R R R R R R R R R
  R       R       R
  R       R       R
  R       R       R
R R R R R R R R R R R
  R       R       R
```

This group doesn't contain any rotations, reflections or glide reflections — I used the letter R to rule those out. It only contains translations in two directions, the bare minimum allowed by the definition of a wallpaper group.

If we take the plane and mod out by this group, all the points labelled x get counted as "the same":

```
  R       R       R
R R R R R R R R R R R
  R       R       R
  x       x       x
  R       R       R
R R R R R R R R R R R
  R       R       R
  x       x       x
  R       R       R
R R R R R R R R R R R
  R       R       R
```

Similarly, all these points labelled y get counted as "the same":

```
 R     R     R
RRR y RRR y RRR
 R     R     R
 R     R     R
 R     R     R
RRR y RRR y RRR
 R     R     R
 R     R     R
 R     R     R
RRR y RRR y RRR
 R     R     R
```

So, when we take the plane and mod out by the group $p1$, we get a rectangle with its right and left edges glued together, and with its top and bottom edges glued together. This is just a torus. A torus is a 2d manifold, which is a specially dull case of a 2d orbifold.

Now let's do a slightly more interesting example:

```
 T   T   T
TTTTTTTTTTT
 T   T   T
 T   T   T
 T   T   T
TTTTTTTTTTT
 T   T   T
 T   T   T
 T   T   T
TTTTTTTTTTT
 T   T   T
```

The letter T is more symmetrical than the letter R: you can reflect it, and it still looks the same. So, the symmetry group of this wallpaper pattern, called pm, is bigger than $p1$: it also contains reflections and glide reflections along a bunch of parallel lines. So now, all these points labelled x get counted as the same when we mod out:

```
 T     T     T
TTTTTTTTTTTTTTT
 T     T     T
 T x x T x x T
 T     T     T
TTTTTTTTTTTTTTT
 T     T     T
 T x x T x x T
 T     T     T
TTTTTTTTTTTTTTT
 T     T     T
```

and similarly for all these points labelled y:

```
  T       T       T
T T y T y T y T y T T
  T       T       T
  T       T       T
  T       T       T
T T y T y T y T y T T
  T       T       T
  T       T       T
  T       T       T
T T y T y T y T y T T
  T       T       T
```

but look how these points labelled z work:

```
  T       T       T
T T T T T T T T T T T
  T       T       T
  T   z   T   z   T
  T       T       T
T T T T T T T T T T T
  T       T       T
  T   z   T   z   T
  T       T       T
T T T T T T T T T T T
  T       T       T
```

There are only half as many z's per rectangle, since they lie on reflection lines.

Because of this subtlety, this time when we mod out we get an orbifold that's not a manifold! It's the torus of the previous example, but now folded in half. We can draw it as *half* of one of the rectangles above, with the top and bottom glued together, but not the sides:

```
T T ·
T   ·
T   ·
T   ·
T T ·
```

So, it's a cylinder... but in a certain technical sense the points at the ends of this cylinder count as "half points": they lie on reflection lines, so they've been "folded in half".

This particular orbifold looks a lot like a 2d manifold "with boundary". That's a generalization of a 2d manifold where some points — the "boundary" points — have a neighborhood that looks like a half-plane. But 2d orbifolds can also have "cone points" and "mirror reflector" points.

What's a cone point? It's like the tip of a cone. Take a piece of paper, cut it like a pie into n equal wedges, take one wedge, and glue its edges together! This gives a cone — and the tip of this cone is a "cone point". We say it has "order n", because the angle around it is not 2π but $2\pi/n$.

Here's a more sophisticated way to say the same thing: take a regular n-gon and mod out by its rotational symmetries, which form a group with n elements. When we're done, the point in the center is a cone point of order n.

We could also mod out by the rotation *and* reflection symmetries of the n-gon, which form a group with $2n$ elements. This is harder to visualize, but when we're done, the point in the center is a "corner reflector of order $2n$".

To see one of these fancier possibilities, let's look at the orbifold coming from a wallpaper pattern with even more symmetries:

```
 o   o   o
ooooooooooo
 o   o   o
 o   o   o
 o   o   o
ooooooooooo
 o   o   o
 o   o   o
 o   o   o
ooooooooooo
 o   o   o
```

These are supposed to be rectangles, not squares. So, 90-degree rotations are not symmetries of this pattern. But, in addition to all the symmetries we had last time, now we have reflections about a bunch of horizontal lines. We get a wallpaper group called pmm.

What orbifold do we get now? It's a torus folded in half *twice!* That sounds scary, but it's not really. We can draw it as a *quarter* of one of the rectangles above:

```
...
. .
...
```

Now no points on the edges are glued together. So, it's just a rectangle. The points on the edges are boundary points, and the corners are corner reflection points of order 4.

In a certain technical sense — soon to be explained — points on the edges of this rectangle count as "half points", since they lie on a reflection line and have been folded in half. But the corners count as "$1/4$ points", since they lie on *two* reflection lines, so they've been folded in half *twice!*

This is where it gets really cool. There's a way to define an "Euler characteristic" for orbifolds that generalizes the usual formula for 2d manifolds. And, it can be a fraction!

The usual formula says to chop our 2d manifold into polygons and compute

$$V - E + F$$

That is: the number of vertices, minus the number of edges, plus the number of faces.

In a 2d orbifold, we use the same formula, but with some modifications. First, we require that every cone point or corner reflector be one of our vertices. Then:

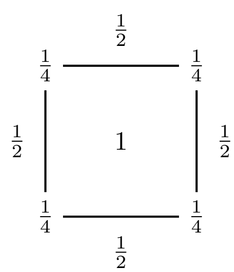
- We count edges and vertices on the boundary for $1/2$ the usual amount.
- We count a cone point of order n as $1/n$th of a point.
- We count a corner reflector of order $2n$ as $1/(2n)$th of a point.

The idea is that these features have been "folded over" by a certain amount, so they count for a fraction of what they otherwise would.

It turns out that if we calculate the Euler characteristic of a 2d orbifold coming from a wallpaper group, we always get zero. And, there are just 17 possibilities!

In fact, wallpaper groups are secretly *the same* as 2d orbifolds with vanishing Euler characteristic! So, they're not just mathematical curiosities: they're almost as fundamental as 2d manifolds.

The torus and the cylinder, which we've already seen, are two examples. These are well-known to have Euler characteristic zero. Of course, we should be careful: now we're dealing with the cylinder as an orbifold, so the points on the boundary count as "half points" — but its Euler characteristic still vanishes. A more interesting example is the square we get from the group pmm. Let's chop it into vertices, edges, and one face, and figure out how much each of these count:



So, the Euler characteristic of this orbifold is

$$(1/4 + 1/4 + 1/4 + 1/4) - (1/2 + 1/2 + 1/2 + 1/2) + 1 = 0$$

This is different than the usual Euler characteristic of a rectangle!

As usual, Conway has figured out a charming way to explain all this:

7) John Conway, Peter Doyle, Jane Gilman and Bill Thurston, "Geometry and the Imagination in Minneapolis", available at `http://www.geom.uiuc.edu/docs/doyle/mpls/handouts/handouts.html`

Especially see the sections near the end entitled "Symmetry and orbifolds", "Names for features of symmetrical pattern", "Names for symmetry groups and orbifolds", "The orbifold shop" and "The Euler characteristic of an orbifold". He has a way of naming 2d orbifolds that lets you easily see how they "cost". Any orbifold that costs 2 dollars corresponds to a wallpaper group, and if you list them, you see there are exactly 17!

(I only know two places where the number 17 played an important role in mathematics — the other is much more famous.)

Why exactly 2 dollars? This is related to this formula for the Euler characteristic of a g-handled torus:

$$V - E + F = 2 - 2g$$

where g is the number of handles. At the orbifold shop, each handle costs 2 dollars. So, if you buy one handle, you're done: you get an ordinary torus, which has Euler characteristic zero. This is the result of taking the plane and modding out by the most boring wallpaper group in the world. If you don't waste all your money on a handle, you can buy more interesting orbifolds.

Devoted readers of This Week's Finds can guess why I'm talking about this. It's not just that I like the Alhambra. The usual Euler characteristic is a generalization of the cardinality of a finite set that allows *negative* values — but not fractional ones. There's also something called the "homotopy cardinality" of a space, which allows *fractional* values — but not negative ones!

If we combine these ideas, we get the orbifold Euler characteristic, which allow both negative and fractional values. This has various further generalizations, like the Euler characteristic of a differentiable stack — and Leinster's Euler characteristic of a category, explained in "Week 244". We should be able to use these to categorify a lot of math involving rational numbers.

But, it's especially cool how this game of listing all 2d orbifolds with Euler characteristic 0 fits together with things like the "Egyptian fractions" approach to ADE Dynkin diagrams — as explained in "Week 182". Here I used the Euler characteristic to list all ways to regularly tile a sphere with regular polygons. This gave the McKay correspondence linking Platonic solids to the simply-laced Lie algebras A_n, D_n, E_6, E_7 and E_8. Taking different values of the Euler characteristic, the same idea let us classify regular tilings of the plane or hyperbolic plane by regular polygons, and see how these correspond to "affine" or "hyperbolic" simply-laced Kac-Moody algebras. Even better, compact quotients of these tilings give some very nice modular curves, like Klein's quartic curve:

8) John Baez, "Klein's quartic curve", `http://math.ucr.edu/home/baez/klein.html`

So, there a lot of connections to be made here. . . and I can tell I haven't made them all yet! Why don't you give it a try?

To add to the fun, my friend Eugene Lerman has just written a very nice survey paper on orbifolds:

9) Eugene Lerman, "Orbifolds as stacks?", available as `arXiv:0806.4160`.

This describes some deeper ways to think about orbifolds. For example, when we form an orbifold by taking the plane and modding out by a wallpaper group, we shouldn't really say two points on the plane become *equal* if there's a symmetry carrying one to the other. Instead, we should say they are *isomorphic* — with the symmetry being the isomorphism. This gives us a groupoid, whose objects are points on the plane and whose morphisms are symmetries taking one point to another. It's a "Lie groupoid", since there's a manifold of objects and a manifold of morphisms, and everything in sight is smooth.

So, orbifolds can be thought of as Lie groupoids. This leads to the real point of Lerman's paper: orbifolds form a 2-category! This should be easy to believe, since there's a 2-category with

- groupoids as objects,
- functors as morphisms, and
- natural transformations as 2-morphisms.

In "Week 75" and "Week 80" I explained the closely related 2-category with *categories* as objects; the same idea works for groupoids. So, to get a 2-category of *Lie* groupoids, we just need to take this idea and make everything "smooth" in a suitable sense.

This turns out to be trickier than you might at first think — and that's where "differentiable stacks" come in. I should explain these someday, but not today. For now, try these nice introductions:

10) J. Heinloth, "Some notes on differentiable stacks", *Mathematisches Institut Universititt Gttingen, Seminars 2004–2005*, ed. Yuri Tschinkel, 1–32. Available as `http://www.math.nyu.edu/~tschinke/WS04/pdf/book.pdf` or separately as `http://www.uni-essen.de/~hm0002/stacks.pdf`

11) Kai Behrend and Ping Xu, "Differentiable stacks and gerbes", available as `arXiv:math/0605694`.

Now, besides the Alhambra, Granada also has a wonderful Department of Algebra. Yes — a whole department, just for algebra! And this department has many experts on categorical groups, also known as 2-groups. So it's worth noting that there are 2-groups lurking in the Alhambra.

Any object in any category has a group of symmetries. Similarly, any object in any 2-category has a 2-group of symmetries. So, any orbifold has a 2-group of symmetries. We should be able to get some interesting 2-groups this way.

The group of *all* symmetries of a manifold — its "diffeomorphism group" — is quite huge. That's because you can warp it and bend it any way you like, as long as that way is smooth. Similarly, the 2-group of *all* symmetries of an orbifold will often be quite huge.

To cut down the symmetry group of a manifold, we can equip it with a Riemannian metric — a nice distance function — and then consider only symmetries that preserve distances. We can get lots of nice groups this way, called "isometry groups". For example, the group E_8, which has been in the news disturbingly often of late, is the isometry group of a 128-dimensional Riemannian manifold called the "octooctonionic projective plane".

So, maybe we can get some nice 2-groups as "isometry 2-groups" of "Riemannian orbifolds". Of course, for this to make sense, we need to know what we mean by a Riemannian metric on on orbifold! I'm no expert on this, but I'm pretty sure the idea makes sense. And I'm pretty sure that the 2d orbifold we get from a specific wallpaper pattern has a Riemannian metric coming from the usual distance function on the plane.

(Warning: the same wallpaper group can arise as symmetries of wallpaper patterns that are different enough to give different Riemannian orbifolds!)

So, here's a potentially fun question: what 2-groups show up as isometry 2-groups of Riemannian orbifolds coming from wallpaper patterns? Try to work out some examples. I don't expect the answer to be staggeringly profound — but it sets up a link between the Alhambra and 2-groups, and that's cool enough for me!

By the way, I obtained copies of some very interesting theses in Granada:

13) Antonio Martinez Cegarra, *Cohomologia Varietal*, Ph.D. thesis, Departamento de Algebra y Fundamentos, Universidad de Santiago de Compostela.

14) Pilar Carrasco, *Complejos Hipercruzados: Cohomologia y Extensiones*, Ph. D. thesis, Cuadernos de Algebra 6, Departamento de Algebra y Fundamentos, Universidad de Granada, 1987.

Antonio Cegarra was the one who brought 2-group theory to Granada, and Pilar Carrasco was his student. It's unfortunate that these theses come from the day before electronic typesetting. Luckily, Carrasco's was later turned into a paper:

15) Pilar Carrasco and Antonio Martinez Cegarra, "Group-theoretic algebraic models for homotopy types", *Jour. Pure Appl. Algebra* **75** (1991), 195–235. Also available at `http://www.sciencedirect.com/science/article/pii/002240499190133M` (click "Download PDF" at top left).

This tackles the ever-fascinating, ever-elusive problem of taking the information in the homotopy type of a topological space and packaging it in some manageable way. If our space is connected, with a chosen basepoint, and it has vanishing homotopy groups above the 2nd, a 2-group will do the job quite nicely! The same idea should work for numbers larger than 2, but n-groups get more and more elaborate as n increases. Carrasco and Cegarra package all the information into a "hypercrossed complex", and I would really like to understand this better.

Carrasco and Cegarra's paper is quite dense. So, I'm very happy to hear that Carrasco plans to translate her thesis into English!

Before I finish, let me mention one more paper about 2-groups:

16) Joo Faria Martins, "The fundamental crossed module of the complement of a knotted surface", available as arxiv:0801.3921.

Martins was unable to attend the Barcelona workshop on 2-groups, but I met him later in Lisbon, and he explained some of the ideas here to me.

A crossed module is just another way of thinking about a 2-group. So, translating the language a bit, the basic concept behind this paper is the "fundamental 2-group" of (X, A, x). Here X is a topological space that contains a subspace A that contains a point x.

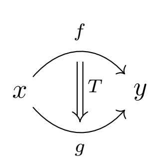
Here's how it goes. A 2-group is a 2-category with one object:

$$x$$

a bunch of morphisms:

$$x \xrightarrow{f} x$$

(which must all be invertible), and a bunch of 2-morphisms:



(which must also be invertible).

So, we get the fundamental 2-group of (X, A, x) as follows:

- Let the only object be the point x.
- Let the morphisms be paths in A starting and ending at x.
- Let the 2-morphisms be homotopy classes of paths of paths in X.

If you let A be all of X, you get the fundamental 2-group of (X, x), and this is what people mean when they say connected pointed homotopy 2-types are classified by 2-groups. But the generalization is also quite nice.

In the above paper, Martins uses this generalization, and a bunch of other ideas, to give an explicit presentation of the fundamental 2-group of the complement of a 2-knot (a sphere embedded in 4d Euclidean space). In a certain sense this generalizes the usual "Wirtinger presentation" of the fundamental group of the complement of a knot. But, it's a bit different.

Addenda: This looks like a good book for fans of Islamic tile patterns:

17) Eric Broug, *Islamic Geometric Patterns*, Thames & Hudson, 2008.

Also, James Propp suggests that people interested in tilings and orbifolds and the work of Conway may enjoy this book:

18) John H. Conway, Heidi Burgiel, and Chaim Goodman-Strauss, *The Symmetries of Things*, A. K Peters Limited, 2008.

I should mention the definition of a wallpaper group: it's just a discrete subgroup of the isometry group of the plane that includes translations in two linearly independent directions. We need the right equivalence relation on these groups to get just 17 of them: they're equivalent if you can conjugate one by an affine transformation of the plane and get the other.

Alas, over at the n-Category Caf Richard Hepworth has shown that all the isometry 2-groups of orbifolds coming from wallpaper groups are equivalent to mere groups. It's a pity! But, at least his remarks shed a lot of light on the general theory of isometry 2-groups. First he wrote:

> *Here is a recipe for computing the isometry 2-groups of a Riemannian orbifold $X//G$, with G a discrete group acting on a connected simply connected manifold X. I think these are precisely the sorts of orbifolds you are interested in. Apologies for the nasty presentation!*
>
> *There is one object.*
>
> *The arrows are pairs (f, φ) where f is an isometry of X, φ an automorphism of G, and f is equivariant with respect to φ, i.e. $f(gx) = \varphi(g)f(x)$.*
>
> *The 2-arrows from (f, φ) to (k, κ) are the elements g of G for which $k = gf$ and $\kappa = g\varphi g^{-1}$.*

I'll leave you to work out the various composition maps.

Of course, I haven't told you what a Riemannian metric on an orbifold (or groupoid or stack) is. You can find definitions in various places, but in the case in point these things are all equivalent to putting a G-invariant metric on X. (Does one always exist? Yes, but the fact that G acts with finite stabilizers is essential here.)

I also haven't said how I get the above answer. If you already knew the answer for $X = \mathrm{point}$ then the above would be your first guess. The reason the guess is correct is thanks to the assumption that X is connected and simply connected: all G-valued functions on X are constant, and all G-bundles are trivial.

Maybe if you're interested I can go into more detail.

Then he wrote:

Hmm, John and David's discussion has reminded me of a result I once proved:

Suppose we are given a morphism $f\colon X \to Y$ of orbifolds and a 2-automorphism $\varphi\colon f \Rightarrow f$. Then φ is trivial if and only if its restriction to any point of X is trivial.

One consequence of this is the following:

Let X be an "effective" orbifold. Then any self-equivalence of X has no nontrivial automorphisms.

And in particular:

The isometry 2-group of an effective orbifold is equivalent to a group.

What does "effective" mean? It means that the automorphism group of any point in X acts effectively on its tangent space, and consequently that almost all points of the orbifold have no inertia at all. This includes all of the orbifolds you are discussing. The fact that the isometry 2-groups are all equivalent to groups is apparent in the description I gave earlier: if we have a 2-arrow $g\colon (f, \varphi) \Rightarrow (f, \varphi)$ then $gf = f$, so that g is the identity.

Of course, there are many interesting noneffective orbifolds. Some of them are called gerbes. (Gerbes with band a finite group.) Maybe you want to compute their isometry 2-groups? Here's a fact for you:

The isometry 2-group of the nontrivial $\mathbb{Z}/2$-gerbe over S^2 is itself a $\mathbb{Z}/2$-gerbe over $\mathrm{O}(3)$, the isometry group of S^2. In particular, it is not equivalent to any group.

The gerbe over $\mathrm{O}(3)$, however, is trivial. But is there a trivialization that respects the 2-group structure?

Actually not *all* the orbifolds coming from wallpaper groups are "effective" in the above sense. For the orbifold to be effective, I believe the corresponding group must include eitherreflections or glide reflections across two different axes, or a rotation by less than 180 degrees. For example, the torus and "cylinder" described above are noneffective orbifolds. But, most of the interesting examples are covered by Hepworth's results, and the others seem also to have isometry 2-groups that are equivalent to mere groups.

For details try:

19) Richard Hepworth, "The age grading and the Chen-Ruan cup product", available as arXiv:0706.4326.

Richard Hepworth, "Morse inequalities for orbifold cohomology", available as arXiv:0712.2432.

According to Hepworth, the first paper "contains the little fact that implies that the automorphism 2-group of an effective orbifold is equivalent to a group". Of course it contains lots of other stuff, too! The second discusses Morse theory on differentiable Deligne-Mumford stacks (these are the proper tale ones). It defines Morse functions, vector fields and Riemannian metrics on differentiable DM stacks. It also proves that Morse functions are generic and that vector fields can be integrated.

You can see more discussion of this Week's Finds at the n-Category Caf.

> Think of one and minus one. Together they add up to zero, nothing, nada, niente, right? Picture them together, then picture them separating, peeling part.... Now you have something, you have two somethings, where you once had nothing.
>
> — *John Updike*

Week 268

August 6, 2008

This Week will be all about Frobenius algebras and modular tensor categories. But first, here's a beautiful photo of Io, the volcanic moon of Jupiter that I introduced back in "Week 266":

1) NASA Photojournal, "A new year for Jupiter and Io", http://photojournal.jpl.nasa.gov/catalog/PIA02879

Io looks awfully close to Jupiter here! It's actually 2.5 Jupiter diameters away... but that's close enough to cause the intense tidal heating that leads to sulfur volcanoes.

I told you about Frobenius algebras in "Week 174" and "Week 224", but I think it's time to talk about them again! In the last few weeks, I've run into them — and their generalizations — in a surprising variety of ways.

First of all, Jamie Vicary visited me here in Paris and explained how certain Frobenius algebras can be viewed as classical objects living in a quantum world — governed by quantum logic.

Mathematicians in particular are used to thinking of the quantum world as a mathematical structure resting on foundations of classical logic: first comes set theory, then Hilbert spaces on top of that. But what if it's really the other way around? What if classical mathematics is somehow sitting inside quantum theory? The world is quantum, after all.

There are a couple of papers so far that discuss this provocative idea:

2) Bob Coecke and Dusko Pavlovic, "Quantum measurements without sums", in *The Mathematics of Quantum Computation and Technology*, eds. Chen, Kauffman and Lomonaco, Chapman and Hall/CRC, New York, pp. 559–596. Also available as quant-ph/0608035.

3) Jamie Vicary, "Categorical formulation of quantum algebras", available as arXiv:0805.0432.

Second, Paul-Andr Mellis, the computer scientist and logician who's my host here, has been telling me how logic can be nicely formulated in certain categories — "$*$-autonomous categories" — which can be seen as *categorified* Frobenius algebras. Here the idea goes back to Ross Street:

4) Ross Street, "Frobenius monads and pseudomonoids", *J. Math. Physics* **45** (2004) 3930–3948. Available as `http://www.math.mq.edu.au/~street/Frob.pdf`

Paul-Andr is teaching a course on this and related topics; you can see the slides for his course here:

5) Paul-Andr Mellis, "Groupoides quantiques et logiques tensorielles: une introduction", course notes at `http://www.pps.jussieu.fr/~mellies/teaching.html`

See especially the fourth class.

But to get you ready for this material, I should give a quick introduction to the basics!

If you're a normal mathematician, the easiest definition of "Frobenius algebra" is something like this. For starters, it's an "algebra": a vector space with an associative product that's linear in each argument, and an identity element 1. But what makes it "Frobenius" is that it's got a nondegenerate bilinear form g satisfying this axiom:

$$g(ab, c) = g(a, bc)$$

I'm calling it "g" to remind geometers of how nondegenerate bilinear forms are used as "metrics", like the metric tensor at a point of a Riemannian or Lorentzian manifold. But beware: we'll often work with complex instead of real vector spaces. And, we won't demand that $g(a, b) = g(b, a)$, though this holds in many examples.

Let's see some examples! For starters, we could take the algebra of $n \times n$ matrices and define

$$g(a, b) = \mathrm{tr}(ab)$$

where "tr" is the usual trace. Or, we could perversely stick any nonzero number in this formula, like

$$g(a, b) = -37\,\mathrm{tr}(ab)$$

Or, we could take a bunch of examples like this and take their direct sum. This gives us the most general "semisimple" Frobenius algebra.

So, semisimple Frobenius algebras are pathetically easy to classify. There's also a vast wilderness of non-semisimple ones, which will never be classified. But for a nice step in this direction, try Prop. 2 in this paper:

6) Steve Sawin, "Direct sum decompositions and indecomposable TQFTs", *J. Math. Phys.* **36** (1995) 6673–6680. Also available as q-alg/9505026.

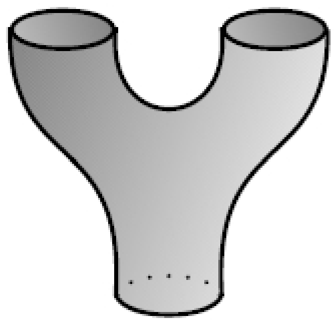
This classifies all commutative Frobenius algebras that are "indecomposable" — not a direct sum of others.

Note the mention of topological quantum field theories, or TQFTs. Here's why. Suppose you have an n-dimensional TQFT. This gives vector spaces for $(n-1)$-dimensional manifolds describing possible choices of "space", and operators for n-dimensional manifolds going between these, which describe possible choices of "spacetime".

So, it gives you some vector space for the $(n-1)$-sphere, say A. And, this vector space is a commutative Frobenius algebra!

Let me sketch the proof. I'll use lots of hand-wavy reasoning, which is easy to make rigorous using the precise definition of a TQFT.

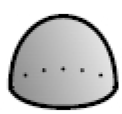
For starters, there's the spacetime where two spherical universes collide and fuse into one. Here's what it looks like for $n = 2$:



This gives the vector space A a multiplication:

$$\begin{aligned} m\colon A \otimes A &\to A \\ a \otimes b &\mapsto ab \end{aligned}$$

Next there's the spacetime where a spherical universe appears from nothing — a "big bang":



This gives A an identity element, which we call 1:

$$\begin{aligned} i\colon \mathbb{C} &\to A \\ 1 &\mapsto 1 \end{aligned}$$

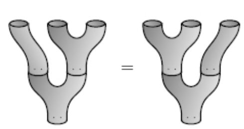
Here $\mathbb{C}$ stands for the complex numbers, but mathematicians could use any field.

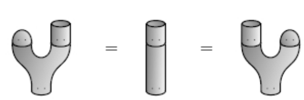
Now we can use topology to show that A is an algebra — namely, that it satisfies the associative law:

$$(ab)c = a(bc)$$

and the left and right unit laws:

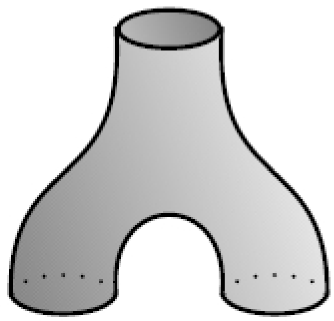
$$1a = a = 1$$





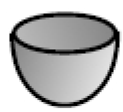
But why is it a Frobenius algebra? To see this, let's switch the future and past in our previous argument! The spacetime where a spherical universe splits in two gives A a "comultiplication":

$$\Delta\colon A \to A \otimes A$$

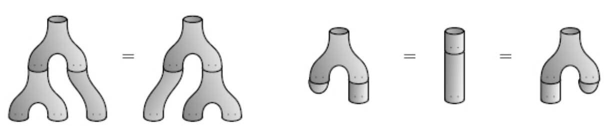


The spacetime where a spherical universe disappears into nothing — a "big crunch" — gives A a trace, or more precisely a "counit":

$$e\colon A \to \mathbb{C}$$

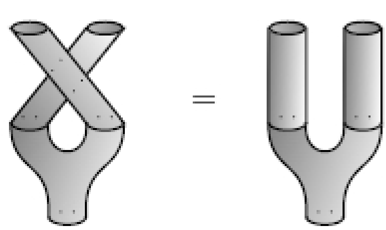


And, a wee bit of topology shows that these make A into a "coalgebra", satisfying the "coassociative law" and the left and right "counit laws":



Everything has just been turned upside down!

It's easy to see that the multiplication on A is commutative, at least for $n > 1$:



Similarly, the comultiplication is "cocommutative" — just turn the above proof upside down!

But why is A a Frobenius algebra? The point is that the algebra and coalgebra structures interact in a nice way. We can use the product and counit to define a bilinear form:

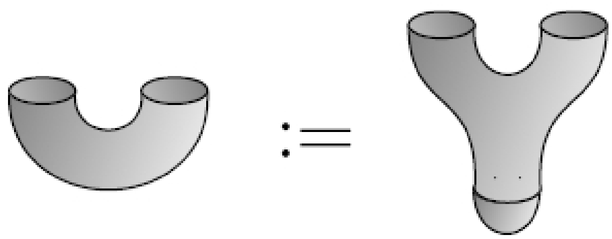
$$g(a,b) = e(ab)$$

This is just what we did in our matrix algebra example, where e was a multiple of the trace.

We can also think of g as a linear operator

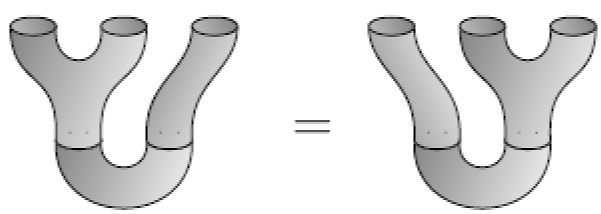
$$g\colon A \otimes A \to \mathbb{C}$$

But now we see this operator comes from a spacetime where two universes collide and then disappear into nothing:



To check the Frobenius axiom, we just use associativity:

$$g(ab,c) = e((ab)c) = e(a(bc)) = g(a,bc)$$



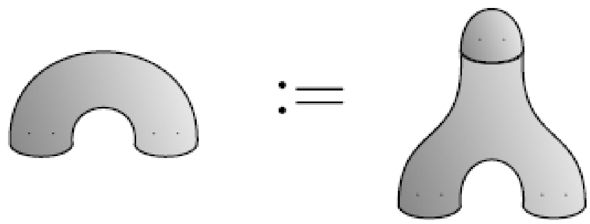
But why is g nondegenerate? I'll just give you a hint. The bilinear form g gives a map from A to the dual vector space A^*:

$$a \mapsto g(a,-)$$

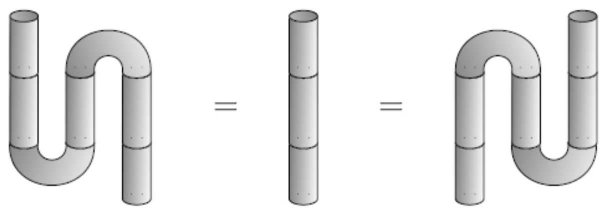
Physicists would call this map "lowering indices with the metric g". To show that g is nondegenerate, it's enough to find an inverse for this map, which physicists would call "raising indices". This should be a map going back from A^* to A. To build a map going back like this, it's enough to get a map

$$h\colon \mathbb{C} \to A \otimes A$$

and for this we use the linear operator coming from this spacetime:



The fact that "raising indices" is the inverse of "lowering indices" then follows from the fact that you can take a zig-zag in a piece of pipe and straighten it out!



So, any n-dimensional TQFT gives a Frobenius algebra, and in fact a commutative Frobenius algebra for $n > 1$.

In general there's more to the TQFT than this Frobenius algebra, since there are spacetimes that aren't made of the building blocks I've drawn. But in 2 dimensions, every spacetime can be built from these building blocks: the multiplication and unit, comultiplication and counit. So, with some work, one can show that

> ***a 2d tqft is the same as***
> ***a commutative frobenius algebra.***

This idea goes back to Dijkgraaf:

7) Robbert H. Dijkgraaf, *A Geometric Approach To Two-Dimensional Conformal Field Theory*, PhD thesis, University of Utrecht, 1989.

and a formal proof was given by Abrams:

8) Lowell Abrams, "Two-dimensional topological quantum field theories and Frobenius algebra", *Jour. Knot. Theory and its Ramifications* **5** (1996), 569–587.

This book is probably the best place to learn the details:

9) Joachim Kock, *Frobenius Algebras and 2d Topological Quantum Field Theories*, Cambridge U. Press, Cambridge, 2004.

but for a goofier explanation, try this:

10) John Baez, *Winter 2001 Quantum Gravity Seminar*, Track 1, weeks 11–17, http://math.ucr.edu/home/baez/qg-winter2001/

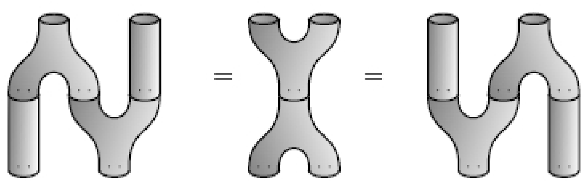
To prove the equivalence of 2d TQFTs and commutative Frobenius algebras, it's handy to use a different definition of Frobenius algebra, equivalent to the one I gave. I said a Frobenius algebra was an algebra with a nondegenerate bilinear form satisfying

$$g(ab, c) = g(a, bc).$$

But this is equivalent to having an algebra that's also a coalgebra, with multiplication and comultiplication linked by the "Frobenius equations":

$$(\Delta \otimes 1_A)(1_A \otimes m) = \Delta m = (m \otimes 1_A)(1_A \otimes \Delta)$$

These equations are a lot more charismatic in pictures!



We can also interpret them conceptually, as follows. If you have an algebra A, it becomes an (A, A)-bimodule in an obvious way... well, obvious if you know what this jargon means, at least. $A \otimes A$ also becomes an (A, A)-bimodule, like this:

$$a(b \otimes c)d = ab \otimes cd$$

Then, a Frobenius algebra is an algebra that's also a coalgebra, where the comultiplication is an (A, A)-bimodule homomorphism! This scary sentence has the Frobenius equations hidden inside it.

The Frobenius equations have a fascinating history, going back to Lawvere, Carboni and Walters, Joyal, and others. Joachim Kock's website includes some nice information about this. Read what Joyal said about Frobenius algebras that made Eilenberg ostentatiously rise and leave the room!

11) Joachim Kock, "Remarks on the history of the Frobenius equation", http://mat.uab.es/~kock/TQFT.html#history

The people I just mentioned are famous category theorists. They realized that Frobenius algebra can be generalized from the category of vector spaces to any "monoidal category" — that is, any category with tensor products. And if this monoidal category is "symmetric", it has an isomorphism between $X \otimes Y$ and $Y \otimes X$ for any objects X and Y, which lets us generalize the notion of a *commutative* Frobenius object.

For a nice intro to these ideas, try the slides of this talk:

12) Ross Street, "Frobenius algebras and monoidal category", talk at the annual meeting of the Australian Mathematical Society, September 2004, available at http://www.maths.mq.edu.au/~street/FAMC.pdf

These ideas allow for a very slick statement of the slogan I mentioned:

a 2d tqft is the same as
a commutative frobenius algebra.

For any n, there's a symmetric monoidal category nCob, with:

- compact oriented $(n-1)$-manifolds as objects;
- compact oriented n-dimensional cobordisms as morphisms.

The objects are choices of "space", and the morphisms are choices of "spacetime".

The sphere is a very nice object in nCob; let's call it A. Then all the pictures above show that A is a Frobenius algebra in nCob! It's commutative when $n > 1$. And when $n = 2$, that's all there is to say! More precisely:

2Cob ***is the***
free symmetric monoidal category on a commutative frobenius algebra.

So, to define a 2d TQFT, we just need to pick a commutative Frobenius algebra in Vect (the category of vector spaces). By "freeness", this determines a symmetric monoidal functor

$$Z\colon \mathsf{2Cob} \to \mathsf{Vect}$$

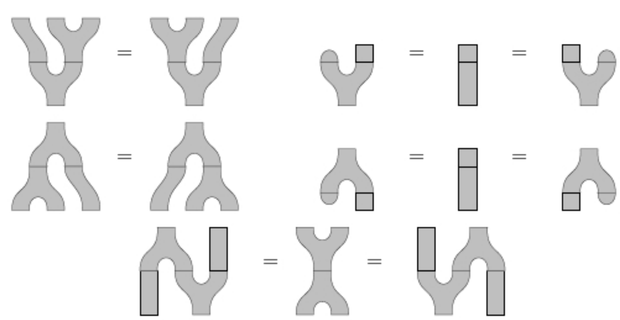
and that's precisely what a 2d TQFT is!

If you don't know what a symmetric monoidal functor is, don't worry — that's just what I'd secretly been using to translate from pictures of spacetimes to linear operators in my story so far. You can get a precise definition from those seminar notes of mine, or many other places.

Now let's talk about some variations on the slogan above.

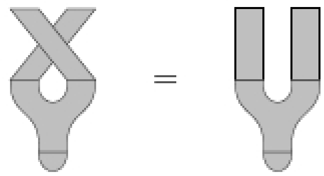
We can think of the 2d spacetimes we've been drawing as the worldsheets of "closed strings" — but ignoring the geometry these worldsheets usually have, and keeping only the topology. So, some people call them "topological closed strings".

We can also think about topological *open* strings, where we replace all our circles by intervals. Just as the circle gave a commutative Frobenius algebra, an interval gives a Frobenius algebra where the multiplication comes from two open strings joining end-to-end to form a single one:



This open string Frobenius algebra is typically noncommutative — draw the picture and see! But, it's still "symmetric", meaning:

$$g(a, b) = g(b, a)$$



This is very nice. But physically, open strings like to join together and form closed strings, so it's better to consider closed and open strings together in one big happy family... or category.

The idea of doing this for topological strings was developed by Moore and Segal:

13) Greg Moore, "Lectures on branes, K-theory and RR charges", *Clay Math Institute Lecture Notes* (2002), available at `http://www.physics.rutgers.edu/~gmoore/clay1/clay1.html`

Lauda and Pfeiffer developed this idea and proved that this category has a nice description in terms of Frobenius algebras:

14) Aaron Lauda and Hendryk Pfeiffer, "Open-closed strings: two-dimensional extended TQFTs and Frobenius algebras", *Topology Appl.* **155** (2008) 623–666. Also available as `math.AT/0510664`.

Here's what they prove, encoded as a mysterious slogan:

the category of open-closed topological strings is the free symmetric monoidal category on a "knowledgeable" frobenius algebra.

If you like the pictures I've been drawing so far, you'll *love* this paper — since that's where I got most of these pictures! And, it's just the beginning of a longer story where Lauda and Pfeiffer build 2d TQFTs using state sum models:

15) Aaron Lauda and Hendryk Pfeiffer, "State sum construction of two-dimensional open-closed topological quantum field theories", *J. Knot Theory and its Ramifications* **16** (2007), 1121–1163. Also available as `arXiv:math/0602047`.

This generalizes a construction due to Fukuma, Hosono and Kawai, explained way back in "Week 16" and also in my seminar notes mentioned above. Then Lauda and Pfeiffer use this machinery to study knot theory!

16) Aaron Lauda and Hendryk Pfeiffer, "Open-closed TQFTs extend Khovanov homology from links to tangles", available as `math/0606331`.

Alas, explaining this would be a vast digression. I want to keep talking about basic Frobenius stuff.

I guess I should say a bit more about semisimple versus non-semisimple Frobenius algebras.

Way back at the beginning of this story, I said you can get a Frobenius algebra by taking the algebra of $n \times n$ matrices and defining

$$g(a,b) = k\,\mathrm{tr}(ab)$$

for any nonzero constant k. Direct sums of these give all the semisimple Frobenius algebras.

But any algebra acts on itself by left multiplication:

$$L_a \colon b \mapsto ab$$

so for any algebra we can try to define

$$g(a,b) = \mathrm{tr}(L_a L_b)$$

This bilinear form is nondegenerate precisely when our algebra is "strongly separable":

17) Marcelo Aguiar, "A note on strongly separable algebras", available at `http://www.math.tamu.edu/~maguiar/strongly.ps.gz`

Over the complex numbers, or any field of characteristic zero, an algebra is strongly separable iff it's finite-dimensional and semisimple. The story is trickier over other fields — see that last paper of Lauda and Pfeiffer if you're interested.

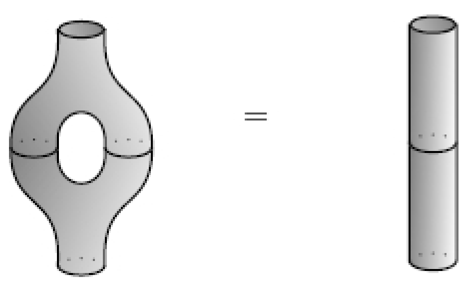
Now, for $n \times n$ matrices,

$$g(a,b) = \mathrm{tr}(L_a L_b)$$

is n times the usual $\mathrm{tr}(ab)$. But it's better, in a way. The reason is that for any strongly separable algebra,

$$g(a,b) = \mathrm{tr}(L_a L_b)$$

gives a Frobenius algebra with a cute extra property: if we comultiply and then multiply, we get back where we started!



This is easy to see if you write the above formula for g using diagrams. Frobenius algebras with this cute extra property are sometimes called "special".

If we use a commutative special Frobenius algebra to get a 2d TQFT, it fails to detect handles! That seems sad. But these papers:

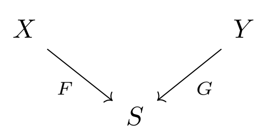
18) Stephen Lack, "Composing PROPs", *Theory and Applications of Categories* **13** (2004), 147–163. Available at `http://www.tac.mta.ca/tac/volumes/13/9/13-09abs.html`

19) R. Rosebrugh, N. Sabadini and R.F.C. Walters, "Generic commutative separable algebras and cospans of graphs", *Theory and Applications of Categories* **15** (Proceedings of CT2004), 164–177. Available at `http://www.tac.mta.ca/tac/volumes/15/6/15-06abs.html`

makes that sad fact seem good! Namely:

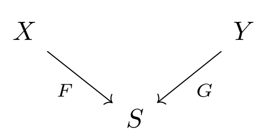
> Cospan(FinSet) ***is the***
> ***free symmetric monoidal category***
> ***on a commutative special frobenius algebra.***

Here Cospan(FinSet) is the category of "cospans" of finite sets. The objects are finite sets, and a morphism from X to Y looks like this:



If you remember the "Tale of Groupoidication" starting in "Week 247", you'll know about spans and how to compose spans using pullback. This is just the same only backwards: we compose cospans using pushout.

But here's the point. A 2d cobordism is itself a kind of cospan:



with two collections of circles included in the 2d manifold S. If we take connected components, we get a cospan of finite sets. Now we've lost all information about handles! And the circle — which was a commutative Frobenius algebra — becomes a mere one-point set — which is a *special* commutative Frobenius algebra.

Now for a few examples of *non*-semisimple Frobenius algebras.

First, take the exterior algebra $\wedge V$ over an n-dimensional vector space V, and pick any nonzero element of degree n — what geometers would call a "volume form". There's a unique linear map

$$e\colon \wedge V \to \mathbb{C}$$

which sends the volume form to 1 and kills all elements of degree $< n$. This is a lot like "integration" — and so is taking a trace. So, you should want to make $\wedge V$ into a Frobenius algebra using this formula:

$$g(a, b) = e(a \wedge b)$$

where $\wedge$ is the product in the exterior algebra. It's easy to see this is nondegenerate and satisfies the Frobenius axiom:

$$g(ab, c) = e(a \wedge b \wedge c) = g(a, bc)$$

So, it works! But, this algebra is far from semisimple.

If you know about cohomology, you should want to copy this trick replacing the exterior algebra by the deRham cohomology of a compact oriented manifold, and replacing e by "integration". It still works. So, every compact manifold gives us a Frobenius algebra!

If you know about algebraic varieties, you might want to copy *this* trick replacing the compact manifold by a complex projective variety. I'm no expert on this, but people seem to say that it only works for Calabi-Yau varieties. Then you can do lots of cool stuff:

20) Kevin Costello, "Topological conformal field theories and Calabi-Yau categories", available as math/0412149.

Here a "Calabi-Yau category" is just the "many-object" version of a Frobenius algebra — a Calabi-Yau category with one object is a Frobenius algebra. There's much more to say about this wonderful paper, but I'm afraid for now you'll have to read it... I'm getting worn out, and I want to get to the new stuff I just learned!

But before I do, I can't resist rounding off one corner I cut. I said that Frobenius algebras show up naturally by taking string theory and watering it down: ignoring the geometrical structure on our string worldsheets and remembering only their topology. A bit more precisely, 2d TQFTs assign linear operators to 2d cobordisms, but *conformal* field theories assign operators to 2d cobordisms *equipped with conformal structures*. Can we describe conformal field theories using Frobenius algebras?

Yes!

21) Ingo Runkel, Jens Fjelstad, Jurgen Fuchs, Christoph Schweigert, "Topological and conformal field theory as Frobenius algebras", available as arXiv:math/0512076.

But, you need to use Frobenius algebras inside a modular tensor category!

I wish I had more time to study modular tensor categories, and tell you all about them. They are very nice braided monoidal categories that are *not* symmetric. You can use them to build 3d topological quantum field theories, and they're also connected to other branches of math.

For example, you can modular tensor categories consisting of nice representations of quantum groups. You can also can get them from rational conformal field theories — which is what the above paper by Runkel, Fjelstad, Fuchs and Schweigert is cleverly turning around. You can also get them from von Neumann algebras!

If you want to learn the basics, this book is great — there's a slightly unpolished version free online:

22) B. Bakalov and A. Kirillov, Jr., *Lectures on Tensor Categories and Modular Functors*, American Mathematical Society, Providence, Rhode Island, 2001. Preliminary version available at http://www.math.sunysb.edu/~kirillov/tensor/tensor.html

But if a book is too much for you, here's a nice quick intro. It doesn't say much about topological or conformal field theory, but it gives a great overview of recent work on the algebraic aspects of tensor categories:

23) Michael Mger, "Tensor categories: a selective guided tour", available as arXiv:0804.3587.

Here's a quite different introduction to recent developments, at least up to 2004:

24) Damien Calaque and Pavel Etingof, "Lectures on tensor categories", available as arXiv:math/0401246.

Still more recently, Hendryk Pfeiffer has written what promises to be a fundamental paper describing how to think of any modular tensor category as the category of representations of an algebraic gadget — a "weak Hopf algebra":

25) Hendryk Pfeiffer, "Tannaka-Krein reconstruction and a characterization of modular tensor categories", available as arXiv:0711.1402.

And here's a paper that illustrates the wealth of examples:

26) Seung-moon Hong, Eric Rowell, Zhenghan Wang, "On exotic modular tensor categories", available as arXiv:07108.5761.

The abstract of this makes me realize that people have bigger hopes of understanding all modular tensor categories than I'd imagined:

> *It has been conjectured that every $(2+1)$-dimensional TQFT is a Chern-Simons-Witten (CSW) theory labelled by a pair (G, k), where G is a compact Lie group, and k in $H^4(BG, Z)$ is a cohomology class. We study two TQFTs constructed from Jones' subfactor theory which are believed to be counterexamples to this conjecture: one is the quantum double of the even sectors of the E_6 subfactor, and the other is the quantum double of the even sectors of the Haagerup subfactor. We cannot prove mathematically that the two TQFTs are indeed counterexamples because CSW TQFTs, while physically defined, are not yet mathematically constructed for every pair (G, k). The cases that are constructed mathematically include:*
>
> - *G is a finite group — the Dijkgraaf-Witten TQFTs;*
> - *G is a torus T^n;*
> - *G is a connected semisimple Lie group — the Reshetikhin-Turaev TQFTs.*
>
> *We prove that the two TQFTs are not among those mathematically constructed TQFTs or their direct products. Both TQFTs are of the Turaev-Viro type: quantum doubles of spherical tensor categories. We further prove that neither TQFT is a quantum double of a braided fusion category, and give evidence that neither is an orbifold or coset of TQFTs above. Moreover, the representation of the braid groups from the half E_6 TQFT can be used to build universal topological quantum computers, and the same is expected for the Haagerup case.*

Anyway, now let me say what Vicary and Mellis have been explaining to me. I'll give it in a highly simplified form... and all mistakes are my own.

First, from what I've said already, every commutative special Frobenius algebra over the complex numbers looks like

$$\mathbb{C} \oplus \mathbb{C} \oplus \ldots \mathbb{C} \oplus \mathbb{C}$$

It's a direct sum of finitely many copies of $\mathbb{C}$, equipped with its god-given bilinear form

$$g(a, b) = \mathrm{tr}(L_a L_b)$$

So, this sort of Frobenius algebra is just an algebra of complex functions on a *finite set*. A map between finite sets gives an algebra homomorphism going back the other way. And the algebra homomorphisms between two Frobenius algebras of this sort *all* come from maps between finite sets.

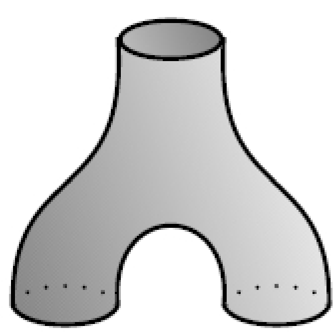
So, the category with:

- commutative special complex Frobenius algebras as objects;
- algebra homomorphisms as morphisms

is equivalent to $\mathsf{FinSet}^{\mathrm{op}}$. This means we can find the category of finite sets — or at least its opposite, which is just as good — lurking inside the world of Frobenius algebras!

Coecke, Pavlovic and Vicary explore the ramifications of this result for quantum mechanics, using Frobenius algebras that are Hilbert spaces instead of mere vector spaces. This lets them define a "†-Frobenius algebra" to be one where the comultiplication and counit are adjoint to the the multiplication and unit. They show that making a finite-dimensional Hilbert space into a commutative special †-Frobenius algebra is the same as *equipping it with an orthonormal basis*.

There's no general way to duplicate quantum states — "you can't clone a quantum" — but if you only want to duplicate states lying in a chosen orthonormal basis you can do it. So, you can think of commutative special †-Frobenius algebras as "classical data types", which let you duplicate information. That's what the comultiplication does: duplicate!



Any commutative special †-Frobenius algebra has a finite set attached to it: namely, the set of basis elements. So, we now see how to describe finite sets starting from Hilbert spaces and introducing a notion of "classical data type" formulated purely in terms of quantum concepts.

The papers by Coecke, Pavlovic and Vicary go a lot further than my summary here. Jamie Vicary even studies how to *categorify* everything I've just mentioned!

A subtlety: it's a fun puzzle to show that in any monoidal category, morphisms between Frobenius algebras that preserve *all* the Frobenius structure are automatically *isomorphisms*. See the slides of Street's talk if you get stuck: he shows how to construct the inverse, but you still get the fun of proving it works.

So, the category with:

- commutative special complex Frobenius algebras as objects;
- Frobenius homomorphisms as morphisms

is equivalent to the *groupoid* of finite sets. We get $\mathsf{FinSet}^{\mathrm{op}}$ if we take algebra homomorphisms, and I guess we get FinSet if we take coalgebra homomorphisms.

Finally, a bit about categorified Frobenius algebras and logic!

I'm getting a bit tired, so I hope you believe that the concept of Frobenius algebra can be categorified. As I already mentioned, Frobenius algebras make sense in any monoidal category — and then they're sometimes called "Frobenius monoids". Similarly, categorified Frobenius algebras make sense in any monoidal bicategory, and then they're sometimes called "Frobenius pseudomonoids". These were introduced in Street's paper "Frobenius monads and pseudomonoids", cited above — but if you like pictures, you may also enjoy learning about them here:

27) Aaron Lauda, "Frobenius algebras and ambidextrous adjunctions", *Theory and Applications of Categories* **16** (2006), 84–122, available at `http://tac.mta.ca/tac/volumes/16/4/16-04abs.html`. Also available as `arXiv:math/0502550`.

I explained some of the basics behind this paper in "Week 174".

But now, I want to give a definition of $*$-autonomous categories, which simultaneously makes it clear that they're natural structures in logic, and that they're categorified Frobenius algebras!

Suppose A is any category. We'll call its objects "propositions" and its morphisms "proofs". So, a morphism

$$f: a \to b$$

is a proof that a implies b.

Next, suppose A is a symmetric monoidal category and call the tensor product "or". So, for example, given proofs

$$f : a \to b, f' : a' \to b'$$

we get a proof

$$f \,\mathrm{or}\, f' : a \,\mathrm{or}\, a' \to b \,\mathrm{or}\, b'$$

Next, suppose we make the opposite category A^{op} into a symmetric monoidal category, but with a completely different tensor product, that we'll call "and". And suppose we have a monoidal functor:

$$\mathrm{not}: A \to A^{\mathrm{op}}$$

So, for example, we have

$$\mathrm{not}(a\,\mathrm{or}\,b) = \mathrm{not}(a) and \mathrm{not}(b)$$

or at least they're isomorphic, so there are proofs going both ways.

Now we can apply "op" and get another functor I'll also call "not":

$$\mathrm{not}\colon A^{\mathrm{op}} \to A$$

Using the same name for this new functor could be confusing, but it shouldn't be. It does the same thing to objects and morphisms; we're just thinking about the morphisms as going backwards.

Next, let's demand that this new functor be monoidal! This too is quite reasonable; for example it implies that

$$\mathrm{not}(a\,\mathrm{and}\,b) = \mathrm{not}(a)\,\mathrm{or}\,\mathrm{not}(b)$$

or at least they're isomorphic.

Next, let's demand that this pair of functors:

$$A \underset{\mathrm{not}}{\overset{\mathrm{not}}{\rightleftarrows}} A^{\mathrm{op}}$$

be a monoidal adjoint equivalence. So, for example, there's a one-to-one correspondence between proofs

$$\mathrm{not}(a) \to b$$

and proofs

$$\mathrm{not}(b) \to a$$

Now for the really fun part. Let's define a kind of "bilinear form":

$$g\colon A \times A \to \mathsf{Set}$$

where $g(a, b)$ is the set of proofs

$$\mathrm{not}(a) \to b$$

And let's demand that g satisfy the Frobenius axiom! In other words, let's suppose there's a natural isomorphism:

$$g(a\,\mathrm{or}\,b, c) \cong g(a, b\,\mathrm{or}\,c)$$

Then A is a "$*$-autonomous category"! And this is a sensible notion, since it amounts to requiring a natural one-to-one correspondence between proofs

$$\mathrm{not}(a\,\mathrm{or}\,b) \to c$$

and proofs

$$\mathrm{not}(a) \to b\,\mathrm{or}\,c$$

So, categorified Frobenius algebras are a nice framework for propositional logic!

In case it slipped by too fast, let me repeat the definition of $*$-autonomous category I just gave. It's a symmetric monoidal category A with a monoidal adjoint equivalence called "not" from A (with one tensor product, called "or") to A^{op} (with another, called "and"), such that the functor

$$\begin{aligned} g\colon A \times A &\to \mathsf{Set} \\ (a,b) &\mapsto \mathrm{Hom}(\mathrm{not}(a), b) \end{aligned}$$

is equipped with a natural isomorphism

$$g(a \,\mathrm{or}\, b, c) \cong g(a, b \,\mathrm{or}\, c)$$

I hope I didn't screw up. I want this definition to be equivalent to the usual one, which was invented by Michael Barr quite a while ago:

28) Michael Barr, *$*$-Autonomous Categories*, Lecture Notes in Mathematics **752**, Springer, Berlin, 1979.

By now $*$-autonomous categories become quite popular among those working at the interface of category theory and logic. And, there are many ways to define them. Brady and Trimble found a nice one:

29) Gerry Brady and Todd Trimble, "A categorical interpretation of C. S. Peirce's System Alpha", *Jour. Pure Appl. Alg.* **149** (2000), 213–239.

Namely, they show a $*$-autonomous category is the same as a symmetric monoidal category A equipped with a *contravariant* adjoint equivalence

$$\mathrm{not}\colon A \to A$$

which is equipped with a "strength", and where the unit and counit of the adjunction respect this strength.

Later, in his paper "Frobenius monads and pseudomonoids", Street showed that $*$-autonomous categories really do give Frobenius pseudomonoids in a certain monoidal bicategory with:

- categories as objects;
- profunctors (also known as distributors) as morphisms;
- natural transformations as 2-morphisms.

Alas, I'm too tired to explain this now! It's a slicker way of saying what I already said. But the cool part is that this bicategory is like a categorified version of Vect, with the category of finite sets replacing the complex numbers. That's why in logic, the "nondegenerate bilinear form" looks like

$$g\colon A \times A \to \mathsf{Set}$$

So: Frobenius algebras are lurking all over in physics, logic and quantum logic, in many tightly interconnected ways. There should be some unified explanation of what's going on! Do you have any ideas?

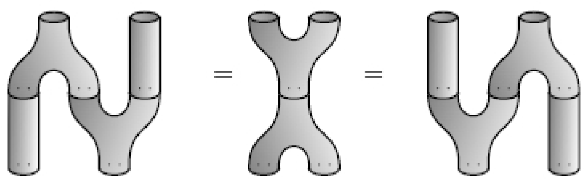
Finally, here are two books on math and music that I should read someday. The first seems more elementary, the second more advanced:

30) Trudi Hammel Garland and Charity Vaughan Kahn, *Math and Music — Harmonious Connections*, Dale Seymour Publications, 1995. Review by Elodie Lauten on her blog *Music Underground*, `http://www.sequenza21.com/2007/04/microtonal-math-heads.html`

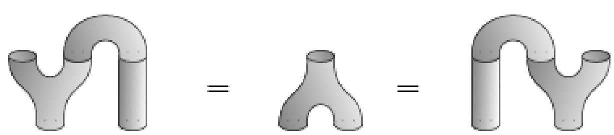
31) Serge Donval, *Histoire de l'Acoustique Musicale* (History of Musical Acoustics), Editions Fuzaeau, Bressuire, France, 2006. Review at Music Theory Online, `http://mto.societymusictheory.org/mto-books.html?id=11`

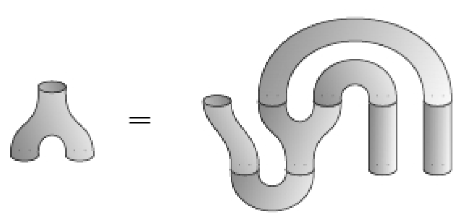
Addenda: I thank Bob Coecke, Robin Houston, Steve Lack, Paul-Andr Mellis, Todd Trimble, Jamie Vicary, and a mysterious fellow named Stuart for some very helpful corrections.

You can't really appreciate the pictorial approach to Frobenius algebras until you use it to prove some things. Try proving that every homomorphism of Frobenius algebras is an isomorphism! Or for something easier, but still fun, start by assuming that a Frobenius algebra is an algebra and coalgebra satisfying the Frobenius equations



and use this to prove the following facts:





For more discussion, visit the n-Category Caf. In particular, you'll see there's a real morass of conflicting terminology concerning what I'm calling "special" Frobenius algebras and "strongly separable" algebras. But if we define them as I do above, they're very nicely related.

More precisely: an algebra is strongly separable iff it can be given a comultiplication and counit making it into a special Frobenius algebra. If we can do this, we can do it in a unique way. Conversely, the underlying algebra of a special Frobenius algebra is strongly separable.

For more details, see:

32) nLab, "Frobenius algebra", `http://ncatlab.org/nlab/show/Frobenius+algebra`

and:

33) nLab, "Separable algebra", `http://ncatlab.org/nlab/show/separable+algebra`

> 'Interesting Truths' referred to a kind of theorem which captured subtle unifying insights between broad classes of mathematical structures. In between strict isomorphism — where the same structure recurred exactly in different guises — and the loosest of poetic analogies, Interesting Truths gathered together a panoply of apparently disparate systems by showing them all to be reflections of each other, albeit in a suitably warped mirror.
>
> — *Greg Egan,* Incandescence

Week 269

August 30, 2008

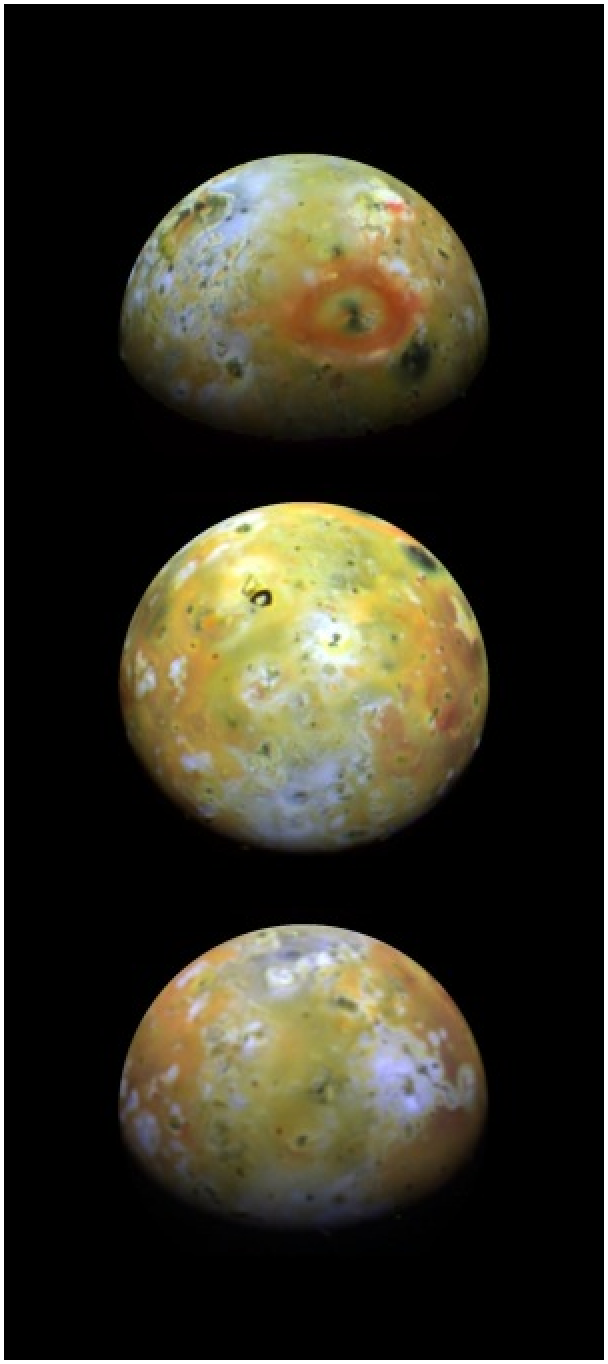
No fancy math today. I've been working hard with Aristide Baratin, Laurent Freidel and Derek Wise on infinite-dimensional representations of 2-groups. It's a gnarly mix of higher category theory and analysis. But I won't bother you with that until the paper is done. I need a break!

So, today I want to talk about the sulfur geysers on Io, honeycombs, the work of Kelvin, the Weaire-Phelan structure, and gas clathrates.

I already teased you with pictures of Jupiter's moon Io in "Week 266" and "Week 268". There's a reason. Tortured by powerful tidal forces, Io is the most geologically active object in the Solar System! It has mountains taller than Mount Everest, and over 400 active volcanos. These put out the hottest lava ever seen — and a lot of it, too. A big eruption in 1997 produced more than 3500 square kilometers of the stuff!

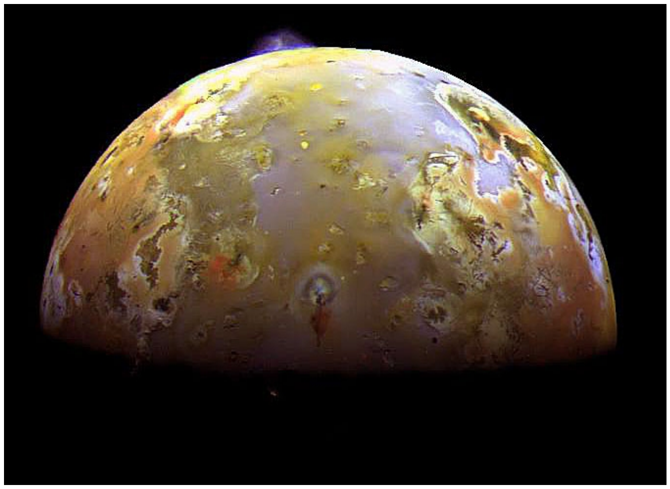
Most moons in the outer Solar System are pale and icy. Io looks like an evil pizza. The ghastly red ring in the top view here is sulfur spewed out by Pele, the biggest volcano on

Io:



1) NASA Photojournal, "Three views of Io", `http://photojournal.jpl.nasa.gov/catalog/PIA00292`

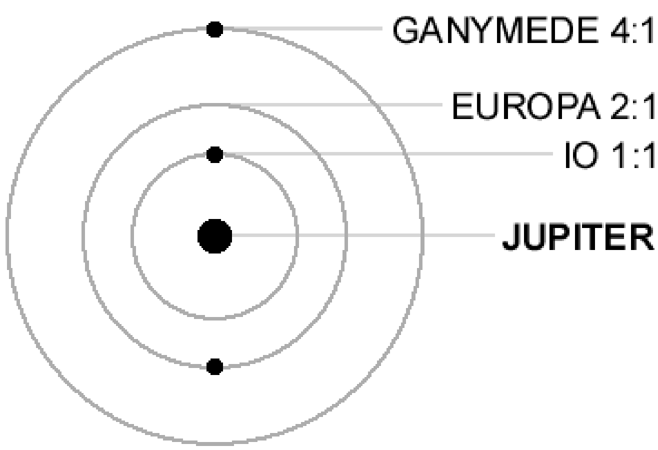
Io is big on sulfur. It has lakes of molten sulfur… pale sulfur dioxide snow… and geysers spew sulfur dioxide up to 500 kilometers high! Here's a picture of two such geysers, taken by the Galileo spacecraft in 1997:



2) Astronomy Picture of the Day, "Io: the Prometheus plume", `http://apod.nasa.gov/apod/ap070211.html`

At the top horizon you can see a cloud of bluish-white haze. It may not look big, but remember: Io is about 3600 kilometers in diameter, roughly the size of our Moon. So, that hazy cloud is actually *huge*. It's a geyser plume rising 140 kilometers above a massive volcano called Pillan Patera. If you look carefully you can also see the Prometheus plume, dead center, as a blue-grey ring. This plume has been active at least since 1979 — but it's *moved 85 kilometers west* during that time! Scary.

Ironically, all this chaotic activity on Io may be caused by the "music of the spheres". Io is locked in a 2:1 orbital resonance with the moon Europa, and a 4:1 resonance with Ganymede. These keep Io's orbit a bit eccentric, which causes tidal heating — to the tune of about 100 trillion watts.



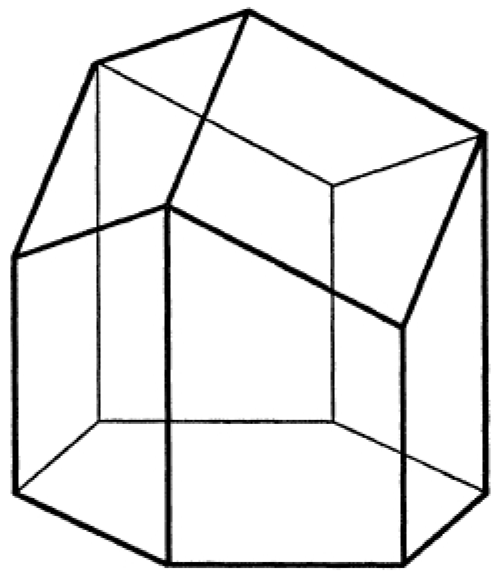
Another interesting thing is that the red spots on Io are made of sulfur, but so are the yellow plains. Of course there are lots of compounds involving sulfur, but even the pure element has a lot of different forms, or allotropes. I've always been fascinated by those.

Back here on Earth, the Weaire-Phelan structure was in the news recently! If you watched the Olympics in Beijing, you may have seen the National Aquatics Center — a building also called the "Water Cube":

3) "Spectacular mathematical bubble design at the Olympics", Math in the News, Mathematical Association of America, August 8, 2008, `http://mathdl.maa.org/mathDL?pa=mathNews&sa=view&newsId=392`

The design of this building is based on the Weaire-Phelan structure. But the story behind the design of this building goes back to 36 BC, when Marcus Terentius Varro described two competing theories for why bees have hexagonal honeycombs. The first said: because bees have six legs! The second said: efficiency! You see, a hexagonal lattice lets you divide the plane into cells of equal area with the least possible perimeter per cell. So if the bees want to save wax, that's the pattern they'll pick.

The second theory seems more plausible. But is it true? I'm not even sure how to resolve that question. But it's worth noting that honeycomb cells are actually 3-dimensional — and the *end* of each cell consists of three rhombi that meet at the same angle as bubbles in soap suds!



Now, soap films minimize surface area subject to whatever constraints they encounter. So, a single bubble that holds a given amount of air will form a sphere. But soap suds with lots of bubbles do more complicated things. Take a bubble bath and pay careful attention! You'll see that three bubble faces meet along each edge, at precisely 120 degree angles. And when four bubbles meet at a vertex, they form a pattern with tetrahedral symmetry, with edges meeting at an angle of $\arccos(-1/3)$, or about 109.5 degrees.

So if honeycombs display these patterns, we can guess area is being minimized.

But as usual, things get more complicated when you look deeper. First, while everyone *believed* for a long time that a hexagonal honeycomb is the way to divide the plane into equal-area cells with minimal perimeters, this was only *proved* much later: in 1999, by Thomas Hales.

It's an interesting story. Hales had just finished his epic proof of Kepler's conjecture about the densest way to pack equal-sized spheres — a proof so complicated that the referees "ran out of energy" trying to check it. That's quite a tale in itself... but to avoid an infinite sequence of nested digressions, I'll refer you to these:

4) George G. Szpiro, *Kepler's Conjecture*, John Wiley and Sons, 2003. Reviewed by Frank Morgan in *Notices Amer. Math. Soc.* **52** (2005), 44–47. Also available at `http://www.ams.org/notices/200501/rev-morgan.pdf`

5) Thomas Hales, "The Kepler Conjecture", `http://www.math.pitt.edu/~thales/kepler98/`

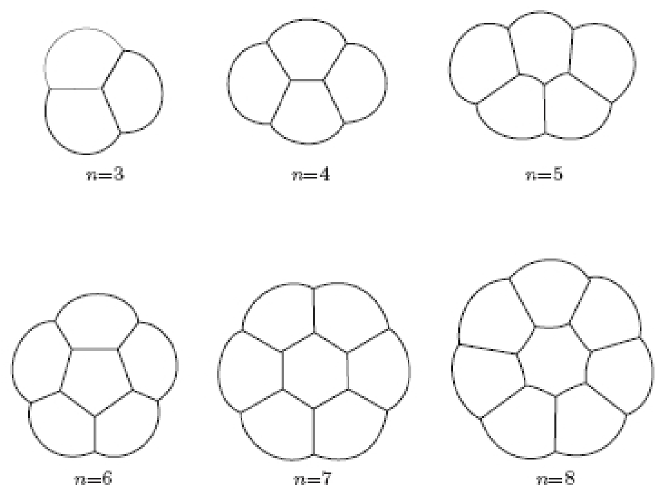
Anyway, after Hales proved the Kepler Conjecture, Denis Weaire suggested that he tackle the Hexagonal Honeycomb Conjecture — and Hales promptly solved that too! He said, "In contrast with the years of forced labor that gave the Kepler Conjecture, I felt as if I had won the lottery."

6) Thomas C. Hales, "The Honeycomb Conjecture", `http://www.math.pitt.edu/~thales/kepler98/honey/`

Hales wasn't the first to make progress on the Hexagonal Honeycomb Conjecture. A guy named Fejes Tth had already proved it's true if we assume the cells are polygons:

7) L. Fejes Tth, *Regular Figures*, Macmillan, New York, 1964.

So what Hales had to do is rule out cells with curved edges. This is harder than you might think. In fact, for clusters of finitely many cells, the optimal shapes can be curved, even near the middle!

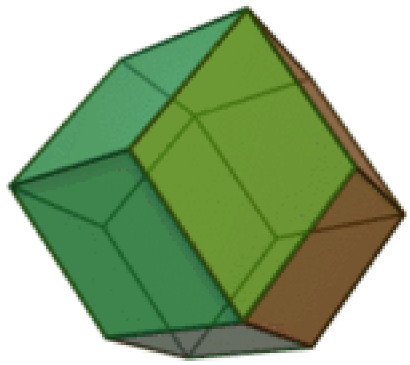


7) S. J. Cox, M. Fatima Vas, C. Monnereua-Pittet and N. Pittet, "Minimal perimeter for N identical bubbles in two dimensions: calculations and simulations", *Phil. Mag.* **83** (2003), 1393–1406.

Another thing Tth did is carefully define the 3d optimization problem that bees might be trying to solve, and find a slightly better solution:

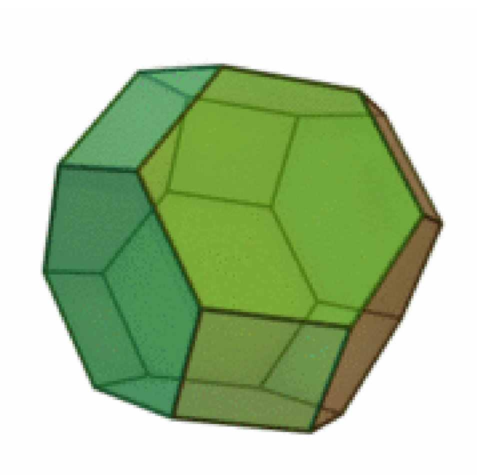
9) L. Fejes Tth, "What the bees know and what the bees do not know", *Bull. Amer. Math. Soc.* **70** (1964), 468–481. Also available at `http://projecteuclid.org/euclid.bams/1183526078`

I won't try to describe his results in detail, since the paper is freely available and well-written. But here's the basic idea. The end of a bee's honeycomb cell looks just like a corner of a rhombic dodecahedron:

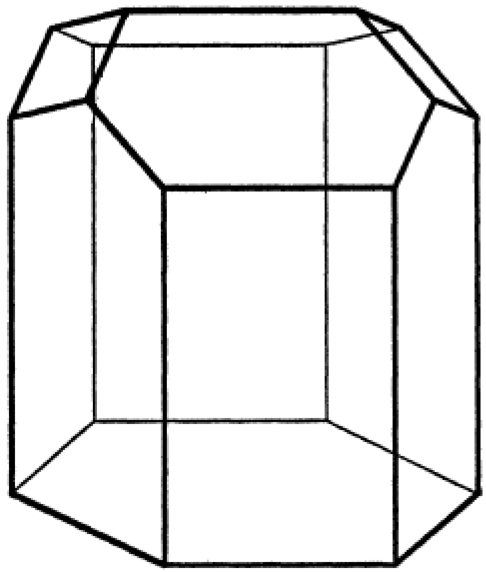


This is a 12-sided solid that you can pack to completely fill space. This makes sense, because the ends of one layer of cells in a honeycomb should neatly fit against those of the next layer.

However, it's been known since the work of Kelvin that there's another solid you can use to pack space more efficiently: that is, with less surface area per cell. This is the truncated octahedron:



Using this, Tth found a design for the end of a honeycomb cell that would be more efficient than what bees use:



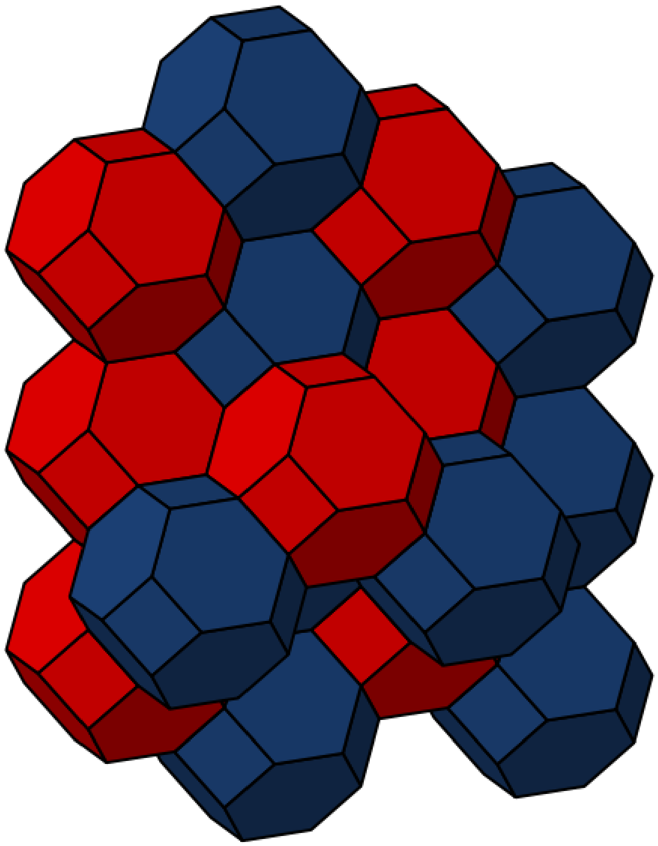
How much more efficient? How much area did Tth manage to shave off? Almost 0.35% of the area of cell's opening! In the eternal battle of man against bee, we triumph yet again! It makes me proud to be human.

Tth is more modest:

> *We must admit that all this has no practical consequence.... Besides, the building style of the bees is definitely simpler than that described above. So we would fail in shaking someone's conviction that the bees have a deep geometrical intuition.*

I doubt "intuition" is the right word for it, but they're definitely good at what they do.

Now, back to Kelvin! When he bumped into the truncated octahedron, he was actually studying the 3d version of the 2d Hexagonal Honeycomb Conjecture. In other words, he was trying to chop 3d space into cells of equal volume with the least surface area per cell. And he conjectured that the answer was very similar to filling space with truncated octahedra, as shown here:



9) Wikipedia, "Bitruncated cubic honeycomb", `http://en.wikipedia.org/wiki/Bitruncated_cubic_honeycom`

I say "very similar" because it's actually more efficient if you let the hexagonal faces in this structure be slightly *curved*. So, the possibility Hales ruled out in the 2d case actually matters here! In his 1887 paper on this subject, Kelvin wrote:

> *No shading could show satisfactorily the delicate curvature of the hexagonal faces, though it may be fairly well seen on the solid model made as described in Section 12. But it is shown beautifully, and illustrated in great perfection, by making a skeleton model of 36 wire arcs for the 36 edges of the complete figure, and dipping it in soap solution to fill the faces with film, which is easily done for all the faces but one. The curvature of the hexagonal film on the two sides of the plane of its six long diagonals is beautifully shown by reflected light.*

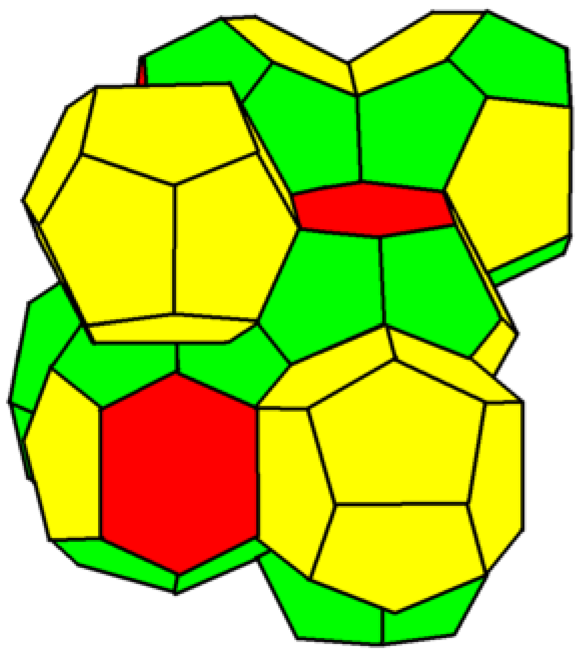
I think this is a nice passage. We may remember Kelvin for his profound work on electromagnetism and thermodynamics — or his 1900 lecture on two "dark clouds" hanging over physics: the Michelson-Morley experiment (which foreshadowed special relativity) and black body radiation (which foreshadowed quantum mechanics). We may not imagine him playing around with soap bubbles! But it shows that good science stems from curiosity, and curiosity knows no bounds.

You can read Kelvin's paper here:

10) Lord Kelvin, "On the division of space with minimum partitional area", *Phil. Mag.* **24** (1887), 503. Also available at `http://zapatopi.net/kelvin/papers/on_the_division_of_space.html`

His lively mind is evident from the selection of papers on this site. For example: "On Vortex Atoms", where he unsuccessfully tried to build atoms out of knotted electromagnetic field lines, and wound up giving birth to knot theory. Some others I hadn't heard of: "On the origin of life", "The sorting demon of Maxwell", and "Windmills must be the future source of power".

Anyway: for over a century the so-called "Kelvin structure" was believed to be the best solution to the problem of chopping space into equal volume cells with minimal surface area. But in 1993 two physicists at Trinity College in Dublin — Denis Weaire and Robert Phelan — found a solution that has 0.3% less surface area! It looks like this:

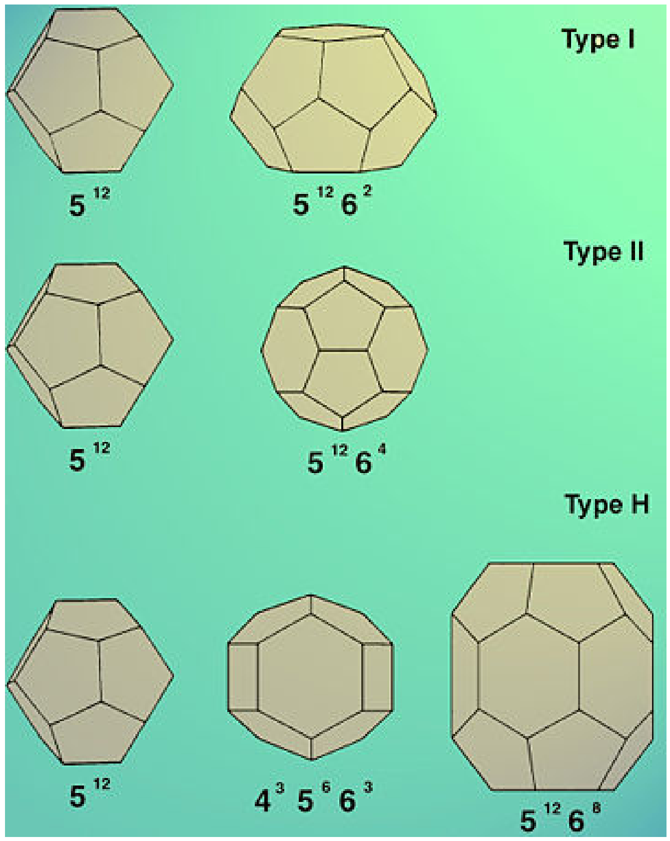


11) Wikipedia, "Weaire-Phelan structure", `http://en.wikipedia.org/wiki/Weaire--Phelan_structure`

It's built from *two* kinds of cells — a 12-sided one and a 14-sided one. Was that allowed in Kelvin's original puzzle? I can't tell!

In fact, this so-called "Weaire-Phelan structure" was no bolt out of the blue. The basic pattern had already been seen in certain cage-like crystals called "clathrates". For example, down at the bottom of the ocean there's somewhere between 500 and 2500 billion tons of methane hydrate, a funny substance in which methane molecules are trapped in polyhedral cages formed by water molecules. It looks like ice, but you can ignite it with a cigarette lighter! If you think global warming is bad now, just wait until people figure out how to mine this stuff. . . .

Anyway, methane hydrate is just one of a collection of gas hydrates with different geometries. And it's an example of a "type I" gas hydrate, where water molecules form cages patterned after the Weaire-Phelan structure!



12) Wikipedia, "Clathrate hydrate", `http://en.wikipedia.org/wiki/Clathrate_hydrate`

So, it's nicely appropriate to use the Weaire-Phelan structure for a building called the Water Cube. Since this structure is perfectly periodic, the engineer for the Water Cube cut it at an odd angle to make it look more exciting. The MAA article cited above says:

> *Made of a plastic known as ethylene tetrafluoroethylene and filled with air, the bubbles are attached to a steel framework outlining the bubble edges. Surface tension holds the bubbles together and tends to pull them into a structure with least surface area.*
>
> *The building "really looks like nothing else in the world," Tristam Carfrae told the New York Times. "It's a box made of bubbles." Carfrae is the structural engineer who designed the center.*

On the math side of things, there's plenty left to be done. Nobody has proved that the Weaire-Phelan structure is the best solution to Kelvin's problem. According to Frank Morgan, the expert on minimal surface who reviewed Spziro's book on Kepler's problem,

> *Proving the Weaire-Phelan structure optimal looks perhaps a century beyond current mathematics to me, but I understand that Hales is already thinking about it.*

More generally, minimal surface theory is a lively subject that uses a lot of deep tools. Morgan is really big on explaining math, so his book is probably the place to start if you want to dig deeper:

13) Frank Morgan, *Geometric Measure Theory: a Beginner's Guide*, Academic Press, New York, 2000.

Personally, I'm more in love with symmetry than minimization. So, I want to learn more about the 28 "convex uniform honeycombs" — ways of uniformly packing 3d space with uniform solids:

14) Wikipedia, "Convex uniform honeycomb", `http://en.wikipedia.org/wiki/Convex_uniform_honeycomb`

They're related to Coxeter groups and "crystallographic groups", which are the 3d analogues of the wallpaper groups I discussed back in "Week 267". In fact, we can study honeycombs and their symmetry groups in any dimension, both in flat space and in positively curved (spherical) and negatively curved (hyperbolic) space. A lot is known about them. . . .

Addenda: I thank Jim Stasheff and Mike Stay for drawing my attention to the Water Cube and its use of the Weaire-Phelan structure. I thank Blake Stacey for catching a mistake.

> My suggestion is that Aepinus' fluid consists of exceedingly minute equal and similar atoms, which I call *electrons*, much smaller than the atoms of ponderable matter.
>
> — *Lord Kelvin, Aepinus Atomized*

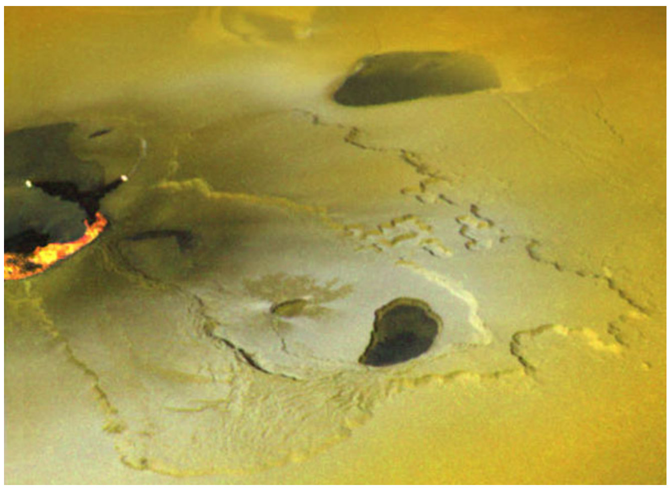
> One word characterizes the most strenuous efforts for the advancement of science I have made perseveringly during fifty-five years; that word is *Failure*. I know no more of electric or magnetic force, or of the relation between ether, electricity and ponderable matter, or of chemical affinity, than I knew and tried to teach to my students of natural philosophy fifty years ago in my first session as professor.
>
> — *Lord Kelvin, at a celebration of his life's work attended by more than 2000 guests*

Week 270

October 11, 2008

Greg Egan has a new novel out, called "Incandescence" — so I want to talk about that. Then I'll talk about three of my favorite numbers: 5, 8, and 24. I'll show you how each regular polytope with 5-fold rotational symmetry has a secret link to a lattice living in twice as many dimensions. For example, the pentagon is a 2d projection of a beautiful shape that lives in 4 dimensions. Finally, I'll wrap up with a simple but surprising property of the number 12.

But first: another picture of Jupiter's moon Io! Now we'll zoom in much closer. This was taken in 2000 by the Galileo probe:



1) "A continuous eruption on Jupiter's moon Io", Astronomy Picture of the Day, `http://apod.nasa.gov/apod/ap000606.html`

Here we see a vast plain of sulfur and silicate rock, 250 kilometers across — and on the left, glowing hot lava! The white dots are spots so hot that their infrared radiation oversaturated the detection equipment. This was the first photo of an active lava flow on another world.

If you like pictures like this, maybe you like science fiction. And if you like hard science fiction — "diamond-scratching hard", as one reviewer put it — Greg Egan is your man. His latest novel is one of the most realistic evocations of the distant future I've ever read. Check out the website:

2) Greg Egan, *Incandescence*, Night Shade Books, 2008. Website at `http://www.gregegan.net/INCANDESCENCE/Incandescence.html`

The story features two parallel plots. One is about a galaxy-spanning civilization called the Amalgam, and two of its members who go on a quest to our Galaxy's core, which is home to enigmatic beings that may be still more advanced: the Aloof. The other is about the inhabitants of a small world orbiting a black hole. This is where the serious physics comes in.

I might as well quote Egan himself:

> *"Incandescence" grew out of the notion that the theory of general relativity — widely regarded as one of the pinnacles of human intellectual achievement — could be discovered by a pre-industrial civilization with no steam engines, no electric lights, no radio transmitters, and absolutely no tradition of astronomy.*
>
> *At first glance, this premise might strike you as a little hard to believe. We humans came to a detailed understanding of gravity after centuries of painstaking astronomical observations, most crucially of the motions of the planets across the sky. Johannes Kepler found that these observations could be explained if the planets moved around the sun along elliptical orbits, with the square of the orbital period proportional to the cube of the length of the longest axis of the ellipse. Newton showed that just such a motion would arise from a universal attraction between bodies that was inversely proportional to the square of the distance between them. That hypothesis was a close enough approximation to the truth to survive for more than three centuries.*
>
> *When Newton was finally overthrown by Einstein, the birth of the new theory owed much less to the astronomical facts it could explain - such as a puzzling drift in the point where Mercury made its closest approach to the sun — than to an elegant theory of electromagnetism that had arisen more or less independently of ideas about gravity. Electrostatic and magnetic effects had been unified by James Clerk Maxwell, but Maxwell's equations only offered one value for the speed of light, however you happened to be moving when you measured it. Making sense of this fact led Einstein first to special relativity, in which the geometry of space-time had the unvarying speed of light built into it, then general relativity, in which the curvature of the same geometry accounted for the motion of objects free-falling through space.*
>
> *So for us, astronomy was crucial even to reach as far as Newton, and postulating Einstein's theory — let alone validating it to high precision, with atomic clocks on satellites and observations of pulsar orbits — depended on a wealth of other ideas and technologies.*
>
> *How, then, could my alien civilization possibly reach the same conceptual heights, when they were armed with none of these apparent prerequisites? The short answer is that they would need to be living in just the right environment: the accretion disk of a large black hole.*
>
> *When SF readers think of the experience of being close to a black hole, the phenomena that most easily come to mind are those that are most exotic from our own perspective: time dilation, gravitational blue-shifts, and massive distortions of the view of the sky. But those are all a matter of making astronomical observations, or at least arranging some kind of comparison between the near-black-hole experience and the experience of other beings who have kept their distance. My aliens would probably need to be sheltering deep inside some rocky structure to protect them from the radiation of the accretion disk — and the glow of the disk itself would also render astronomy immensely difficult.*

Blind to the heavens, how could they come to learn anything at all about gravity, let alone the subtleties of general relativity? After all, didn't Einstein tell us that if we're free-falling, weightless, in a windowless elevator, gravity itself becomes impossible to detect?

Not quite! To render its passenger completely oblivious to gravity, not only does the elevator need to be small, but the passenger's observations need to be curtailed in time just as surely as they're limited in space. Given time, gravity makes its mark. Forget about black holes for a moment: even inside a windowless space station orbiting the Earth, you could easily prove that you were not just drifting through interstellar space, light-years from the nearest planet. How? Put on your space suit, and pump out all the station's air. Then fill the station with small objects — paper clips, pens, whatever — being careful to place them initially at rest with respect to the walls.

Wait, and see what happens.

Most objects will eventually hit the walls; the exact proportion will depend on the station's spin. But however the station is or isn't spinning, some objects will undergo a cyclic motion, moving back and forth, all with the same period.

That period is the orbital period of the space station around the Earth. The paper clips and pens that are moving back and forth inside the station are following orbits that are inclined at a very small angle to the orbit of the station's center of mass. Twice in every orbit, the two paths cross, and the paper clip passes through the center of the space station. Then it moves away, reaches the point of greatest separation of the orbits, then turns around and comes back.

This minuscule difference in orbits is enough to reveal the fact that you're not drifting in interstellar space. A sufficiently delicate spring balance could reveal the tiny "tidal gravitational force" that is another way of thinking about exactly the same thing, but unless the orbital period was very long, you could stick with the technology-free approach and just watch and wait.

A range of simple experiments like this — none of them much harder than those conducted by Galileo and his contemporaries — were the solution to my aliens' need to catch up with Newton. But catching up with Einstein? Surely that was beyond hope?

I thought it might be, until I sat down and did some detailed calculations. It turned out that, close to a black hole, the differences between Newton's and Einstein's predictions would easily be big enough for anyone to spot without sophisticated instrumentation.

What about sophisticated mathematics? The geometry of general relativity isn't trivial, but much of its difficulty, for us, revolves around the need to dispose of our preconceptions. By putting my aliens in a world of curved and twisted tunnels, rather than the flat, almost Euclidean landscape of a patch of planetary surface, they came better prepared for the need to cope with a space-time geometry that also twisted and curved.

The result was an alternative, low-tech path into some of the most beautiful truths we've yet discovered about the universe. To add to the drama, though,

> *there needed to be a sense of urgency; the intellectual progress of the aliens had to be a matter of life and death. But having already put them beside a black hole, danger was never going to be far behind.*

As you can tell, this is a novel of ideas. You have to be willing to work through these ideas to enjoy it. It's also not what I'd call a feel-good novel. As with "Diaspora" and "Schild's Ladder", the main characters seem to become more and more isolated and focused on their work as they delve deeper into the mysteries they are pursuing. By the time the mysteries are unraveled, there's almost nobody to talk to. It's a problem many mathematicians will recognize. Indeed, near the end of "Diaspora" we read: "In the end, there was only mathematics".

So, this novel is not for everyone! But then, neither is This Week's Finds.

In fact, I was carrying "Incandescence" with me when in mid-September I left the scorched and smoggy sprawl of southern California for the cool, wet, beautiful old city of Glasgow. I spent a lovely week there talking math with Tom Leinster, Eugenia Cheng, Bruce Bartlett and Simon Willerton. I'd been invited to the University of Glasgow to give a series of talks called the 2008 Rankin Lectures. I spoke about my three favorite numbers, and you can see the slides here:

3) John Baez, "My favorite numbers", available at `http://math.ucr.edu/home/baez/numbers/`

I wanted to explain how different numbers have different personalities that radiate like force fields through diverse areas of mathematics and interact with each other in surprising ways. I've been exploring this theme for many years here. So, it was nice to polish some things I've written and present them in a more organized way. These lectures were sponsored by the trust that runs the Glasgow Mathematical Journal, so I'll eventually publish them there. I plan to add a lot of detail that didn't fit in the talks.

I began with the number 5, since the golden ratio and the five-fold symmetry of the dodecahedron lead quickly to a wealth of easily enjoyed phenomena: from Penrose tilings and quasicrystals, to Hurwitz's theorem on approximating numbers by fractions, to the 120-cell and the Poincare homology sphere.

After giving the first talk I discovered the head of the math department, Peter Kropholler, is a big fan of Rubik's cubes. I'd never been attracted to them myself. But his enthusiasm was contagious, especially when he started pulling out the unusual variants that he collects, eagerly explaining their subtleties. My favorite was the Rubik's dodecahedron, or "Megaminx":

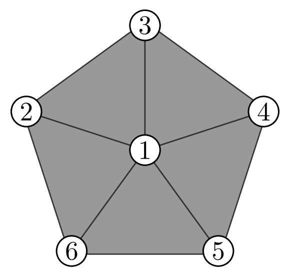
4) Wikipedia, "Megaminx", `http://en.wikipedia.org/wiki/Megaminx`

Then I got to thinking: it would be even better to have a Rubik's icosahedron, since its symmetries would then include M_{12}, the smallest Mathieu group. And it turns out that such a gadget exists! It's called "Dogic":

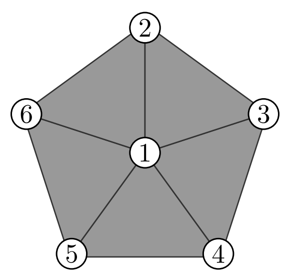
5) Wikipedia, "Dogic", `http://en.wikipedia.org/wiki/Dogic`

The Mathieu group M_{12} is the smallest of the sporadic finite simple groups. Someday I'd like to understand the Monster, which is the biggest of the lot. But if the Monster is the Mount Everest of finite group theory, M_{12} is like a small foothill. A good place to start.

Way back in "Week 20", I gave a cute description of M_{12} lifted from Conway and Sloane's classic book. If you get 12 equal-sized balls to touch a central one of the same size, and arrange them to lie at the corners of a regular icosahedron, they don't touch their neighbors. There's even room to roll them around in interesting ways! For example, you can twist 5 of them around clockwise so that this arrangement:



becomes this:



We can generate lots of permutations of the 12 outer balls using twists of this sort — in fact, all even permutations. But suppose we only use moves where we first twist 5 balls around clockwise and then twist 5 others counterclockwise. These generate a smaller group: the Mathieu group M_{12}.

Since we can do twists like this in the Dogic puzzle, I believe M_{12} sits inside the symmetry group of this puzzle! In a way it's not surprising: the Dogic puzzle has a vast group of symmetries, while M_{12} has a measly

$$8 \times 9 \times 10 \times 11 \times 12 = 95040$$

elements. But it'd still be cool to have a toy where you can explore the Mathieu group M_{12} with your own hands!

The math department lounge at the University of Glasgow has some old books in the shelves waiting for someone to pick them up and read them and love them. They're sort of like dogs at the pound, sadly waiting for somebody to take them home. I took one that explains how Mathieu groups arise as symmetries of "Steiner systems":

6) Thomas Beth, Dieter Jungnickel, and Hanfried Lenz, *Design Theory*, Cambridge U. Press, Cambridge, 1986.

Here's how they get M_{12}. Take a 12-point set and think of it as the "projective line over $\mathbb{F}_{11}$" — in other words, the integers $\mod 11$ together with a point called infinity. Among the integers $\mod 11$, six are perfect squares:

$$\{0, 1, 3, 4, 5, 9\}$$

Call this set a "block". From this, get a bunch more blocks by applying fractional linear transformations:

$$z \mapsto \frac{az + b}{cz + d}$$

where the matrix

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix}$$

has determinant 1. These blocks then form a "$(5, 6, 12)$ Steiner system". In other words: there are 12 points, 6 points in each block, and any set of 5 points lies in a unique block.

The group M_{12} is then the group of all transformations of the projective line that map points to points and blocks to blocks!

If I make more progress on understanding this stuff I'll let you know. It would be fun to find deep mathematics lurking in mutant Rubik's cubes.

Anyway, in my second talk I turned to the number 8. This gave me a great excuse to tell the story of how Graves discovered the octonions, and then talk about sphere packings and the marvelous E_8 lattice, whose points can also be seen as "integer octonions". I also sketched the basic ideas behind Bott periodicity, triality, and the role of division algebras in superstring theory.

If you look at my slides you'll also see an appendix that describes two ways to get the E_8 lattice starting from the dodecahedron. This is a nice interaction between the magic powers of the number 5 and those of the number 8. After my talk, Christian Korff from the University of Glasgow showed me a paper that fits this relation into a bigger pattern:

7) Andreas Fring and Christian Korff, "Non-crystallographic reduction of Calogero-Moser models", *Jour. Phys. A* **39** (2006), 1115–1131. Also available as `hep-th/0509152`.

They set up a nice correspondence between some non-crystallographic Coxeter groups and some crystallographic ones:

- the H_2 Coxeter group and the A_4 Coxeter group,
- the H_3 Coxeter group and the D_6 Coxeter group,
- the H_4 Coxeter group and the E_8 Coxeter group.

A Coxeter group is a finite group of linear transformations of $\mathbb{R}^n$ that's generated by reflections. We say such a group is "non-crystallographic" if it's not the symmetries of any lattice. The ones listed above are closely tied to the number 5:

- H_2 is the symmetry group of a regular pentagon.
- H_3 is the symmetry group of a regular dodecahedron.

- H_4 is the symmetry group of a regular 120-cell.

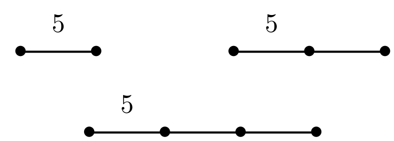
Note these live in 2d, 3d and 4d space. Only in these dimensions are there regular polytopes with 5-fold rotational symmetry! Their symmetry groups are non-crystallographic, because no lattice can have 5-fold rotational symmetry.

A Coxeter group is "crystallographic", or a "Weyl group", if it *is* symmetries of a lattice. In particular:

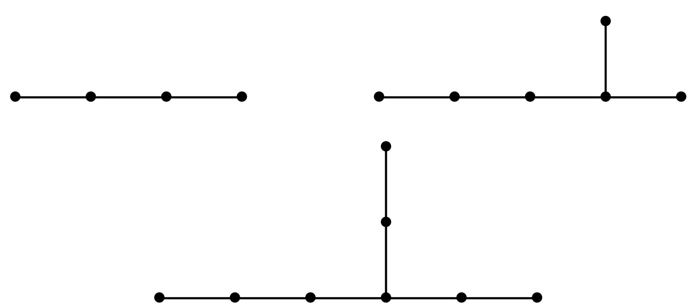
- A_4 is the symmetry group of a 4-dimensional lattice also called A_4.
- D_6 is the symmetry group of a 6-dimensional lattice also called D_6.
- E_8 is the symmetry group of an 8-dimensional lattice also called E_8.

You can see precise descriptions of these lattices in "Week 65" — they're pretty simple.

Both crystallographic and noncrystallographic Coxeter groups are described by Coxeter diagrams, as explained back in "Week 62". The H_2, H_3 and H_4 Coxeter diagrams look like this:



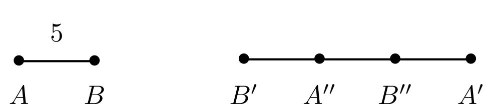
The A_4, D_6 and E_8 Coxeter diagrams (usually called Dynkin diagrams) have twice as many dots as their smaller partners H_2, H_3 and H_4:

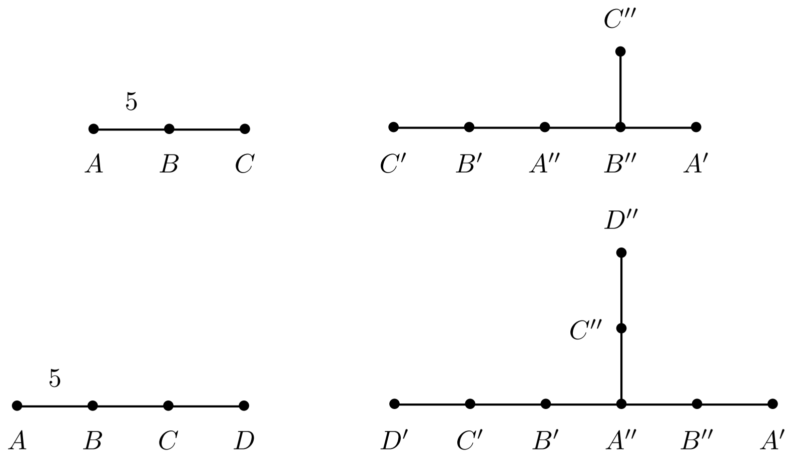


I've drawn these in a slightly unorthodox way to show how they "grow".

In every case, each dot in the diagram corresponds to one of the reflections that generates the Coxeter group. The edges in the diagram describe relations — you can read how in "Week 62".

All this is well-known stuff. But Fring and Korff investigate something more esoteric. Each dot in the big diagram corresponds to 2 dots in its smaller partner:





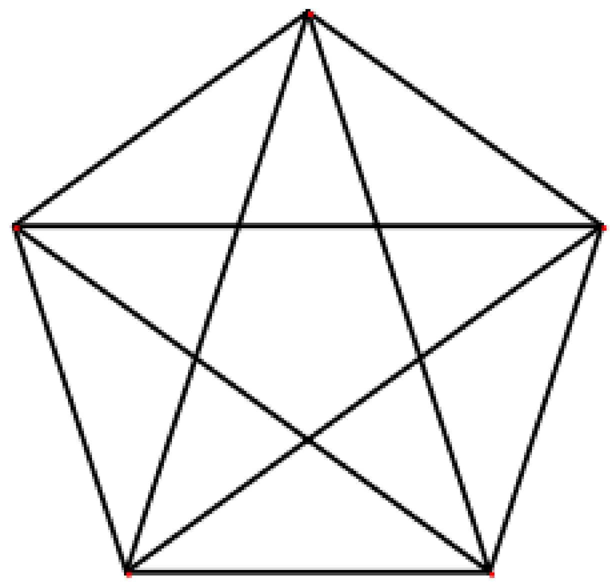
If we map each generator of the smaller group (say, the generator D in H_4) to the product of the two corresponding generators in the bigger one (say, $D'D''$ in E_8), we get a group homomorphism.

In fact, we get an *inclusion* of the smaller group in the bigger one!

This is just the starting point of Fring and Korff's work. Their real goal is to show how certain exactly solvable physics problems associated to crystallographic Coxeter groups can be generalized to these three noncrystallographic ones. For this, they must develop more detailed connections than those I've described. But I'm already happy just pondering this small piece of their paper.

For example, what does the inclusion of H_2 in A_4 really look like?

It's actually quite beautiful. H_2 is the symmetry group of a regular pentagon, including rotations and reflections. A_4 happens to be the symmetry group of a 4-simplex. If you draw a 4-simplex in the plane, it looks like a pentagram:



So, any symmetry of the pentagon gives a symmetry of the 4-simplex. So, we get an inclusion of H_2 in A_4.

People often say that Penrose tilings arise from lattices in 4d space. Maybe I'm finally starting to understand how! The A_4 lattice has a bunch of 4-simplices in it — but when we project these onto the plane correctly, they give pentagrams. I'd be very happy if this were the key.

What about the inclusion of H_3 in D_6?

Here James Dolan helped me make a guess. H_3 is the symmetry group of a regular dodecahedron, including rotations and reflections. D_6 consists of all linear transformations of R^6 generated by permuting the 6 coordinate axes and switching the signs of an even number of coordinates. But a dodecahedron has 6 "axes" going between opposite pentagons! If we arbitrarily orient all these axes, I believe any rotation or reflection of the dodecahedron gives an element of D_6. So, we get an inclusion of H_3 in D_6.

And finally, what about the inclusion of H_4 in E_8?

H_4 is the symmetry group of the 120-cell, including rotations and reflections. In 8 dimensions, you can get 240 equal-sized balls to touch a central ball of the same size. E_8 acts as symmetries of this arrangement. There's a clever trick for grouping the 240 balls into 120 ordered pairs, which is explained by Fring and Korff and also by Conway's "icosian" construction of E_8 described at the end of my talk on the number 8. Each element of H_4 gives a permutation of the 120 faces of the 120-cell — and thanks to that clever trick, this gives a permutation of the 240 balls. This permutation actually comes from an element of E_8. So, we get an inclusion of H_4 in E_8.

My last talk was on the number 24. Here I explained Euler's crazy "proof" that

$$1 + 2 + 3 + \ldots = -\frac{1}{12}$$

and how this makes bosonic strings happy when they have 24 transverse directions to wiggle around in. I also touched on the 24-dimensional Leech lattice and how this gives a version of bosonic string theory whose symmetry group is the Monster: the largest sporadic finite simple group.

A lot of the special properties of the number 24 are really properties of the number 12 — and most of these come from the period-12 behavior of modular forms. I explained this back in "Week 125". I recently ran into these papers describing yet another curious property of the number 12, also related to modular forms, but very easy to state:

8) Bjorn Poonen and Fernando Rodriguez-Villegas, "Lattice polygons and the number 12". Available at `http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.43.2555`

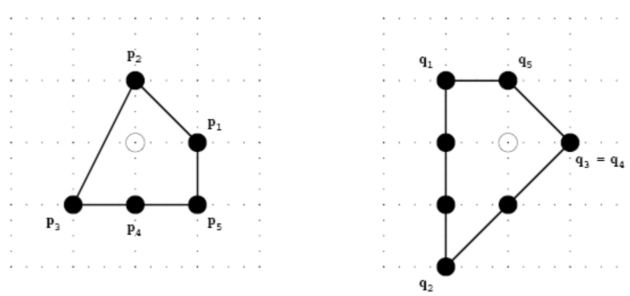
9) John M. Burns and David O'Keeffe, "Lattice polygons in the plane and the number 12", *Irish Math. Soc. Bulletin* **57** (2006), 65–68. Also available at `http://www.maths.tcd.ie/pub/ims/bull57/M5700.pdf`

Consider the lattice in the plane consisting of points with integer coordinates. Draw a convex polygon whose vertices lie on this lattice. Obviously, the *differences* of successive vertices also lie on the lattice. We can create a new convex polygon with these differences as vertices. This is called the "dual" polygon.

Say our original polygon is so small that the only lattice point in its interior is $(0, 0)$. Then the same is true of its dual! Furthermore, the dual of the dual is the original polygon!

But now for the cool part. Take a polygon of this sort, and add up the number of lattice points on its boundary and the number of lattice points on the boundary of its dual. The total is 12.

You can see an example in Figure 1 of the paper by Poonen and Rodriguez-Villegas:



Note that $p_2 - p_1 = q_1$ and so on. The first polygon has lattice 5 points on its boundary; the second, its dual, has 7. The total is 12.

I like how Poonen and Rodriguez-Villegas' paper uses this theorem as a springboard for discussing a big question: what does it mean to "explain" the appearance of the number 12 here? They write:

> *Our reason for selecting this particular statement, besides the intriguing appearance of the number 12, is that its proofs display a surprisingly rich variety of methods, and at least some of them are symptomatic of connections between*

> *branches of mathematics that on the surface appear to have little to do with one another. The theorem (implicitly) and proofs 2 and 3 sketched below appear in Fulton's book on toric varieties. We will give our new proof 4, which uses modular forms instead, in full.*

Addenda: I thank Adam Glesser and David Speyer for catching mistakes.

The only noncrystallographic Coxeter groups are the symmetry groups of the 120-cell (H_4), the dodecahedron (H_3), and the regular n-gons where $n = 5, 7, 8, 9, \ldots$. The last list of groups is usually called I_n — or better, $\mathrm{I}_2(n)$, so that the subscript denotes the number of dots in the Dynkin diagram, as usual. But Fring and Korff use "H_2" as a special name for $\mathrm{I}_2(5)$, and that's nice if you're focused on 5-fold symmetry, because then H_2 forms a little series together with H_3 and H_4.

If you examine Poonen and Rodriguez-Villegas' picture carefully, you'll see a subtlety concerning the claim that the dual of the dual is the original polygon. Apparently you need to count every boundary point as a vertex! Read the papers for more precise details.

For more discussion visit the n-Category Caf.

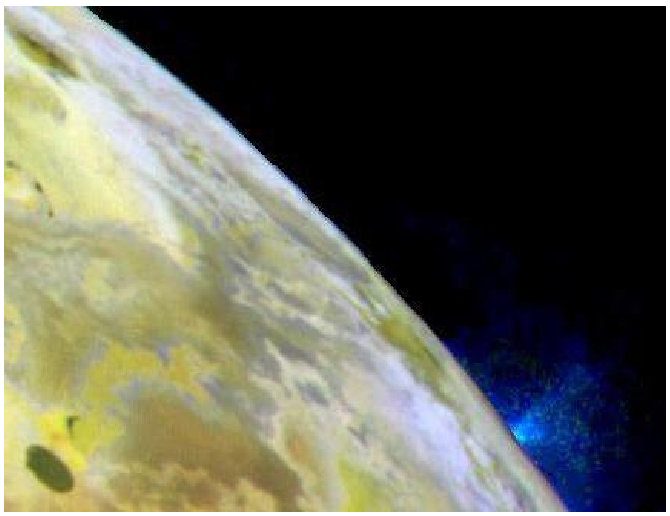
> When the blind beetle crawls over the surface of a globe, he doesn't realize that the track he has covered is curved. I was lucky enough to have spotted it.
>
> — *Albert Einstein*

Week 271

October 26, 2008

This week I'll talk about quasicrystals and how they arise from the interplay between crystallographic and noncrystallographic Coxeter groups. I'll describe Jeffrey Morton's new paper on groupoids and 2-vector spaces, and Stephen Summers' review of new work on constructive quantum field theory. But first — more pictures of Jupiter's moon Io!

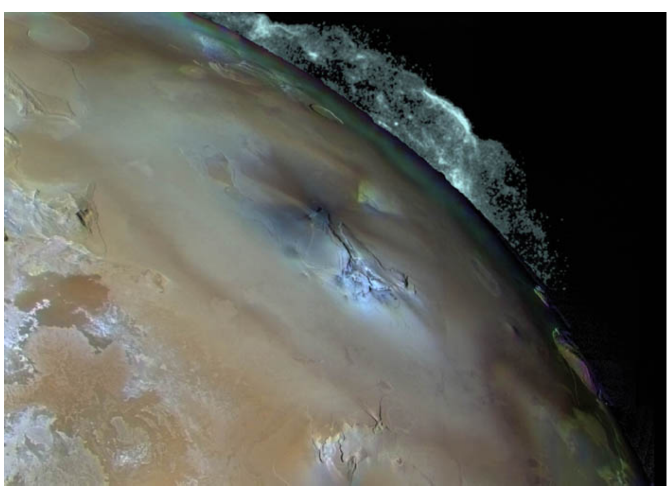
Here's a great photo of volcanic activity on Io — the "Masubi plume":



1) NASA Photojournal, "Masubi plume on Io", `http://photojournal.jpl.nasa.gov/catalog/PIA02502`

You can see hot gas and dust shooting 100 kilometers up into the atmosphere!

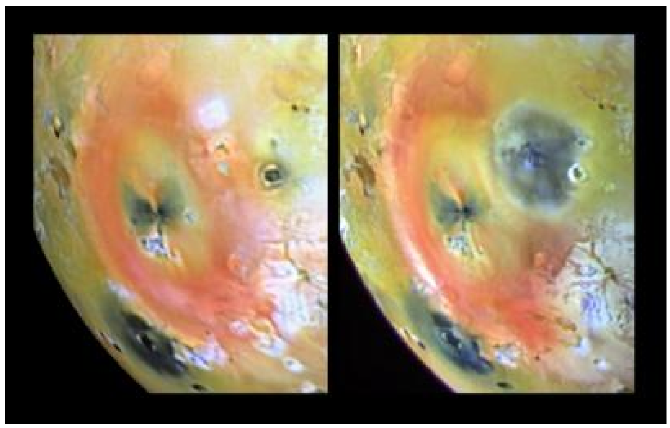
Here's another:



2) Solarviews, "Pele volcano and Pillan Patera", `http://www.solarviews.com/eng/iopele.htm`

In front we see a volcanic feature called Pillan Patera. Over the horizon we see an enormous eruption 300 kilometers high coming from the most intense persistent hot spot on Io: Pele. This seems to be an active lava lake inside a volcanic depression, or "patera", about 20×30 kilometers in size.

But Pillan Patera is no slouch either when it comes to eruptions. Look at these "before and after" pictures taken 5 months apart in 1997:



3) NASA Photojournal, "Arizona-sized Io eruption", `http://photojournal.jpl.nasa.gov/catalog/PIA00744`

The big red ring is sulfur spewed out by Pele. But the exciting new feature in the "after" picture is the dark blotch centered at Pillan Patera. It's 400 kilometers in diameter, roughly the size of Arizona. It consists of about 50 cubic kilometers of lava laid down by a big eruption. At the peak of the activity, 10,000 cubic meters of lava were spewing out each second. This was the largest volcanic eruption ever seen, anywhere!

For more, try these:

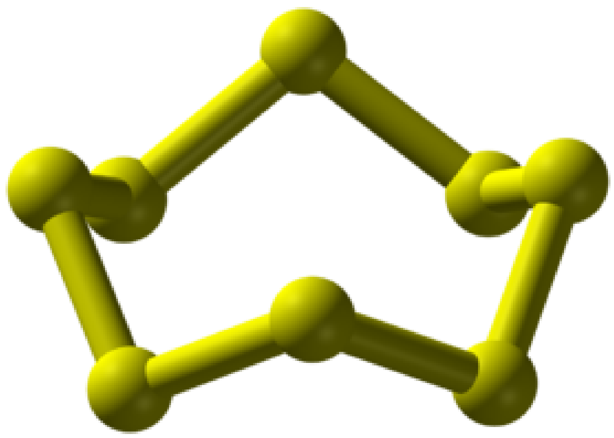
4) A.G. Davies et al, "Thermal signature, eruption style and eruption evolution at Pele and Pillan on Io", *Jour. Geophys. Research* **106** (2001), 33,079–33,103. Also available at `http://europa.la.asu.edu/pgg/associates/members/williams/gw/pdf/2001Daviesetal.pdf`

5) Jani Radebaugh et al, "Observations and temperatures of Io's Pele Patera from Cassini and Galileo spacecraft images", *Icarus* **169** (2004), 65–79.

In case you're wondering about the red sulfur around Pele versus the yellow sulfur you saw last week, let me say a bit about that. Sulfur comes in an incredible number of forms, or allotropes:

6) Wikipedia, "Allotropes of sulfur", `http://en.wikipedia.org/wiki/Allotropes_of_sulfur`

It can form different molecules consisting of 2 to 20 atoms. The most common form on Earth is α-sulfur: rhombic crystals made of ring-shaped molecules containing 8 atoms each:



α-sulfur is lemon yellow, but above 95 degrees Celsius it gradually turns into pale yellow β-sulfur: the ring-shaped molecules reorganize to form crystals with less symmetry — monoclinic crystals, to be precise.

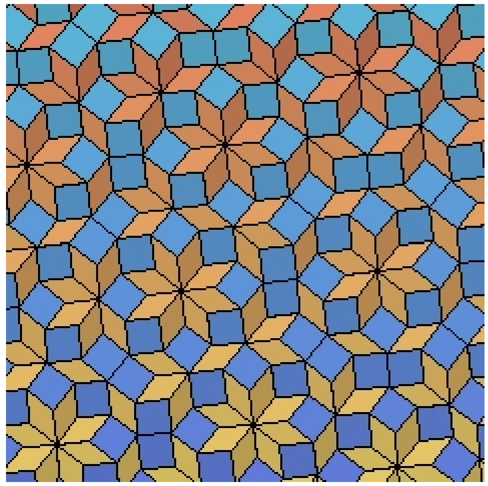
Sulfur melts around 115 Celsius. But when you heat it above 160 Celsius, something weird happens: contrary to the usual pattern for liquids, it gets more viscous as it gets hotter! The reason: the atoms start forming long chain polymers, called "catena sulfur". As these predominate, the stuff gets darker in color: first orange, then red, then dark red, and finally almost black. Blecch! If you then cool it suddenly, it can form a red amorphous solid. And that, presumably, is what we see in the ring around Pele.

Now, from crystals to quasicrystals...

Last week I asked if quasicrystals with approximate 5-fold symmetry could be obtained by slicing lattices in higher dimensions. Greg Egan answered — yes! He even has a beautiful Java applet that demonstrates it:

7) Greg Egan, "deBruijn", `http://www.gregegan.net/APPLETS/12/12.html`

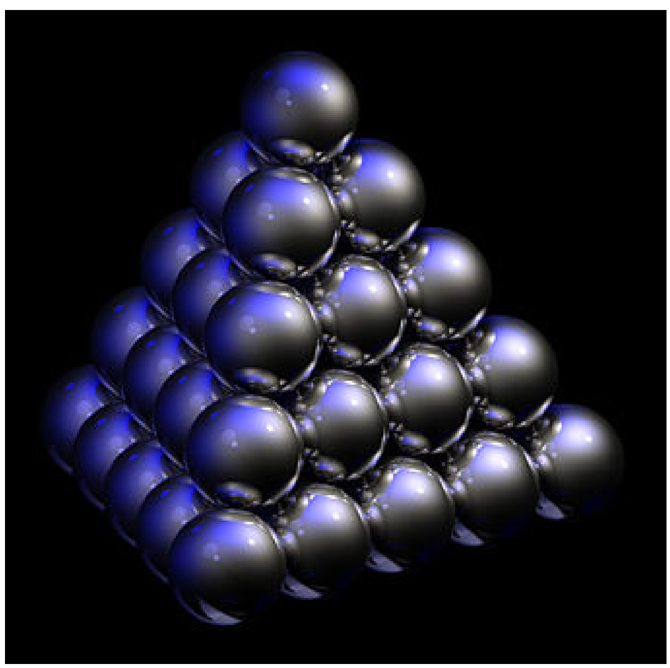
It shows some nice quasiperiodic tilings of the plane with approximate n-fold symmetry, made by cleverly slicing a cubical lattice in n-dimensional space. Here's a piece of one for $n = 4$:



The idea comes from this paper:

8) N. G. deBruijn, "Algebraic theory of Penrose's nonperiodic tilings of the plane, I, II", *Nederl. Akad. Wetensch. Indag. Math.* **43** (1981), 39–52, 53-66.

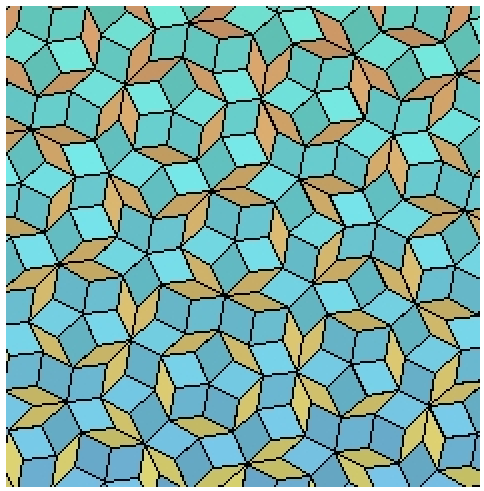
When n is odd, we can also get deBruijn's tiling by slicing the A_{n-1} lattice in $(n-1)$-dimensional space. You're probably most familiar with the A_3 lattice, which shows up when you stack oranges:



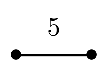
You'll notice this pattern has tetrahedral symmetry. The symmetry group of the tetrahedron is also called the A_3 Coxeter group. It's the group of all permutations of 4 things (the corners of the tetrahedron). This contains the symmetry group of the square, since that group contains some but not all permutations of the 4 corners of the square. Indeed, if you view a regular tetrahedron from the correct angle, it looks like a square!

This pattern goes on for higher n. Last week I spoke about the A_4 lattice, whose symmetry group consists of all permutations of 5 things — namely the 5 corners of a 4-simplex, which is the 4d analogue of a tetrahedron. I explained how this group contains the symmetry group of the pentagon. Indeed, if you view a 4-simplex from the correct angle, it looks like a pentagon!

So, it's not surprising that we can get a quasiperiodic tiling of the plane with approximate 5-fold symmetry by taking a 2d slice of the A_4 lattice and doing a few other tricks. Here's a piece:

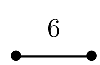


But this generalizes: the symmetries of an $(n-1)$-simplex include the symmetries of a regular n-gon. In other words, just as this Coxeter group, the symmetry group of the pentagon:



sits inside the A_4 Coxeter group:

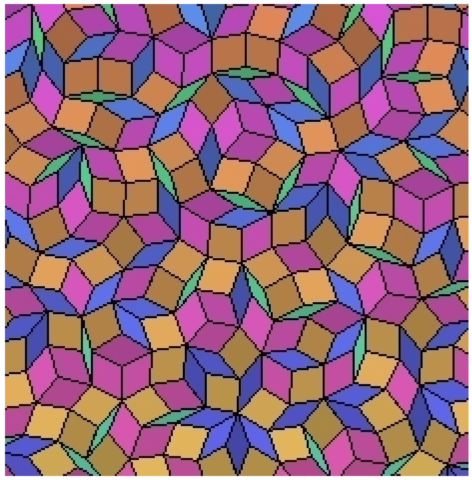
similarly the symmetries of a hexagon:



sit inside the A_5 Coxeter group:

and so on: the noncrystallographic Coxeter groups $\mathrm{I}_2(n)$ sit nicely inside the Coxeter groups A_{n+1}. But the really cool part is how deBruijn uses these to get quasiperiodic tilings of the plane!

You can see details on Greg Egan's page above. Here's a piece of the tiling for $n = 7$, again courtesy of Egan's applet:



And this idea generalizes to the *other* two noncrystallographic Coxeter groups. Remember, there are just two more:

- H_3, the symmetry group of the dodecahedron, with 120 elements;
- H_4, the symmetry group of the 120-cell, with 120×120 elements.

We can get 3d quasicrystals with approximate dodecahedral symmetry by cleverly slicing the 6-dimensional D_6 lattice. This is actually practical, since there really *are* such quasicrystals in nature. And we can get 4d quasicrystals with approximate 120-cell symmetry by cleverly slicing the E_8 lattice! This is just incredibly cool as pure mathematics:

9) Veit Elser and Neil Sloane, "A highly symmetric four-dimensional quasicrystal", *J. Phys. A* **20** (1987), 6161–6168. Also available at `http://akpublic.research.att.com/~njas/doc/Elser.ps`

10) J. F. Sadoc and R. Mosseri, "The E_8 lattice and quasicrystals: geometry, number theory and quasicrystals", *J. Phys. A* **26** (1993), 1789–1809.

11) Robert V. Moody and J. Patera, "Quasicrystals and icosians", *J. Phys. .* **26** (1993), 2829–2853.

Yes, this is the same Moody who helped invent Kac-Moody algebras! For the last decade or so he's been working on quasicrystals. In "Week 20" I explained the "icosians" — a subring of the quaternions built from the symmetries of a dodecahedron — and how Conway and Sloane used them to construct the E_8 lattice. Moody's article uses the icosians to study the 4d quasicrystals that we get by slicing the E_8 lattice.

While they may seem remote from the real world, these 4d quasicrystals can be further sliced to give 3d quasicrytals with approximate dodecahedral symmetry. So in some sense, the quasicrystals we find in nature are "shadows" of the E_8 lattice... trying their best to have a symmetry that can only exist in 8 dimensions, but never quite succeeding.

I love this idea, because it's gotten me over my fear of quasicrystals. They look unruly and complicated, but now I see that some of them have close ties to the beautiful, perfectly symmetrical world of Dynkin diagrams. The "noncrystallographic" Coxeter groups are really "quasicrystallographic"!

Next let me discuss this paper by Jeffrey Morton:

11) Jeffrey Morton, "2-vector spaces and groupoids", available as arXiv:0810.2361.

It's an important new twist in the Tale of Groupoidification! As part of this tale, in "Week 256" I described a functor from the category with

- finite groupoids as objects,
- equivalence classes of spans of finite groupoids as morphisms

to the category with

- finite-dimensional vector spaces as objects,
- linear operators as morphisms.

I called this "degroupoidification". The idea is that a lot of linear algebra has an underlying purely combinatorial "skeleton" that doesn't involve the complex numbers — just symmetry in its purest form. Groupoidification is quest to strip the fat off linear algebra and do it using groupoids.

Jeffrey boosts this idea up one notch, getting a 2-functor from the 2-category with

- finite groupoids as objects,
- spans of finite groupoids as morphisms,
- equivalence classes of spans of spans of finite groupoids as 2-morphisms

to the 2-category with

- finite-dimensional 2-vector spaces as objects,
- linear functors as morphisms,
- natural transformations as 2-morphisms.

Here by "finite-dimensional 2-vector space" I really mean a "Kapranov-Voevodsky 2-vector space". That's a category equivalent to Vect^n for some n, where Vect is the category of finite-dimensional vector spaces. A "linear functor" is one that's linear on each homset. More concretely, we can describe a linear functor

$$F\colon \mathsf{Vect}^n \to \mathsf{Vect}^m$$

as an $m \times n$ matrix of finite-dimensional vector spaces, just as we can describe a linear operator

$$F\colon \mathbb{C}^n \to \mathbb{C}^m$$

as a $m \times n$ matrix of complex numbers.

This suggests that Jeffrey is secretly talking about a categorified version of Heisenberg's "matrix mechanics" — and that's true. I want to explain that. But I'm getting really sick of saying "finite" and "finite-dimensional". So, henceforth I'll leave out those adjectives... but they're really always there. Okay?

Degroupoidification turns each groupoid into a vector space... but in fact it gives more: a Hilbert space! Similarly, Jeffrey's process actually turns each groupoid into a 2-Hilbert space. I proved that a long time ago:

12) John Baez, Higher-dimensional algebra II: 2-Hilbert spaces, Adv. Math. 127 (1997), 125-189. Also available as q-alg/9609018.

So, just as degroupoidification reveals that a fair amount of quantum mechanics can be done with groupoids instead of vector spaces, Jeffrey's process reveals that a fair amount of *categorified* quantum mechanics can also be done with groupoids!

Categorified quantum mechanics becomes important when we go from the physics of particles (which is really field theory on 1d spacetimes) to the physics of strings (which is really field theory on 2d spacetimes). The simplest case is "topological string theory", also known as "extended 2d topological quantum field theory". And the simplest example of such a theory is the "Dijkgraaf-Witten model": a gauge theory with a finite gauge group.

In his thesis:

13) Jeffrey Morton, *Extended TQFT's and Quantum Gravity*, Ph.D. thesis, U. C. Riverside, 2007. Available at arXiv:0710.0032.

Jeffrey showed that a special case, the "untwisted" Dijkgraaf-Witten model, gives a weak 2-functor from the weak 2-category with

- 0d manifolds as objects,
- 1d cobordisms between these as morphisms,
- equivalence classes of 2d cobordisms between these as 2-morphisms

to the weak 2-category with

- finite-dimensional 2-vector spaces as objects,
- linear functors as morphisms,
- natural transformations as 2-morphisms.

Composing this 2-functor with the 2-functor I just described, he gets the untwisted Dijkgraaf-Witten model as an extended TQFT! And in fact, he does it in all dimensions, not just dimension 2.

By the way, most of the 2-categories and 2-functors here are "weak". Also by the way, Jeffrey constructed the above cobordism 2-category in an earlier paper, which I discussed in "Week 242". He recently polished up this paper, changing the title to make it focus on the algebraic essence of his construction:

14) Jeffrey Morton, "Double bicategories and double cospans", available as arXiv:math/0611930.

There's a lot more I could say about this, but not a lot more time. So, let me wrap up with a pointer to Stephen Summers' review of new work on constructive quantum field theory.

Constructive quantum field theory is the branch of mathematical physics where you try to rigorously construct examples of quantum field theories. I did my Ph.D. thesis on this subject under Irving Segal, but it was too hard for me, and my heart was never really in it, so I soon fled — first to classical field theory, and then further.

I recently met Stephen Summers at a conference in honor of von Neumann, and he tried to call me back to my roots. It turns out there's been a lot of interesting progress in constructive quantum field theory! I'll probably keep working on topological quantum field theory and other wimpy subjects — but it's great to hear someone out there is doing the hard work of getting physically realistic quantum field theories to make rigorous mathematical sense.

Here's some of what he has to say:

> *The development of the tools and techniques of algebraic quantum field theory (AQFT) has reached the point where they can be turned upon the question of existence of quantum field models. Although the program of constructing models via AQFT is still in its infancy and only a few researchers are working in the field, already some encouraging successes can be displayed. I personally find it stimulating that the ideas employed go well beyond the range of the semiclassical ideas which were mathematically developed by researchers in constructive quantum field theory in the 70's and 80's. There is no appeal to Lagrangians, actions and perturbation theory, nor does one "work in the Euclidean realm", and one generally avoids a direct construction of strictly local quantum field operators (as these either do not exist or are prohibitively difficult to construct), preferring to construct more physically relevant quantities such as the scattering amplitudes and local "observables". Some of the constructed models are local and free, some are local and have nontrivial S-matrices, and yet others manifest only certain remnants of locality, although these remnants suffice to enable the computation of nontrivial two-particle S-matrix elements. This includes models with nontrivial scattering in four spacetime dimensions.*

This is just the beginning of a fascinating review. Check it out:

15) Stephen J. Summers, "Constructive AQFT", http://www.math.ufl.edu/~sjs/construction.html

Also check out his big AQFT page, which lists textbooks and many more references:

16) Stephen J. Summers, "Algebraic quantum field theory", http://www.math.ufl.edu/~sjs/aqft.html

During the journey we commonly forget its goal. Almost every profession is chosen as a means to an end but continued as an end in itself. Forgetting our objectives is the most frequent act of stupidity.

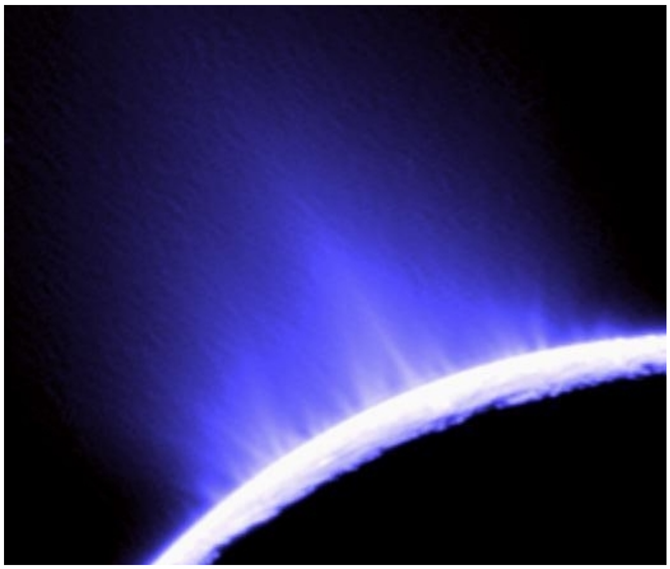
— *Friedrich Nietzsche*

Week 272

November 28, 2008

Today I want to talk about the Enceladus plumes, the Io flux tube, special properties of the number 6 — and the wonders of standard Borel spaces, commutative von Neumann algebras and Polish groups.

Let's start by leaving the fire and brimstone of Io. Let's sail out to the icy splendor of Saturn's moon Enceladus, and gaze at its geysers:



1) NASA Photojournal, "Jet blue", `http://photojournal.jpl.nasa.gov/catalog/PIA08386`

We've seen Enceladus once before, in "Week 231". I showed you a photo of the icy geysers near its south pole, taken by the Cassini probe back in 2005. Above is a prettier version of the same picture, released this August to celebrate Cassini's return to Enceladus. You can see the geysers shooting plumes of ice into outer space!

You see, this year NASA has been using the Cassini probe to take a better look at large cracks on the icy surface of Enceladus. They're called "tiger stripes", and this where the geysers are. On August 11th, the Cassini probe shot past Enceladus at a distance of just

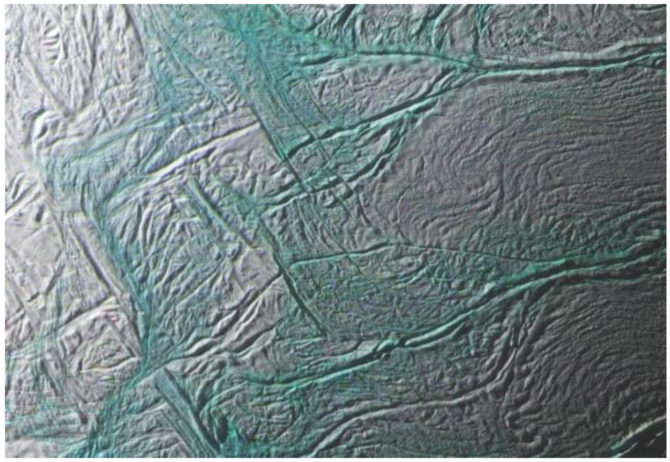
50 kilometers. It took some wonderful photos as it approached:

2) NASA Photojournal, "Great southern land", `http://photojournal.jpl.nasa.gov/catalog/PIA11112`

Here you see the four main tiger stripes on Enceladus. They're the big greenish cracks near the south pole, running from left to right. For some reason these tiger stripes are named after cities mentioned in that wonderful book of tales called One Thousand and One Nights. From top to bottom: Damascus Sulcus, Baghdad Sulcus, Cairo Sulcus and Alexandria Sulcus. "Sulcus" is Latin for a depression or fissure. It's often used as a medical term, especially for features of the brain — but on Enceladus it just means "tiger stripe".

These tiger stripes are about 2 kilometers wide and 500 meters deep. They're flanked by ridges that are about 2-4 kilometers wide and 100 meters tall. The Damascus and

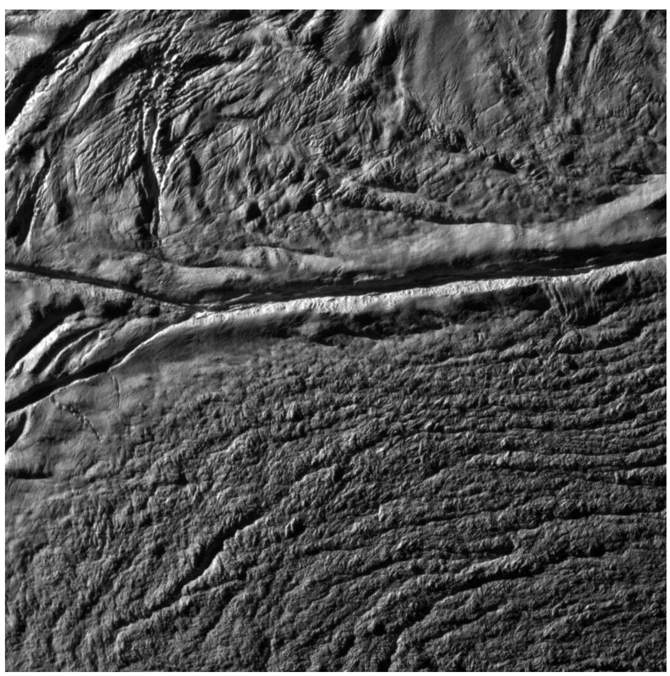
Baghdad Sulci are the most active ones, as far as geysers go:



Alexandria Sulcus is the least active.

The white stuff in this false-color image is fine-grained ice. The green stuff is bigger chunks of ice — even house-sized boulders. These bigger chunks are concentrated along valley floors and walls, and along the upraised flanks of the tiger stripes.

At its closest approach, Cassini took much higher resolution photographs of the tiger stripes, like this closeup of the big fork in Damascus Sulcus:



3) NASA Photojournal, "Damascus Sulcus on Enceladus", `http://photojournal.jpl.nasa.gov/catalog/PIA11113`

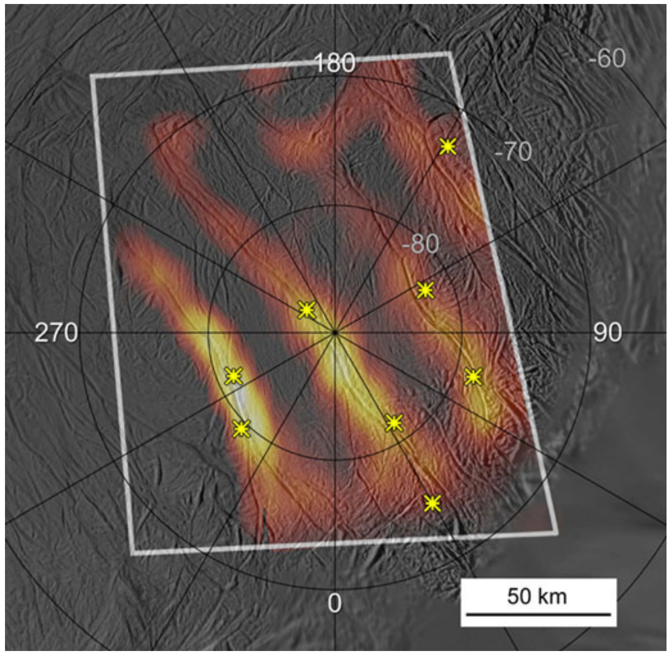
The hard part was compensating for the fact that the spacecraft was whizzing by the moon. The NASA team used a clever maneuver they called a "skeet shoot" to tackle this problem:

> *On Earth, skeet shooting is an outdoor shotgun sport that simulates shooting game birds in flight. A small Frisbee-like ceramic disk, called a clay pigeon, is launched through the air, usually diagonally across in front of a shooter armed with a shotgun. The skill in successfully hitting the moving clay target with the birdshot is for the shooter to point a little ahead of the clay pigeon, and match its angular velocity when the trigger is pulled. The clay pigeon then passes into the bird shot at exactly when the shot arrives at its destination in the path of the moving target. So far so good. At and just after closest-approach on the Enceladus 4 flyby, relative to Cassini and Optical Remote Sensing boresight directions, Enceladus was streaking too quickly across the sky for the spacecraft to be able to stably target and track any geological feature on the surface. Borrowing from the firearms sport, the trick was to turn the spacecraft as fast as possible in the same direction as Enceladus' path across the sky. The plan was to be leading Enceladus and match its angular velocity at the exact times when our targets of interest passed into our camera's field of view.*

It worked amazingly well. They got some photos that could see features as small as 10 meters! It's great fun to read blogs of the scientists involved, and watch a video of the maneuver:

4) NASA Blogs, "Enceladus — Aug08", `http://blogs.nasa.gov/cm/blog/cassini-aug08/`

During this flyby, an infrared spectrometer on the Cassini probe saw that the tiger stripes are significantly warmer than their surroundings. Here's a temperature map with approximate locations of active geysers. From left to right you can see Damascus Sulcus, Baghdad Sulcus, Cairo Sulcus and Alexandria Sulcus:



5) NASA Photojournal, "Jet spots in tiger stripes", http://photojournal.jpl.nasa.gov/catalog/PIA10361

Other instruments detected a plume of water vapor, ice, methane, carbon dioxide, nitrogen, and more complicated organic compounds. All this is good evidence for "cryovolcanism": a version of volcanic activity that can happen on really cold worlds, with water playing the role of lava. Cryovolcanism has also been seen on other moons of outer planets: Triton, Europa, Titan, Ganymede and Miranda.

Cassini made two more flybys in October, and another in November:

6) NASA Blogs, "Enceladus", http://blogs.nasa.gov/cm/blog/enceladus

And just this week, Candice Hansen and her coauthors came out with a new paper about the geysers of Enceladus:

7) C. J. Hansen, L. W. Esposito, A. I. F. Stewart, B. Meinke, B. Wallis, J. E. Colwell, A. R. Hendrix, K. Larsen, W. Pryor and F. Tian, "Water vapour jets inside the plume of gas leaving Enceladus", *Nature* **456** (27 November 2008), 477–479.

8) NASA, "Enceladus Jets: Are They Wet or Just Wild?", http://www.nasa.gov/mission_pages/cassini/whycassini/cassinif-20081126.html

They analyzed data from the ultraviolet spectrometer on Cassini and saw four separate jets of water blocking out the light of distant stars. With further clever reasoning they estimated the velocity of these jets. They seem to be moving at supersonic speeds — up to 2000 kilometers per hour.

Then, they argue that such high speeds are only possible if there's lots of underground liquid water near the surface on this part of Enceladus!

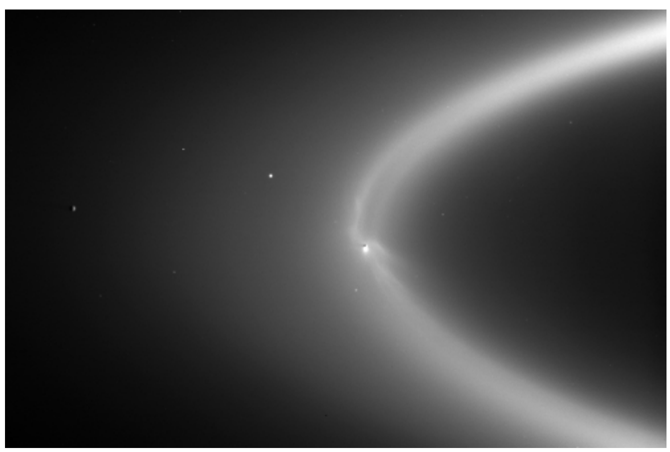
This conclusion is still controversial. Susan Kieffer of the University of Illinois at Urbana-Champaign has a competing theory, which says the water is in the form of gas clathrates. I showed you pictures of these back in "Week 269": they're cage-shaped ice crystal structures that trap gas molecules inside. There are about 6 trillion tons of gas clathrates on the ocean floors here on Earth. Maybe they're also on Enceladus:

9) Susan W. Kieffer, Xinli Lu, Craig M. Bethke, John R. Spencer, Stephen Marshak, Alexandra Navrotsky, "A clathrate reservoir hypothesis for Enceladus' south polar plume", *Science* **314** (15 December 2006), 1764–1766.

Only time will tell.

What's not controversial is that the geysers of Enceladus shoot out plumes of stuff — mainly water vapor and ice, but also other gases and dust — at speeds exceeding escape velocity. Enceladus is small: just 500 kilometers across. So its gravity is weak; its escape velocity is low. So it's easy for geysers to blast stuff straight out into space.

And then what? Then it spreads out and form a huge ring around Saturn: the "E ring". The E ring is much more diffuse than the really famous rings of Saturn. It's much further out, and usually almost invisible. But here's a great photo of the E ring, again taken by the hero of this story — the Cassini probe:



10) NASA Photojournal, "Ghostly fingers of Enceladus", `http://photojournal.jpl.nasa.gov/catalog/PIA08321`

The trick was to take the shot with the sun almost directly behind the camera. Unexpected wispy patterns may hint at interactions between the ring and Saturn's magnetic field. Enceladus is the white blob in the middle of the ring. If you look carefully, you can see its geysers... and you can even see it carving a path through the E ring!

By the way, the volcanic activity on Io also produces a kind of ring: a of ionized sulfur, oxygen, sodium, and chlorine. And since the magnetic field of Jupiter is very strong, and Io is close by, this causes dramatic effects. For example, charged particles from this plasma torus are funnelled down something called the "Io flux tube" to Jupiter's surface, where they create light shows 1000 times brighter than the aurora borealis here on earth! You can see pictures here:

11) Astronomy Picture of the Day, "Jovian aurora", http://apod.nasa.gov/apod/ap980123.html

Next: some exceptional properties of the number 6.

Every set has a group of symmetries, but so does every group. The symmetries of a group are called "automorphisms" of the group. Every symmetry of a set gives a symmetry of its group of its symmetries, called an "inner automorphism". But of all finite sets, only the six-element set has a symmetry group with an **extra** symmetry — a symmetry that doesn't come from a symmetry of that set! This is called an "outer automorphism".

In more jargonesque terms: of all the permutation groups S_n, only S_6 has an outer automorphism. It has just one, and I described it here:

12) John Baez, "Some thoughts on the number six", http://math.ucr.edu/home/baez/six.html

Recently my friend Bruce Westbury told me some more cool facts about the number 6. One of these is another nice description of the outer automorphism of S_6. He heard this from the group theorist Brian Bowditch, who heard it from Chris Penrose (Roger's son). I haven't checked it! I'll just pass it on, as gossip, and let you see if it's true.

It goes like this. Start with the icosahedron, drawn as a graph on the sphere, and identify oposite points. This gives a graph on the projective plane. This graph is called K_6: the "complete graph on 6 vertices". In other words, it's the graph with 6 vertices, each one connected to every other one by a single edge. The faces of this graph are triangles. Apparently there are twelve embeddings of K_6 in the projective plane and they come in six pairs. Each permutation of the six vertices of K_6 induces a permutation of these six pairs. This process gives an automorphism of S_6. And apparently this is the outer automorphism!

Westbury later pointed out this related paper:

13) Ben Howard, John Millson, Andrew Snowden, Ravi Vakil, "A description of the outer automorphism of S_6", and the invariants of six points in projective space, available as arXiv:0710.5916.

I haven't checked to see if the results here imply the claim above.

Another group theorist, Derek Holt, told Westbury about an intriguing conjecture which he believes but has not had the energy to check.

Take a finite simple group and choose two elements at random. What is the probability that they generate the group?

One cool fact is that this probability is never zero. Every finite simple group can be generated by two elements! So, they're all quotients of the free group on two generators.

Another cool fact is that this probability tends to 1 as the order of the group tends to infinity:

14) M. Liebeck and A. Shalev, "The probability of generating a finite simple group", *Geometrica Dedicata* **56** (1995), 103–111.

This implies that this probability attains a *mininum value* for some finite simple group. For A_6 — the group of even permutations of a 6-element set — the probability is $53/90$. Holt's conjecture is that this is the minimum among all finite simple groups.

Here are the probabilities for a few finite simple groups, listed in order of increasing size:

A_2	1	$= 1.000$
A_3	$\frac{8}{9}$	$\sim .889$
$\mathrm{PSL}(2,3) \cong A_4$	$\frac{2}{3}$	$\sim .667$
$\mathrm{PSL}(2,4) \cong \mathrm{PSL}(2,5) \cong A_5$	$\frac{19}{30}$	$\sim .633$
$\mathrm{PSL}(2,7) \cong \mathrm{PSL}(3,2)$	$\frac{19}{28}$	$\sim .679$
$\mathrm{PSL}(2,8)$	$\frac{71}{84}$	$\sim .845$
$\mathrm{PSL}(2,9) \cong A_6$	$\frac{53}{90}$	$\sim .589$
$\mathrm{PSL}(2,11)$	$\frac{127}{165}$	$\sim .770$
A_7	$\frac{229}{315}$	$\sim .727$
$\mathrm{PSL}(4,2) \cong A_8$	$\frac{133}{180}$	$\sim .739$
A_9	$\frac{15403}{18144}$	$\sim .849$

Here A_n is the group of even permutations of an n-element set, while $\mathrm{PSL}(n,q)$ is the group of $n \times n$ matrices with determinant 1 having entries in the field with q elements, mod multiples of the identity matrix. Of the groups listed above, only A_4 is not simple. I included this one just because I wanted to list *all* the unexpected isomorphisms between "alternating" groups (A_n's) and "projective special linear" groups (PSL's).

(Each of these isomorphisms makes a wonderful story in itself — but not for today! If you want to know these stories, try the first of Conway's "Three Lectures on Exceptional Groups" from his book with Sloane on Sphere Packings, Lattices and Groups. Also try "Week 79", to see how these stories are related to Galois' fatal duel and the buckyball.)

So, you see the number 6 has several special properties, all related to permutations of a 6-element set. What do these special properties really mean? I don't know! They're just little clues waiting for a big mystery story to come along.

Lately I've been finishing up a big paper on infinite-dimensional representations of 2-groups, which I'm writing along with my former student Derek Wise, the physicist Laurent Freidel, and his student Aristide Baratin. Derek and Aristide are doing all the really hard work, but I used to do functional analysis when I was a youngster, and there's a lot of measure theory in our paper, so sometimes it's my job to dig up papers on this subject and see if they help. And this sort of work can actually be fun, once I get into the mood.

I don't want to try to explain this paper now. I just feel like explaining some tidbits of measure theory that I've picked up while writing it.

A measure space is basically a space that you can do integrals over. But some measure spaces are better than others. Some of them are immensely huge and nasty. But "standard Borel spaces" include all the examples I care about, and they're incredibly nice.

So, what's a standard Borel space?

It's a kind of "measurable space", meaning a space equipped a collection of subsets that's closed under countable intersections, countable unions and complement. Such a collection is called a "sigma-algebra", and we call the sets in here "measurable". A "measure" on a measurable space assigns a number between 0 and $+\infty$ to each measurable set, in such a way that for any countable disjoint union of measurable sets, the measure of their union is the sum of their measures.

A nice way to build a measurable space is to start with a topological space. Then you take its open sets and keep taking countable intersections, countable unions and complements until you get a sigma-algebra. This may take a long time, but if you believe in transfinite induction you're bound to eventually succeed. The sets in this sigma-algebra are called "Borel sets".

A basic result in real analysis is that if you put the usual topology on the real line, and use this to cook up a sigma-algebra as just described, there's a unique measure on the resulting measurable space that assigns to each interval its usual length. This is called "Lebesgue measure".

Some topological spaces are big and nasty. But separable complete metric spaces are not so bad.

We don't care about the metric in this game. So, we use the term "Polish space" for a topological space that's *homeomorphic* to a complete separable metric space.

And often we don't even care about the topology. So, we use the term "standard Borel space" for a measure space whose measurable sets are the Borel sets for some topology making it into a Polish space.

In short: every complete separable metric space has a Polish space as its underlying topological space, and every Polish space has a standard Borel space as its underlying measurable space.

Now, it's hopeless to classify complete separable metric spaces. It's even hopeless to classify Polish spaces. But it's not hopeless to classify standard Borel spaces! The reason is that metric spaces are like diamonds: you can't bend or stretch them at all without breaking them entirely. But topological spaces are like rubber... and measurable spaces are like dust. So, it's very hard for two metric spaces to be isomorphic, but it's easier for their underlying topological spaces — and even easier for their underlying measurable spaces.

For example, the line and plane are isomorphic, if we use their usual sigma-algebras of Borel sets to make them into measurable spaces! And the plane is isomorphic to $\mathbb{R}^n$ for every n, and all these are isomorphic to a separable Hilbert space! As measurable spaces, that is.

In fact, every standard Borel space is isomorphic to one of these:

- a countable set with its sigma-algebra of all subsets,
- the real line with its sigma-algebra of Borel subsets.

That's pretty amazing. It means that standard Borel spaces are classified by just their *cardinality*, which can only be finite, countably infinite, or the cardinality of the continuum. The "continuum hypothesis" says there's no cardinality between the countably infinite one and the cardinality of the continuum — but we don't need the continuum hypothesis to prove this result.

This amazing result has a nice relative in the world of von Neumann algebras.

Starting from a measure space X, we can form the Hilbert space $L^2(X)$. We can also form $L^\infty(X)$, the space of equivalence classes of bounded measurable complex functions on X, where we identify functions that agree except on a set of measure zero.

$L^\infty(X)$ is an algebra where we multiply functions pointwise. We can think of it as an algebra of multiplication operators on $L^2(X)$. In fact, it forms a "von Neumann algebra" — a kind of algebra that's great for quantum theory. I defined von Neumann algebras back in "Week 175", so I won't do it again here.

$L^\infty(X)$ is a very special sort of von Neumann algebra, namely a *commutative* one. There's a nice theorem: every commutative von Neumann algebra is isomorphic (as an abstract C^*-algebra) to one of this form. Even better, every commutative von Neumann algebra of operators on a *separable* Hilbert space is isomorphic to $L^\infty(X)$ for one of these choices of X:

- a countable set (equipped with counting measure),
- the real line (with Lebesgue measure),
- the disjoint union of a countable set and the real line (with the obvious measure).

We're really talking about measure theory now, even when it seems I'm talking about commutative von Neumann algebras. But sometimes topology sneaks into the game. This especially true if we're studying groups. We can talk about "standard Borel groups": standard Borel spaces made into groups where all the group operations are measurable. But nobody seems to know much about these! What people know about are "Polish groups": Polish spaces made into groups where all the group operations are continuous.

If you like keeping your categories straight, it may seem annoying to put a topology on your group when you're trying to do measure theory. But apparently some topology is a good thing... either that, or people haven't found the best framework for this subject.

It's a bit slippery, you see. Not every standard Borel group comes from a Polish group. But the counterexamples seem to be horrid. And more importantly, every measurable homomorphism between Polish groups is automatically continuous! So there must be some way to define Polish groups as standard Borel groups with an extra *property* — not extra structure.

There are other similar results with the same flavor. For example, suppose we have a Polish group G acting as measurable transformations on a standard Borel space X. Then we can make X into a Polish space so that the action of G on X becomes continuous!

(Of course we also require that the new Borel sets of X are the same as its original measurable sets.)

There are also interesting results saying that Polish spaces and Polish groups "aren't too big". Some of these theorems mention the "Hilbert cube" — that is, a countable product of copies of $[0, 1]$, with its product topology. This space sounds big, but thanks to Tychonoff's theorem it's compact! It's also metrizable: that is, its topology comes from a metric. And it's separable: it has a countable dense subset. In fact, a topological space is separable and metrizable iff it's homeomorphic to a subset of the Hilbert cube.

Here's how the Hilbert cube relates to Polish spaces: a topological space is a Polish space iff it's homeomorphic to a countable intersection of open subsets of the Hilbert cube!

And here's how the Hilbert cube relates to Polish groups: a topological group is a Polish group iff it's isomorphic to a subgroup of the group of homeomorphisms of the Hilbert cube!

I'll wrap up with a nice fact about measures on standard Borel spaces. A "probability measure" on a measure space X is one that assigns the number 1 to the whole set X. Then the measure of a measurable subset S of X can be interpreted as the probability of a point being in X. This idea of using measures to study probabilities lies at the foundation of modern probability theory.

But here's a lesser-known fact: the set of probability measures on X, say $M(X)$, is itself a measurable space!

How does this work? First we give $M(X)$ its "weak topology". This is the topology where a bunch of measures μ_i converge to μ if for every bounded continuous function f on X,

$$\int f d\mu_i \to \int f d\mu$$

Starting with this topology and taking the Borel sets, $M(X)$ becomes a measurable space.

It then turns out that $M(X)$ is a standard Borel space iff X is!

In fact, M is what category theorists call a "monad" on the category of standard Borel spaces. I said what a monad is back in "Week 89", so I won't do it again, but here it involves the presence of god-given measurable maps

$$M(M(X)) \to M(X)$$

and

$$X \to M(X)$$

The first map is the most interesting: a probability measure on the space of probability measures of X gives a probability measure on X! In layman's terms: if you're uncertain about how uncertain you are, you might as well just say you're uncertain.

The second map just sends any point in X to the Dirac delta measure at that point. In layman's terms: if you're certain, you might as well say you're uncertain (just not very much). That is, if you're completely certain about something, you can still describe your state of knowledge by a probability distribution.

You can see some applications of this monad here:

15) David Corfield, "Category theoretic probability theory II", `http://golem.ph.utexas.edu/category/2007/02/category_theoretic_probability_1.html`

16) Ernst-Erich Doberkat, "Characterizing the Eilenberg-Moore algebras for a monad of stochastic relations", *Universitat Dortmund, Fachbereit Informatik, Lehrstule fuer Software-Technologie*, Memo **147**. Also available at `https://eldorado.uni-dortmund.de/bitstream/2003/2717/1/147.pdf`

For more on standard Borel spaces in probability theory, try this:

17) K. R. Parthasrathy, *Probability Measures on Metric Spaces*, Academic Press, San Diego, 1967.

For standard Borel spaces and von Neumann algebras, try this:

18) W. Arveson, *An Invitation to C^*-Algebra*, Springer, Berlin, 1976.

And for really hard-core results on standard Borel spaces and Polish groups, try this:

19) H. Becker and A. S. Kechris, *Descriptive Set Theory of Polish Group Actions*, Cambridge U. Press, Cambridge, 1996.

I can't imagine any of the normal readers of This Week's Finds will enjoy this book, but I could be wrong: there could be some really scary people who read this column.

Addenda: I thank Greg Egan, Benoit Jubin, Ted Nitz and Ultrawaffle for corrections and improvements. Originally I had a less complete table of probabilities for finite simple groups, and I begged my readers for help. Ted Nitz and Greg Egan stepped into save me.

Ted Nitz calculated the odds that a pair of elements in A_4 generates the group and got $2/3$:

> *I decided to take your challenge. For A4 the odds are 96/144 = 2/3 that a pair generates. For reference, the sage code that gave the probability is:*

```
A4 = AlternatingGroup(4)
order = A4.order()
i = 0
j = 0
for g in A4:
    for h in A4:
        i += 1
        if A4.subgroup([g,h]).order() == order:
            j += 1
print (j,i)
j/i
```

> *It was blindingly fast for A4, and if it ever finishes for A9, I'll let you know the probability there.*
>
> *-Ted*

He later realized that the A_9 calculation would take at least a century. Greg Egan wrote a Mathematica program based on a more efficient algorithm and got the answer in less than a day.

For more discussion, including the idea behind Egan's algorithm, visit the n-Category Caf.

> The splitting into something discrete and something continuous seems to me to be a basic issue in all morphology.
>
> — *Hermann Weyl*

Week 273

December 14, 2008

Today I'd like to talk about the history of the Earth, and then say a bit about locally compact abelian groups. But first, a few more words about Enceladus.

Last week we visited the geysers of Saturn's moon Enceladus. Afterwards, George Musser pointed me to an article on this subject by Carolyn Porco, leader of the imaging team for the Cassini-Huygens mission — the team that's been taking the photos I showed you. It's a great article, leading up to some intriguing theories about what powers these geysers:

1) Carolyn Porco, "Enceladus: secrets of Saturn's strangest moon", *Scientific American*, November 2008, available at `http://www.sciam.com/article.cfm?id=enceladus-secrets`

And it's free online! — at least for now. I've criticized the Scientific American before here, but if they keep coming out with articles like this, I'll change my tune. For one thing, it's well-written:

> *There is obviously a tale writ on the countenance of this little moon that tells of dramatic events in its past, but its present, we were about to find out, is more stunning by far. In its excursion over the outskirts of the south polar terrain, Cassini's dust analyzer picked up tiny particles, apparently coming from the region of the tiger stripes. Two other instruments detected water vapor, and one of them delivered the signature of carbon dioxide, nitrogen and methane. Cassini had passed through a tenuous cloud.*
>
> *What is more, the thermal infrared imager sensed elevated temperatures along the fractures — possibly as high as 180 kelvins, well above the 70 kelvins that would be expected from simple heating by sunlight. These locales pump out an extraordinary 60 watts per square meter, many times more than the 2.5 watts per square meter of heat arising from Yellowstone's geothermal area. And smaller patches of surface, beyond the resolving power of the infrared instrument, could be even hotter.*

For another, it tackles a fascinating mystery. Where does all this power come from? The geysers near the south pole of Enceladus emit about 6 gigawatts of heat. Enceladus is too small to have that much radioactive heating at its core — only about 0.3 gigawatts, probably. The rest must come from tidal heating. This happens when stuff sloshes back and forth in a changing gravitional field: friction converts this motion to heat.

So, what causes tides on Enceladus? It may be important that Enceladus has a $2:1$ resonance with Dione: it orbits Saturn twice for each orbit of that larger moon. This sort of resonance is known to cause tidal heating. For example, in "Week 269", I showed you how Jupiter's moon Io is locked in resonances with Europa and Ganymede. The resulting tidal heat powers its mighty volcanos.

Unfortunately, the resonance with Dione doesn't seem powerful enough to produce the heat we see on Enceladus. Unless something funny is going on, there should only be 0.1 gigawatts of tidal heating — not nearly enough! At least that's what Porco estimated in 2006:

2) Carolyn Porco et al, "Cassini observes the active south pole of Enceladus", *Science* **311** (2006), 1393–1401.

So, we need to dream up a more complicated story.

Here's one: there could be a kind of slow cycle where the orbit of Enceladus gets more eccentric, tidal heating increases, ice beneath its surface melts, more sloshing water causes more tidal heating, and then the release of heat energy damps its eccentric orbit, until it freezes solid and the whole cycle starts over. We could be near the end of such a cycle right now.

Here's another: maybe Enceladus has an sea of liquid water under the frozen surface of its south pole. With enough water sloshing around, there could be a lot more tidal heating than you'd naively guess... and this heating, in turn, could keep the water liquid. The fun thing about this second scenario is that a permanent liquid ocean on Enceladus raises the possibility of life!

Nobody knows for sure what's going on — but Carolyn Porco examines the options in a clear and engaging way. If you like celestial mechanics, also try this paper:

3) Jennifer Meyer, Jack Wisdom, "Tidal heating in Enceladus", *Icarus* **188** (2007), 535–539. Also available at `http://groups.csail.mit.edu/mac/users/wisdom/meyerwisdom1.pdf`

I wrote about Jack Wisdom's work back in "Week 107" — it's fascinating stuff. He knows a lot about resonances. For related work on the Jupiter-Saturn resonance, the Neptune-Pluto resonance, and the math of continued fractions, also try the addenda to "Week 222".

Next I'd like to give you a quick trip through the Earth's history. In "Week 196" we looked back into the deep past, all the way to the electroweak phase transition 10 picoseconds after the Big Bang. On the other hand, here:

4) John Baez, "The end of the universe", `http://math.ucr.edu/home/baez/end.html`

you can zip forwards into the deep future — for example, 10^{19} years from now, when the galaxies boil off, shooting dead stars into the the vast night.

But now I'd like to zoom in closer to home and quickly tell the history of the Earth, focusing on an aspect you may never have thought about. You see, Kevin Kelly recently pointed me to this fascinating paper on "mineral evolution":

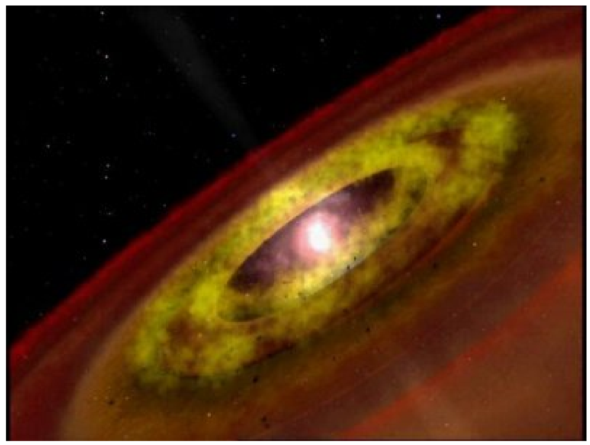
5) Robert M. Hazen, Dominic Papineau, Wouter Bleeker, Robert T. Downs, John M. Ferry, Timothy J. McCoy, Dmitri A. Sverjensky and Henxiong Yang, "Mineral evolution", *American Mineralogist* **91** (2008), 1693–1720.

Ever since it was formed, the number of different minerals on Earth has kept going up — and ever since *life* ran wild, it's soared! Some examples are obvious: seashells become limestone, which gets squashed into marble. Some are less so: for example, there wasn't much *clay* before the advent of life.

Here's a timeline loosely taken from this paper:

- **The era of planetary formation.**

- Primary chondrite minerals (> 4.56 billion years ago): *60 species of mineral*.
  Chondrites are stony meteorites that formed early in the history of the solar system. They're made of chondrules — millimeter-sized spheres of olivine, pyroxene and other minerals — together with nuggets called CAIs (calcium-aluminum rich inclusions) and other stuff. These chondrules began life as molten droplets back when the Sun was a T Tauri star, heated only by gravitational collapse.



Artist's image of the Sun as a T Tauri Star.

- Aqueous alteration, thermal alteration, and shocks form achondrites and iron meteorites (4.56 to 4.55 billion years ago): *250 species of mineral*.
  This is the era when the disk of dust circling the early Sun started forming lumps. As these lumps collided, they got bigger and bigger, eventually forming the asteroids and planets we see today. Some of these protoplanets melted, letting heavier metals sink to their core while lighter material stayed on top. But then some crashed into each other, shattering and forming new kinds of meteorites: iron-nickel meteorites, and stony meteorites called achondrites. Thanks to radioactive dating of these, scientists claim a shockingly precise knowledge of when all this happened: sometime between 4.56 and 4.55 billion years ago.

- **The era of crust and mantle reworking**.
  - Igneous rock evolution (4.55 to 4 billion years ago): *350 species of mineral*.
    The Earth's history is divided into four eons: the Hadean, Archean, Proterozoic, Phanerozoic. Back when I was a kid, the Cambrian era seemed really old — but that's just the start of the current eon, when multicellular life emerged: the Phanerozoic. We're digging much deeper now: the Phanerozoic will be *end* of today's story. The Hadean eon began with a bang: the event that formed the Moon around 4.55 billion years ago! What made the moon? The current most popular explanation is the "giant impact theory" — sometimes called the Big Splat Theory. Dana Mackenzie spends a lot of time writing about math, but he's also written a book about this:

6) Dana Mackenzie, *The Big Splat, or How Our Moon Came To Be*, Wiley, New York, 2003.

The idea is that another planet formed in one of the "Lagrange points" of Earth's orbit — a stable spot 60 degrees ahead or behind the Earth:

7) John Baez, "Lagrange points", `http://math.ucr.edu/home/baez/lagrange.html`

But when this planet reached about the mass of Mars, it would no longer be stable in this location. So, it gradually drifted toward Earth, and eventually smacked right into us! The impact was incredibly energetic, melting the Earth's entire crust and outer mantle. The iron core of this other planet sank into Earth's core, while about 2% of the outer part formed an orbiting ring of debris. Within a century, about half of this ring formed the Moon we know and love.

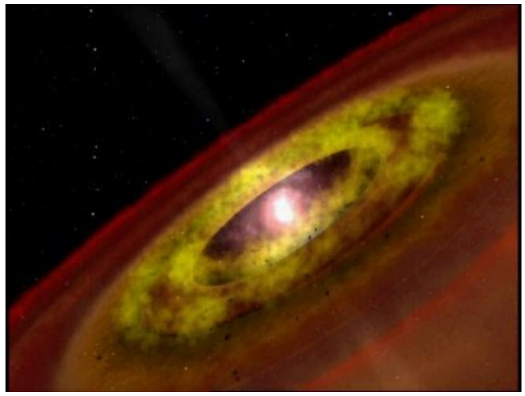
It's an amazing story, but most of the evidence seems to support it. The early Moon is known to have been much closer to Earth than it is now — it's been receding ever since. For this and many other reasons, the giant impact theory is sufficiently solid that the hypothetical doomed planet that hit Earth has a name: Theia! You can even watch a simulation of it hitting Earth, produced by Robin Canup:

8) Dana Mackenzie, "The Big Splat (animation)", `http://www.danamackenzie.com/big_splat_animation.htm`

Let me quote Mackenzie on this:

> *The simulation shows the first twenty-four hours after the giant impact. It begins with Theia about to strike the Earth. After the impact, one hemisphere of the Earth is sheared off and flung into space. The remaining part of Earth is very lopsided, and sets up a "gravitational torque" on the debris. This boosts some of the debris into orbit. (Without such a boost, it would all simply fall back down again.)*
>
> *Within a few hours, the debris has formed an "arm" that smashes spectacularly back into the Earth. This crash is nearly as explosive as the original impact! (The second explosion can be seen much more vividly in the video than in the still frames published in my book.) Notice how the temperature of the Earth has risen, from the blues and greens of the early frames to yellows and reds, indicating more than 2000 degrees*

Kelvin. Earth has literally become a blast furnace.



Robin Canup's simulation of Earth and Theia, 50 minutes after their initial collision.

> *As the fateful day continues, the debris gets more uniformly distributed in a disk around the Earth. Notice, though, that this disk is not stable like the rings of Saturn. It develops shock waves that whirl around the Earth, collecting material into spiral arms. According to Alastair Cameron, another modeler the giant impact, these spiral arms also play an important role in the development of the Moon, by "siphoning" debris up from lower orbits into higher ones. Scientists have estimated that a mass at least twice the present mass of the Moon had to be lifted beyond the "Roche limit," roughly twelve thousand miles or three Earth radii above the surface. Any debris that does not make it past the Roche limit will be torn apart by tidal forces, and cannot form a permanent moon. This simulation stops after 24 hours, a long time before the disk of debris condenses into our Moon. The Moon was not formed in a day! However, it did form much more rapidly than you might expect; current estimates range from 1 to 100 years. This is astounding, compared to ordinary geological time scales. An entire new planet was born within the life span of a single human.*

No rocks on Earth are known to survive from before 4.03 billion years ago, so the details of this time period are hotly debated. However, many igneous rocks, especially basalt, must have been formed at this time.

Even after the surface cooled enough to form a crust, volcanoes continued to release steam, carbon dioxide, and ammonia. This led to what is called the Earth's "second atmosphere". The "first atmosphere" was mainly hydrogen and helium; the second was mainly carbon dioxide and water vapor, with some nitrogen but almost no oxygen. This second atmosphere had about 100 times as much gas as today's "third atmosphere"!

As the Earth cooled, oceans formed, and much of the carbon dioxide dissolved into the seawater and later precipitated out as carbonates.

– Granitoid formation and the first cratons (4 to 3.2 billion years ago): *1000 species of mineral*.

Between 4 and 3.8 billion years ago there was another scary time: the Late Heavy Bombardment. A lot of large craters on the Moon date to this period, so probably the Earth, Venus and Mars got hit too. Why so many impacts after a period of relative calm? One theory is that Jupiter and Saturn locked into their current $2:1$ resonance at this time, causing a big shakedown in the asteroid belt and Kuiper belt. Wikipedia has a nice quick review of this and other theories:

9) Wikipedia, "Late heavy bombardment", `http://en.wikipedia.org/wiki/Late_heavy_bombardment`

This time also marked the rise of "cratons". Cratons are a bit like small early "plates" in the sense of plate tectonics: they're ancient tightly-knit pieces of the earth's crust and mantle, many of which survive today. While most cratons only finished forming 2.7 billion years ago, nearly all started growing earlier, in the Eoarchean era.

Cratons are made largely of granitoids. Granitoids are more sophisticated igneous rocks than basalt. Modern granite is one of these. Granite is made in a variety of ways, for example by the remelting of sedimentary rock. Early granitoids were probably simpler.

- Emergence of plate tectonics (3.2 to 2.8 billion years ago): *1500 species of mineral*.

  In the Paleoarchean and Mesoarchean eras, plate tectonics as we know it began. A key aspect of this process is the recycling of the Earth's crust through subduction: oceanic plates slide under continental plates and get pushed down into the mantle. Another feature is underwater volcanism and hydrothermal activity.

- Anoxic biology leading up to photosynthesis (3.9 to 2.5 billion years ago): *1500 species of mineral*.

  The earliest hints of life include some banded iron formations that date back 3.85 billion years. The real fun starts with the rise of photosynthesis leading up to the Great Oxidation Event about 2.5 billion years ago — more on that later. But organisms from the domain Archea can do well in a wide variety of extreme environments without oxygen, and as their name suggests, many of these organisms are very ancient. These organisms gave rise to an active sulfur cycle and deposits of sulfate ores starting in the Paleoarchean era. They also made the atmosphere increasingly rich in methane throughout the Mesoarchean and Neoarchean. So, life was already beginning to affect mineral evolution.

- **The era of bio-mediated mineral formation**.
  - The Great Oxidation Event (2.5 to 1.9 billion years ago): *over 4000 species of mineral*. The Archean eon ended and the Proterozoic began with the Great Oxidation Event 2.5 billion years ago. In this event, also known as the Oxygen Catastrophe, photosynthesis put enough oxygen into the atmosphere to make it lethal to most organisms of the time. Luckily evolution found a way out of this impasse. The oyxgen-rich atmosphere in turn led to a wide variety of new minerals.
  - The intermediate ocean (1.9 to 1 billion years ago): *over 4000 species of mineral*. In the Mesoproterozoic era, increased oxygen levels in the ocean put an end to many anoxic life forms. For example, around 1.85 billion years ago, banded iron formations suddenly ceased. The next gigayear was rather static and dull — if you're mainly interested in new minerals, that is. The term "intermediate ocean" means that during this period, the seawater contained a lot more oxygen than before, but still much less than today.
  - Snowball Earth and the Neoproterozoic oxygenation events (1 to 0.54 billion years ago): *over 4000 species of mineral*.

    The Neoproterozoic era probably saw several Snowball Earth events: episodes of runaway glaciation during which most or all the Earth was covered with ice. Since ice reflects sunlight, making the Earth even colder, it's easy to guess how this runaway feedback might happen. The opposite sort of feedback is happening now, as melting ice makes the Earth darker and thus even warmer. The interesting questions are why this instability doesn't keep driving the Earth to extreme temperatures one way or another — and what stopped the Snowball Earth events back then!

    Here's a currently popular answer to the second question. Ice sheets slow down the weathering of rock. Weathering of rock is one of the main long-term processes that use up atmospheric carbon dioxide, by converting it into various carbonate minerals. On the other hand, even on an ice-covered Earth, volcanic activity would keep putting carbon dioxide into the atmosphere. So, eventually carbon dioxide would build up, and the greenhouse effect would warm things up again. This process might be very dramatic, with perhaps as much as 13% of the atmosphere being carbon dioxide (350 times what we see today), and temperatures soaring to 50 Celsius! But the details are still the subject of much controversy.

At the end of these glacial cycles, it's believed that oxygen increased from 2% of the atmosphere to 15%. (Now it's 21%.) This may be why multi-celled oxygen-breathing organisms date back to this time. Others argue that the "freeze-fry" cycle imposed tremendous evolutionary pressure on life and led to the rise of multicellular organisms. Both these theories could be true.

During the glacial cycles, few new minerals were formed — unless you count ice. Afterwards, surface rocks were weathered in new ways involving oxidation.

– Phanerozoic biomineralization (0.54 billion years ago to now): *over 4300 species of mineral*.

  The Phanerozoic eon, beginning with the Cambrian 540 million years ago, marks the rise of of life as we know it. During this time, sea life has given rise to extensive deposits of biominerals such as calcite, aragonite, dolomite, hydroxylapatite, and opal. There has also been increased production of clay and many different types of soil.

This is the end of our story — but of course the story isn't over. We're now in the Anthropocene epoch of the Cenozoic era of the Phanerozoic eon. New things are happening. Humans are boosting atmospheric carbon dioxide levels. If the temperature rises one more degree, the Earth's temperature will be the hottest it's been in 1.35 million years, before the Ice Ages began. There's no telling when this trend will stop. We're filling the oceans and land with plastic and other debris. In millions of years, these may form new species of minerals. Regardless, there will probably still be rocks — but we'll either be gone or drastically changed.

Next: Pontryagin duality! Like last week's math topic, I needed to learn more about this for my work on infinite-dimensional representations of 2-groups. And like last week's math topic, it involves a lot of analysis. But it also involves a lot of algebra and category theory.

You may know about Fourier series, which lets you take a sufficiently nice complex-valued function on the circle and write it like this:

$$f(x) = \sum_k g_k \exp(ikx)$$

Here k ranges over all integers, so what you're really doing here is taking a function on the circle:

$$f \colon S^1 \to \mathbb{C}$$

and expressing it in terms of a function on the integers:

$$g \colon \mathbb{Z} \to \mathbb{C}$$

More precisely, any L^2 function on the circle can be expressed this way for some L^2 function on the integers — and conversely. In fact, if we normalize things right, the Fourier series gives a unitary isomorphism between the Hilbert spaces $L^2(S^1)$ and $L^2(\mathbb{Z})$.

You may also know about the Fourier transform, which lets you take a sufficiently nice complex-valued function on the real line and write it like this:

$$f(x) = \int g(k) \exp(ikx) dk$$

Here k also ranges over the real line, so what you're really doing is taking a function on the line:

$$f \colon \mathbb{R} \to \mathbb{C}$$

and expressing it in terms of another function on the line:

$$g \colon \mathbb{R} \to \mathbb{C}$$

In fact, any L^2 function $f \colon \mathbb{R} \to \mathbb{C}$ can be expressed this way for some L^2 function $g \colon \mathbb{R} \to \mathbb{C}$. And if we normalize things right, the Fourier transform is a unitary isomorphism from $L^2(\mathbb{R})$ to itself.

Pontryagin duality is the grand generalization of these two examples! Any locally compact Hausdorff abelian group A has a "dual" A^* consisting of all continuous homomorphisms from A to S^1. The dual is again a locally compact Hausdorff abelian group — or "LCA group", for short. When you take duals twice, you get back where you started. And the Fourier transform gives a unitary isomorphism between the Hilbert spaces $L^2(A)$ and $L^2(A^*)$.

It's fun to take the Pontryagin duals of specific groups, or specific classes of groups, and see what we get. We've already seen that the dual of S^1 is $\mathbb{Z}$, the dual of $\mathbb{Z}$ is S^1, and the dual of $\mathbb{R}$ is $\mathbb{R}$. More generally the dual of the n-dimensional torus is $\mathbb{Z}^n$, and vice versa, while the dual of $\mathbb{R}^n$ is isomorphic to $\mathbb{R}^n$. What can we glean from these examples?

Well, any discrete abelian group is an LCA group — a good example is $\mathbb{Z}^n$. So is any compact Hausdorff abelian group — a good example is the n-dimensional torus. And there's a nice general theorem saying that the dual of any group of the first kind is a group of the second kind, and vice versa!

In particular, if we have an abelian group that's both compact and discrete, its dual must be too. But the only abelian groups like this are the *finite* abelian groups — products of finite cyclic groups Z/n. So, this collection of groups is closed under Pontryagin duality!

In fact, it's easy to see that for any finite abelian group, A^* is isomorphic to A. But not canonically! To get a canonical isomorphism we need to take duals twice: for any LCA group, we get a canonical isomorphism between A and A^{**}. This should remind you of duality for finite-dimensional vector spaces — another famous collection of LCA groups that's closed under Pontryagin duality.

You can take any collection of LCA groups, stare at it through the looking-glass of Pontryagin duality, and see what it looks like. I've mentioned a few examples so far:

- A is compact iff A^* is discrete.

- A is finite iff A^* is finite.
- A is a finite-dimensional vector space iff A^* is a finite-dimensional vector space.

Here are some fancier ones:

- A is torsion-free and discrete iff A^* is connected and compact.
- A is compact and metrizable iff A^* is countable.
- A is a Lie group iff A^* has finite rank.
- A is metrizable iff A^* is σ-compact.
- A is second countable iff A^* is second countable.

If you know more snappy results like this, tell me! I'm collecting them — they're sort of addictive.

Because Pontryagin duality turns compact LCA groups into discrete ones — and vice versa — we can use it to turn some topology questions into algebra questions, and vice versa. After all, a discrete abelian group has no more structure than an "abstract" abelian group — one without a topology!

Sometimes this change of viewpoint helps, but sometimes it merely reveals how hard a problem really is.

For example, here's an innocent-sounding question: what are the compact path-connected LCA groups? The obvious example is the circle. More generally, we could take any product of circles — even an *infinite* product. Are there any others?

It turns out that this question cannot be settled by Zermelo-Fraenkel set theory together with the axiom of choice!

Here's why. An LCA group is compact and path-connected iff its dual is a "Whitehead group". What's that? It's an abelian group A such any short exact sequence of abelian groups like this splits:

$$0 \to \mathbb{Z} \to B \to A \to 0$$

where $\mathbb{Z}$ is the integers and B is any abelian group.

We call this sort of short exact sequence an "extension of A by $\mathbb{Z}$". So, if you want to show off your sophistication, you can say that A is a Whitehead group if "$\mathrm{Ext}(A, \mathbb{Z}) = 0$".

The obvious examples of Whitehead groups are free abelian groups. Indeed, these are precisely the guys whose Pontryagin dual is a product of circles! So the question is: are there any others? Or is every Whitehead group a free abelian group?

This is a famous old problem, called the Whitehead problem:

10) Wikipedia, "Whitehead problem", `http://en.wikipedia.org/wiki/Whitehead_problem`

In 1971, the logician Saharon Shelah showed the answer to this problem was undecidable using the axioms of ZFC! This was one of the first problems mathematicians really cared about that turned out to be undecidable.

If you want an easy introduction to Pontryagin duality and the structure of LCA groups, you can't beat this:

10) Sidney A. Morris, *Pontryagin Duality and the Structure of Locally Compact Abelian Groups*, Cambridge U. Press, Cambridge, 1977.

This classic treatment is still great, too:

14) Lev S. Pontrjagin, *Topological Groups*, Princeton University Press, Princeton, 1939.

To dig deeper, you need to read this — it's a real mine of information:

15) E. Hewitt and K. A. Ross, *Abstract Harmonic Analysis I*, Springer, Berlin, 1979.

This book has a lot of interesting newer results:

16) David L. Armacost, *The Structure of Locally Compact Abelian Groups*, Dekker, New York, 1981.

In particular, this is where I learned about path-connected LCA groups and the Whitehead problem.

I'd like to dedicate this issue of This Week's Finds to my father, Peter Baez, who died yesterday around midnight at the age of 87. His health had been failing for a long time, so this did not come as a shock. It's a curious coincidence that I was already writing an issue about minerals, since my dad majored in chemistry and returned to school for a master's in soil science after serving in the Army in World War II. After that he worked in the Blackfeet Nation in Browning Montana, riding around in a jeep, digging up soil samples, and testing them back at the lab for the Army Corps of Engineers. When he found "medicine wheels" — stone circles laid down by the native Americans for ritual reasons — he would report them to his friend the archeologist Tom Kehoe. Later he moved to California, became an editor for the Forest Service, and met my mother.

He got me interested in science at an early age because he was always taking me to museums, bringing me books from the public library, and so on. As a little kid, when I spilled something, he'd say "So you don't believe in the law of gravity?" He liked to joke around. Whenever I said an ungrammatical sentence, he'd tease me for it. "I'm not that hungry." "What do you mean? You're not *how* hungry?"

I learned a lot of math, physics and chemistry from his 1947 edition of the CRC Handbook of Chemistry and Physics — an edition so old that it listed "mesothorium" among the radioactive isotopes. He brought home the book "From Frege to Gdel" — a sourcebook in mathematical logic — because it was in the math section of the library and he misread "Gdel" as "Googol": he knew I liked large numbers! I didn't understand much of it, but it had a big effect on me.

I owe a lot to him.

Addenda: I thank Michael Barr, Kevin Buzzard and Mike Stay for some interesting comments. Mike Stay pointed out an interesting book on how humans may affect the future of mineral evolution:

17) Jan Zalasiewicz, *The Earth After Us: What Legacy Will Humans Leave in the Rocks?*, Oxford University Press, Oxford, 2009.

It's not 2009 yet, but the best books about the future are actually published in the future. Here's a quote:

> *The surface of the Earth is no place to preserve deep history. This is in spite of — and in large part because of — the many events that have taken place on it. The surface of the future Earth, one hundred million years now, will not have preserved evidence of contemporary human activity. One can be quite categorical about this. Whatever arrangement of oceans and continents, or whatever state of cool or warmth will exist then, the Earth's surface will have been wiped clean of human traces.*
>
> *Thus, one hundred million years from now, nothing will be left of our contemporary human empire at the Earth's surface. Our planet is too active, its surface too energetic, too abrasive, too corrosive, to allow even (say) the Egyptian Pyramids to exist for even a hundredth of that time. Leave a building carved out of solid diamond — were it even to be as big as the Ritz — exposed to the elements for that long and it would be worn away quite inexorably.*
>
> *So there will be no corroded cities amid the jungle that will, then, cover most of the land surface, no skyscraper remains akin to some future Angkor Wat for future archaeologists to pore over. Structures such as those might survive at the surface for thousands of years, but not for many millions.*

Kevin Buzzard had some interesting comments on Pontryagin duality in number theory:

> *I don't know any more general theorems of the form "G has X iff its dual has Y" but, since lecturing on Tate's thesis, I learnt some more nice examples of Pontrjagin duals.*
>
> *As you know well, number theorists like to complete fields with respect to norms. The rational numbers are too rich to understand completely, so we choose a norm on them, and then we complete with respect to that norm, and we get either the reals, or, a couple of thousand years later, the p-adics. Now a complete field is a much better gadget to have because there's a chance we'll be able to do analysis on this field. Indeed, for example, it's possible to set up a theory of Banach spaces etc for any complete field, and this isn't just for fun — e.g. Serre showed in the 1960s how to simplify some of Dwork's work on zeta functions of hypersurfaces using standard theorems of analysis of Banach spaces, applied to Banach spaces over the p-adic numbers, and there are oodles of other examples within number theory (leading up to an entire "p-adic Langlands programme" nowadays). But ideally, as well as continuity and differentiation etc, it's nice to be able to do integration as well, and for that you might need a measure, like Haar measure for example, and if you want to use Haar measure then you want your complete field to be locally compact too.*
>
> *Now, perhaps surprisingly, Weil (I think it was Weil; it might have been earlier though and perhaps I'm doing someone a disservice) managed to completely classify normed fields which were both complete and locally compact. They are: the reals and finite field extensions, the p-adic numbers and finite field*

extensions, and the fields $\mathbb{F}_p((t))$ and finite field extensions. (Given a complete normed field, there's a unique extension of the norm to any finite field extension and the extension is still complete — for the same sorts of reasons that there's only one vector space norm on Rˆn up to equivalence and it's complete.)

Tate in his thesis proves the following result (it's not too hard but it's crucial for him): if K is a complete locally compact normed field (considered as a locally compact abelian group under addition), then K is isomorphic to its own Pontrjagin dual. The isomorphism is non-canonical because you have to decide where 1 goes, but after you've made that decision there's a unique natural topological and algebraic isomorphism. So $\mathbb{R}$ and $\mathbb{C}$ are their own Pontrjagin duals — but the p-adic numbers are also self-dual in this way.

One of the reasons (perhaps historically the main reason) that number theorists are interested in complete fields is that given a more "global" object, like a number field (a finite extension of the rationals), one way of understanding it is by understanding its completions. Before I lectured my class on Tate's thesis, if someone had asked me to motivate the definition of the adeles, I would have said that to study a number field k it's easiest to think about it locally, so we complete with respect to a norm, but we can't choose a natural norm, so we choose all of them, and then we "multiply them all together" so we can get to them all at once. This is not really an ideal answer.

But here's a completely different way of motivating them. Classically, people interested in automorphic forms would typically consider functions on a Lie group G which were invariant, or transformed well, under a well-chosen discrete subgroup: for example one might want to consider smooth functions on $\mathbb{R}$ satisfying $f(x) = f(x+1)$ — a very interesting class of functions — or perhaps functions on $\mathrm{GL}_2(\mathbb{R})$ which are invariant under $\mathrm{GL}_2(\mathbb{Z})$ (and now you're well on the way to inventing/discovering the theory of modular forms). In fact $\mathbb{Z}$ lives in $\mathbb{R}$ very nicely:

$$0 \to \mathbb{Z} \to \mathbb{R} \to \mathbb{R}/\mathbb{Z} \to 0$$

and furthermore there's a bit of magic in this picture: it's self-dual with respect to Pontrjagin duality!

But $\mathbb{Z}$ is awkward. It's not a field. You really begin to see the awkwardness if you're Hecke in the 1930s trying to figure out what the correct notion of a Dirichlet character is when working with the integers not of $\mathbb{Q}$ but of a finite extension of $\mathbb{Q}$. The problem is that the integers in a general number field aren't a principal ideal domain so it's sometimes hard to get your hands on "local information": prime ideals aren't in general principal so you can't always evaluate a global object at one number *(analogous to the prime number p) to get local information.*

So let's try and fix this up. Let k be a number field. k is definitely a global object, like $\mathbb{Z}$, and it's also much easier to manage — it's a field rather than just a ring. The question is: what is the analogue of

$$0 \to \mathbb{Z} \to \mathbb{R} \to \mathbb{R}/\mathbb{Z} \to 0$$

if we replace $\mathbb{Z}$ by k, a number field? (Let's give k the discrete topology, because it has no natural topology other than this.) Even replacing $\mathbb{Z}$ by the rationals $\mathbb{Q}$ (with the discrete topology) is an interesting question! "$\mathbb{Z}$ is to $\mathbb{R}$ as $\mathbb{Q}$ is to... what?".

Well, Tate proposes the following: $\mathbb{Z}$ is to $\mathbb{R}$ as $\mathbb{Q}$ is to the adeles of $\mathbb{Q}$! More generally $\mathbb{Z}$ is to $\mathbb{R}$ as k (a number field) is to its adeles. And the argument he could use to justify this isn't number-theoretic at all, in some sense — it's coming entirely from Pontrjagin duality! Tate shows that the Pontrjagin dual of a number field k is A_k/k, where A_k is the adeles of k, and k is embedded diagonally! Now the analogue of the beautiful self-dual picture $0 \to \mathbb{Z} \to \mathbb{R} \to \mathbb{R}/\mathbb{Z} \to 0$ is going to be

$$0 \to k \to ??? \to A_k/k \to 0$$

and the natural candidate for $???$ is of course now the adeles A_k. These gadgets have appeared "magically" in some sense — the argument seems to me to be topological rather than arithmetic (although of course there is more than one choice for $???$ and perhaps the argument that the adeles are the right thing to put in the middle is number-theoretic).

Here's Tate's proof. Pontrjagin duality sends direct sums to direct products and vice-versa. So neither of them in particularly "symmetric" — both get changed. But Tate observes that restricted *direct products get sent to restricted direct products! Let k be a number field. Let k_v denote a typical completion of k (so if $k = \mathbb{Q}$ then k_v is either $\mathbb{R}$ or $\mathbb{Q}_p$). We know k_v is complete and it's easy to check it's locally compact (this doesn't use Weil's classification — it's the easy way around). So k_v is self-dual. So the restricted product of all the k_v (that is, the adeles), is locally compact, and dual to the restricted product of all the $(k_v)^*$, and $(k_v)^*$ is k_v again, so the adeles of k are self-dual!! Tate then checks that k (embedded diagonally in A_k) is discrete and equal to its own annihilator (if you choose all the local isomorphisms $k_v = (k_v)^*$ just right), and hence by the "Galois correspondence" between closed subgroups of G and closed subgroups of G^* he deduces that A_k/k is the dual of k. In particular*

$$0 \to k \to A_k \to A_k/k \to 0$$

looks like a very natural analogue of $0 \to \mathbb{Z} \to \mathbb{R} \to \mathbb{R}/\mathbb{Z} \to 0$; the quotient is compact, the sub is discrete, and the diagram is self-Pontjragin-dual.

Within about 10 years of Tate's thesis it's visibly clear in the literature that there has been a seismic shift: there seem to be as many people studying $G(\mathbb{Q})\backslash G(\text{adeles})$ as there are studying $G(\mathbb{Z})\backslash G(\mathbb{R})$ in the theory of automorphic forms, and the adelic approach has the advantage that, although less concrete, it has "truly local" components, thus motivating the representation theory of p-adic groups, the Langlands programme, and lots of other things.

Kevin

Michael Barr wrote:

> *Did you know that there is a $*$-autonomous category of topological abelian groups that includes all the LCA groups and whose duality extends that of Pontrjagin? The groups are characterized by the property that among all topological groups on the same underlying abelian group and with the same set of continuous homomorphisms to the circle, these have the finest topology. It is not obvious that such a finest exists, but it does and that is the key.*

He has a paper on this:

18) Michael Barr, "On duality of topological abelian groups", available at `ftp://ftp.math.mcgill.ca/pub/barr/pdffiles/abgp.pdf`

For more discussion, visit the n-Category Caf.

> People like us, who believe in physics, know that the distinction between past, present and future is only a stubbornly persistent illusion.
>
> — *Albert Einstein*

Week 274

March 12, 2009

Whew! It's been a long time since I wrote my last Week's Finds. I've been too busy. But luckily, I've been too busy writing papers about math and physics. So, let me talk about one of those.

First, the astronomy picture of the week:

1) NASA Photojournal, " 'Victoria Crater' at Meridiani Planum", `http://photojournal.jpl.nasa.gov/catalog/PIA08813`

This is the crater that NASA's rover called Opportunity has been exploring. It's 800 meters across. I like this picture just because it's beautiful. It was taken by the High Resolution Imaging Science Experiment on NASA's Mars Reconnaissance Orbiter.

Now, on to business! I want to talk about this paper, which took over 2 years to write:

2) John Baez, Aristide Baratin, Laurent Freidel and Derek Wise, Representations of 2-groups on infinite-dimensional 2-vector spaces, available as arXiv:0812.4969.

We can dream up the notion of "2-vector space" by pondering this analogy chart:

numbers	vector spaces
addition	direct sum
multiplication	tensor product

0	the 0-dimensional vector space
1	the 1-dimensional vector space

Just as you can add and multiply numbers, you can add and multiply vector spaces — but people call these operations "direct sum" and "tensor product", to make them sound more intimidating. These new operations satisfy axioms similar to the old ones. However, what used to be equations like this:

$$x + y = y + x$$

now become isomorphisms like this:

$$X + Y \cong Y + X.$$

This means we're "categorifying" the concepts of plus and times.

The unit for addition of vector spaces is the 0-dimensional vector space, and the unit for multiplication of vector spaces is the 1-dimensional vector space.

But here's the coolest part. Our chart is like a snake eating its own tail. The first entry of the first column matches the last entry of the second column! The set of all "numbers" is the same as "the 1-dimensional vector space". If by "numbers" we mean complex numbers, these are both just $\mathbb{C}$.

This suggests continuing the chart with a third column, like this:

numbers ($\mathbb{C}$)	vector spaces (Vect)	2-vector spaces 2Vect
addition	direct sum	direct sum
multiplication	tensor product	tensor product
0	$\mathbb{C}^0$	Vect^0
1	$\mathbb{C}^1$	Vect^1

Here $\mathbb{C}^0$ is short for the 0-dimensional vector space, while $\mathbb{C}^1$ is short for the 1-dimensional vector space — in other words the complex numbers, $\mathbb{C}$. Vect is the category of all vector spaces. So, whatever a "2-vector space" is, to make the chart nice we'd better have Vect be the 1-dimensional 2-vector space. We can emphasize this by calling it Vect^1.

In fact, about 15 years ago Kapranov and Voevodsky invented a theory of 2-vector spaces that makes all this stuff work:

3) Mikhail Kapranov and Vladimir Voevodsky, "2-categories and Zamolodchikov tetrahedra equations", in *Algebraic Groups and Their Generalizations: Quantum and Infinite-Dimensional Methods*, Proc. Sympos. Pure Math. **56**, Part 2, AMS, Providence, RI, 1994, pp. 177–259.

They mainly considered *finite-dimensional* 2-vector spaces. Every finite-dimensional vector space is secretly just $\mathbb{C}^n$, or at least something isomorphic to that. Similarly, every finite-dimensional 2-vector space is secretly just Vect^n, or at least something equivalent to that.

(You see, when we categorify once, equality becomes isomorphism. When we do it again, isomorphism becomes "equivalence".)

What's Vect^n, you ask? Well, what's $\mathbb{C}^n$? It's the set where an element is an n-tuple of numbers:

$$(x_1, \ldots, x_n)$$

So, Vect^n is the category where an object is an n-tuple of vector spaces:

$$(X_1, \ldots, X_n)$$

It's all pathetically straightforward. Of course we also need to know what's a morphism in Vect^n. What's a morphism from

$$(X_1, \ldots, X_n)$$

to

$$(Y_1, \ldots, Y_n)?$$

It's just the obvious thing: an n-tuple of linear operators

$$(f_1 \colon X_1 \to Y_1, \ldots, f_n \colon X_n \to Y_n)$$

And we compose these in the obvious way, namely "componentwise".

This may seem like an exercise in abstract nonsense, extending formal patterns just for the fun of it. But in fact, 2-vector spaces are all over the place once you start looking. For example, take the category of representations of a finite group, or the category of vector bundles over a finite set. These are finite-dimensional 2-vector spaces!

Here I can't resist a more sophisticated digression, just to impress you. The whole theory of Fourier transforms for finite abelian groups categorifies nicely, using these examples. Any finite abelian group G has "Pontryagin dual" G^* which is again a finite abelian group. I explained how this works back in "Week 273". The Fourier transform is a map from functions on G to functions on G^*. So, it's a map between vector spaces. But, lurking behind this is a map between 2-vector spaces! It's a map from representations of G to vector bundles over G^*.

You can safely ignore that last paragraph if you like. But if you want more details, try section 6.1 of this old paper:

4) John Baez, "Higher-dimensional algebra II: 2-Hilbert spaces", *Adv. Math.* **127** (1997), 125–189. Also available as `q-alg/9609018`.

As you can see from the title, I was trying to go beyond 2-vector spaces and think about "2-Hilbert spaces". That's because in quantum physics, we use Hilbert spaces to describe physical systems. Recent work on physics suggests that we categorify this idea and study 2-Hilbert spaces, 3-Hilbert spaces and so on — see "Week 58" for details. In the above paper I defined and studied finite-dimensional 2-Hilbert spaces. But a lot of the gnarly fun details of Hilbert space theory show up only for infinite-dimensional Hilbert spaces — and we should expect the same for 2-Hilbert spaces.

After these old papers on 2-vector spaces and 2-Hilbert spaces, various people came along and improved the whole story. For example:

5) Martin Neuchl, *Representation Theory of Hopf Categories*, Ph.D. dissertation, University of Munich, 1997. Chapter 2: "2-dimensional linear algebra". Available at `http://math.ucr.edu/home/baez/neuchl.ps`

6) Josep Elgueta, "A strict totally coordinatized version of Kapranov and Voevodsky 2-vector spaces", to appear in *Math. Proc. Cambridge Phil. Soc.* Also available as arXiv:math/0406475.

7) Bruce Bartlett, "The geometry of unitary 2-representations of finite groups and their 2-characters", available as arXiv/0807.1329.

In the last of these, Bruce worked out how finite-dimensional 2-Hilbert spaces arise naturally in certain topological quantum field theories!

Just as we can study representations of groups on vector spaces, we can study representations of "2-groups" on 2-vector spaces:

8) Magnus Forrester-Barker, *Representations of crossed modules and* cat^1*-groups*, Ph.D. thesis, Department of Mathematics, University of Wales, Bangor, 2004. Available at http://www.maths.bangor.ac.uk/research/ftp/theses/forrester-barker.pdf

9) John W. Barrett and Marco Mackaay, "Categorical representations of categorical groups", *Th. Appl. Cat.* **16** (2006), 529–557. Also available as arXiv:math/0407463.

10) Josep Elgueta, "Representation theory of 2-groups on finite dimensional 2-vector spaces", available as math.CT/0408120.

A group is a category with one object, all of whose morphisms are invertible. Similarly, a 2-group is a 2-category with one object, all of whose morphisms and 2-morphisms are invertible. Just as we can define "Lie groups" to be groups where the group operations are smooth, we can define "Lie 2-groups" to be 2-groups where all the 2-group operations are smooth. Lie groups are wonderful things, so we can hope Lie 2-groups will be interesting too. There are already lots of examples known. You can see a bunch here:

11) John Baez and Aaron Lauda, "Higher-dimensional algebra V: 2-groups", *Theory and Applications of Categories* **12** (2004), 423–491. Available at http://www.tac.mta.ca/tac/volumes/12/14/1$2$-14abs.html and as arXiv:math/0307200.

However, Barrett and Mackaay discovered something rather upsetting. While Lie groups have lots of interesting representations on finite-dimensional vector spaces, Lie 2-groups don't have many representations on finite-dimensional 2-vector spaces!

In fact, the problem already shows up for representations of plain old Lie *groups* on 2-vector spaces. A Lie group can be seen as a special sort of Lie 2-group, where the only 2-morphisms are identity morphisms.

The problem is that unlike a vector space, a 2-vector space has a unique basis — at least up to isomorphism. In $\mathbb{C}^n$ there's an obvious basis consisting of vectors like

$$(1,0,0,\ldots), \quad (0,1,0,\ldots), \quad (0,0,1,\ldots), \quad \ldots$$

and so on, but there are lots of other bases too. But in Vect^n the only basis goes like this:

$$(\mathbb{C}^1,\mathbb{C}^0,\mathbb{C}^0,\ldots), \quad (\mathbb{C}^0,\mathbb{C}^1,\mathbb{C}^0,\ldots), \quad (\mathbb{C}^0,\mathbb{C}^0,\mathbb{C}^1,\ldots), \quad \ldots$$

Well, I'm exaggerating slightly: we could replace $\mathbb{C}^1$ here by any other 1-dimensional vector space, and $\mathbb{C}^0$ by any other 0-dimensional vector space. That would give other bases — but they'd still be *isomorphic* to the basis shown above.

So, if we have a group acting on a finite-dimensional 2-vector space, it can't do much more than permute the basis elements. So, any representation of a group on a finite-dimensional 2-vector space gives an action of this group as permutations of a finite set!

That's okay for finite groups, since these can act in interesting ways as permutations of finite sets. But it's no good for Lie groups. Lie groups are usually infinite: they're manifolds. So, they have lots of actions on *manifolds*, but not many actions on finite sets.

This suggests that to study representations of Lie groups (or more general Lie 2-groups) on 2-vector spaces, we should invent some notion of "infinite-dimensional 2-vector space", where the basis can be not a finite set but an infinite set — indeed, something more like a manifold.

Luckily, such a concept was already lurking in the mathematical literature!

In the categorification game, it's always good when the concepts you invent shed light on existing issues in mathematics. And it's especially fun when you categorify a concept and get a concept that turns out to have been known — or at least partially known — under some other name. Then you're not just making up new stuff: you're seeing that existing math already had categorification built into it! This happens surprisingly often. That's why I take categorification so seriously.

The concept I'm talking about here is called a "field of Hilbert spaces". Roughly speaking, the idea is that you pick a set X, possibly infinite. X could be the real line, for example. Then a "field of Hilbert spaces" assigns to each point x in X a Hilbert space H_x.

As I've just described it, a measurable field of Hilbert spaces is an object in what we might call Hilb^X — a hairier, scarier relative of the tame little Vect^n that I've been talking about.

Let's think about how Hilb^X differs from Vect^n. First, the the finite number n has been replaced by an infinite set X. That's why Hilb^X deserves to be thought of as an *infinite-dimensional* 2-vector space.

Second, Vect has been replaced by Hilb — the category of Hilbert spaces. This suggests that Hilb^X is something more than a mere infinite-dimensional 2-vector space. It's closer to an infinite-dimensional 2-*Hilbert* space! So, we've departed somewhat from our original goal of inventing a notion of infinite-dimensional vector space. But that's okay, especially if we're interested in applications to quantum physics that involve analysis.

And here I must admit that I've left out some important details. When studying fields of Hilbert spaces, people usually bring in some analysis to keep the Hilbert space H_x from jumping around too wildly as x varies. They restrict attention to "measurable" fields of Hilbert spaces. To do this, they assume X is a "measurable space": a space with a sigma-algebra of subsets, like the Borel sets of the real line. Then they assume H_x depends in a measurable way on x.

The last assumption must be made precise. I won't do that here — you can see the details in our paper. But, here's an example of what I mean. Take X and partition it into countably many disjoint measurable subsets. For each one of these subsets, pick some Hilbert space H and let $H_x = H$ for points x in that subset. So, the dimension of the Hilbert space H_x can change as x moves around, but only in a "measurable way". In fact, every measurable field of Hilbert spaces is isomorphic to one of this form.

So, a measurable field of Hilbert spaces on X is like a vector bundle over X, except the fibers are Hilbert spaces and there's no smoothness or continuity — the dimension of the fiber can "jump" in a measurable way.

If you've studied algebraic geometry, this should remind you of a "coherent sheaf". That's another generalization of a vector bundle that allows the dimension of the fiber to jump — but in an algebraic way, rather than a measurable way. One reason algebraic geometers like categories of coherent sheaves is because they need a notion of infinite-dimensional 2-vector space. Similarly, one reason analysts like measurable fields of Hilbert spaces is because they want *their own* notion of infinite-dimensional 2-vector space. Of course, they don't know this — if you ask, they'll strenuously deny it.

We learned most of what we know about measurable fields of Hilbert spaces from this classic book:

12) Jacques Dixmier, *Von Neumann Algebras*, North-Holland, Amsterdam, 1981.

This book was also helpful:

13) William Arveson, *An Invitation to C^*-Algebra*, Chapter 2.2, Springer, Berlin, 1976.

As you might guess from the titles of these books, measurable fields of Hilbert spaces show up when we study representations of operator algebras that arise in quantum theory. For example, any commutative von Neumann algebra A is isomorphic to the algebra $L^\infty(X)$ for some measure space X, and every representation of A comes from a measurable field of Hilbert spaces on X.

The following treatment is less detailed, but it explains how measurable fields of Hilbert spaces show up in group representation theory:

14) George W. Mackey, *Unitary Group Representations in Physics, Probability and Number Theory*, Benjamin-Cummings, New York, 1978.

I'll say a lot more about this at the very end of this post, but here's a quick, rough summary. Any sufficiently nice topological group G has a "dual": a measure space G^* whose points are irreducible representations of G. You can build any representation of G from a measurable field of Hilbert spaces on G^* together with a measure on G^*. You build the representation by taking a "direct integral" of Hilbert spaces over G^*. This is a generalization of writing a representation as a direct sum of irreducible representation. Direct integrals generalize direct sums — just as integrals generalize sums!

By the way, Mackey calls measurable fields of Hilbert spaces "measurable Hilbert space bundles". Those who like vector bundles will enjoy his outlook.

But let's get back to our main theme: representations of 2-groups on infinite-dimensional 2-vector spaces.

We don't know the general definition of an infinite-dimensional 2-vector space. However, for any measurable space X, we can define measurable fields of Hilbert spaces on X. We can also define maps between them, so we get a category, called $\mathsf{Meas}(X)$. Crane and Yetter call these "measurable categories".

I believe someday we'll see that measurable categories are a halfway house between infinite-dimensional 2-vector spaces and infinite-dimensional 2-Hilbert spaces. In fact, when we move up to n-vector spaces, it seems there could be $n + 1$ different levels of "Hilbertness".

The conclusions of our paper include a proposed definition of 2-Hilbert space that can handle the infinite-dimensional case. So, why work with measurable categories? One reason is that they're they're well understood, thanks in part to the work of Dixmier — but also thanks to Crane and Yetter:

15) David Yetter, "Measurable categories", *Appl. Cat. Str.* **13** (2005), 469–500. Also available as arXiv:math/0309185.

16) Louis Crane and David N. Yetter, "Measurable categories and 2-groups", *Appl. Cat. Str.* **13** (2005), 501–516. Also available as arXiv:math/0305176.

The paper by Crane and Yetter studies representations of discrete 2-groups on measurable categories. Our paper pushes forward by studying representations of *topological* 2-groups, including Lie 2-groups. Topology really matters for infinite-dimensional representations. For example, it's a hopeless task to classify the infinite-dimensional unitary representations of even a little group like the circle, $\mathrm{U}(1)$. But it's easy to classify the *continuous* unitary representations.

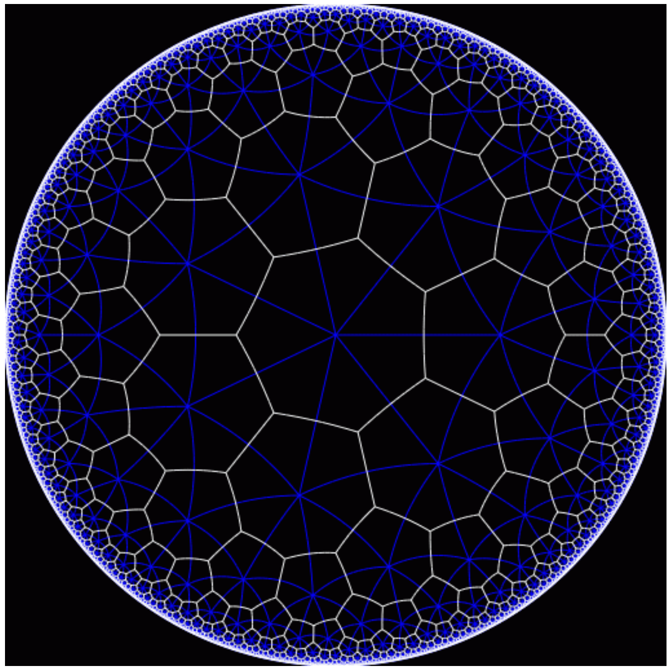
A group has a category of representations, but a 2-group has a 2-category of representations! So, as usual, we have representations and maps between these , which physicists call "intertwining operators" or "intertwiners" for short. But we also have maps between intertwining operators, called "2-intertwiners".

This is what's really exciting about 2-group representation theory. Indeed, intertwiners between 2-group representations resemble group representations in many ways — a fact noticed by Elgueta. It turns out one can define direct sums and tensor products not only for 2-group representations, but also for intertwiners! One can also define "irreducibility" and "indecomposability", not just for representations, but also for intertwiners.

Our paper gives nice geometrical descriptions of these notions. Some of these can be seen as generalizing the following paper of Crane and Sheppeard:

17) Louis Crane and Marnie D. Sheppeard, "2-categorical Poincare representations and state sum applications", available as arXiv:math/0306440.

Crane and Sheppeard studied the 2-category of representations of the "Poincare 2-group". It turns out that we can get representations of the Poincare 2-group from discrete subgroups of the Lorentz group. Since the Lorentz group acts as symmetries of the hyperbolic plane, these subgroups come from symmetrical patterns like these:



18) Don Hatch, "Hyperbolic planar tesselations", `http://www.plunk.org/~hatch/HyperbolicTesselations/`

But Crane and Sheppeard weren't just interested in beautiful geometry. They developed their example as part of an attempt to build new "spin foam models" in 4 dimensions. I've talked about such models on and off for many years here. The models I've discussed were usually based on representations of groups or quantum groups. Now we can build models using 2-groups, taking advantage of the fact that we have not just representations and intertwiners, but also 2-intertwiners. You can think of these models as discretized path integrals for gauge theories with a "gauge 2-group". To compute the path integral you take a 4-manifold, triangulate it, and label the edges by representations, the triangles by intertwiners, and the tetrahedra by 2-intertwiners. Then you compute a number for each 4-simplex, multiply all these numbers together, and sum the result over labellings.

Baratin and Freidel have done a lot of interesting computations in the Crane-Sheppeard model. I hope they publish their results sometime soon.

To wrap up, I'd like to make a few technical remarks about group representation theory and measurable fields of Hilbert spaces. In "Week 272" I talked about a class of measurable spaces called standard Borel spaces. Their definition was frighteningly general: any measurable space X whose measurable subsets are the Borel sets for some complete separable metric on X is called a "standard Borel space". But then I described a theorem saying these are all either countable or isomorphic to the real line! They are, in short, the "nice" measurable spaces — the ones we should content ourselves with studying.

In our work on 2-group representations, we always assume our measurable spaces are standard Borel spaces. We need this to get things done. But standard Borel spaces also show up ordinary group representation theory. Let me explain how!

To keep your eyes from glazing over, I'll write "rep" to mean a strongly continuous unitary representation of a topological group on a separable Hilbert space. And, I'll call an irreducible one of these guys an "irrep".

Mackey wanted to build all the reps of a topological group G starting from irreps. This will only work if G is nice. Since Haar measure is a crucial tool, he assumed G was locally compact and Hausdorff. Since he wanted $L^2(G)$ to be separable, he also assumed G was second countable.

For a group with all these properties — called an "lcsc group" by specialists wearing white lab coats and big horn-rimmed glasses — Mackey was able to construct a measure space G^* called the "unitary dual" of G.

The idea is simple: the points of G^* are isomorphism classes of irreps of G. But let's think about some special cases....

When G is a finite group, G^* is a finite set.

When G is abelian group, not necessarily finite, G^* is again an abelian group, called the "Pontryagin dual" of G. I talked about this a lot in "Week 273".

When G is both finite and abelian, so of course is G^*.

But the tricky case is the general case, where G can be infinite and nonabelian! Here Mackey described a procedure which is a grand generalization of writing a rep as a direct sum of irreps.

If we choose a sigma-finite measure μ on G^* and a measurable field H_x of Hilbert spaces on G^*, we can build a rep of G. Here's how. Each point x of G^* gives an irrep of G, say R_x. These form another measurable field of Hilbert spaces on G^*. So, we can tensor H_x and R_x, and then form the "direct integral"

$$\int_x (H_x \otimes R_x) d\mu(x)$$

As I already mentioned, a direct integral is a generalization of a direct sum. The result of doing this direct integral is a Hilbert space, and in this case it's a rep of G. The Hilbert spaces H_x specify the "multiplicity" of each irrep R_x in the representation we are building.

The big question is whether we get *all* the reps of G this way.

And the amazing answer, due to James Glimm, is: yes, *if G^* is a standard Borel space!*

In this case we say G is "type I". People know lots of examples. For example, an lcsc group will be type I if it's compact, or abelian, or a connected real algebraic group, or a connected nilpotent Lie group. That covers a lot of ground. However, there are plenty of groups, even Lie groups, that aren't type I. The representation theory of these is more tricky!

If you want to know more, either read Mackey's book listed above, or this summary:

19) George W. Mackey, "Infinite-dimensional group representations", *Bull. Amer. Math. Soc.* **69** (1963), 628–686. Available from Project Euclid at `http://projecteuclid.org/euclid.bams/1183525453`

The most fascinating thing about algebra and geometry is the way they struggle to help each other to emerge from the chaos of non-being, from those dark depths of subconscious where all roots of intellectual creativity reside.

— *Yuri Manin*

Week 275

June 14, 2009

Long time no see! I've been really busy, but now classes are over, and like last summer, I'm visiting Paul-Andr Mellis, who works on logic, computer science and categories.

I should be finishing up some more papers, but let me take a little break, and tell you about an old dream that's starting to come true. People are finally starting to understand extended topological quantum field theories using n-categories!

Back in 1995, Jim Dolan and I argued that n-dimensional extended TQFTS were representations of a certain n-category called $n\mathrm{Cob}$ in which:

- objects are 0-dimensional manifolds: that is, collections of points,
- morphisms are 1-dimensional manifolds with boundary going between collections of points,
- 2-morphisms are 2-dimensional manifolds with corners going between 1-dimensional manifolds with boundary going between collections of points,

... and so on, up to dimension n. In particular, any n-dimensional manifolds is an n-morphism in $n\mathrm{Cob}$.

And, we thought we could glimpse a purely algebraic description of $n\mathrm{Cob}$. We called this the "cobordism hypothesis", and we explained it here:

1) John Baez and James Dolan, "Higher-dimensional algebra and topological quantum field theory", *J. Math. Phys.* **36** (1995), 6073–6105. Also available as q-alg/9503002.

I talked about this back in "Week 49". For more, try these talks:

2) Eugenia Cheng, "n-Categories with duals and TQFT", 4 lectures at the Fields Institute, January 2007. Audio available at http://www.fields.utoronto.ca/audio/06-07/#crs-ncategories and lecture notes by Chris Brav at http://math.ucr.edu/home/baez/fields/eugenia.pdf

Now Jacob Lurie has come out with a draft of an expository paper that outlines a massive program, developed with help from Mike Hopkins, to reformulate the cobordism hypothesis using more ideas from homotopy theory, and prove it:

3) Jacob Lurie, "On the classification of topological field theories", available as arXiv:0905.0465.

He's running around giving talks about this work, and you can see some here:

4) Jacob Lurie, "TQFT and the cobordism hypothesis", four lectures at the Geometry Research Group of the University of Texas at Austin, January 2009. Videos available at http://lab54.ma.utexas.edu:8080/video/lurie.html and lecture notes by Braxton Collier, Parker Lowrey and Michael Williams at http://www.ma.utexas.edu/users/plowrey/dev/rtg/notes/perspectives_TQFT_notes.html

Excited by this new progress, I decided to run around giving some talks about it myself — just to explain the basic intuitions to people who'd never thought about this stuff before. You can see my slides here:

5) John Baez, "Categorification and topology", available at `http://math.ucr.edu/home/baez/cat/`

A key feature of Lurie's approach is that instead of using n-categories he uses (∞, n)-categories, which are ∞-categories where everything is invertible above dimension n. This is what gets ideas from homotopy theory into the game. I should talk about this more someday.

Meanwhile, Chris Schommer-Pries has written a thesis on 2d extended TQFTs which follows an approach much closer to what Jim and I had originally imagined. You could say he gives more of an individually hand-crafted treatment of the $n = 2$ case, as compared with Lurie's high-tech industrial approach that clobbers all n at once:

6) Chris Schommer-Pries, *The Classification of Two-Dimensional Extended Topological Field Theories*, Ph.D. theis, U.C. Berkeley, 2009. Available at `http://sites.google.com/site/chrisschommerpriesmath/`

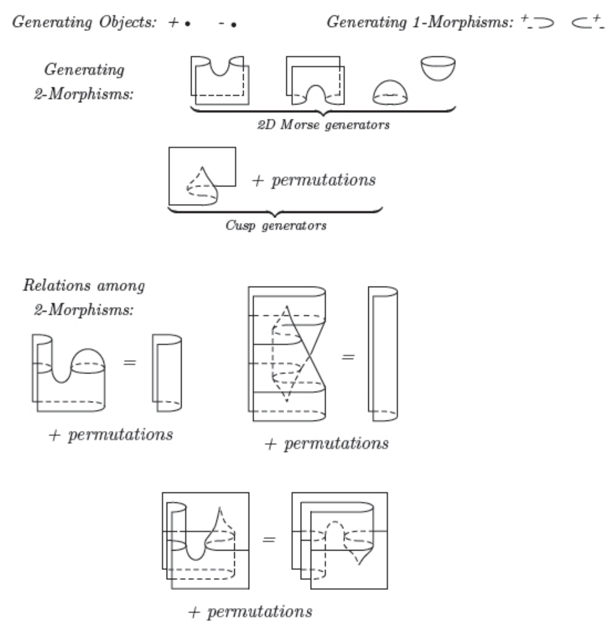
Instead of (∞, n)-categories, Schommer-Pries just uses n-categories — and since he's doing 2d TQFTs, that means 2-categories. Or more precisely, "weak" 2-categories, where all the laws hold only up to equivalence. Like most people, he calls these "bicategories". And one of the charms of his thesis is that he gives a detailed treatment of the $n = 2$ column of the periodic table of n-categories — which in his language looks like this:

Table 15: k-tuply monoidal n-categories

	$n = 0$	$n = 1$	$n = 2$
$k = 0$	sets	categories	2-categories
$k = 1$	monoids	monoidal categories	monoidal 2-categories
$k = 2$	commutative monoids	braided monoidal categories	braided monoidal 2-categories
$k = 3$	" "	symmetric monoidal categories	sylleptic monoidal 2-categories
$k = 4$	" "	" "	symmetric monoidal 2-categories
$k = 5$	" "	" "	" "

A k-tuply monoidal n-category is an $(n + k)$-category that's boring at the bottom k levels. For example, a category with just one object is a monoid. As we increase k, we get more and more commutative flavors of n-category. But after k hits $n + 2$, we expect that increasing k further has no effect. At this point we say our n-category is "stable".

If the cobordism hypothesis is true, nCob is a stable n-category. For $n = 2$, such a gadget is often called a "symmetric monoidal bicategory". Schommer-Pries shows that 2Cob is indeed a symmetric monoidal bicategory. Even better, he gives a "generators and relations" description of this gadget, which is just the sort of thing we need for the 2d version of the cobordism hypothesis:



At this point, any n-category theorist could finish off the job.

(Well, the really nice statement of the cobordism hypothesis involves *framed* oriented cobordisms, and we may need a topologist to tell us how those work — but there's also a version of the hypothesis for plain old oriented cobordisms, and that's what Schommer-Pries' thesis will give.)

For more on nCob as an n-category, try this:

7) Eugenia Cheng and Nick Gurski, "Toward an n-category of cobordisms", *Theory and Applications of Categories* **18** (2007), 274–302. Available at `http://www.tac.mta.ca/tac/volumes/18/10/18-10abs.html`

I should add that a lot of the 2-category theory in Schommer-Pries' thesis relies on a thesis by a student of Ross Street:

8) Paddy McCrudden, "Balanced coalgebroids", *Theory and Applications of Categories* **7** (2000), 71–147. Available at `http://www.tac.mta.ca/tac/volumes/7/n6/7-06abs.html`

Two students of mine should read the stuff about symmetric monoidal bicategories in this thesis! One is Alex Hoffnung, whose work on Hecke algebras uses the symmetric monoidal bicategory where:

- objects are groupoids,
- morphisms are spans of groupoids where the legs are fibrations,
- 2-morphisms are maps of spans of groupoids.

The other is Mike Stay, whose work on computer science uses the symmetric monoidal bicategory where:

- objects are categories,
- morphisms are profunctors,
- 2-morphisms are natural transformations between profunctors.

A profunctor is a categorified version of a matrix. More precisely, a profunctor from $\mathcal{C}$ to $\mathcal{D}$ is a functor

$$F\colon \mathcal{C} \times \mathcal{D}^{\mathrm{op}} \to \mathsf{Set}$$

so it's like a matrix of sets. A span of groupoids where the legs are fibrations is also a categorified version of a matrix, since by a theorem of Grothendieck we can reinterpret it as a weak 2-functor

$$F\colon \mathcal{C} \times \mathcal{D}^{\mathrm{op}} \to \mathsf{Gpd}$$

where now $\mathcal{C}$ and $\mathcal{D}$ are groupoids. So, both these students are studying aspects of "categorified matrix mechanics"... and we need symmetric monoidal bicategories to provide the proper context for such work. This should connect up to the 2d version of the cobordism hypothesis in some interesting ways.

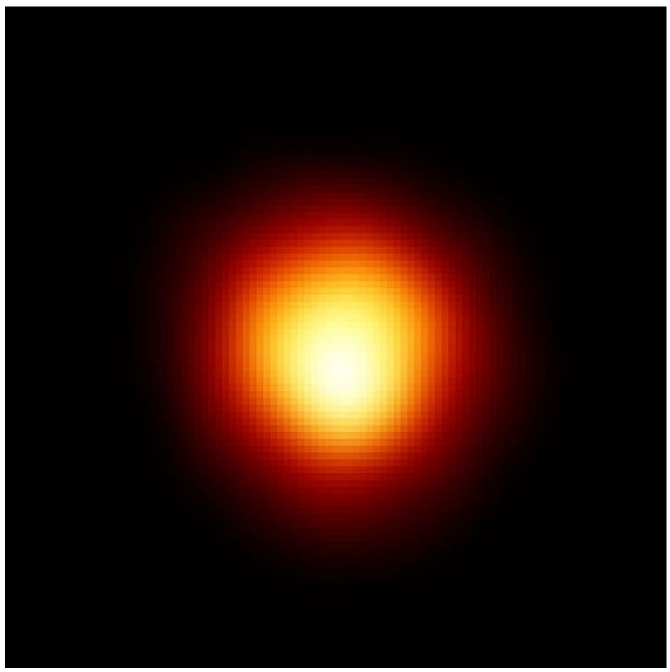
> As for your problems... I am so tired of mathematics and hold it in such low regard, that I could no longer take the trouble to solve them myself.
>
> — *Descartes to Mersenne*

Week 276

June 20, 2009

Math is eternal, but I'll start with some news that may be time-sensitive. Betelgeuse is shrinking!

1) Stefan Scherer, "Shrinking Betelgeuse", http://backreaction.blogspot.com/2009/06/shrinking-betelgeuse.html



Betelgeuse is that big red star in the shoulder of Orion. It's a red supergiant, one of the largest stars known. It's only 20 times the mass of the Sun, but it's about 1000 times as big across — about 5 times the size of the Earth's orbit. For more of a sense of what that means, watch this video. If you've got kids, have them watch it too:

2) Hansie0Slim, 'Largest stars this side of the Milky Way', http://www.youtube.com/watch?v=u70UBs7BWY8

But, it's shrinking. These authors claim its radius has shrunk 15% since 1993:

3) C. H. Townes, E. H. Wishnow, D. D. S. Hale and B. Walp, "A systematic change with time in the size of Betelgeuse", *The Astrophysical Journal Letters* **697** (2009), L127–L128.

That's about 1000 kilometers per hour!

Of course, it's a bit tricky to estimate the size of Betelgeuse — besides being rather far, it's so diffuse that its surface isn't very precisely defined. And it's a variable star, so maybe a little shrinkage isn't a big deal. But the two known cycles governing its oscillations have periods of one year and 6 years. So, the authors of the above paper think this longer-term shrinkage has some other cause.

It could be just another cycle, with a longer period. But there's another possibility that's a lot more exciting. Maybe Betelgeuse is about to collapse and go supernova!

Indeed this seems likely in the long term, since that's the usual fate of such massive stars. And the long term may not even be so long, since Betelgeuse is about 8.5 million years old — quite old for stars this big, which live fast and go out in a blaze of glory.

What if Betelgeuse went supernova? How would it affect the US economy, and the next Presidential election? Could this be the Republican party's best hope?

Sorry, I'm being a bit parochial... let me try that again. How would it affect the insignificant inhabitants of a puny speck called Earth, located about 500 or 600 light years away from Betelgeuse? According to Brad Schaefer at Louisiana State University, it would be "brighter than a million full moons", but it wouldn't hurt us — in part because of the distance, and in part because we're not lined up with its pole.

(Perhaps just to build up the suspense, Schaefer added that Betelgeuse could already have gone supernova, in which case we're just waiting for its light to reach us.)

Actually, I have trouble believing that Betelgeuse gone supernova would be brighter than a million full moons. First of all, the full moon is $1/449,000$ times as bright as the Sun. So, "brighter than a million full moons" is just an obscure way of saying "more than twice as bright as the Sun."

Second, let's try the calculation ourselves. There are various kinds of supernovae, with different luminosities. I guess Betelgeuse is most likely to become a type II supernova. Such supernovae show quite a bit of variation in their behavior. But anyway, it seems they get to be 1 billion times as bright as the Sun, or maybe at most — let's look at a worst-case scenario — 10 billion times as bright. So, between 10^9 and ten times that.

On the other hand, Betelgeuse is about 600 light years away, and there are 63,239 astronomical units in a light year, so it's about

$$600 \times 63,000 \approx 4 \times 10^7$$

times as far away as the Sun — no point trying to be too precise here. Brightness scales as one over distance squared, so supernova Betelgeuse should look between

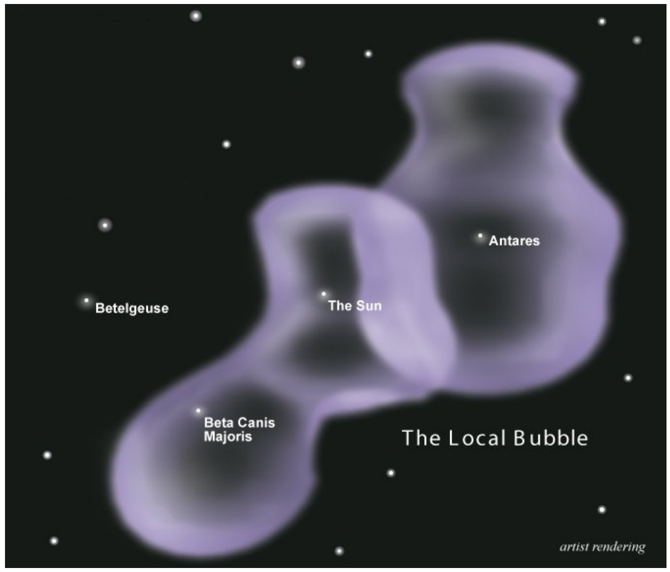
$$\frac{10^9}{(4 \times 10^7)^2} \approx 7 \times 10^{-7}$$

as bright as the Sun, and ten times that bright.

As I mentioned, the full Moon is about 2×10^{-6} times as bright as the Sun. So, supernova Betelgeuse should be roughly between $1/3$ as bright as the full Moon, and 3 times as bright. This is a rough calculation, but I've done it a few different ways and gotten similar answers. So, it's safe to say that "brighter than a million full moons" is a vast exaggeration.

Whew.

It's worth recalling that not too long ago, a supernova exploded at a roughly comparable distance from us, forming the "Local Bubble" — a peanut-shaped region of hot thin gas about 300 light years across, containing our Sun. The gas in the Local Bubble is about 1000 times less dense than ordinary interstellar space, and vastly hotter.



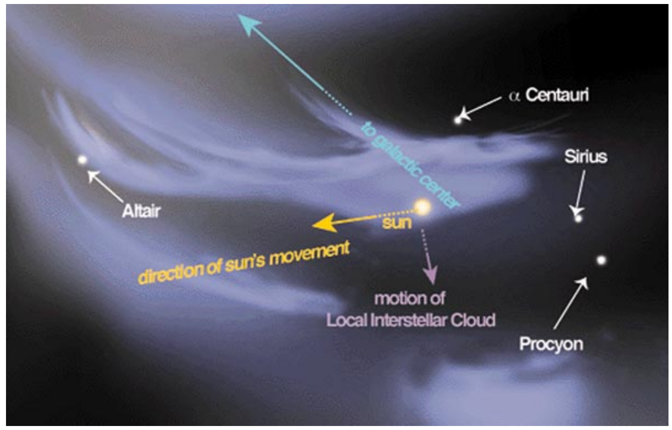
What do I mean by "not too long ago"? Well, nobody is sure, but back in "Week 144" I reported a bunch of evidence for a theory that the Local Bubble was formed just 340,000 years ago, when a star called Geminga went supernova, perhaps 180 light years away.

Now I'm getting a sense that the situation is more complex. It seems our Sun is near the boundary of the Local Bubble and another one, called the "Loop I Bubble". This other bubble seems to have formed earlier — perhaps 2 million years ago, at the Pliocene-Pleistocene transition, when a bunch of ultraviolet-sensitive marine creatures mysteriously died:

4) NASA, "Near-earth supernovas", `http://science.nasa.gov/headlines/y2003/06jan_bubble.htm`

The Loop I Bubble may have been caused by a supernova in "Sco-Cen", a cloud in the directions of Scorpius and Centaurus. It's about 450 light years away now, but it used to be considerably closer.

In the last few million years, some wisps of interstellar gas have drifted into the Local Bubble. Our solar system is immersed in one of these filaments, charmingly dubbed the "local fluff". It's much cooler than the hot gas of the Local Bubble: 7000 Kelvin instead of roughly 1 million. It's also much denser — about 0.1 atoms per cubic centimeter instead of 0.05 or so. But Sco-Cen is sending interstellar cloudlets in our direction that are denser still, by a factor of 100. These might actually have some effect on the Sun's magnetic field when they reach us.



Map of the local interstellar medium within 10 years of the Sun, prepared by Linda Huff and Priscilla Frisch.

I'm sure we'll get a clearer story as time goes by. In 2003, NASA launched a satellite called the Cosmic Hot Interstellar Plasma Spectrometer, or CHIPS for short, to study this sort of thing:

5) NASA/UC Berkeley, "Overview of CHIPS Science", `http://chips.ssl.berkeley.edu/science.html`

It sounds pretty interesting. Unfortunately the latest news on the CHIPS homepage dates back to 2005, before they'd done much science. What's up?

You can't do much about Betelgeuse. But you can do something about mathematics! For example, if you're into categories or n-categories, you can help out the nLab:

6) nLab, `http://ncatlab.org/nlab`

The nLab is like the library, or laboratory, in the back room of the n-Category Caf. The nCaf is a place to chat: it's a blog. The nLab is a place to work: it's a wiki. It's been operating since November 2008. There's quite a lot there by now, but it's really just getting started.

Check it out! You'll find explanations of many concepts, which you may be able to improve, and the beginnings of some big projects, which you may want to join.

So far the main contributors include Urs Schreiber, Mike Shulman, Toby Bartels, Tim Porter, Ronnie Brown, Todd Trimble, David Roberts, Andrew Stacey, Bruce Bartlett, Zoran koda, Eric Forgy and myself. Jim Dolan recently joined in with a page on algebraic geometry for category theorists — I'll say more about this someday. And like the nCaf, technical aspects of the nLab are largely run by Jacques Distler — it uses some wiki software called Instiki which he is helping develop.

Finally, a bit of actual math. Here's a paper by the fellow I'm working with here in Paris, and a grad student of his:

7) Paul-Andr Mllis and Nicolas Tabareau, "Free models of T-algebraic theories computed as Kan extensions", available at `http://hal.archives-ouvertes.fr/hal-00339331/fr/`

I really need to understand this for my work with Mike Stay.

In "Week 200" I talked about Lawvere's work on algebraic theories; I'll assume you read that and pick up from there. In its narrowest sense, an "algebraic theory" is a category with finite products where every object is a product of copies of some fixed object c. We use algebraic theories to describe various types of mathematical gadgets: to be precise, any type of gadget that consists of a set with a bunch of n-ary operations satisfying a bunch of purely equational laws.

For any type of gadget like this, there's an algebraic theory $\mathcal{C}$; I explained how you get this back in "Week 200". If we have a functor

$$F\colon \mathcal{C} \to \mathsf{Set}$$

that preserves finite products, then $F(c)$ becomes a specific gadget of the given type. Conversely, any specific gadget of the given type determines a functor like this.

So, we define a "model" of the theory $\mathcal{C}$ to be a functor

$$F\colon \mathcal{C} \to \mathsf{Set}$$

that preserves finite products. But actually, this is just a model of $\mathcal{C}$ in the world of sets! We could replace Set by any other category with finite products, say X, and define a "model of the theory $\mathcal{C}$ in the environment X" to be a functor

$$F\colon \mathcal{C} \to X$$

that preserves finite products.

For example, if $\mathcal{C}$ is the theory of groups and X is Set, a model of $\mathcal{C}$ in X is a group. If instead X is the category of topological spaces, a model of $\mathcal{C}$ in X is a topological group. And so on. In general people call a model of this particular theory $\mathcal{C}$ in any old X a "group object in X".

But as you might fear, we want to understand more than a single model of $\mathcal{C}$ in X. As category theorists, we want to understand the whole *category* of models of $\mathcal{C}$ in X. This category, which I'll call $\mathsf{Mod}(\mathcal{C}, X)$, has:

- functors $F\colon \mathcal{C} \to X$ that preserve finite products as its objects;
- natural transformations between these as its morphisms.

For example, if $\mathcal{C}$ is the theory of groups and X is the category of topological spaces, $\mathsf{Mod}(\mathcal{C}, X)$ is the category of topological groups and continuous homomorphisms.

So far I've just been reviewing at a fast pace. What happens next? Well, there's always a forgetful functor

$$R\colon \mathsf{Mod}(\mathcal{C}, X) \to X$$

sending any model to its underlying object in X. But what we'd really like is for R to have a left adjoint

$$L\colon X \to \mathsf{Mod}(\mathcal{C}, X)$$

sending any object of X to the free gadget on that object. Then we could follow L by R to get a functor

$$RL\colon X \to X$$

called a "monad". One reason this would be great is that monads are another popular way to study algebraic gadgets. I explained monads very generally back in "Week 89", and said how to get them from adjoint functors in "Week 92"; in "Week 257" I gave some links to some great videos by the Catsters explaining monads and what they're good for. So, I won't say more about monads now: I'll just assume you love them. Given this, you must be dying to know when the functor R has a left adjoint.

In fact it does whenever X has colimits that distribute over the finite products! For example, it does when $X = \mathsf{Set}$. And Mllis and Tabareau give a very nice modern explanation of this fact before generalizing the heck out of it.

The key is to note that

$$R\colon \mathsf{Mod}(\mathcal{C}, X) \to X$$

is just an extreme case of forgetting *some* of the structure on an algebraic gadget: namely, forgetting *all* of it. More generally, suppose we have any map of algebraic theories

$$Q\colon \mathcal{B} \to \mathcal{C}$$

that is, a finite-product-preserving functor that sends the special object b in $\mathcal{B}$ to the special object c in $\mathcal{C}$. Then composition with Q gives a functor

$$Q^*\colon \mathsf{Mod}(\mathcal{C}, X) \to \mathsf{Mod}(\mathcal{B}, X)$$

For example, if $\mathcal{B}$ is the theory of groups and $\mathcal{C}$ is the theory of rings, $\mathcal{C}$ is "bigger", so we get an inclusion

$$Q\colon \mathcal{B} \to \mathcal{C}$$

and then Q^* is the functor that takes a ring object in X and forgets some of its structure, leaving us a group object in X. But when $\mathcal{B}$ is is the most boring algebraic theory in the world, the "theory of a bare object", then Q^* forgets everything: it's our forgetful functor

$$R\colon \mathsf{Mod}(\mathcal{C}, X) \to \mathsf{Mod}(\mathcal{B}, X) = X$$

So, we should ask quite generally when any functor like

$$Q^*\colon \mathsf{Mod}(\mathcal{C}, X) \to \mathsf{Mod}(\mathcal{B}, X)$$

has a left adjoint. And, the answer is: it always does!

The proof uses a left Kan extension followed by what Mllis and Tabaraeu call a "miracle" — see page 5 of their paper. And, it's this miracle they want to understand and generalize.

Here's the basic idea. If we write $\mathsf{Hom}(\mathcal{C}, X)$ for the category with

- arbitrary functors $F\colon \mathcal{C} \to X$ as its objects;
- natural transformations between these as its morphisms

then composition with Q gives a functor

$$\mathsf{Hom}(\mathcal{C}, X) \to \mathsf{Hom}(\mathcal{B}, X)$$

and this has a left adjoint

$$\mathsf{Hom}(\mathcal{B}, X) \to \mathsf{Hom}(\mathcal{C}, X)$$

using a well-known trick called "Kan extension", or more precisely "left Kan extension". Since $\mathsf{Mod}(\mathcal{B}, X)$ is included in $\mathsf{Hom}(\mathcal{B}, X)$, we can compose this inclusion with the functor above:

$$\mathsf{Mod}(\mathcal{B}, X) \to \mathsf{Hom}(\mathcal{B}, X) \to \mathsf{Hom}(\mathcal{C}, X)$$

And now comes the miracle: this composite functor actually lands us in $\mathsf{Mod}(\mathcal{C}, X)$, which sits inside $\mathsf{Hom}(\mathcal{C}, X)$. This gives us a functor

$$\mathsf{Mod}(\mathcal{B}, X) \to \mathsf{Mod}(\mathcal{C}, X)$$

which turns out to be what we wanted: the left adjoint of

$$Q^*\colon \mathsf{Mod}(\mathcal{C}, X) \to \mathsf{Mod}(\mathcal{B}, X).$$

Kan extensions are a very general concept, so the hard part is understanding and generalizing this miracle.

To do this Mllis and Tabareau first generalize algebraic theories to "T-algebraic theories" where T is any pseudomonad on Cat. I already said that monads are a trick for studying very general algebraic gadgets. Similarly, pseudomonads are a trick for studying very general *categorified* algebraic gadgets, like "categories with finite products" or "monoidal categories" or "braided monoidal categories" or "symmetric monoidal categories".

Each of these types of categories allows us to define a type of "theory":

- monoidal categories let us define "PROs"
- braided monoidal categories let us define "PROBs"
- symmetric monoidal categories let us define "PROPs"
- categories with finite products let us define "algebraic theories"

I explained all these, along with monads, here:

8) John Baez, "Universal algebra and diagrammatic reasoning", available as `http://math.ucr.edu/home/baez/universal/`

Take my word for it: they're great. In particular, they're more "quantum" in flavor than algebraic theories. So, we would like to generalize Lawvere's original results to these other kinds of theories, which are all examples of "T-algebraic theories". But, it's not automatic! For example, it doesn't always work with PROPs.

A typical kind of algebraic gadget we could define with a PROP is a "bialgebra". While there's always a free group on a set, there's not usually a free bialgebra on a vector space! The problem is not the category of vector spaces: it's that bialgebras have not only "operations" like multiplication, but also "co-operations" like comultiplication.

So, Mllis and Tabareau have their work cut out for them. But they tackle it very elegantly, using profunctors and a certain generalization thereof: Richard Wood's concept of "proarrow equipment". This lets them generalize the "miracle" to any situation where we have a little T-algebraic theory sitting inside a bigger one

$$Q\colon \mathcal{B} \to \mathcal{C}$$

and the bigger one only has extra operations, not co-operations.

"Proarrow equipment" sounds pretty scary — it's taken me about a decade to overcome my fear of it. So I'll stop my summary here, right around page 12 of the paper — right when the fun is just getting started!

Addenda: Charles McElwain kindly responded to my plea for information about what would happen if Betelgeuse went supernova. He wrote:

> *As you mention, there's not a lot of (quality) work out there. Most of what I found briefly would score fairly high on the "crank index".*
>
> *Of course, near supernovas have a positive and essential role in life on earth, in there* being *an earth, rather than just a star. . .*
>
> *A few that I found that weren't immediately eliminated as cranks, that might repay further investigation:*
>
> 9) *Michael Richmond, "Will a nearby supernova endanger life on Earth?". Available at* `http://stupendous.rit.edu/richmond/answers/snrisks.txt`
>
> *Perhaps the closest to the number-crunching you're looking for.*
>
> 10) *S. E Thorsett, "Terrestrial implications of cosmological gamma-ray burst models",* Astrophys. J. ***444*** *(1995), L53. Also available as* `astro-ph/9501019`.
>
> *Specifically, nitric oxide increases/ozone decreases.*
>
> 11) *Steven I. Dutch, "Life (briefly) near a supernova",* Journal of Geoscience Education*, 2005. Available at* `http://nagt.org/files/nagt/jge/abstracts/Dutch_v53n1.pdf`
>
> *The conceit here is what would happen if the Sun went supernova; acknowledged as impossible, but a very interesting exercise almost as a "Fermi problem", spinning out the real implications of the classic Arthur C. Clarke story "Rescue Party", and interesting also pedagogically.*

Using this and other information, I checked that if Betelguese went supernova, it would not be anywhere nearly as bright as "a million full moons".

You can see one of my calculations above.

Andrew Platzer looked into what happened to CHIPS, the satellite that was supposed to study hot gas in the Local Bubble. And, he found a fascinating newspaper article about this satellite's quixotic, sad, but ultimately rather mysterious quest. Andrew wrote:

> *I am interested in space and I did a little bit of googling about the CHIP satellite. Turns out it was turned off about a year ago after a 5 year mission. Unfortunately, it never detected the UV signal of the local bubble according to the article. There's a full story in local California newspaper:*

12) *Chris Thompson, "Goodbye Mr. CHIPS",* East Bay Express*, July 2, 2008. Also available at* `http://www.eastbayexpress.com/ebx/PrintFriendly?oid=780923`

And a couple of papers in the arxiv by M. Hurwitz referencing CHIPS; the more recent one is about spectra of comets:

13) *M. Hurwitz, T. P. Sasseen and M. M. Sirk, "Observations of diffuse EUV emission with the Cosmic Hot Interstellar Plasma Spectrometer (CHIPS)",* Astrophys. J. ***623*** *(2005), 911–916. Also available as* `astro-ph/0411121`

14) *T. P. Sasseen, M. Hurwitz et al, "A search for EUV emission from comets with the Cosmic Hot Interstellar Plasma Spectrometer (CHIPS)",* Astrophys. J. ***650*** *(2006), 461–469. Also available as* `astro-ph/0606466`.

The null result seems interesting since a signal was expected.

Still up there. TLE from NORAD:

```
CHIPSAT
1 27643U 03002B 09177.47579469 .00000388 00000-0 34685-4 0 1094
2 27643 94.0213 313.0310 0014359 84.1512 276.1301 14.97271142352353
```

Andrew Platzer

"TLE" refers to the "two-line element" format for transmitting satellite locations.

The short version of the CHIPS story — which completely leaves out all the fascinating twists and turns you'll find in that newspaper article above — is that this satellite failed to detect the extreme ultraviolet radiation (EUV) that people expected from the hot gas of the Local Bubble. It doesn't seem like a malfunction. So, something we don't understand is going on!

Todd Trimble gave a snappy proof that the forgetful functor from bialgebras to vector spaces has no left adjoint. If it did, it would need to preserve limits. In particular, it would send the terminal bialgebra to the terminal vector space. But the terminal bialgebra is 1-dimensional, while the terminal vector space is 0-dimensional — a contradiction.

For more discussion visit the n-Category Caf.

> The question you raise, "how can such a formulation lead to computations?" doesn't bother me in the least! Throughout my whole life as a mathematician, the possibility of making explicit, elegant computations has always come out by itself, as a byproduct of a thorough conceptual understanding of what was going on. Thus I never bothered about whether what would come out would be suitable for this or that, but just tried to understand — and it always turned out that understanding was all that mattered.
>
> — *Grothendieck*

Week 277

August 10, 2009

I'm very happy to have finished writing a huge paper with Aaron Lauda... a paper we've been working on for 5 years! It's a chronology of 20th century math and physics, focused on why people are starting to use n-categories in physics:

1) John Baez and Aaron Lauda, "A prehistory of n-categorical physics", to appear in *Deep Beauty: Mathematical Innovation and the Search for an Underlying Intelligibility of the Quantum World*, edited by Hans Halvorson. Available at `http://math.ucr.edu/home/baez/history.pdf`

This is a companion to a paper I talked about in "Week 261":

2) John Baez and Mike Stay, "Physics, topology, logic and computation: a Rosetta Stone", to appear in *New Structures in Physics*, edited by Bob Coecke. Available as `arXiv:0903.0340`.

Both of them are supposed to be gentle explanations of how "diagrammatic thinking" unifies and clarifies our perspective on many subjects. I can imagine merging them and expanding them to form a book someday... but not today. Today I want to take a break from n-categories, and talk about some basic physics.

Let's start with a riddle.

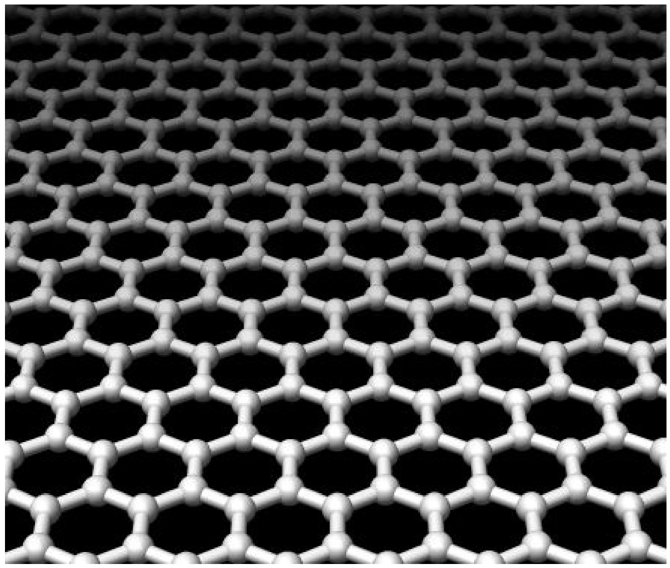
What's a million times thinner than paper, stronger than diamond, a better conductor than copper, and absorbs exactly

$$\pi\alpha \sim \frac{3.14159}{137.035} \sim 2.29254$$

of the light you shine through it?

Hint: α is the "fine structure constant" — a fundamental dimensionless constant that specifies the strength of the electromagnetic force.

Can't guess? Okay, here's the answer: graphene!



Graphene is what you get when you take one slice of a crystal of graphite. It's a hexagonal array of carbon atoms, each connected to three neighbors. I mentioned this substance back in "Week 262", when I was visiting the National University of Singapore, because researchers there were working on graphene as a possible substitute for silicon chips, which might operate 1000 times faster. Now a group there led by Barbaros zyilmaz is trying to use graphene for storing information:

3) Prachi Patel, "A step towards superfast carbon memory", *Technology Review*, Wednesday April 1, 2009, available at `http://www.technologyreview.com/computing/22377/`.

No, this is not an April Fool's joke.

Graphene was only discovered in 2004, but the easiest way to make some is surprisingly lowtech. You press a chunk of graphite onto some scotch tape, and hope a thin layer sticks:

4) "Making graphene 101", zyilmaz' group, YouTube video, available at `http://www.youtube.com/watch?v=rphiCdR68TE`

Graphene has some amazing properties. For starters, it acts like a toy universe containing massless spin-$1/2$ particles, which zip around at the speed of light. But in this toy universe, light moves 300 times slower than in the real universe!

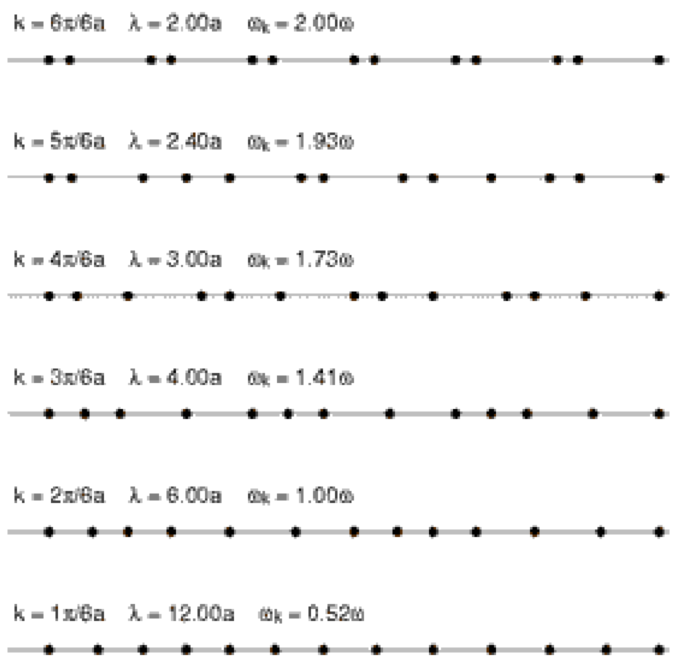
To understand this, you have to start by realizing that any sort of wave also acts like a particle when you take quantum mechanics into account. The idea is that when something can wiggle back and forth at some frequency, it can wiggle a little or a lot — but it can't wiggle just *any* amount. The amount of wiggling is "quantized" — it comes in discrete steps. These steps are too small to see in normal life, where for example you might see a rubber band seem to vibrate with any amplitude it wants. But these quantized steps are there nonetheless, no matter what is doing the wiggling — it appears to be a completely general principle.

When the wiggles are waves that are moving along, these quantized steps are called “particles”. So, for example, when we have a wave of light of some particular frequency that contains 500 times more energy than the bare minimum, we say it consists of 500 “particles” called photons. At first this may sound weird, but it turns out that many things we normally consider particles — like electrons and protons and neutrons — really are just quantized wiggles of some sort or another.

In cases like these, the stuff that’s doing the wiggling is rather absract: for photons, we say it’s the “electromagnetic field”, and for other particles we say it’s various other fields.

But since the principle is completely general, there are also cases where the stuff doing the wiggling is quite mundane. For example, if you have a crystal, the crystal’s atoms can wiggle. This is of course how *sound* propagates. But this should mean that sound comes in quantized packets called “phonons”. And indeed, experiments with crystals show that this is true!

5) Wikipedia, “Phonon”, `http://en.wikipedia.org/wiki/Phonon`



Different vibrational modes of the crystal have different numbers of wiggles per *distance*, and different numbers of wiggles per *time*; these give phonons with different *momentum* and *energy*. The relation is simple:

$$\text{momentum} = \hbar(\text{wiggles per distance})$$
$$\text{energy} = \hbar(\text{wiggles per time})$$

where $\hbar$ is Planck's constant, a tiny little constant.

(Why am I talking about sound in crystals, rather than air? It's because our story applies in its simplest form to vibrational modes that don't interact much with other modes or get damped out quickly by friction. These give rise to particles that don't interact much with other particles, and don't decay very fast — so they're easy to see.)

There are other things about a crystal that can vibrate besides the atoms. For example, the atoms may have an angular momentum, or "spin", that can change directions. Often each atom's spin affects its neighbors through magnetic forces. This allows waves of changed spin - "spin waves" — to propagate through the crystal. And again, these waves come in quantized packets called "magnons":

6) Wikipedia, "Spin wave", `http://en.wikipedia.org/wiki/Spin_wave`

A crystal can also have a "hole": an atom with a missing electron These holes can move around, so they too act like particles. This example is perhaps a bit more obvious than the previous ones... so here I should probably emphasize that just as sound waves or spin waves come in discrete particle-like units, these holes act like waves!

Similarly, a crystal can have atom with an *extra* electron. Here it's even *more* obvious that these electrons act like particles — you probably want to just say they *are* particles. But at this point it's crucial to emphasize that this example is really like all the rest. In particular (pardon the pun), we need to understand these extra electrons in terms of waves, and we need to compute their energy and momentum using these formulas:

$$\text{momentum} = \hbar(\text{wiggles per distance})$$
$$\text{energy} = \hbar(\text{wiggles per time})$$

so the relation between the energy and momentum depends completely on the details of the crystal: you can't just use some formula you may happen to know about a "free" electron in empty space. So, electrons in crystals can do all sorts of crazy things that you'd never expect, and indeed this is what makes things like conductors and transistors possible.

Let's talk about relations between energy and momentum. For a particle of mass m in empty space, special relativity says that

$$\text{energy}^2 = \text{momentum}^2 + \text{mass}^2$$

in units where the speed of light is 1. If we work in units where the speed of light is some number c, the formula looks messier:

$$\text{energy}^2 = \text{momentum}^2 c^2 + \text{mass}^2 c^4$$

If you look at a particle at rest, its momentum is zero so a little calculation reveals that:

$$\text{energy} = \text{mass} c^2$$

which is a pretty famous formula.

But if we go to the other extreme, and look at "massless" particle, meaning one with mass $= 0$, we get

$$\text{energy} = \text{momentum} \cdot c$$

which is also famous, but less so. If we apply the formulas

$$\begin{aligned} \text{momentum} &= \hbar(\text{wiggles per distance}) \\ \text{energy} &= \hbar(\text{wiggles per time}) \end{aligned}$$

we get

$$\text{wiggles per time} = c(\text{wiggles per distance})$$

This fact is easy to believe if we imagine light as a wave moving along, and c as the speed of this wave. Indeed, this fact was important in helping guess the relation between momentum and "wiggles per distance", and energy and "wiggles per time", back when they were first inventing quantum mechanics.

Now, the above formulas apply to photons in empty space. But my real point is this: if we have any sort of substance and any sort of wave that can move around in this substance, obeying these formulas, it *acts like a massless particle*. So, we can use a lot of the same math and intuition that we use for photons. But, quite likely the value of c will be different than the real-world speed of light!

And this in fact is what happens for graphene. The calculation is pathetically simple by ordinary physics standards, yet complicated enough that I don't feel like presenting the details here. You can see it near the beginning of this wonderful paper:

7) Jiannis K. Pachos, "Manifestations of topological effects in graphene", to appear in *Contemp. Phys.*. Also available as arXiv:0812.1116.

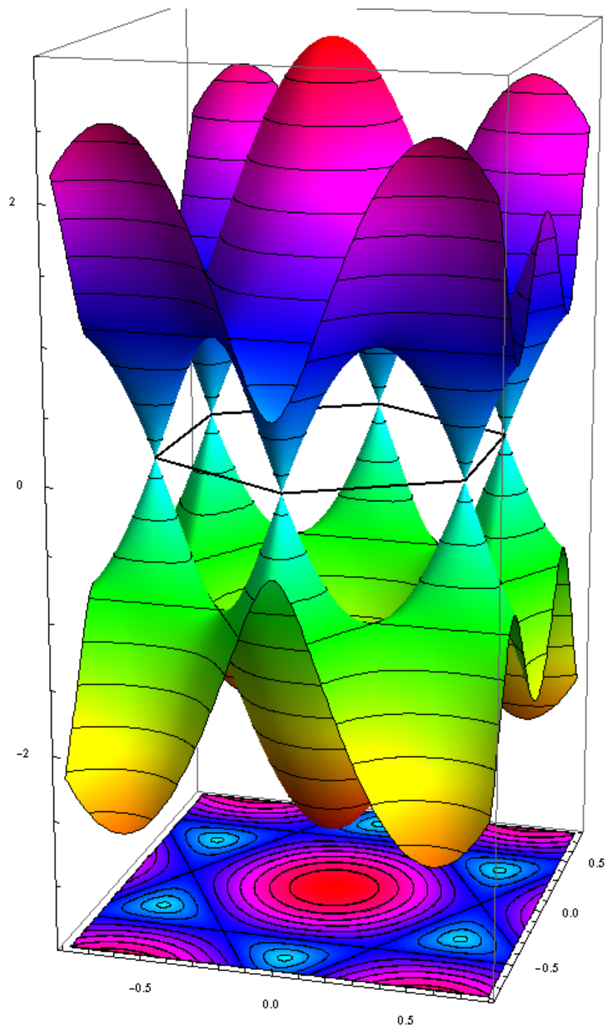
You start with a really simplified model: a planar hexagonal honeycomb that can either have or lack an electron at each vertex. You ignore the spin of the electron for some reason. It's a bit surprising you can get away with that! You assume the energy is a bit less whenever you've got electrons at both ends of any given edge of your hexagonal honeycomb.

It helps to color the vertices of the honeycomb alternately black and white. Then there are two kinds of electrons: "black" and "white" ones. The formula for the energy can be expressed nicely in terms of these, because it's a sum over pairs of neighboring vertices, one black and one white. Note that whenever an electron moves, it changes from black to white or vice versa.

Then you do a little calculating: you do a Fourier transform here, you diagonalize a 2×2 matrix there, and… presto!

There turn out to be two different kinds of waves, each consisting of a certain linear combination of black and white electrons. The energy of these waves (or if you prefer, their number of wiggles per time) is related to their momentum (or wiggles per distance) by a complicated formula.

To write down this formula, we have to treat momentum as a *vector*, since the energy of a wave depends on which direction it's going: the hexagonal honeycomb means that not all direction behave the same. If you plot the energy E as a function of the momentum vector $p = (p_x, p_y)$, you get a graph like this:

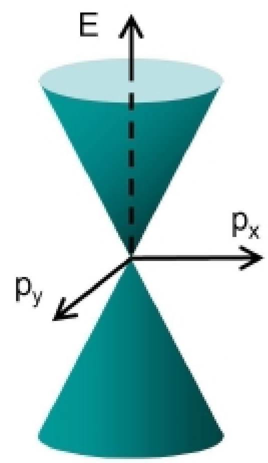


It's pretty fancy! But you can see it has hexagonal symmetry, as you'd expect. And it's also periodic. This comes from the periodicity of the crystal lattice — but in sneaky way: the crystal lattice in actual space gives rise to a "dual lattice" in the space of momentum vectors, and the energy stays the same if you take the momentum and add any vector in this dual lattice. This is no big deal, it happens for any crystal:

8) Wikipedia, "Reciprocal lattice", `http://en.wikipedia.org/wiki/Reciprocal_lattice`

In crystal jargon, the original crystal lattice is called the "Bravais lattice" and its dual is called the "reciprocal lattice".

The big deal is that if you look carefully at certain points in the graph, it looks almost like a cone.



If we move the tip of this cone to the origin, the formula for it is:

$$E^2 = (p_x^2 + p_y^2)c^2$$

or in other words:

$$\text{energy}^2 = \text{momentum}^2 c^2$$

for some constant c. And this is just the relation we had for a massless particle!

A digression: when I wrote this relation before, I took a square root and got

$$\text{energy} = \text{momentum} \cdot c$$

But the above graph includes negative energies as well. What do the negative energies mean, physically? That's an interesting puzzle, which people had to confront when they were first trying to combine special relativity and quantum mechanics. I don't want to talk about it here, but if you're curious, see this:

9) M. I. Katnelson, K. S. Novoselov, A. K. Geim, "Chiral tunnelling and the Klein paradox in graphene", *Nature Physics* **2** (2006), 620.

Anyway, the upshot is that near one of these special points, graphene acts like it has two kinds of massless particles in it: "black" and "white". They are approximately described by the usual equation for massless spin-$1/2$ particles, the Dirac equation — but in a universe with just 2 dimensions of space! The speed c is about $1/300$th of the real-world speed of light. Also, a spin-$1/2$ particle in 2d space usually comes in two states: it can rotate clockwise or counterclockwise. But here those two states are certain linear combinations of "black" and "white".

Furthermore, because the formula

$$\text{energy}^2 = \text{momentum}^2 c^2$$

remains unchanged when you rescale both distances and times by the same factor, graphene reacts exactly the same way to light of different wavelengths — at least within some range where all the approximations we've made are good. And, it turns out to absorb $\pi\alpha$ of the light you shine through it:

10) A. B. Kuzmenko, E. van Heumen, F. Carbone, and D. van der Marel, "Universal infrared conductance of graphite", *Phys. Rev. Lett.* **100** (2008), 117401.

11) R. R. Nair, P. Blake, A. N. Grigorenko, K. S. Novoselov, T. J. Booth, T. Stauber, N. M. R. Peres, and A. K. Geim, A. K., "Fine structure constant defines visual transparency of graphene", *Science* **320** (2008), 1308. Also available at `http://onnes.ph.man.ac.uk/nano/Publications/Science_2008fsc.pdf`

So, graphene is an amazing physical system, but so far I've just scratched the surface. Graphene also has "topological defects", and "anyons", and there are wonderful applications of Euler's theorem and its big brother, the Atiyah-Singer index theorem. All this in a physical system you can make by pressing graphite against a piece of tape!

I should say more about all this, but I need to go to bed and then catch an early morning plane. So, I'll quit here — but I highly recomend Pachos' paper "Manifestations of topological effects in graphene".

Addenda: For more discussion visit the n-Category Caf.

Error and doubt no longer encumber us with mist;
We are now admitted to the banquets of the Gods;
We may deal with laws of heaven above; and we now have
The secret keys to unlock the obscure earth.

— *Halley*

Week 278

August 29, 2009

Mathematicians love to use groups to describe symmetries. Symmetries like to be symmetries *of* something, so groups like to act as symmetries of sets, vector spaces and other mathematical gadgets. Of all these choices, it's possible that mathematicians love *vector spaces* most of all — so there's an enormous subject called "group representation theory" devoted to studying how groups can act as symmetries of vector spaces.

Why vector spaces and not, say, sets? Maybe because mathematicians, despite whatever sophistication they may have obtained through years of schooling, are still most comfortable with numbers. We like to say "math is not just about numbers", which is true, but when nobody is looking many of us will secretly add, subtract, multiply or divide a few — and it's very satisfying.

A vector space is a bunch of vectors, and a vector can be seen as a list of numbers. A symmetry of a vector space is a linear transformation, and a linear transformation can be seen as a box full of numbers. So, group representation theory has the charm of combining beautiful sophisticated ideas with something mathematicians know how to handle: *lots of numbers*.

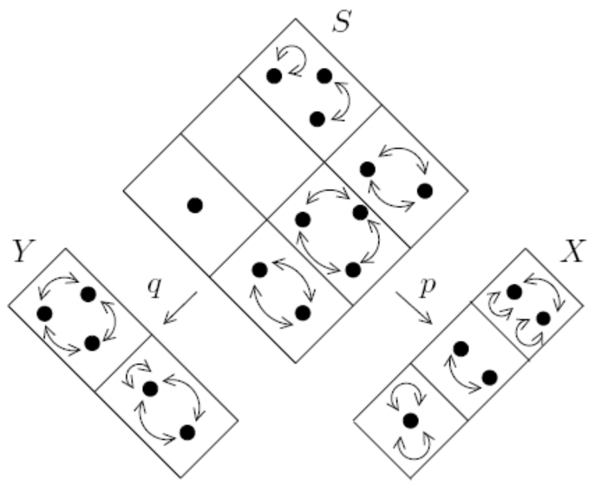
This is great, because it means when you get stuck and don't understand something in group representation theory, you can just grind away - adding, multiplying, subtracting and dividing — until you get a good idea.

But at a deeper level, it's a bit weird that of all the structures that groups can act on, we spend so much time studying their actions on vector spaces. The reason is obvious: it's amazingly useful. But what's a bit mysterious to me is *why* it's so amazingly useful!

One way to get a new perspective on this is "groupoidification". Groupoidification is a program for taking familiar math and redoing it with groupoids replacing vector spaces. Since a group can be seen as a special case of a groupoid, this puts a new spin on group representation theory. Instead of having groups in one corner and vector spaces in the other, now it's all about groupoids!

This sheds a whole new light on quantum mechanics. Early in the history of quantum mechanics, back when Heisenberg was still a grad student, he developed "matrix mechanics". In this idea, any physical process is described by a box of numbers. The number in the ith column and jth row says the amplitude for the system to hop from its ith state to its jth state.

Later it was realized that these matrices described linear operators. But with groupoidification, we replace these matrices by "spans of groupoids", which look like this:



These are even more primitive than boxes of numbers!

I've been thinking about this for a long time. Luckily, today I don't need to, because Alex Hoffnung, Christopher Walker and I just finished a big paper on groupoidification and put it on the arXiv:

1) John Baez, Alexander Hoffnung and Christopher Walker, "Higher-dimensional algebra VII: groupoidification", available at arxiv:0908.4305.

You may remember the Tale of Groupoidification, which started in "Week 247", went on for several weeks, and then morphed into a series of videotaped lectures. You can find all that stuff here:

2) John Baez, "The tale of groupoidification", available at `http://math.ucr.edu/home/baez/groupoidification`

Our new paper "HDA7" crystallizes some of this tale into theorems, but there's a lot more left. That's why Alex and I are writing "HDA8" on the groupoidification of Hecke algebras, and Christopher and I will — I hope — write "HDA9" on the groupoidification of quantum groups (or more precisely, Hall algebras).

But today I'm tired of writing about math, so this Week's Finds will mainly be about astrophysics, with a little bit about geophysics and a tiny bit about graphene. If you want to understand my cryptic comments about spans of groupoids and how matrix mechanics gets a new look after groupoidification, just read the introduction to HDA7.

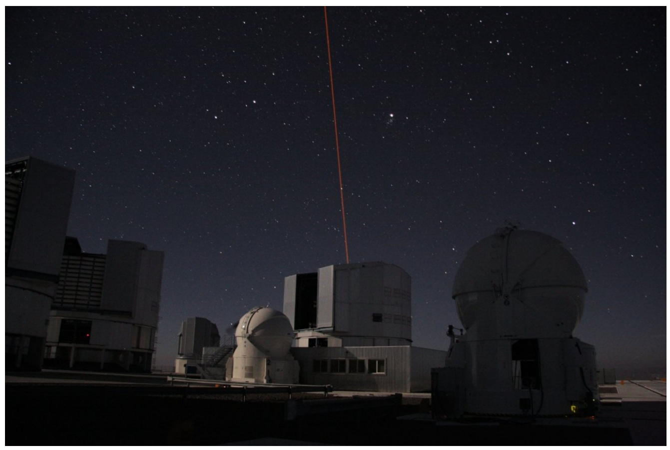
In "Week 277" I said the star Betelgeuse may be shrinking rapidly — shrinking at a rate of 1000 kilometers per hour, according to one group of astrophysicists. This is exciting because Betelgeuse is a red supergiant, a big and shortlived type of star that's stopped burning hydrogen and is busy burning heavier elements. When it runs out of fuel, its core will collapse and then explode into a supernova, briefly becoming between 1 and 10 billion times as luminous as the Sun. From here it will look about as bright as the moon! Then its core will become a neutron star or black hole.

This could happen soon, or it could take a million years: no one knows.

Now two other teams have taken photos of Betelgeuse using the VLT — the "Very Large Telescope". And they've found something quite dramatic.

I showed you photos of the VLT back in "Week 226". It's actually a bunch of telescopes that can function as a single unit, built in the high Atacama Desert of northern Chile to minimize atmospheric effects — you know, that "twinkle twinkle little star" stuff.

The VLT also uses "adaptive optics" to counteract the effect of twinkling. For this, they shoot a laser beam into the sky:



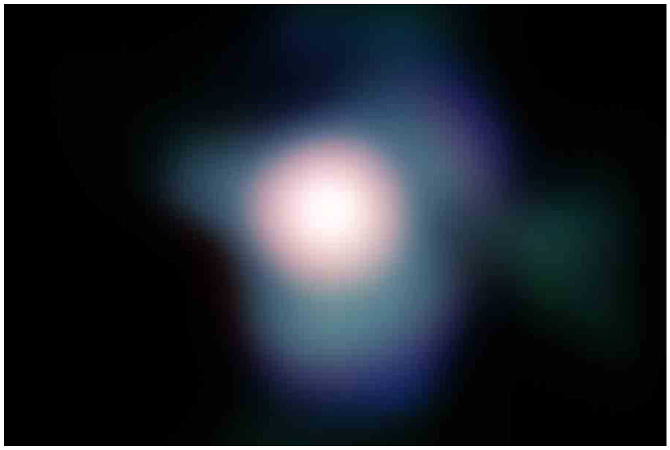
3) "Free from the atmosphere", European Space Observatory (ESO), `http://www.eso.org/public/outreach/press-rel/pr-2007/pr-27-07.html`

The beam excites sodium ions 100 kilometers up — ions created by tiny meteorites that are constantly hitting the atmosphere and vaporizing. Then the ions glow and create an "artificial star". The astronomers down below can watch the twinkling of this "star" and use the data to correct for the effect of atmospheric distortion.

Thanks to tricks like this, the VLT can take pictures with a resolution of 37 milliarcseconds — about the size of a tennis ball on the International Space Station, as seen from the ground!

This is how a team of astronomers led by Pierre Kervella discovered that Betelgeuse is spewing out a huge plume of gas that extends out 6 times the diameter of the star —

in other words, the distance between the Sun and Neptune!



That's mighty impressive. Could it mean that Betelgeuse is about to go supernova? Nobody knows.

Another team has detected carbon monoxide coming out of Betelgeuse. This ties into another favoite subject of mine: dust!

In "Week 257" I talked about the dust spewed out by "asymptotic giant branch stars". Our Sun will become one of these someday — as will indeed most stars between 0.6 and 8 solar masses, in a late stage of their life. Right before they completely run out of fuel they can burn, these stars turn red, expand, and become unstable, periodically puffing out dust like smokestacks.

"Red supergiants" like Betelgeuse are different. Betelgeuse is about 20 times as massive as our Sun. Stars this big are rare, but they pump out a lot more dust. A well-studied example is α Herculis, known to some early astronomers as Ras Algethi. It's sort of small for a red supergiant, only 14 times the mass of our Sun. Some people classify it as a "bright giant" instead. It's part of a triple star system. It emits a stellar wind blowing outwards at about 10 kilometers per second, and it loses at least 6×10^{19} tons of mass each year. That's about 1% of the Earth's mass every year! Most of this stuff is hydrogen and helium, but about 0.5% of it condenses to form dust as it cools.

This dust includes organic compounds like polycyclic aromatic hydrocarbons, or PAHs. I said a lot about these in "Week 257". It also includes a lot of calcium-aluminum silicates and magnesium silicates: predecessors of some of the most common minerals here on Earth!

And indeed, that's how we got here: when a red supergiant finally goes supernova, all the dust surrounding it gets blown outwards by the shock wave, mixed in with really heavy elements formed by the supernova core collapse itself. This dust floats around. . . and eventually some of it forms clouds that can block starlight, cool down, collapse under their own gravity, and form solar systems like ours.

So, I like thinking about Betelgeuse as part of a rich galactic life cycle that also includes us.

For more details, see:

4) "Sharpest views of star Betelgeuse reveal how supergiant stars lose mass", *Sci-*

enceDaily, August 3, 2009, http://www.sciencedaily.com/releases/2009/07/090729074525.htm

5) "Betelgeuse resolved", Astronomy Picture of the Day, August 5, 2009, http://apod.nasa.gov/apod/ap090805.html

6) Pierre Kervella et al, "The close circumstellar environment of Betelgeuse. Adaptive optics spectro-imaging in the near-IR with VLT/NACO", *Astronomy and Astrophysics*, 2009. Also available as arXiv:0907.1843.

7) Keiichi Ohnaka et al, "Spatially resolving the inhomogeneous structure of the dynamical atmosphere of Betelgeuse with VLTI/AMBER", *Astronomy and Astrophysics*, 2009. Also available as arXiv:0906.4792.

8) Jacco Th. van Loon, "The effects of red supergiant mass loss on supernova ejecta and the circumburst medium", in *Hot and Cool: Bridging Gaps in Massive Star Evolution*, Pasadena, November 2008. Also available as arXiv:0906.4855.

Speaking of dust, there's a great blog entry by Stefan Scherer about dust in a planetary system 95 light years away — silica dust that seems to have been created by a high-speed collision of planets only a few thousand years ago!

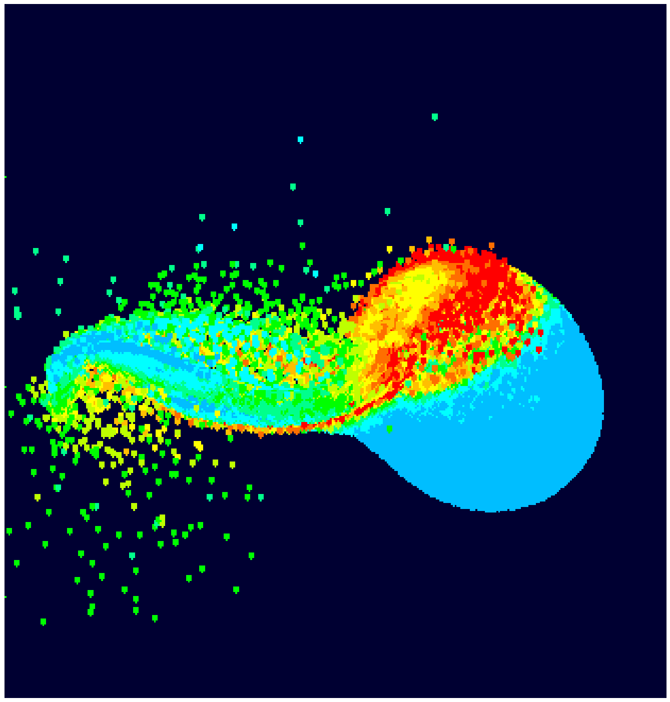
9) Stefan Scherer, "News from other worlds", *Backreaction*, http://backreaction.blogspot.com/2009/08/news-from-other-worlds.html

10) C. M. Lisse et al., "Abundant circumstellar silica dust and SiO gas created by a giant hypervelocity collision in the ˜12 Myr HD172555 System", *Astrophys. J.* **701** (2009), 2019–2032. Also available as arXiv:0906.2536.

If you have some pent-up aggression and feel the need for catharsis, try watching an animated movie of this collision:

11) The Bad Astronomer, "When worlds collide", http://blogs.discovermagazine.com/badastronomy/2009/08/10/when-worlds-collide/

It makes the collision that seems to have created our Moon look pretty tame by comparison.



Robin Canup's simulation of Earth and Theia, 50 minutes after their initial collision. Color indicates temperature.

By the way, you can read my tale of *that* collision, and some other dramatic developments in the Earth's history, here:

12) John Baez, "The Earth — for physicists", *PhysicsWorld*, July 31, 2009. Available at http://physicsworld.com/cws/article/print/39959 or http://math.ucr.edu/home/baez/earth.html

Moving from big splats to little flashes, here's an interesting item pointed out by Daniel Rocha. I described one way to make graphene in "Week 277" — by taking graphite crystals and carefully peeling off layers with the help of scotch tape. There are other methods but they're expensive and annoying. Now they've found a better way: take a sheet of "paper" made of graphite oxide and zap it with the flash of an ordinary camera!

13) "Northwestern researchers create graphene in a flash", *DailyTech*, http://www.dailytech.com/Northwestern+Researchers+Create+Graphene+in+a+Flash/article15976.htm

Okay, that's all for this week. I'm still passionately interested in math, and I've been learning so much beautiful stuff lately that I almost despair of explaining it all. But I've been spending so much time writing papers that I need a little break!

Addenda: For more discussion visit the n-Category Caf.

> Abstract work, if one wishes to do it well, must be allowed to destroy one's humanity; one raises a monument which is at the same time a tomb, in which, voluntarily, one slowly inters oneself.
>
> — *Bertrand Russell*

(I hope it ain't true.)

Week 279

September 5, 2009

I just finished a paper with John Huerta, a math grad student who really likes particle physics:

1) John Baez and John Huerta, "Division algebras and supersymmetry", available as arXiv:0909.0551.

You can think of this paper as our sequel to:

2) John Baez, "$\mathbb{OP}^1$ and Lorentzian geometry", http://math.ucr.edu/home/baez/octonions/node11.html

A "normed division algebra" is an algebraic gadget where you can add, multiply, subtract, and divide, satisfying all the usual laws *except* the commutative and associative laws for multiplication, and where every element has an "absolute value" or "norm" satisfying the usual rules, including most notably:

$$|xy| = |x||y|$$

The most popular example is the real numbers. The second most popular example is the complex numbers. Then comes the quaternions, which are noncommutative... and then, trailing in a distant fourth place, comes the octonions, which are noncommutative and nonassociative.

Our paper aims to give a clear and self-contained treatment of the amazing relation between normed division algebras and supersymmetric Yang-Mills theory. Let me explain the basic idea! I'll cut some corners, but you can see all the details in our paper.

Suppose $\mathbb{K}$ is a normed division algebra of dimension n. There are just four choices:

- $\mathbb{K} = \mathbb{R}$, the real numbers, with $n = 1$.
- $\mathbb{K} = \mathbb{C}$, the complex numbers, with $n = 2$.
- $\mathbb{K} = \mathbb{H}$, the quaternions, with $n = 4$.
- $\mathbb{K} = \mathbb{O}$, the octonions, with $n = 8$.

We get all of these by taking the real numbers and throwing in square roots of -1. So, any guy in $\mathbb{K}$ has a "real part" and an "imaginary part" — and we can "conjugate" it by switching the sign of its imaginary part.

This means we can talk about hermitian matrices with entries in $\mathbb{K}$: that is, matrices that stay the same when you transpose them and then conjugate each entry. Let's use $\mathrm{h}_2(\mathbb{K})$ to mean the set of hermitian 2×2 matrices with entries in $\mathbb{K}$.

Then a nice thing happens: $\mathrm{h}_2(\mathbb{K})$ is the same as $(n + 2)$-dimensional Minkowski spacetime! To see this, note that any guy in $\mathrm{h}_2(\mathbb{K})$ has this form:

$$A = \begin{pmatrix} t + x & y \\ y^* & t - x \end{pmatrix}$$

where t and x are real elements of $\mathbb{K}$, and y is an arbitrary element. Since $\mathbb{K}$ has dimension n, $\mathrm{h}_2(\mathbb{K})$ has dimension $n+2$. And check out its determinant:

$$\det(A) = t^2 - x^2 - yy^*$$

Note that $yy^* = y^*y = |y|^2$, just as in the complex numbers. So, $\det(A)$ is a Minkowski metric with one positive or "timelike" direction, namely t, and $n+1$ negative or "spacelike" directions, namely x and the n components of y.

So: - $\mathrm{h}_2(\mathbb{R})$ is 3-dimensional Minkowski spacetime. - $\mathrm{h}_2(\mathbb{C})$ is 4-dimensional Minkowski spacetime. - $\mathrm{h}_2(\mathbb{H})$ is 6-dimensional Minkowski spacetime. - $\mathrm{h}_2(\mathbb{O})$ is 10-dimensional Minkowski spacetime.

And — lo and behold! — these are just the dimensions where classical superstring theory and super-Yang-Mills theory work best!

More precisely, these are the dimensions where you can write down the Lagrangian for the "Green-Schwarz superstring" and "pure super-Yang-Mills theory". There are fancier tricks that give superstring theories and super-Yang-Mills theories in other dimensions, but these are mainly offshoots of the four cases listed here.

So now we have to ask: why do these supersymmetric theories feel so happy when spacetime is secretly $\mathrm{h}_2(\mathbb{K})$?

Well, supersymmetry is a kind of symmetry that mixes bosons and fermions. In the simple cases I'm talking about, this means mixing vectors and spinors. Since vectors are the same as points in Minkowski spacetime — once we pick an origin — vectors in dimensions 3, 4, 6, or 10 are nicely described by elements of $\mathrm{h}_2(\mathbb{K})$. And it turns out that supersymmetry works well in these dimensions because we can also describe *spinors* using $\mathbb{K}$. A spinor consists of 2 guys in $\mathbb{K}$: in other words, an element of $\mathbb{K}^2$.

Indeed, if you've studied physics, you may know that in 4d Minkowski spacetime, where we apparently live, we use $\mathbb{C}^2$ to describe spinors. I talked quite a bit about this example and also the example of 3d Minkowski spacetime back in "Week 196". So go there if you want more details. For now what matters is this:

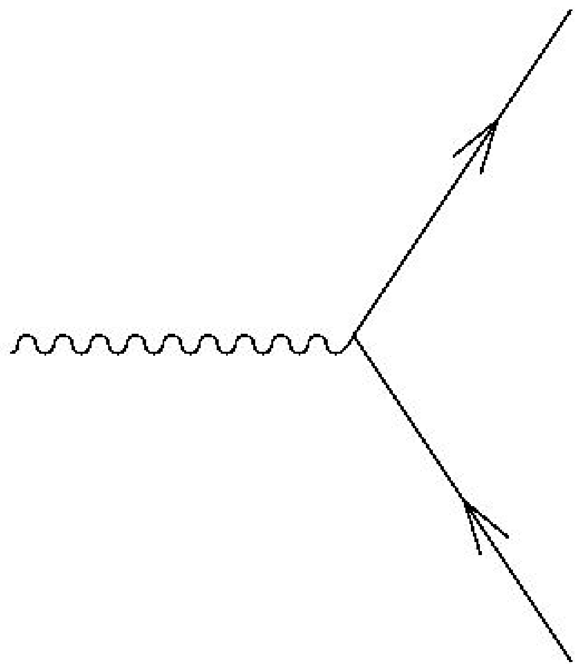
- $\mathbb{R}^2$ is the space of spinors in 3-dimensional Minkowski spacetime.
- $\mathbb{C}^2$ is the space of spinors in 4-dimensional Minkowski spacetime.
- $\mathbb{H}^2$ is the space of spinors in 6-dimensional Minkowski spacetime.
- $\mathbb{O}^2$ is the space of spinors in 10-dimensional Minkowski spacetime.

This is a bit oversimplified, because physicists use various kinds of spinors, and I'm not saying which kinds show up here. But I explained all these kinds back in "Week 93", and I don't want to distract you with that here. I'll say more about it later.

Now, from what we've seen so far, there's an obvious way to take a vector and a spinor and get a new spinor. This is what matrices were born for! Just take your matrix in $\mathrm{h}_2(\mathbb{K})$, multiply your spinor in $\mathbb{K}^2$ by that matrix, and you get a new spinor in $\mathbb{K}^2$.

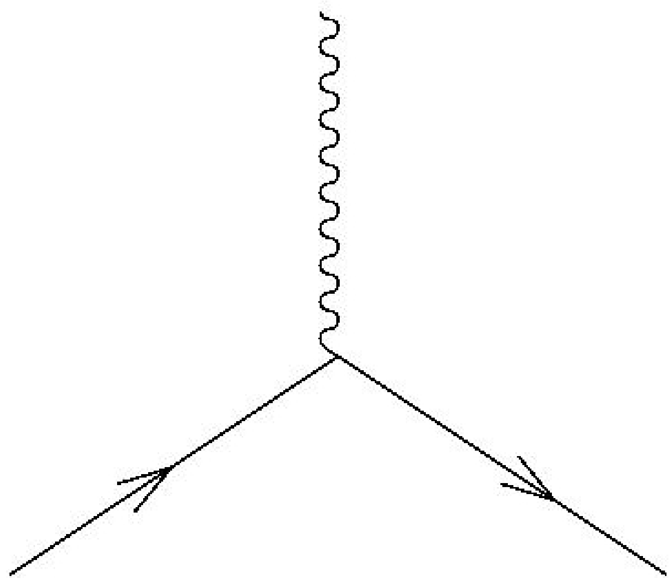
In fact, we see this process at work whenever an electron absorbs a photon. Quite literally, we *see* it — because that's how our eyes work! A photon is described by a vector, an electron is described by a spinor, and part of the math involved when an electron absorbs a photon is this business of matrix multiplication.

Physicists would draw this operation using a Feynman diagram where a wiggly line (the vector) and a straight one (the spinor) come in and a straight one goes out:



Mathematicians would write it as the operation that takes A in $\mathrm{h}_2(\mathbb{K})$ and ψ in $\mathbb{K}^2$ and multiplies them to get $A\psi$ in $\mathrm{h}_2(\mathbb{K})$.

Now, one cool thing about Feynman diagrams is that you can turn them around and read them in different ways, and they still make sense. So as soon as we have a process where a spinor absorbs a vector, we also get a process where two spinors collide and form a vector:



Now what is this process, mathematically speaking? Well, it's some operation that takes two spinors, say ψ in $\mathbb{K}^2$ and φ in $\mathbb{K}^2$, and creates a vector in $\mathrm{h}_2(\mathbb{K})$ that I'll call $\psi \cdot \varphi$. If you want the explicit formula for this operation, read our paper.

So, we've got an operation that takes a vector and a spinor and creates a spinor, and an operation that takes two spinors and creates a vector. Actually, these operations exist for *any* dimension of spacetime! In general we need to describe them using the language of Clifford algebras. Only in dimensions 3, 4, 6 and 10 can we describe them using the language of normed division algebras, as I've done here.

But it's this special language that gives the prettiest explanation of a certain astounding fact. Supersymmetry for the Green-Schwarz superstring and pure super-Yang-Mills theory relies on a special identity which is true only in dimensions 3, 4, 6 and 10:

$$(\psi \cdot \psi)\psi = 0$$

This is an example of a "Fierz identity". These equations show up whenever you work with spinors, and they should probably be called "fierce identities", because they can be pretty scary. In particular, it's a bit scary how some of them — like this one — hold only in certain special dimensions.

But this particular one has a beautiful proof in terms of normed division algebras! It follows from a special property shared by these algebras: they're all "alternative". In other words, the "associator"

$$[x, y, z] = (xy)z - x(yz)$$

changes sign whenever we switch any of the two variables. The associator is just zero for $\mathbb{R}$, $\mathbb{C}$, and $\mathbb{H}$, since these algebras are associative. So the only really interesting case is the octonions, which are not associative, but still alternative. And this is the case that matters for superstring theory in 10 dimensions!

Anyway, what our paper does is describe the basic operations involving spinors and vectors using the normed division algebras, then use this description to prove the identity

$$(\psi \cdot \psi)\psi = 0,$$

and then explain how this identity is crucial in supersymmetric Yang-Mills theory. None of this is particularly new! What's new, we hope, is that we explain everything in one place, in a way that people who don't know about division algebras or supersymmetry can follow. Some of the proofs use a little Clifford algebra technology, but most of them amount to simple calculations.

Now let me tell you a tiny bit about the history of this subject, with references. I would love to hear more details from people who were around at the time. As far as I can tell, this is the first paper that explained super-Yang-Mills theory and why it only works in dimensions 3, 4, 6 and 10:

3) Lars Brink, John Schwarz and Joel Scherk, "Supersymmetric Yang-Mills theory", *Nucl. Phys. B* **121** (1977), 77–92.

Well, actually 2, 4, 6, and 10, but never mind! This is the paper that did it for superstring theory:

4) Michael Green and John Schwarz, "Covariant description of superstrings", *Phys. Lett.* **B136** (1984), 367–370.

The bible of string theory contains proofs of these facts, based on the properties of Clifford algebras in various dimensions:

5) Michael B. Green, John H. Schwarz and Edward Witten, *Superstring Theory*, Volume 1, Cambridge U. Press, Cambridge, 1987. Appendix 4.A, "Super Yang-Mills Theory", pages 244–247. Section 5.1.2: "The supersymmetric string action", pp. 253–255.

Back in 1983, Kugo and Townsend showed how spinors in dimension 3, 4, 6, and 10 get special properties from the normed division algebras. They formulated a supersymmetric model in 6 dimensions using the quaternions, and speculated about a similar formalism in 10 dimensions using the octonions:

6) Taichiro Kugo and Paul Townsend, "Supersymmetry and the division algebras", *Nucl. Phys. B* **221** (1983), 357–380. Also available at `http://ccdb4fs.kek.jp/cgi-bin/img_index?198301032`

Later, Evans showed that supersymmetry of nonabelian Yang-Mills fields coupled to massless spinors in $(n+2)$-dimensional spacetime implies the existence of a normed division algebra of dimension n:

7) J. M. Evans, "Supersymmetric Yang-Mills theories and division algebras", *Nucl. Phys. B* **298** (1988), 92–108. Also available at `http://ccdb4fs.kek.jp/cgi-bin/img_index?8801412`

Shortly after Kugo and Townsend's work, Tony Sudbery used division algebras to construct vectors, spinors and Lorentz groups in Minkowski spacetimes of dimensions 3, 4, 6, and 10:

8) Anthony Sudbery, "Division algebras, (pseudo)orthogonal groups and spinors", *Jour. Phys.* **A17** (1984), 939–955.

He then refined his construction with the help of a grad student:

9) Kwok-Wai Chung and Anthony Sudbery, "Octonions and the Lorentz and conformal groups of ten-dimensional space-time", *Phys. Lett.* **B198** (1987), 161–164.

Tony Sudbery and Corinne Manogue then used these ideas to give an octonionic proof that $(\psi \cdot \psi)\psi = 0$.

10) Corinne Manogue and Anthony Sudbery, "General solutions of covariant superstring equations of motion", *Phys. Rev.* **D12** (1989), 4073–4077.

Together with her husband and Jason Janesky, Manogue later simplified the proof, and our argument is based on theirs:

11) Tevian Dray, Jason Janesky and Corinne Manogue, "Octonionic hermitian matrices with non-real eigenvalues", *Adv. Appl. Clifford Algebras* **10** (2000), 193–216. Appendix B, "The 3-ψ's rule". Also available as arXiv:math/0006069.

They give a little more history of this wondrous identity. They say that Manogue's student Schray called it "the 3-ψ's rule" in his hard-to-obtain thesis:

12) Jrg Schray, *Octonions and supersymmetry*, Ph.D. thesis, Department of Physics, Oregon State University, 1994.

and also here:

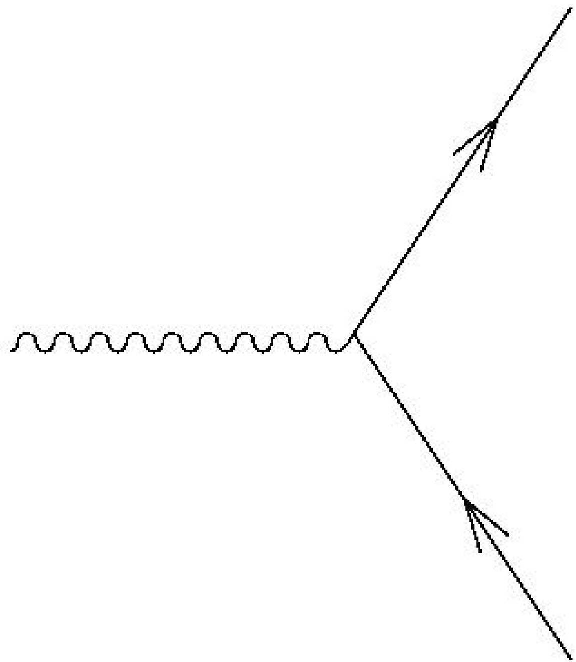
13) Jrg Schray, "The general classical solution of the superparticle", *Class. Quant. Grav.* **13** (1996), 27–38. Also available as hep-th/9407045.

They also write:

> *It is well-known that the 3-ψ's rule holds for Majorana spinors in 3 dimensions, Majorana or Weyl spinors in 4 dimensions, Weyl spinors in 6 dimensions and Majorana-Weyl spinors in 10 dimensions. Thus, the Green-Schwarz superstring exists only in those cases. As was shown by Fairlie and Manogue, the 3-ψ's rule in all these cases is equivalent to an identity on the gamma-matrices, which holds automatically for the natural representation of the gamma-matrices in terms of the 4 division algebras $\mathbb{R}$, $\mathbb{C}$, $\mathbb{H}$ and $\mathbb{O}$, corresponding precisely to the above 4 types of spinors. Manogue and Sudbery then showed how to rewrite these spinor expressions in terms of 2×2 matrices over the appropriate division algebra, thus eliminating the gamma-matrices completely.*

Now I feel like explaining all this Majorana/Weyl business a bit better — leaving many details to "Week 93".

First I should admit, for the nonexperts, that I've committed a few sins of oversimplification for the sake of a nice clean story line. For starters, remember how I said that the absorption of a photon by an electron:



corresponds to the operation where we take a guy in $\mathrm{h}_2(\mathbb{K})$ and a guy in $\mathbb{K}^2$ and multiply them to get a guy in $\mathrm{h}_2(\mathbb{K})$?

In saying this, I was ignoring everything about energy and momentum, and focusing on the "spin" aspect of this absorption process. It's only the "spin", or intrinsic angular momentum, of a photon that's described by an element of $\mathrm{h}_2(\mathbb{K})$ — with $\mathbb{K} = \mathbb{C}$, since we live in 4-dimensional spacetime. And it's only the spin of the electron that's described by an element of $\mathbb{K}^2$.

But it's even worse than that. In 4-dimensional spacetime, spinors come in left- and right-handed forms. For example, the neutrinos we most easily see — not that easily, actually! — are left-handed spinors, while antineutrinos are right-handed. Electrons come in both left and right-handed forms, so we actually describe them using $\mathbb{C}^2 \oplus \mathbb{C}^2 = \mathbb{C}^4$. We call $\mathbb{C}^4$ the space of "Dirac spinors", and we call the two pieces left- and right-handed "Weyl spinors".

Similar but subtly different things happen in other dimensions. As far as our division algebras story goes, the crucial fact is that besides the "obvious" way for an element of $\mathrm{h}_2(\mathbb{K})$ to act on $\mathbb{K}^2$, there is a less obvious way that involves the "traced-reversed" form of a 2×2 hermitian matrix:

$$\widetilde{A} = A - \mathrm{tr}(A)$$

where the trace $\mathrm{tr}(A)$ is the sum of the diagonal entries. We get one kind of spinors, say

$$S_+ = \mathbb{K}^2$$

upon which the vectors

$$V = \mathrm{h}_2(\mathbb{K})$$

act in the obvious way, and another kind of spinors, say

$$S_- = \mathbb{K}^2$$

in which vectors act in a nonobvious way. As vector spaces S_+ and S_- are the same — but they differ in how vectors act on them, and we should think of this action as interchanging the two kinds of spinor. Here are the formulas:

$$\begin{aligned} V \otimes S_+ &\to S_- \\ A \otimes \psi &\mapsto A\psi \end{aligned}$$

$$\begin{aligned} V \otimes S_- &\to S_+ \\ A \otimes \psi &\mapsto \widetilde{A}\psi \end{aligned}$$

These actions fit together to yield a Clifford algebra action on the direct sum of S_+ and S_-, since

$$A\widetilde{A} = \widetilde{A}A = -\det(A)$$

and the determinant is related to the metric on Minkowski spacetime, so these are the Clifford algebra relations in deep disguise.

What all this really amounts to depends a lot on which of the four division algebras we're looking at! Sometimes S_+ and S_- are secretly isomorphic, sometimes not. They always start out being real vector spaces, since as vector spaces they're just $\mathbb{K}^2$, and the only uniform way to think of all four normed division algebras is as *real* vector spaces. But sometimes S_+ and S_- admit Lorentz-invariant complex structures, so we can think of them as complex vector spaces!

(By "Lorentz-invariant" I really mean invariant under the action of the *double cover* of the Lorentz group. For brevity, let's just call this the Lorentz group.)

In fact, each of the four cases has its own unique personality, with the 4d case being the weirdest — you might call it a "split personality". Let me just summarize the facts, without much explanation. This is one of those things where I write stuff down so I can forget it and look it up later:

- If $\mathbb{K} = \mathbb{R}$, we're in 3d Minkowski spacetime. Then S_+ and S_- are isomorphic as real representations of the Lorentz group — so it's not important to distinguish them. The secret reason for this is that $\mathbb{R}$ is commutative. Since $S_+ \cong S_-$ does not have an invariant complex structure, we call elements of this space "Majorana" spinors, which is the name for real spinors that don't have a particular handedness.

- If $\mathbb{K} = \mathbb{C}$, we're in 4d Minkowski spacetime. Then S_+ and S_- are isomorphic as real representations of the Lorentz group — so it's not important to distinguish them. The secret reason for this is that $\mathbb{C}$ is commutative. If we treat $S_+ \cong S_-$ as a real vector space, we call elements of this space "Majorana" spinors, which is the name for real spinors that don't have a particular handedness.

But in fact this real vector space has two invariant complex structures, and the resulting complex representations are *not* isomorphic! If we think of S_+ and S_- as two nonisomorphic complex representations we call their elements left- and right-handed "Weyl" spinors, respectively — since that's the name for complex spinors that do have a particular handedness.

- If $\mathbb{K} = \mathbb{H}$, we're in 6d Minkowski spacetime. Then S_+ and S_- are not isomorphic as real representations of the Lorentz group — so it's important to distinguish them. The secret reason for this is that $\mathbb{H}$ is not commutative. Furthermore, S_+ and S_- admit invariant complex structures. If we think of S_+ and S_- as complex representations we call their elements left- and right-handed "Weyl" spinors, respectively — since that's the name for complex spinors that do have a particular handedness.
- If $\mathbb{K} = \mathbb{O}$ we're in 10d Minkowski spacetime. Then S_+ and S_- are not isomorphic as real representations of the Lorentz group — so it's important to distinguish them. The secret reason for this is that $\mathbb{O}$ is not commutative. Furthermore, S_+ and S_- do not admit invariant complex structures. So, we must think of S_+ and S_- as real representations, and we call their elements left- and right-handed "Majorana-Weyl" spinors, respectively — since that's the name for real spinors that do have a particular handedness.

Wow, I bet that was thrilling!

Now that I'm done with this paper, my life has undergone a phase change. I've been finishing a lot of old papers for the last 2 years. This is the last of that batch — and the least old. It comes as an incredible relief. Working on old projects is tiring, especially when you have new things you're dying to think about. I feel like I've been way behind myself, running to catch up... but this week I finally caught up and ran past myself! It's a strange sensation.

Adding to this strange sensation, I just got word that I'm free to take leave from UCR and visit Singapore for a year, starting in July 2010. I'll be working at the Centre for Quantum Technologies. That should be a great adventure.

So, I'm feeling peppy, and I'm dying to tell you about all sorts of new cool stuff: Stirling numbers and the Poisson operad, stacks and noncommutative geometry, Adams operations and Galois theory, toric varieties, octonions and rolling balls, the windmill powered by light, and the symplectic geometry of electrical circuits. Each of these deserves a whole Week, but we'll see.

For now, here are a few cool things I *won't* tell you much about, because I don't understand them well enough. First, as pointed out to me by Mike Stay:

14) "Generalized continued fractions and equal temperament", *Doctroidal Dissertations*, April 13th, 2009, `http://doctroid.wordpress.com/2009/04/13/`

This starts with the old problem of trying to find a number x such that $x^n = 2$ and x^k is almost $3/2$. In music jargon, this is called "finding an equal tempered scale that has a good fifth". In math jargon, it amounts to finding a good rational approximation to

$$\frac{\log(3/2)}{\log(2)} \sim 0.584962501$$

The theory of continued fractions gives us candidates:

$$\begin{aligned}\frac{0}{1} &= 0.000000\\ \frac{1}{1} &= 1.000000\\ \frac{1}{2} &= 0.500000\\ \frac{3}{5} &= 0.600000\\ \frac{7}{12} &= 0.583333\\ \frac{24}{41} &= 0.585366\\ \frac{31}{53} &= 0.584906\end{aligned}$$

and the first really good one is $7/12$. Maybe that's why we divide the scale into 12 equal parts, with the the 7th one up playing a crucial role!

But what if you want a scale that has a good fifth and something else too, like a good third? That's where "generalized" continued fractions come in! I won't tell you what those are.

I also won't tell you about the new revolution linking logic to weak ∞-groupoids. For that you'll have to read these:

15) Martin Hofmann and Thomas Streicher, "The groupoid interpretation of type theory", in Sambin, Giovanni, et al, *Twenty-five years of constructive type theory*, Clarendon Press, Oxford, 1998, pp. 83–111. Also available at `http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.37.606`

16) Steve Awodey and Michael A. Warren, "Homotopy theoretic models of identity types", available as `arXiv:0709.0248`.

17) Steve Awodey, Pieter Hofstra, Michael A. Warren, "Martin-Lf Complexes", available as `arXiv:0906.4521`.

18) Benno van den Berg and Richard Garner, "Types are weak omega-groupoids", available at `http://www.dpmms.cam.ac.uk/~rhgg2/Typesom/Typesom.pdf`

Addendum: Francesco Toppan writes:

> *In relation with your recent, interesting,* `arxiv:0909.0551` *paper I would like to signal that division algebras also appear in the N-extended supersymmetric quantum mechanics (in one dimension) for $N = 1, 2, 4, 8$. This is hardly surprising, of course (*`hep-th/0109073` *NPB Pr. Sup.). Perhaps slightly more surprising is the fact that the octonionic structure constants enter, as coupling constants, $N = 8$ invariant actions, like e.g. the $(1, 8, 7)$ model of* `hep-th/0511274` *(also in JHEP). In this example the 7 auxiliary fields can be associated*

> *with the 7 imaginary octonions, preserving the "octonionic covariance". I should add that the representations of N-extended 1D superalgebra are mathematically very interesting and quite intricate. In the last few years basically two groups, my group and the group of S. Gates and his collaborators, worked out with complementary viewpoints and results several features of these representations: total number of fields, grading of the fields, graph interpretation, connectivity of the graphs, etc. etc. Basic references can be found in `hep-th/0010135` (in JMP), `hep-th/0610180` (PoS), or typing my name (and Gates'name) in the arXiv. Perhaps you could find these interesting to have a look at.*

For more discussion visit the n-Category Caf.

> A creation of importance can only be produced when its author isolates himself, it is a child of solitude.
>
> — *Johann Wolfgang von Goethe*

Week 280

September 27, 2009

I have a lot to talk about, since I just got back from this quantum gravity summer school in Corfu:

1) *2nd School and workshop on quantum gravity and quantum geometry*, September 13–20, 2009, organized by John Barrett, Harald Grosse, Larisa Jonke and George Zoupanos. Information at `http://www.physics.ntua.gr/corfu2009/qg.html`

I felt a bit like Rip Van Winkle, the character who fell asleep for 20 years and woke to find everything changed. I gave up working on quantum gravity about 4 years ago because so many problems seemed intractable. Now a lot of these have been solved, or at least seen some progress. It was great!

The school featured courses by Ashtekar, Barrett, Rivasseau, Rovelli and myself — we each gave 5 hours of lectures. All these guys were friends whom I was very happy to see again — except for Rivasseau, whom I'd never met. But it was great to meet him, since he works on mathematically rigorous quantum field theory, the topic I tried to do my PhD on. He had some amazing things to say about the combinatorics of trees and the problem of summing divergent series. Sadly, right now I only have time to summarize the courses by Ashtekar and Rovelli.

Abhay Ashtekar gave a review of recent work on loop quantum cosmology. Starting with the work of Martin Bojowald around 2001, there's been a lot of interest in the possibility that loop quantum gravity could eliminate the singularity at the Big Bang. The big problem is that dynamics in loop quantum gravity is not understood. There are lots of choices for how it might work, and nobody knows which is right, or if there even *is* a right one. Luckily, by imposing the symmetry conditions appropriate to a homogeneous isotropic cosmology one can narrow down the problem of seeking a reasonable dynamics to something much more manageable. Instead of infinitely many degrees of freedom, there are just a few.

Unfortunately, there are still lots of choices involved in guessing a reasonable theory of quantum cosmology inspired by loop quantum gravity. Checking their implications involves both computer calculations and conceptual head-scratching. So, it took about 7 years to find a satisfactory candidate. It might not be right, but at least it's self-consistent and elegant. For a nice survey, see this:

2) Abhay Ashtekar, "Loop quantum cosmology: an overview", *Gen. Rel. Grav.* 41 (2009), 707–741. Also available as arXiv:0812.0177.

Section IVa sketches the history of the subject, but it's best to read the previous stuff first.

Anyway, what are the results? What does the currently popular theory of loop quantum cosmology imply?

In a nutshell: if you follow the history of the Universe back in time, it looks almost exactly like what ordinary cosmology predicts until the density reaches about $1/100$ of the Planck density.

What's the Planck density, you ask?

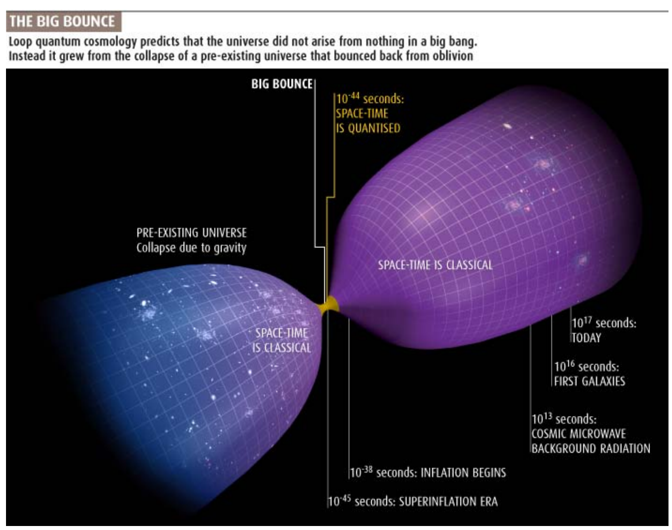
Well, you can cook up units of mass, length and time by saying that Planck's constant and the speed of light and Newton's gravitational constant are all 1 in these units. Unsurprisingly, these units are called:

- the Planck mass: 2.2×10^{-8} kilograms
- the Planck length: 1.6×10^{-35} meters
- the Planck time: 5.4×10^{-44} seconds

The "Planck density" is one Planck mass per cubic Planck length — or in ordinary units, about 5×10^{96} kilograms per cubic meter. That's incredibly dense! It's what you'd get by compressing 10^{23} solar masses into the volume of an atomic nucleus.

According to loop quantum cosmology, at around $1/100$ the Planck density quantum gravity effects come in, creating a force that prevents the universe from shrinking further as we march backwards in time. And at about $.4$ times the Planck density, there's a "bounce". Going further back in time, we see the Universe expand again! Indeed, the Universe is symmetrical in time around the moment of maximum compression.

So, the Big Bang is replaced by a Big Bounce.



The above picture comes from this popular account:

3) Anil Ananthaswamy, "From Big Bang to Big Bounce", *New Scientist*, December 13, 2008, pp. 32–35. Also available at `http://gravity.psu.edu/outreach/articles/bigbounce.pdf`

Interestingly, in this model quantum effects don't create much dispersion as the Universe passes through a big bounce. In other words: if the Universe's wavefunction is sharply peaked around a certain classical geometry, it remains so through the big bounce. It doesn't "smear out" too much.

By the way, I would be very happy if anyone working on loop quantum cosmology could give me a intuitive physical explanation for the force that prevents the singularity.

Mathematically I know that it arises from a kind of discretization. Instead of talking about the curvature of spacetime at infinitesimally small scales, we can only measure curvature by carrying a particle around a finite-sized loop. This has little effect when spacetime is only slightly curved, but when the curvature is big it makes a big difference. This causes an effect very much like a force that prevents the Universe from crushing down to nothing as we follow it backwards in time.

So far, so good. But a good physical intuition would explain the *sign* of this effective force. Why does it prevent the singularity instead of, say, making it worse?

For some more details, try this treatment which resembles the course Ashtekar taught:

4) Abhay Ashtekar, "An introduction to loop quantum gravity through cosmology", *Nuovo Cimento* **122B** (2007), 135–155. Also available as gr-qc/0702030.

Anyway, the real fun will start when people systematically compute the behavior of inhomogeneous perturbations in loop quantum cosmology model. After all, the little ripples in the microwave background radiation are the first interesting thing we see in the Universe.

A lot of work on cosmology studies these inhomogeneities by calculating backwards to a hypothesized "inflationary epoch" about 10^{-35} seconds after the Big Bang — or Big Bounce, if that's your theory. Quantum gravity effects are likely to become important only at much earlier times, since the Planck time is about 10^{-43} seconds. Here I'm using "much earlier" in a funny logarithmic sense. But that's actually appropriate here. The inflationary epoch comes about 100 million Planck times after the Big Bounce. According to Ashtekar's calculations, by then quantum gravity corrections only affect the rate of expansion of the universe by about one part in 100 thousand. So, it's not clear that loop quantum gravity calculations are going to have anything interesting to say about anything we can see today. But who knows?

Next, Carlo Rovelli. His class was an introduction to spin foam models, which are an attempt to pin down a specific dynamics for loop quantum gravity. Here I'm going to get more technical, because this material is closer to my heart. If you need a warmup, try "Week 109"–"Week 113" for the basics.

I worked on spin foams for about 5 years. I love them because they offer the hope of building spacetime from abstract algebra — higher category theory, in fact. But I gave up because a lot of puzzle pieces just didn't seem to be fitting together. Back then, the best candidate for a spin foam model of gravity was the Barrett-Crane model. But there were three big problems:

A) *The Barrett-Crane model used spin networks of a different kind from the usual ones in loop quantum gravity. Instead of spin networks with edges labelled by unitary representations of* $\mathrm{SU}(2)$ *(the double cover of the rotation group), it used unitary representations of* $\mathrm{SL}(2, \mathbb{C})$ *(the double cover of the Lorentz group). This is because it's all about* spacetime*, while loop quantum gravity focuses on* space*. And instead of using spin networks with vertices labelled by arbitrary intertwiners, it only used a special intertwiner called the "Barrett-Crane intertwiner".*

B) *While loop quantum gravity in its modern formulation includes the Immirzi parameter — a dimensionless constant that sets the scale of area* *quantization — the Barrett-Crane model did not. If the currently accepted calculations are right, we need to choose a special and rather peculiar value of the Immirzi parameter if we want loop quantum gravity to get the right answer for the entropy of black holes. So, along with problem A), this makes it even harder to connect the Barrett-Crane model to loop quantum gravity.*

C) *At first people hoped for various clues linking the Barrett-Crane model to general relativity. For example, we hoped that the asymptotic value of the amplitude for a large* 4*-simplex in the Barrett-Crane model was nicely related to the action for general relativity. But this turned out to be false: in the Barrett-Crane model, the amplitude for a large* 4*-simplex is dominated by certain degenerate geometries where the* 4*-simplex is squashed down to 3 dimensions. See "Week 128", "Week 168", "Week 170" and "Week 198" for the story here. Carlo Rovelli raised our hopes again in a more sophisticated way: he tried to compute the propagator for gravitons starting from the Barrett-Crane model. For a beautiful and physically very sensible reason, the degenerate geometries don't dominate this calculation. Rovelli got some promising results for certain components of the graviton propagator, and left a student to work out the rest of the components... but it didn't work!*

It seems all these problems have been solved now. There's a new model sometimes called the EPRL model, after Engle, Pereira, Rovelli, and Livine, although other people were involved as well — I'll list some papers later.

The basic idea of the EPRL model is to start with the Holst Lagrangian for general relativity. In 1995, Soren Holst came up with a nice Lagrangian for gravity:

5) Soren Holst, "Barbero's Hamiltonian derived from a generalized Hilbert-Palatini action", *Phys. Rev.* **D53** (1996), 5966–5969. Also available as gr-qc/9511026.

It looks like this:

$$\mathrm{tr}(e \wedge e \wedge *F) + \frac{1}{\gamma}\,\mathrm{tr}(e \wedge e \wedge F)$$

I'll explain this in detail later, because there was a student who twice asked about the math behind this Lagrangian, and Rovelli and I brushed the question off by saying "it's just like Palatini Lagrangian". I feel guilty, so someone find that student and tell him to read my explanation below.

But that gets a bit technical, so for now let me say: "it's just like the Palatini Lagrangian". Namely, the first term is the usual Palatini Lagrangian for gravity. The second term involves the Immirzi parameter, γ. The second term doesn't affect the classical equations of motion, because its variation is a total derivative. But it does affect the quantum theory!

If we triangulate spacetime and carry out a spin foam quantization of this theory — which is a bit like doing lattice gauge theory — we can show (in a rough-and-ready physicist's way) that the partition function of the quantum theory is computed as a sum over spin foams where the spin foams are labelled by certain special representations of $\mathrm{SL}(2,\mathbb{C})$.

Physicists don't learn the unitary representations of the Lorentz group in school the way they do for the Poincare group. But the unitary representations of the Lorentz group — or its double cover $\mathrm{SL}(2,\mathbb{C})$ — are very nice. Except for the trivial representation they're all infinite-dimensional, which is a bit scary at first... but there's a bunch called the "principal series" indexed by a spin $j = 0, 1/2, 1, 3/2, \ldots$ and a nonnegative real number I'll call k. Very roughly speaking the spin j has to do with rotations, while k is an analogous quantity related to boosts. If you want more details, the only online explanation I can find is this:

6) Wikipedia, "Representation theory of the Lorentz group", `http://en.wikipedia.org/wiki/Representation_theory_of_the_Lorentz_group`

It may be better to read some of the many books cited there.

Anyway, the special representations of $\mathrm{SL}(2,\mathbb{C})$ that show up in the EPRL model are those with

$$k = \gamma j$$

This is beautiful because there's one for each spin. So, the category of these representations and their direct sums is equivalent to the category of finite-dimensional unitary representations of $\mathrm{SU}(2)$!

This is how the EPRL model gets around problem A) listed above. Spin networks in this new model are nicely compatible with spin networks in loop quantum gravity, because you can think of their edges *either* as labelled by special representations of $\mathrm{SL}(2,\mathbb{C})$, *or* as labelled by arbitrary representations of $\mathrm{SU}(2)$. The first is the "spacetime" or Lagrangian viewpoint, the second is the "space" or Hamiltonian viewpoint.

This is also the key to how the EPRL model gets around problem B). The Immirzi parameter is built into the model in a very natural way. As a result, the quantization of area and volume in this model is compatible with that in loop quantum gravity.

I don't think I'll describe the rest of the model, which consists of a formula for computing the amplitude for a 4-simplex with edges labelled by spins. But it's this formula that solves problem C). The EPRL model gets the graviton propagator right!

Of course there are even bigger tests still ahead for this spin foam model. We need to see if it reduces to general relativity in the classical limit. In other words, we need to get Einstein's equations out of it. And we need to see if it reduces to the usual perturbative theory of quantum gravity in some other limit. In other words, we need to compute, not just graviton propagators (which describe the probability of a lone graviton zipping from here to there on the background of Minkowski spacetime), but graviton scattering amplitudes (which describe the probability of various outcomes when two or more gravitons collide).

Both these tasks are both computationally and *conceptually* difficult. In other words, it's not just hard to do the calculations: it's hard to know what calculations to do! When I said "in some limit" and "in some other limit", I know what limits these are in a physical sense, but not how to describe them using spin foams. Actually we seem closer to understanding graviton scattering amplitudes, thanks to the work of Rovelli. But it seems miraculous and strange that we can compute graviton propagators (much less scattering amplitudes) using very simple spin foams, as Rovelli and his collaborators have done. Every time I meet him, I ask Rovelli what's going on here: how we can describe the behavior of a graviton in terms of just a few 4-simplices of spacetime.

So, the road is still long, steep, and fraught with danger. But three problems that had everyone completely stumped have now been solved in one elegant blow.

Though there's much more to say, it's dinnertime now. So, let me list some references and then explain the differential geometry behind the Holst Lagrangian, just to make up for not explaining it to that student.

The original "EPR model" was introduced here — but this treated Riemannian rather than Lorentzian metrics, and only in the special case where the second term in the Holst action was left out — so, no Immirzi parameter:

7) Jonathan Engle, Roberto Pereira and Carlo Rovelli, "Flipped spinfoam vertex and loop gravity", *Nucl. Phys.* **B798** (2008), 251–290. Also available as arXiv:0708.1236.

This is a very nice paper which describes a lot of geometry that I haven't had time to mention. However, the full-fledged model appeared later:

8) Jonathan Engle, Etera Livine, Roberto Pereira, and Carlo Rovelli, "LQG vertex with finite Immirzi parameter", *Nucl. Phys.* **B799** (2008), 136–149. Also available as arXiv:0711.0146.

But there are also other people whose work deserves credit! For example, my friends Laurent Freidel and Kirill Krasnov:

9) Laurent Freidel and Kirill Krasnov, "A new spin foam model for 4d gravity", *Class. Quant. Grav.* **25** (2008), 125018. Also available as arXiv:0708.1595.

This paper gives a bit of the history, which I don't know very well, since I wasn't paying attention. Kirill visited me once and tried to get me interested in his new spin foam model, but I wasn't in the mood. Now that everything is nicely polished, of course I like it more.

There are probably lots of other important papers that I'm leaving out, but let me turn to a few papers that discuss graviton propagators.

Here's the paper where Rovelli's student found problems with getting the graviton propagator from the Barrett-Crane model:

10) Emanuele Alesci and Carlo Rovelli, "The complete LQG propagator: I. Difficulties with the Barrett-Crane vertex", *Phys. Rev.* **D76** (2007), 104012. Also available as arXiv:0708.0883.

Then Rovelli and collaborators found numerical evidence that the EPR model seemed to be working better:

11) Elena Magliaro, Claudio Perini and Carlo Rovelli, "Numerical indications on the semiclassical limit of the flipped vertex", *Class. Quant. Grav.* **25** (2008), 095009. Also available as arXiv:0710.5034.

Then Alesci and Rovelli wrote a paper using the new model:

12) Emanuele Alesci and Carlo Rovelli, "The complete LQG propagator: II. Asymptotic behavior of the vertex", *Phys. Rev.* **D77** (2008), 044024. Also available as arXiv:0711.1284.

and then Alesci and Rovelli wrote another paper in their series, with Eugenio Bianchi:

13) Emanuele Alesci, Eugenio Bianchi, Carlo Rovelli, "LQG propagator: III. The new vertex", available as arXiv:0812.5018.

This paper used the work of John Barrett and collaborators, who analyzed the asymptotics of the amplitude for a 4-simplex in the new model:

14) John W. Barrett, R. J. Dowdall, Winston J. Fairbairn, Henrique Gomes and Frank Hellmann, "Asymptotic analysis of the EPRL four-simplex amplitude", available as arXiv:0902.1170.

For a nice treatment of spin foams that generalizes the new model to spin foams that don't come from triangulations of spacetime, try:

15) Wojciech Kaminski, Marcin Kisielowski, Jerzy Lewandowski, "Spin-foams for all loop quantum gravity", available as arXiv:0909.0939.

Everything Lewandowski does is very precise, so if you're a mathematician you might actually want to start here.

There are also lots of other papers in this general line of work. I apologize to everyone whose work I didn't cite — like Dan Christensen and Igor Khavkine, for example!

Anyway, I'm excited about this new work and I hope to write another paper or two about spin foam models. I came up with two fun ideas during the Corfu summer school, which I'd like to work on.

But let me conclude by explaining the Holst Lagrangian for gravity. I explained the Palatini Lagrangian and a whole bunch of other Lagrangians for gravity back in "Week 176". But maybe you were just a kid back then. . . or maybe you weren't paying adequate attention! So, let me repeat my explanation in slightly different words.

Assume spacetime is an orientable smooth 4-manifold M. Pick a vector bundle T that's isomorphic to the tangent bundle TM. Physicists don't have a name for this bundle, but they call any of its fibers the "internal space". I call it the "fake tangent bundle".

We then equip T with a Lorentzian metric and orientation. This lets us describe a Lorentzian metric on M using a vector bundle map

$$e \colon TM \to T$$

This map has lots of names: the "cotetrad", the "soldering form", or the "coframe field". Whatever we call it, we can use it to pull the metric on T back to the tangent bundle. If e is an isomorphism, this gives a Lorentzian metric on M. If it's not, we get something like a metric, but with degenerate directions. For now let's only consider the case where e is an isomorphism.

The cotetrad is one of the two basic fields used to define the Holst action. The other is a metric-compatible connection on T. This connection is usually denoted A and called a "Lorentz connection". Its curvature is denoted F.

Now, what does the Holst Lagrangian

$$\mathrm{tr}(e \wedge e \wedge *F) + \frac{1}{\gamma}\,\mathrm{tr}(e \wedge e \wedge F)$$

actually mean?

First of all, the curvature F is an $\mathrm{End}(T)$-valued 2-form, but using the metric on T we get an isomorphism between T and its dual, so we can also think of the curvature as a 2-form taking values in $T \otimes T$. However, if we do this, the fact that A is metric-compatible means that F is skew-symmetric: it takes values in the second exterior power of T, $\wedge^2(T)$.

Since T has a metric and orientation, we can define a Hodge star operator on the exterior algebra $\wedge(T)$ just as we normally do for differential forms on a manifold with metric and orientation. We call this the "internal" Hodge star operator. Using this we can define $*F$, which is again a 2-form taking values in $\wedge^2(T)$.

Next, note that the cotetrad e can be thought of as a T-valued 1-form. This allows us to define the wedge product $e \wedge e$ as a $\wedge^2(T)$-valued 2-form. This is the same sort of gadget as the curvature F and its internal Hodge dual $*F$! So, we can take the wedge product of the differential form parts of $e \wedge e$ and $*F$ while using the metric on T to pair together their $\wedge^2(T)$ parts to get a number. The overall result is a plain old 4-form, which we call $\mathrm{tr}(e \wedge e \wedge *F)$. This is the Palatini Lagrangian!

If you work out the equations of motion coming from this Lagrangian, they say A that pulls back via e to a *torsion-free* metric-compatible connection on the tangent bundle. This is just the Levi-Civita connection! It follows that F pulls back to the curvature of the Levi-Civita connection. This is just the Riemann tensor! Finally, it turns out that $\mathrm{tr}(e \wedge e \wedge *F)$ is just the Ricci scalar curvature times the volume form on M, so we were doing general relativity all along!

We define $\mathrm{tr}(e \wedge e \wedge F)$ the same sort of way, and throwing this term into the action doesn't affect the classical equations of motion. It's very much like Yang-Mills theory, where you can take the usual action

$$\mathrm{tr}(F \wedge *F)$$

and throw in a "theta term", proportional to "second Chern class"

$$\mathrm{tr}(F \wedge F)$$

without changing the classical equations of motion. But it does affect the quantum theory!

For a more detailed treatment of the Holst action including a cosmological constant term proportional to

$$\mathrm{tr}(e \wedge e \wedge e \wedge e)$$

and three topological terms corresponding to the Pontryagin class, the Euler class and the Nieh-Yan class, see:

16) Danilo Jimenez Rezende and Alejandro Perez, "4d Lorentzian Holst action with topological terms", available as arXiv:0902.3416.

Addenda: I thank Dan Christensen for catching an error in my quick history of the graviton propagator calculations, and Derek Wise for catching some other errors. Urs Schreiber asked a question about how the singularity gets avoided in loop quantum cosmology:

> *My impression is the following, I'd be grateful to hear what is wrong about it.*
>
> *What happens is a special case of this: start with a differential equation whose solutions diverge at the origin. Replace it with a difference equation on a subset of points that does not include the origin. This will have solutions not showing the singularity.*

I replied:

> *This may have been true for some early models — there have been a lot of models between 2001 and now. It's definitely not true for the currently popular models that Ashtekar was explaining.*
>
> *It's true that a differential equation is getting replaced by a difference equation. But the singularity is not being avoided simply by "stepping over it". That would be a cheap trick. In the models under study now, there really is an effective "force" that prevents the singularity. And it arises from the mechanism I sketched:*
>
>> *Instead of talking about the curvature of spacetime at infinitesimally small scales, we can only measure curvature by carrying a particle around a finite-sized loop. This has little effect when spacetime is only slightly curved, but when the curvature is big it makes a big difference. This causes an effect very much like a force that prevents the Universe from crushing down to nothing as we follow it backwards in time.*
>
> *A bit more precisely — but still quite roughly: if the curvature is c, in loop quantum gravity we don't have a quantum operator corresponding to c. Instead we have something more like $\exp(ic)$. So we use something like $\sin(c) = (\exp(ic) - \exp(-ic))/2i$ as a substitute for c in certain equations. When the curvature is small these are almost the same. When it gets big, they're different. This gives an effective force that prevents the singularity. This force starts getting big when the density is about $1/100$ of the Planck density. When the density hits $.41$ times the Planck density, it's enough to create a "bounce".*
>
> *(The math is actually a bit more complicated than what I said, but it's similar in spirit.)*
>
> *In fact, a bunch of calculations have shown that the quantum dynamics are quite nicely approximated by an "effective Friedmann equation" — a differential equation like the usual classical one that describes the Big Bang, but with an extra term.*
>
> *And, the solutions of the difference equation are very close to the solutions of this differential equation.*

Then Urs asked for more detail, and I admitted that the references given above might not satisfy him:

> *... you might prefer Effective equations for isotropic quantum cosmology including matter by Bojowald, Hernandez and Skirzewski.*

> *In Section 2, they start with the Friedmann equation for a homogeneous isotropic cosmology with $k = 0$ and a massless scalar field φ. In Section 2.1 they "deparametrize" this equation, eliminating the time variable by using φ as a clock field, or "internal time" — a standard way of dealing with the problem of time in quantum cosmology. In Section 2.2 they review the standard Wheeler-DeWitt quantization of the resulting theory.*
>
> *Then, in Section 2.3, they switch to loop quantization. They don't give a vast amount of detail on why one should do this, but they do say what is being done. Briefly, instead of the variable $c = da/d\tau$ (the rate of expansion of the universe, closely related to spacetime curvature in this model) we switch to using $\exp(ic)$ and $\exp(-ic)$. In a deeper treatment this comes from using holonomies instead of the curvature at a point. This is what produces the effective force that prevents the singularity.*
>
> *They don't derive difference equations in this paper; instead, they just derive effective corrections to the usual Wheeler-DeWitt equation. Calculations have shown that solutions of the resulting differential equations closely match those of the difference equations that a more thorough approach yields. So, as far as physical intuition goes, perhaps the most important thing is to see where these effective corrections to the Wheeler-DeWitt equation come from, and why they prevent the singularity.*

For more discussion visit the n-Category Caf.

> Many fine physicists have burned away their lives grappling with the problem of quantum gravity.
>
> — *R. P. Woodard*

Week 281

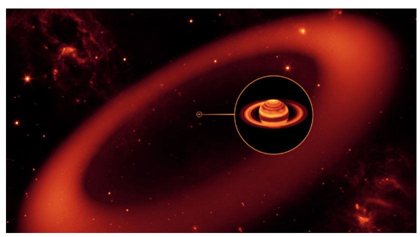
October 19, 2009

This week I'd like to finish my news report from the Corfu summer school on quantum gravity. You'll hear how strings meet loops in BF theory, and how the Poincar 2-group gives a spin foam model that mimics flat Minkowski spacetime.

But first: Timurid tiling patterns with 5-fold and 10-fold quasisymmetry, and the astronomy pictures of the week!

If you listen to the news, you probably heard that NASA discovered an enormous diffuse ring around Saturn. They did it using the Spitzer Space Telescope, a satellite equipped with a telescope that detects infrared light. In "Week 243" I showed you infrared light from the first stars in the Universe, and in "Week 257" I talked about magnesium and iron oxide dust emanating from the Red Rectangle. Both of those were discovered using the Spitzer.

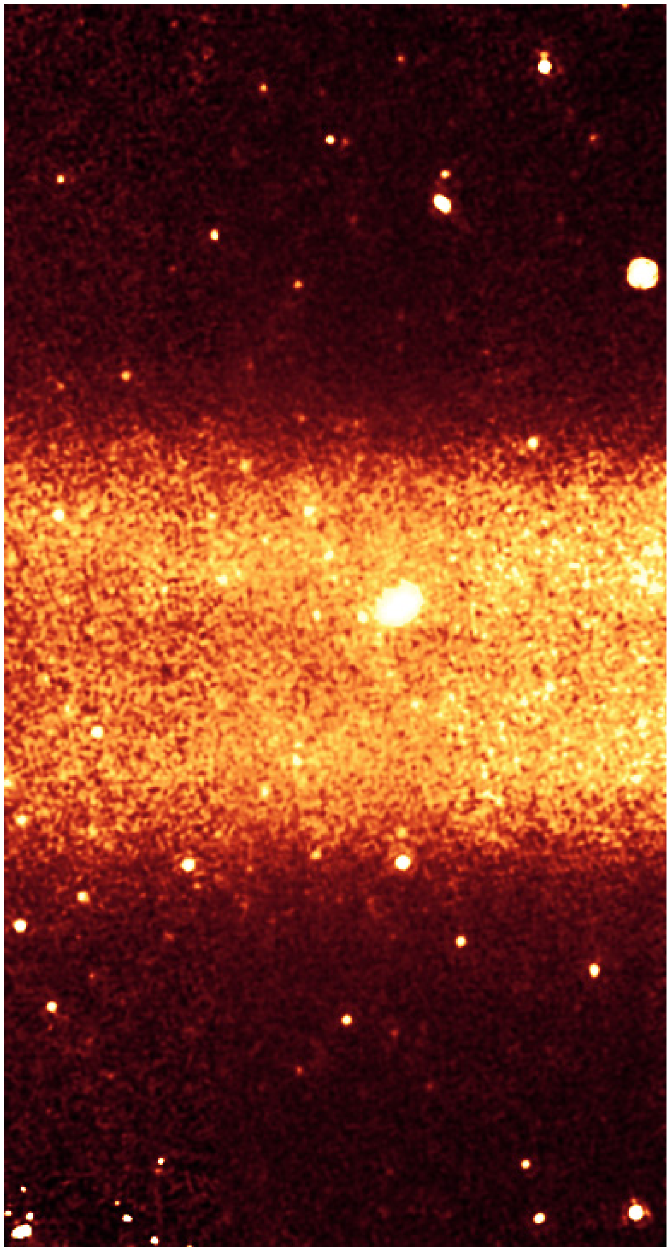
Here's what the new ring would look like if you could see it:



1) Jet Propulsion Laboratory, "NASA space telescope discovers largest ring around Saturn", October 6, 2009, `http://www.jpl.nasa.gov/news/news.cfm?release=2009-150`

As you probably heard, it dwarfs all the visible rings, and it's tilted relative to them.

But even cooler is what the Spitzer Space Telescope actually saw:



2) NASA, "Big band of dust", `http://www.nasa.gov/mission_pages/spitzer/multimedia/spitzer-20091007d.html`

It's an edge-on view of the new ring. It's fat: 20 Saturns thick. And if you look carefully, you'll see that it has two layers, with a bit of a gap in the middle. According to the scientists who discovered it, this is consistent with its origin:

3) Anne Verbiscer, Michael Skrutskie, and Doug Hamilton, "Saturn's largest ring", Nature, October 7, 2009.

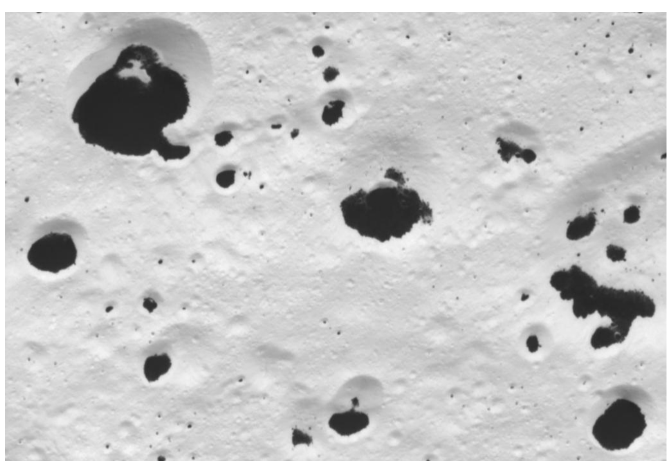
The point is that this ring surrounds the orbit of Saturn's moon Phoebe — a meteor-scarred hulk 100 kilometers across. While Phoebe looks like an asteroid, it's probably an interloper from the outer Solar System, because it's made of ice... but it's covered with a layer of dark material.

The newly discovered ring seems to be made of this dark stuff, blasted away from Phoebe by meteorite collisions. And its discoverers say the double-layered structure is characteristic of rings formed this way from moons with inclined orbits. (Jupiter also has some faint rings like this, poetically known as the "gossamer rings".)

What's really exciting about this new ring is that it explains one of the big mysteries of the Solar System: the dark spot on Saturn's moon Iapetus!

Iapetus is mostly icy, but one side is covered with dark stuff... probably cyanides and carbon-rich minerals. Now it seems this stuff was picked up from the newly discovered ring! It seems to have landed in lumps — mainly on the leading side of Iapetus. You see, this moon is locked in synchronous rotation with Saturn, just like our Moon always shows the same face to Earth. So, one side plows through space and picks up debris, while the other stays clean.

Here's a closeup of some lumps of dark stuff on Iapetus, taken by the Cassini probe:



4) NASA Photojournal, "Spotty Iapetus", `http://photojournal.jpl.nasa.gov/catalog/PIA08382`

NASA Photojournal, "Inky stains on a frozen moon", `http://photojournal.jpl.nasa.gov/catalog/PIA08374`

As you can see, in this region of Iapetus the dark stuff is found at the bottoms of craters. It could have formed these craters by impact, but its presence could also gradually make these craters deeper: the dark stuff should absors more sunlight and warm the nearby ice, making it "sublimate": that is, turn into water vapor.

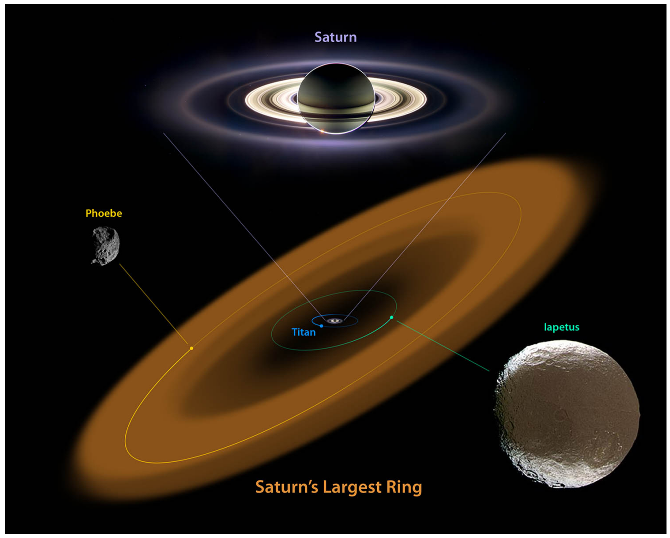
Because it's locked in synchronous rotation with Saturn, the "day" on Iapetus is equal to one period of rotation, namely 79 of our Earth days. So, it's probably the warmest place in the Saturnian system during the daytime. Not very warm: just 113 kelvin on the ice. That's $-160°\mathrm{C}$! But in the dark regions it should be about 138 kelvin. This extra warmth should make more ice sublimate, making them even darker. It's been estimated that over one billion years the very dark regions would lose about 20 meters of ice to sublimation, while the light-colored regions would lose only 10 centimeters, not even counting the ice transferred from the dark regions.

If you want more, there's a great introduction to Saturn's rings in this blog, followed by a nontechnical summary of the new paper on the Phoebe ring:

5) Emily Lakdawalla, "The Phoebe ring", *The Planetary Society Blog*, October 14, 2009, `http://planetary.org/blog/article/00002165/`

As Lakdawalla points out, discovering a big ring was just the beginning:

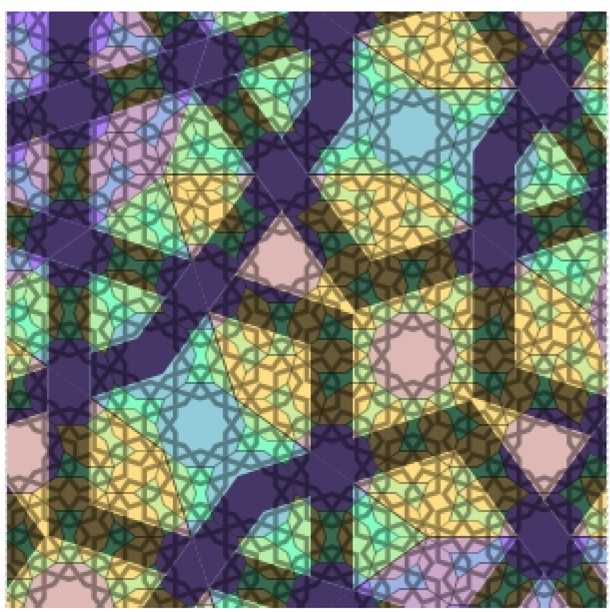
> *So far, it's a cool result but it's sort of like stamp collecting — we discovered a new X and described it, done. Where the paper gets really interesting is when the authors explore what happens to the particles in Phoebe's ring over time, something that you can model by writing down a few equations that describe the orbit of a particle, include Saturn, Phoebe, Iapetus, and Titan, include the masses, densities, and albedos of the particles, and the effects of incident sunlight.*
>
> *What happens to particles depends upon their size. The biggest chunks, several centimeters in size or larger, don't really migrate anywhere, sticking around near Phoebe's orbit until they smack into something — each other or Phoebe. The model simulation suggests that it would take more than the age of the solar system for half of the particles to be removed from the system by re-collision with Phoebe, so most of the biggest chunks are still out there somewhere in Phoebe's orbital space.*
>
> *What about smaller particles? The article says "re-radiation of absorbed sunlight exerts an asymmetric force on dust grains, causing them to spiral in towards Saturn with a characteristic timescale of* $1.5 \times 10^5 r$ *years, where* r *is the particle radius in micrometers. This force brings all centimetre-sized and smaller material to Iapetus and Titan unless mutual particle collisions occur first.... Most material from 10 micrometres to centimetres in size ultimately hits Iapetus, with smaller percentages striking Hyperion and Titan." This would be a slow process that has operated continuously since whenever Phoebe was captured into Saturn's orbit. There might have been bursts of material delivered to Iapetus associated with some of the bigger impacts that have left such large scars on Phoebe, but they would have been blips above a steady background.*



[Next: tilings. The science fiction writer Greg Egan is also a professional programmer, and he's written a remarkable collection of Java applets, which you can see on his website. Here's the latest:]

6) Greg Egan, *Girih*, `http://www.gregegan.net/APPLETS/32/32.html`

This program generates quasiperiodic tilings with approximate 10-fold rotational symmetry using a method called "inflation". The idea of inflation is to take a collection of tiles and repeatedly subdivide each one into smaller tiles from the same collection. Egan's applet shows the process of inflation at work: patterns zooming in endlessly!



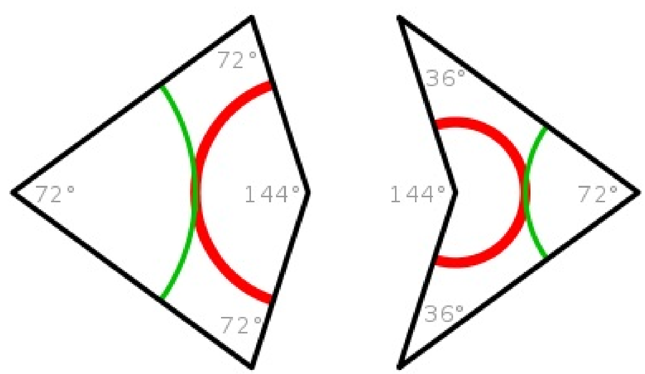
Some of the math behind this is modern, but some goes back to the Timurids: the dynasty founded by the famous conqueror Timur, also known as Tamerlane. By 1400, the Timurid empire was huge. It included most of central Asia, Iran, and Afghanistan, as well as large parts of Pakistan, India, Mesopotamia and the Caucasus. Its capital was the magical city of Samarkand.

The Timurids raised the art of tiling to its highest peak. Islamic artists had already explored periodic tilings with most of the 17 mathematically possible "wallpaper groups" as symmetries — for more on this, see my tour of the Alhambra in "Week 267". What was left to do? Well, periodic tilings can have 2-fold, 3-fold, 4-fold, or 6-fold rotational symmetry, but nothing else. Notice the gap? It's the number 5! So that's what they tackled.

Precisely because you *can't* produce periodic tilings with 5-fold rotational symmetry, it's a delightful artistic challenge to fool the careless eye into thinking you've done just that.

In the 1970's, Penrose discovered quasiperiodic patterns with approximate 5-fold symmetry — for example, patterns made of two tiles, called "kites" and "darts":

- kite: a convex quadrilateral with interior angles of $2\pi/5$, $2\pi/5$, $2\pi/5$ and $4\pi/5$ as you march around it.
- dart: a nonconvex quadrilateral with interior angles of $2\pi/5$, $\pi/5$, $6\pi/5$ and $\pi/5$.



The work of Penrose launched a huge investigation into quasiperiodic tilings and quasicrystals. With their eyes opened, modern scientists saw how fascinating the old Timurid tilings were:

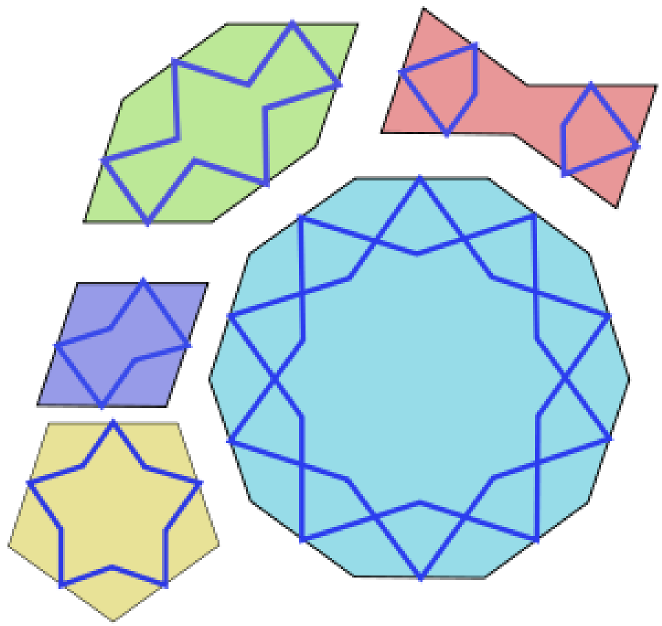
7) Peter J. Lu and Paul J. Steinhardt, "Decagonal and quasi-crystalline tilings in medieval Islamic architecture", *Science* **315** (2007), 1106–1110.

Lu and Steinhardt described a set of 5 tiles which seem to underlie a lot of Timurid designs:

- a regular pentagon with five interior angles of $3\pi/5$.
- a regular decagon with ten interior angles of $4\pi/5$.

- a rhombus with interior angles of $2\pi/5$, $3\pi/5$, $2\pi/5$, $3\pi/5$.
- an elongated hexagon with interior angles of $2\pi/5$, $4\pi/5$, $4\pi/5$, $2\pi/5$, $4\pi/5$, $4\pi/5$.
- a bow tie (non-convex hexagon) with interior angles of $2\pi/5$, $2\pi/5$, $6\pi/5$, $2\pi/5$, $2\pi/5$, $6\pi/5$.

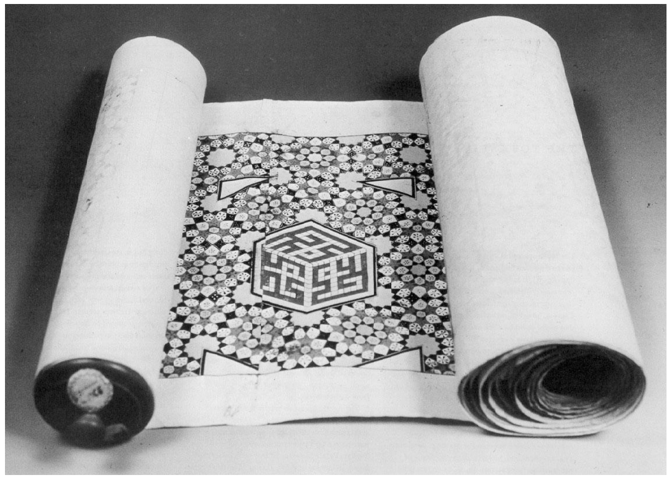
All the edges of all these tiles have the same length:



There are lots of ways to fit them together. The rhombus can be subdivided into a kite and a dart, too!

Lu and Steinhardt call them "girih tiles". But "girih" actually means "strapwork": the braided bands that decorate the tiles in a lot of this art, as shown rather crudely in the picture above. Egan's applet uses three of these tiles: the decagon, the elongated hexagon and the bowtie. As you'll see on his webpage, each can be subdivided into smaller decagons, hexagons and bowties. And that's how "inflation" works.

Did the Timurid artists actually understand the process of inflation, or the idea of a quasiperiodic tiling? Seeking clues, scholars have turned to the Topkapi Scroll, a kind of "how-to manual" for tiling that resides in the Topkapi Palace in Istanbul.



I would love to get my paws on this color reproduction:

8) Gulru Necipoglu and Mohammad al-Asad, *The Topkapi Scroll — Geometry and Ornament in Islamic Architecture*, Getty Publications, 1996.

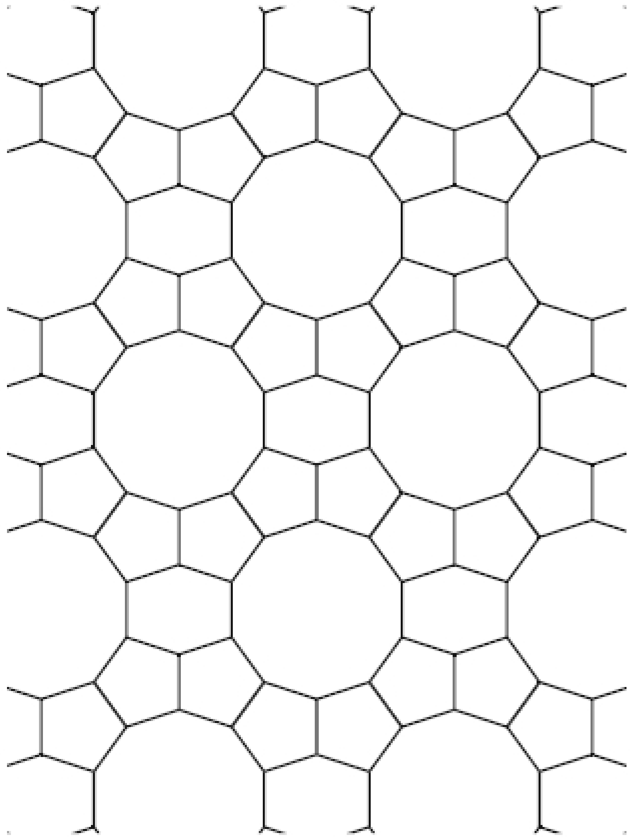
For now, the best substitute I've found is this beautiful article:

9) Peter R. Cromwell, "The search for quasi-periodicity in Islamic 5-fold ornament", *Math. Intelligencer* **31** (2009), 36–56. Also available at `http://www.springerlink.com/content/760261153n347478/?p=405b9dbf45ea4f4793a097b6e12dcb08pi=7`

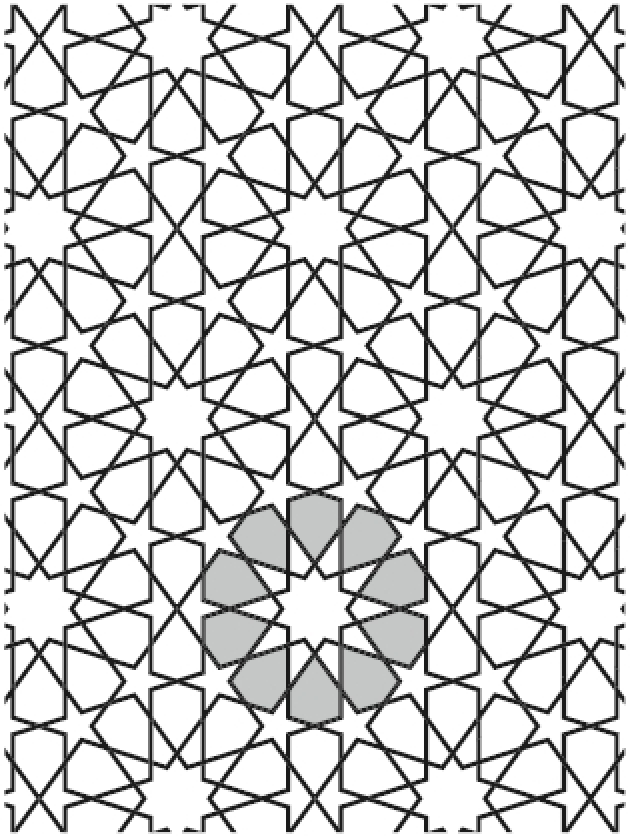
The Mathematical Intelligencer is a wonderful magazine put out by Springer Verlag. It's recently become available online — and to my shock, the above article is free! Springer doesn't give much away, so I can't help but fear this is an oversight on their part, soon to be corrected. So, grab a copy of this article *now*.

Cromwell argues that we shouldn't attribute too much modern mathematical knowledge to the Timurid tile artists. But the really great thing about this article is the detailed information on how some of these tiling patterns are made — including lots of pictures. It repays repeated study.

For an easy example, consider these regular decagons surrounded by regular pentagons and funky hexagons of a different sort than those in the girih tiles:



By replacing each decagon with a 10-pointed star, each pentagon with a 5-pointed star, and extending the lines outward in a clever way, he gets this magnificent design — one of the most widespread star patterns in Islamic art:



Here's a less mathematical and more historical introduction to the Timurid tile artists, also with lots of nice pictures:

10) Sebastian R. Prange, "The tiles of infinity", Saudi Aramco World (October–November 2009), 24–31. Also available at `http://www.saudiaramcoworld.com/issue/200905/the.tiles.of.infinity.htm`

You should also check out Craig Kaplan's work. He's studied Kepler's work on patterns built from decagons, and written software that generates beautiful star patterns:

11) Craig Kaplan, "The trouble with five", *Plus Magazine* **45** (December 2007), available at `http://plus.maths.org/issue45/features/kaplan/`

12) Craig Kaplan, "A meditation on Kepler's Aa, in Bridges 2006: Mathematical Connections" in *Art, Music and Science*, 2006, pp. 465–472. Also available at `http://www.cgl.uwaterloo.ca/~csk/papers/bridges2006a.html`

13) Craig Kaplan, "Taprats: computer generated Islamic star patterns", `http://www.cgl.uwaterloo.ca/~csk/washington/taprats/`

Together with David Salesin, he's also gone beyond the old masters by studying tilings in spherical and hyperbolic geometry:

14) Craig S. Kaplan and David H. Salesin, "Islamic star patterns in absolute geometry", *ACM Transactions on Graphics* **23** (April 2004), 97–119. Also available at http://www.cgl.uwaterloo.ca/~csk/papers/tog2004.html

Another key player in this business is Eric Broug:

15) Broug Ateliers: "Islamic Geometric Design", http://www.broug.com/

Check out the nice photo gallery and the lesson on 5-fold symmetry! He sells beautiful screens and other products. But I bought this book, which explains how to make the patterns yourself:

16) Eric Broug, *Islamic Geometric Patterns* (book with CD-ROM), Thames and Hudson, 2008.

Even if you don't have the skill or time to draw these patterns, the book is worthwhile for the pictures and explanations.

I'll list a bunch more references below, for when I retire and get time to devote myself more deeply to this subject. But now — on to Corfu!

Last time I said a bit about what I learned in Ashtekar and Rovelli's courses. Now I'd like to talk about some other things I learned in Corfu — some things I find even more tantalizing.

In "Week 232", I explained how gravity in 3d spacetime automatically contains within it a theory of point particles, and how a 4d analogue of 3d gravity automatically contains within it a theory of string-like objects. This 4d theory is called BF theory. Like 3d gravity, it describes a world where spacetime is flat. So, it's boring compared to full-fledged 4d gravity — so boring that we can understand it much better! In particular, unlike 4d gravity, we understand a lot about what happens when you take quantum mechanics into account in 4d BF theory.

But when you remove a surface from spacetime in 4d BF theory, it springs to life! In particular, the surface acts a bit like the worldsheet of a string. It doesn't behave like the strings in ordinary string theory. But Winston Fairbairn has been thinking about this a lot:

17) Winston J. Fairbairn and Alejandro Perez, "Extended matter coupled to BF theory", *Phys. Rev.* **D78**:024013, 2008. Also available as arXiv:0709.4235.

18) Winston J. Fairbairn, "On gravitational defects, particles and strings", *JHEP* **0809**:126, 2008. Also available as arXiv:0807.3188.

19) Winston J. Fairbairn, Karim Noui and Francesco Sardelli, "Canonical analysis of algebraic string actions", available as arXiv:0908.0953

And it turns out that if we impose the constraints on BF theory that turn it into general relativity, we obtain the usual Nambu-Goto string, where the action is the area! However, the last of the three papers above shows there are some subtle differences.

I need to think about this a lot more. It was always my hope to reconcile string theory and loop quantum gravity, and this could be the way. Of course, reconciling two things that don't work doesn't necessarily give one that does. A pessimist might say that combining string theory and loop quantum gravity is like combining epicycles and aether. But I'm optimistic. Something interesting is going on here.

In a different but possibly related direction, Aristide Baratin gave a talk on recent work he's been doing with Derek Wise and Laurent Freidel. You can get a feel for this work from this paper:

20) Aristide Baratin, Derek K. Wise, "2-Group representations for spin foams", to appear in proceedings of the *XXV Max Born Symposium: The Planck Scale*, Wroclaw, Poland. Also available as arXiv:0910.1542.

In "Week 235" I mentioned an amazing paper by Baratin and Freidel called "Hidden quantum gravity in 4d Feynman diagrams: emergence of spin foams". They described a spin foam model that acts just like 4-dimensional flat Minkowski spacetime: couple it to interacting point particles, and you get the usual Feynman diagrams described in a new way!

The big news is that this spin foam model comes from the representations of a 2-group, instead of a group. Namely, the Poincar 2-group. This is a 2-group I invented which has Lorentz transformations as objects and translations as endomorphisms of any object.

The Poincar 2-group spin foam model was first studied by Crane, Sheppeard and Yetter. Baratin, Freidel, Wise and I spent a long time developing the theory of infinite-dimensional representations of 2-groups needed to make this model precise — see "Week 274" for more on all this. Now the details are falling into place, and a beautiful picture is emerging.

I should admit that the paper by Baratin and Wise deals with the Euclidean rather the Lorentzian version of this picture. I hope this is merely because the representation theory of the "Euclidean 2-group" is more tractable than that of the Poincar 2-group. I hope everything generalizes to the Lorentzian case.

A lot to think about.

To wrap up, here's a big list of references from Cromwell's paper on tilings I hadn't known so much had been written about this subject!

21) M. Arik and M. Sancak, "Turkish-Islamic art and Penrose tilings", *Balkan Physics Letters* **15** (1 Jul 2007) 1–12.

22) J. Bonner, "Three traditions of self-similarity in fourteenth and fifteenth century Islamic geometric ornament", *Proc. ISAMA/Bridges: Mathematical Connections in Art, Music and Science*, (Granada, 2003), eds. R. Sarhangi and N. Friedman, 2003, pp. 1–12.

23) J. Bonner, "Islamic Geometric Patterns: Their Historical Development and Traditional Methods of Derivation", unpublished manuscript.

24) J. Bourgoin, *Les Elements de l'Art Arabe: Le Trait des Entrelacs*, Firmin-Didot, 1879. Plates reprinted in Arabic Geometric Pattern and Design, Dover Publications, 1973.

25) J.-M. Castira, *Arabesques: Art Decoratif au Maroc*, ACR Edition, 1996.

26) J.-M. Castira, "Zellijs, muqarnas and quasicrystals", *Proc. ISAMA*, (San Sebastian, 1999), eds. N. Friedman and J. Barrallo, 1999, pp. 99–104.

27) G. M. Fleurent, *Pentagon and decagon designs in Islamic art, Fivefold Symmetry*, ed. I. Hargittai, World Scientific, 1992, pp. 263–281.

28) B. Grunbaum and G. C. Shephard, *Tilings and Patterns*, W. H. Freeman, 1987.

29) E. H. Hankin, "On some discoveries of the methods of design employed in Mohammedan art", *J. Society of Arts* **53** (1905) 461–477.

30) E. H. Hankin, "The Drawing of Geometric Patterns in Saracenic Art", *Memoirs of the Archaeological Society of India*, no **15**, Government of India, 1925.

31) E. H. Hankin, 'Examples of methods of drawing geometrical arabesque patterns', *Math. Gazette* **12** (1925), 370–373.

32) E. H. Hankin, "Some difficult Saracenic designs II", *Math. Gazette* **18** (1934), 165–168.

33) E. H. Hankin, "Some difficult Saracenic designs III", *Math. Gazette* **20** (1936), 318–319.

34) A. J. Lee, "Islamic star patterns", *Muqarnas IV: An Annual on Islamic Art and Architecture*, ed. O. Grabar, Leiden, 1987, pp. 182–197.

35) P. J. Lu and P. J. Steinhardt, 'Response to Comment on "Decagonal and quasicrystalline tilings in medieval Islamic architecture"', *Science* **318** (30 Nov 2007), 1383.

36). F. Lunnon and P. Pleasants, "Quasicrystallographic tilings", *J. Math. Pures et Appliques* **66** (1987), 217–263.

37) E. Makovicky, "800-year old pentagonal tiling from Maragha, Iran, and the new varieties of aperiodic tiling it inspired", *Fivefold Symmetry*, ed. I. Hargittai, World Scientific, 1992, pp. 67–86.

38) E. Makovicky, 'Comment on "Decagonal and quasi-crystalline tilings in medieval Islamic architecture"', *Science* **318** (30 Nov 2007), 1383.

39) E. Makovicky and P. Fenoll Hach-Alm, "Mirador de Lindaraja: Islamic ornamental patterns based on quasi-periodic octagonal lattices in Alhambra, Granada, and Alcazar, Sevilla, Spain", *Boletin Sociedad Espanola Mineralogia* **19** (1996), 1–26.

40) E. Makovicky and P. Fenoll Hach-Alm, "The stalactite dome of the Sala de Dos Hermanas — an octagonal tiling?", *Boletin Sociedad Espanola Mineralogia* **24** (2001), 1–21.

41) E. Makovicky, F. Rull Pirez and P. Fenoll Hach-Alm, "Decagonal patterns in the Islamic ornamental art of Spain and Morocco", *Boletmn Sociedad Espanola Mineralogia* **21** (1998), 107–127.

42) J. Rigby, "A Turkish interlacing pattern and the golden ratio", *Mathematics in School* **34** no 1 (2005), 16–24.

43) J. Rigby, "Creating Penrose-type Islamic interlacing patterns", Proc. *Bridges: Mathematical Connections in Art, Music and Science*, (London, 2006), eds. R. Sarhangi and J. Sharp, 2006, pp. 41–48.

44) F. Rull Pirez, "La nocion de cuasi-cristal a traves de los mosaicos arabes", *Boletin Sociedad Espanola Mineralogia* **10** (1987), 291–298.

45) P. W. Saltzman, "Quasi-periodicity in Islamic ornamental design", *Nexus VII: Architecture and Mathematics*, ed. K. Williams, 2008, pp. 153–168.

46) M. Senechal, *Quasicrystals and Geometry*, Cambridge Univ. Press, 1995.

47) M. Senechal and J. Taylor, "Quasicrystals: The view from Les Houches", *Math. Intelligencer* **12** (1990) 54–64.

Reference 24, the book by Bourgoin, is a classic — and the Dover version is probably quite affordable. Cromwell also lists some more websites:

48) ArchNet, "Library of digital images of Islamic architecture", `http://archnet.org/library/images/`

49) E. Harriss and D. Frettlh, "Tilings Encyclopedia", `http://tilings.math.uni-bielefeld.de/`

50) P. J. Lu and P. J. Steinhardt, "Decagonal and quasi-crystalline tilings in medieval Islamic architecture", supporting online material, `http://www.sciencemag.org/cgi/content/full/315/5815/1106/DC1`

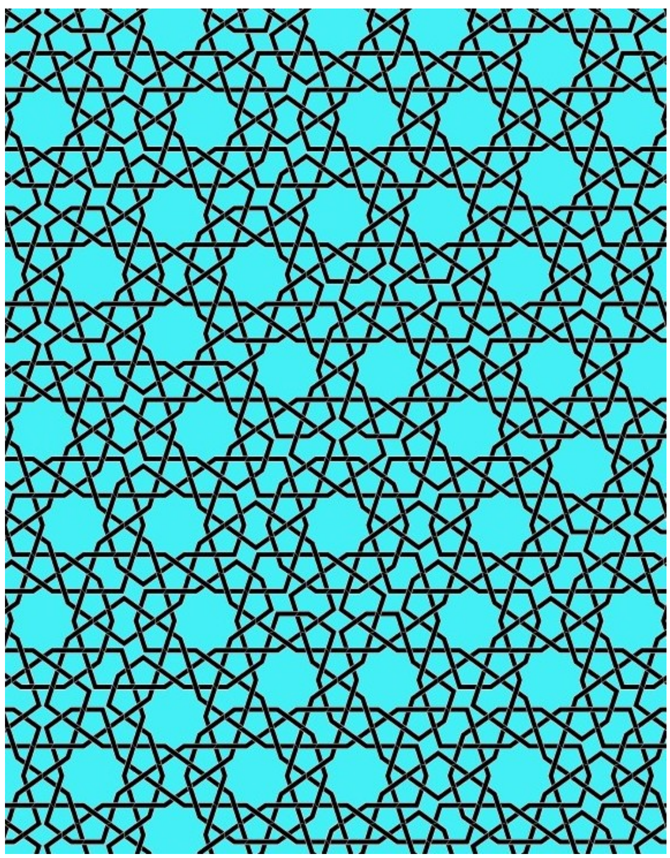
51) D. Wade, "Pattern in Islamic Art: The Wade Photo-Archive", `http://www.patterninislamicart.com/`

The last one is a huge treasure trove of images!

Addenda: Greg Egan writes:

> *I wrote a new version of the Girih applet, which scrolls across an infinite quasiperiodic tiling at a single scale. (I start with a Penrose rhombic tiling that I construct by de Bruijn's method, and then convert into a tiling of decagons, hexagons and bowties.)*
>
> *This one can be run in full-screen mode.*

Here's a sample of what it produces:



I got an email Craig Kaplan, whose wonderful work on tilings I mentioned above. He writes:

Because of the content of your post, I can't help but offer a few notes about what you said. Feel free to use these any way you want, or file them away for later.

I wouldn't say that the Timurids set out to tackle fivefold tilings. They looked at a lot of geometry in general — it's not clear to me that they devoted any more energy to 5 than any other number. But they did produce amazing results!

You should be aware that within the Islamic geometric art community, there's a fair amount of controversy and resentment surrounding the Lu and Steinhardt paper. First, the paper contains very strong claims that aren't supported by evidence. Even if the artisans had some understanding of inflation (which is debatable), I don't think there's any way they would have had a notion of quasiperiodicity. Second, several researchers perceive that L&S muscled their way into unfamiliar territory without really finding out what had been done before — one could argue that most of the work in their paper was well known to the community. Finally, the paper made its mark not because of the originality of its contribution, but because Science *rolled out an enormous publicity machine around the paper's release. This is something that academics can't really control for, and which I still find a bit baffling.*

Man, I'd also love to get my hands on Necipoglu's book on The Topkapi Scroll. I knew of the book when it was in print, and didn't buy it.

Cromwell's article was in part a response to Lu & Steinhardt's. You also might be interested in three upcoming articles of his, to appear in the Journal of Mathematics and the Arts (for which I'm an associate editor):

- *"Islamic geometric designs from the Topkapi Scroll I: Unusual arrangements of stars".*
- *"Islamic geometric designs from the Topkapi Scroll II: A modular design system".*
- *"Hybrid 1-point and 2-point constructions for some Islamic geometric designs".*

Hopefully they'll be out soon.

In the meantime, I might also add that I did a bit of work on understanding the origin of strange tilings like the one you show with decagons, pentagons, and funky hexagons. It's in this paper, which you didn't link to:

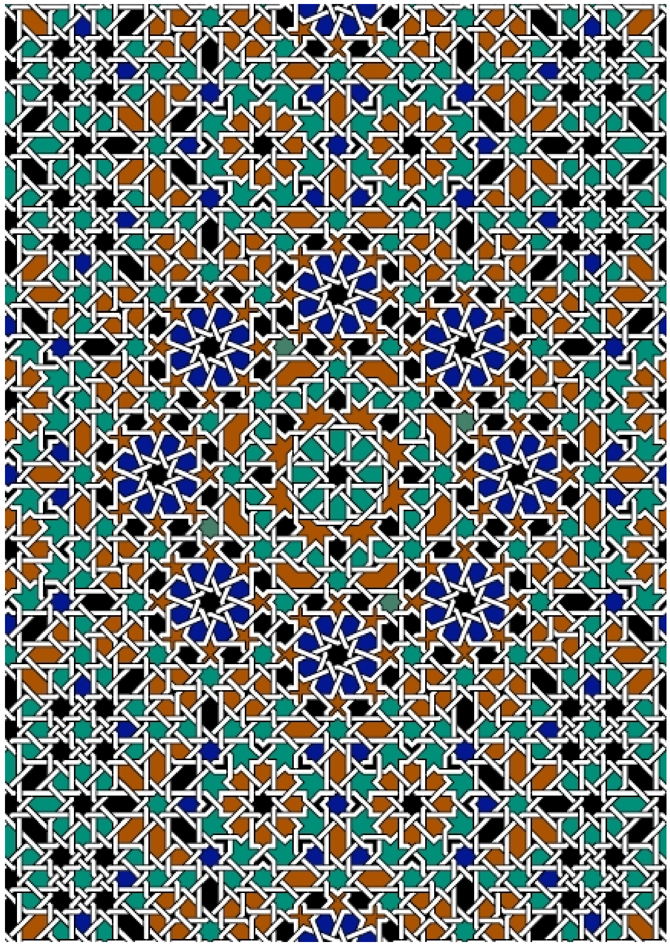
52) Craig S. Kaplan, "Islamic star patterns from polygons in contact", in GI '05: Proceedings of the 2005 conference on Graphics Interface*, 2005. Also available at* `http://www.cgl.uwaterloo.ca/~csk/papers/gi2005.html`

Hope that's useful to you, and thanks for the mention.

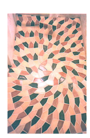
Brian Wichmann pointed out this online database:

53) Brian Wichmann, "A tiling database", `http://www.tilingsearch.org/`

Here's a database entry from the Alhambra:



Michael D. Hirschhorn emailed me to say that nearly 30 years ago, he and David C. Hunt published a paper in the *Journal of Combinatorial Theory* classifying all tilings of the plane by identical convex equilateral pentagons. The most famous appears to be the "Hirschhorn medallion". Bob Jenkins used it to tile his bathroom:



Later Hirschhorn and Hunt extended their result to cover all *non*-convex equilateral tilings, but this has never been published.

Presumably this page is based on Hirschhorn and Hunt's work:

54) MathPuzzle, "The 14 different types of convex pentagons that tile the plane", available at `http://www.mathpuzzle.com/tilepent.html`

For more discussion visit the n-Category Caf.

> The arabesques displayed a profound use of mathematical principles, and were made up of obscurely symmetrical curves and angles based on the quantity of five.
>
> — *H. P. Lovecraft*

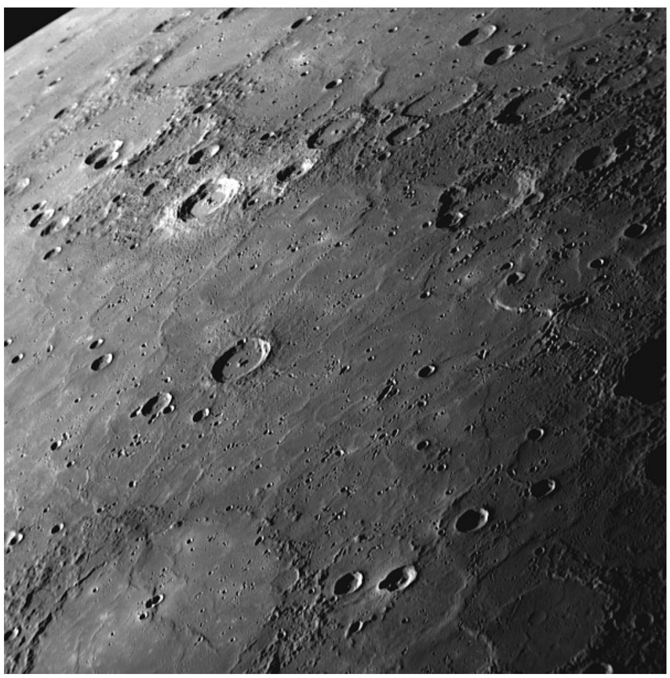
Week 282

October 29, 2009

This week I'll get back to explaining some serious math: the relation between associative, commutative, Lie and Poisson algebras, and how this relates to quantization. There's some beautiful algebra and combinatorics that shows up here: linear operads, their generating functions, and Stirling numbers of the first kind.

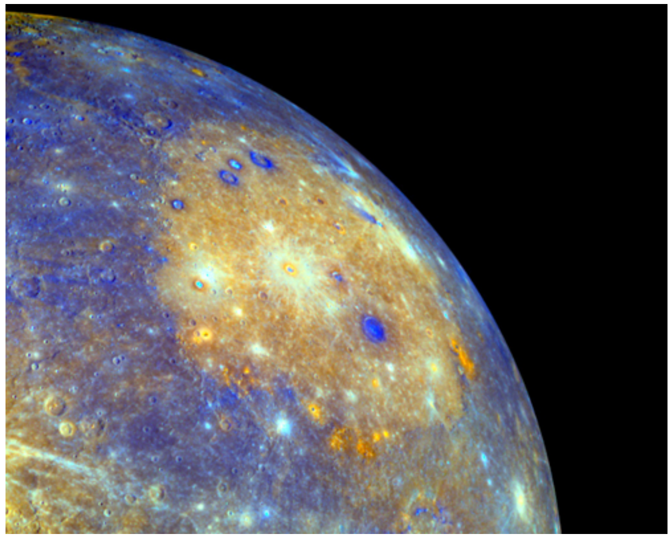
But first: the astronomy picture of the week! Lately we've been exploring the moons of Saturn — first Enceladus in "Week 272" and "Week 273", and then Phoebe and Iapetus in "Week 281". Someday we should talk about Rhea — a moon of Saturn with its own rings. But first let's take a big detour and sail in to Mercury.

In fact, the Messenger probe sailed in to Mercury starting on August 3, 2004. It's flown past this planet several times, and in March 2011 it's scheduled to orbit Mercury for a whole year. It's already taken some detailed photos:



1) Messenger, "Image gallery", http://messenger.jhuapl.edu/gallery/sciencePhotos/

Superficially Mercury looks like the Moon, and thus not very exciting. But it's actually very different. First of all, parts of Mercury get really hot: about 430 Celsius near the equator during the day — considerably above the melting point of lead. Second, permanently shaded regions near the poles are not only cold, they actually have lots of ice! Third, Mercury had a violent past. For example, the Caloris basin on Mercury is one of the solar system's largest impact basins. Formed by a huge asteroid impact long ago, it's about 1,500 kilometers across:

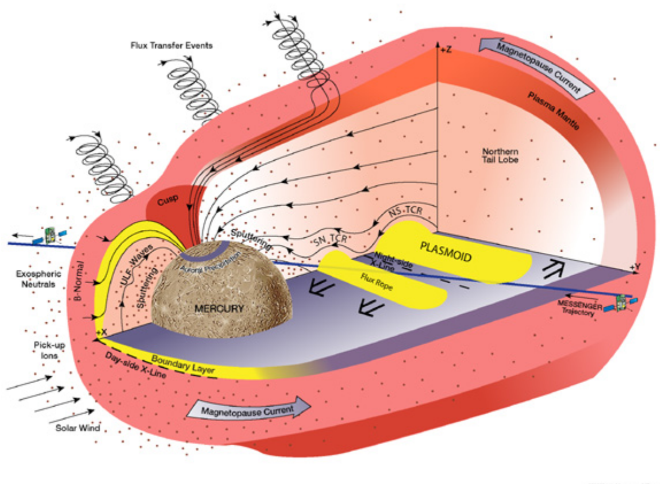


Caloris basin shown in yellow in false color image. Orange hues just inside the basin's rim are features thought to

2) NASA, "New discoveries at Mercury", August 3, 2008. http://science.nasa.gov/headlines/y2008/03jul_mercuryupdate.htm

Fourth, Mercury is the densest planet, with the highest percentage of iron. Why is this? There are various theories. The most widely accepted is reminiscent of the "giant impact theory" for how our Moon formed (see "Week 273"). It goes like this. Once upon a time Mercury was over twice the size it is now, with a more ordinary chemical composition. Then it was hit by another body about 1/6 its own mass! This stripped off a lot of its crust and mantle, leaving a smaller Mercury, whose iron core now accounts for a greater percentage of its mass.

Fifth, and a direct consequence of the previous point, Mercury has a strong magnetic field — like Earth, and unlike Venus, Mars, or our Moon. And this brings me to the picture I really want you to stare at: a diagram of how Mercury's magnetic field interacts with the solar wind, which is very powerful so near the Sun. The Messenger probe learned a lot about this when it flew past Mercury on October 6th, 2008:



3) NASA, "Magnetic tornadoes could liberate Mercury's tenuous atmosphere", `http://www.nasa.gov/mission_pages/messenger/multimedia/magnetic_tornadoes.html`

The pink stuff in this picture is the "magnetopause" — the zone where the solar wind crashes into Mercury's magnetic field. And see the spirals? These are "flux transfer events". Every so often, the solar magnetic field lines reconnect with those of Mercury and ions in the solar wind penetrate the magnetopause and rain down on Mercury's north and south poles. Similar flux transfer events happen here on Earth about every 8 minutes:

4) NASA, "Magnetic portals connect Sun and Earth", October 30, 2008. `http://science.nasa.gov/headlines/y2008/30oct_ftes.htm?list179029`

The physics is complex and just starting to be understood: the basic equations governing the interaction of plasma (that is, ionized gas) and electromagnetism are devilishly nonlinear and hard to deal with. This is one reason fusion reactors that use magnetic confinement are so hard to develop. In particular, there's been a lot of recent work on "reconnection", where magnetic fields pointing in opposite directions cross-link and accelerate plasma in a "magnetic slingshot". Here's a great article on that subject:

5) James L. Burch and James F. Drake, "Reconnecting magnetic fields", *American Scientist* **97** (2009), 392–399. Also available at `http://mms.space.swri.edu/AmSci-Reconnection.pdf`

Finally: do you see the yellow "plasmoid" in the picture above? That's a coherent blob of plasma and magnetic field which forms in the the long "magnetotail" behind Mercury. Again, these also form near the Earth. And again, they're complex and mathematically interesting. So, while Mercury may look dead and boring, it's rich in activity if you know where to look!

Next, some math.

Today I'd like to talk about 4 of my favorite kinds of algebras: associative algebras, commutative algebras, Lie algebras and Poisson algebras. They're all important in quantum mechanics and quantization, and they fit together in a very nice way. There's a lot to say about this, but I just want to explain one thing: how the relation between these 4 kinds of algebras gives a pretty pattern involving Stirling numbers.

If you don't know what Stirling numbers are, don't worry! They'll show up on their own accord, and then we'll see why.

First: the four kinds of algebra. Let's review them.

An "associative algebra" is a vector space equipped with an identity element 1 and a binary operation called multiplication that's linear in each argument. We demand that these obey a few rules:

$$1x = x$$
$$x1 = x$$
$$(xy)z = x(yz)$$

In physics, associative algebras often show up as "algebras of observables" — their elements are things you can measure about a physical system.

A "commutative algebra" is an associative algebra that obeys one extra rule:

$$xy = yx$$

In classical mechanics, the algebras of observables are always commutative. The big deal about quantum mechanics is that we drop this rule and allow more general associative algebras. This wreaks havoc on our intuitions about physics, but in a very nice way.

A "Lie algebra" is a vector space with a "bracket" operation which is linear in each argument. We demand that this obeys two rules:

$$= -[y, x]$$
$$[x, [y, z]] = [[x, y], z] + [y, [x, z]]$$

These rules seem a lot scarier than the rules above — at least when you first meet them! The reason is that while ordinary numbers form an associative and even commutative algebra, they don't form a Lie algebra in any interesting way. Sure, you can define $[x, y] = 0$, and it works, but it's dull. The first nontrivial Lie algebra we meet in school may be the space of vectors in 3d space, where the bracket is the cross product. But most students don't even remember that the cross product satisfies the second rule above — the so-called "Jacobi identity". So to get comfortable with Lie algebras, most people need to start with an associative algebra that's not commutative, and then define the bracket by:

$$[x, y] = xy - yx$$

This is called the "commutator", and it's very important in quantum mechanics, in part because it tells you how far things are from being classical. In classical mechanics, the commutators are zero!

There's also a deeper and more important reason why commutators and Lie algebras are important in quantum theory: they show up when we study *symmetries* of physical systems. But that's another story, tangential to today's tale.

Anyway, it's fun — or at least good for your moral development — to check that the associative law for multiplication implies the Jacobi identity when we define the bracket by $[x, y] = xy - yx$.

So, we've got a recipe for turning an associative algebra into a Lie algebra. We've also seen a pathetically easy recipe for turning a commutative algebra into an associative one: just forget that it's commutative!

In the language of category theory, both these recipes are called "forgetful functors", because they lose information. So, we've got forgetful functors

$$\mathsf{CommAlg} \to \mathsf{AssocAlg} \to \mathsf{LieAlg}$$

and this little diagram is the crux of our tale.

But to see why, I need to introduce the fourth character: Poisson algebras. The idea here is to realize that classical mechanics isn't really true: the world is quantum mechanical. So, even when we think our algebra of observables is commutative, it's probably not. This is probably just an approximation. It's not really true that the commutator $[x, y]$ is zero. Instead, it's just tiny.

How do we formalize this? Well, in reality $[x, y]$ is often proportional to a tiny constant called Planck's constant, $\hbar$. When this happens, we can write

$$[x, y] = \hbar\{x, y\}$$

where $\{x, y\}$ is some other element of our associative algebra.

Mathematically, it's more convenient to treat $\hbar$ as a variable than as a fixed number. So, let's suppose we have an associative algebra A with a special element h that commutes with everything. And let's suppose that A is equipped with a new bracket operation $\{x, y\}$ that satisfies the above equation.

Then let's consider the algebra A/hA, which we define by taking A and imposing the relation $h = 0$. This amounts to neglecting quantum effects, so A/hA is called the "classical limit" of our original algebra A.

What is this new algebra like?

Well, first of all, it's associative. Second, it's commutative, since $[x, y]$ was proportional to h, but now we're setting h equal to zero. And third, it inherits from A a bracket operation $\{x, y\}$, called the "Poisson bracket".

What rules does the Poisson bracket satisfy? Well, since

$$[x, y] = -[y, x]$$

we know that

$$h\{x, y\} = -h\{y, x\}$$

in A. So it seems plausible that

$$\{x, y\} = -\{y, x\}$$

in A/hA. Unfortunately I can't derive this from my meager assumptions thus far, since I'm not allowed to divide by h. So let me also assume that multiplication by h is one-to-one in A. Then I know

$$\{x, y\} = -\{y, x\}$$

in A and thus also in A/hA.

Similarly, from the Jacobi identity for the commutator

$$[x,[y,z]] = [[x,y],z] + [y,[x,z]]$$

we know that

$$\{x,\{y,z\}\} = \{\{x,y\},z\} + \{y,\{x,z\}\}$$

in A, and thus also in A/hA. The same sort of argument also shows that $\{x,y\}$ is linear in each argument.

There's one more rule, too! Note that in A we have

$$\begin{aligned} &= xyz - yzx \\ &= xyz - yxz + yxz - yzx \\ &= [x,y]z + y[x,z] \end{aligned}$$

and thus

$$\{x,yz\} = \{x,y\}z + y\{x,z\}$$

So, this rule holds in A/hA too. This rule says that the operation "bracketing with x" obeys the product rule, just like a derivative.

And so, we've been led to the definition of a Poisson algebra! It's a commutative algebra with an extra operation, the Poisson bracket, which is linear in each argument and obeys these rules:

$$\begin{aligned} \{x,y\} &= -\{y,x\} \\ \{x,\{y,z\}\} &= \{\{x,y\},z\} + \{y,\{x,z\}\} \\ \{x,yz\} &= \{x,y\}z + y\{x,z\} \end{aligned}$$

Physically, the idea here is that the Poisson bracket is the extra structure that we get from the fact that classical mechanics arises from quantum mechanics by neglecting quantities proportional to Planck's constant.

Mathematically, the idea is that a Poisson algebra is both a commutative algebra and a Lie algebra (with the Poisson bracket as its bracket), obeying the compatibility condition

$$\{x,yz\} = \{x,y\}z + y\{x,z\}$$

So, besides the forgetful functors I've already drawn, we have two more:

$$\mathsf{PoissonAlg} \to \mathsf{CommAlg}$$

and

$$\mathsf{PoissonAlg} \to \mathsf{LieAlg}$$

But you'll notice that in my above argument I got ahold of the Poisson algebra axioms starting from an *associative* algebra of a special sort: roughly, one that's "noncommutative, but only up to terms of order h". This suggests that Poisson algebras are a halfway house between associative and commutative algebras. And I'd like to make this more precise!

Technically, these special associative algebras are called "deformations" of commutative algebras. And there's a whole branch of mathematical physics called "deformation quantization" that studies them. So, some experts reading the previous paragraph may think I'm about to explain deformation quantization. But much as I'd love to talk about that, I won't now! That theme will have to remain lurking in the background.

Instead, I just want to show how the concept of Poisson algebra emerges from the forgetful functor

$$\mathsf{CommAlg} \to \mathsf{AssocAlg}$$

And to do this, I'll need operads. I explained these back in "Week 191", so if the word "operad" fills you with bewilderment or terror instead of delight, please reread that. But today I'll be using linear operads, so let me explain those.

The concepts of associative algebra and commutative algebra and Lie algebra and Poisson algebra have a lot in common. In every case we start with a vector space and equip it with a bunch of n-ary operations that are linear in each argument. Moreover, these operations are required to satisfy equations where each variable shows up exactly once in each term, like

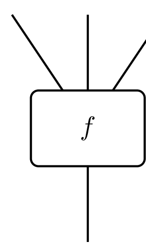
$$\{x, \{y, z\}\} = \{\{x, y\}, z\} + \{y, \{x, z\}\}$$

or

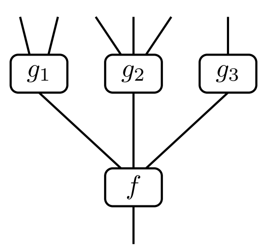
$$\{x, yz\} = \{x, y\}z + y\{x, z\}$$

And this is precisely what linear operads are designed to handle!

More precisely, a linear operad $\mathcal{O}$ consists of a vector space $\mathcal{O}_n$ for each natural number n. We call this the space of "n-ary operations". They're not operations *on* anything yet — they're just "abstract" operations, with names like "multiplication" or "Poisson bracket". We can draw an n-ary operation as a little black box with n wires coming in and one coming out:



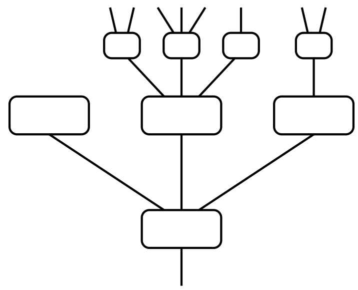
We're allowed to compose these operations in a tree-like fashion:



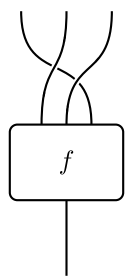
Here we are feeding the outputs of n operations $g_1, \ldots, g_n$ into the inputs of an n-ary operation f, obtaining a new operation which we call

$$f \circ (g_1, \ldots, g_n)$$

Since we're doing *linear* operads todqay, we demand that this composition operation be linear in each argument. Moreover we demand that there be a unary operation serving as the identity for composition, and we impose an "associative law" that makes a composite of composites like this well-defined:

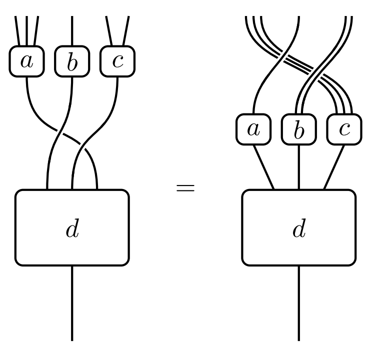


(This picture has a 0-ary operation in it, just to emphasize that this is allowed.) Furthermore, we can permute the inputs of an n-ary operation and get a new operation:



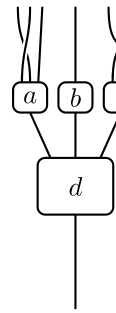
We demand that this give an action of the permutation group on the $\mathcal{O}_n$. And finally, we demand that these permutation group actions be compatible with composition in two ways.

The first way is easy to draw:



We can permute the wires leading into d and then compose it with the operations a, b, c, or compose them in a different order and then permute the wires.

The second way is harder to draw, because both sides of the equation look exactly the same! For example:



Here we can either compose the operations a, b, c with d and then permute the wires leading into the result, or apply permutations to the wires leading into a, b, and c and then compose the resulting operations with d. We get the same answer either way, and indeed the pictures look exactly the same.

We use operads to describe algebras. An "algebra" for a linear operad $\mathcal{O}$ is a vector space V together with maps that turn elements of $\mathcal{O}_n$ into n-ary operations on V that are linear in each argument. If you like representations of groups you might prefer to call this a "representation" of $\mathcal{O}$ on V, since the idea is that elements of $\mathcal{O}_n$ are getting represented as actual operations on the vector space V. Of course we demand that composing operations in $\mathcal{O}$ and permuting their arguments get along with this process.

Let's look at 4 examples.

First, there's an operad Assoc, whose algebras are associative algebras. This operad is generated by one binary operation, called multiplication, and one nullary operation, called 1. We'll write these as if they were actual functions, though it's is not really true until we choose an algebra for this operad. So, we'll write them as

$$(x, y) \mapsto xy$$

and

$$() \mapsto 1$$

The second operation looks funny: it's a "nullary operation", one that takes no inputs. A nullary operation is also known as a "constant", because its output doesn't depend on anything.

Starting from these two operations we can generate lots more by composition and taking linear combinations. Then we impose some relations. First we impose one saying that these two ternary operations are equal:

$$(x, y, z) \mapsto (xy)z$$

and

$$(x, y, z) \mapsto x(yz)$$

I can say this faster, as follows:

$$(xy)z = x(yz)$$

But remember: now I'm not talking about the associative law in any *particular* algebra — I'm talking about an equation that holds in the operad Assoc, and thus in *every* algebra of this operad. We also also impose these laws:

$$1x = xx1 = x$$

This completes our "generators and relations" description of the linear operad Assoc. We could also describe it by saying what all the n-ary operations are, and how to compose them. Either way, it's clear that the dimension of the space of n-ary operations is n factorial:

$$\dim(\mathrm{Assoc}_n) = n!$$

For example, here's a basis of the space of 3-ary operations:

$$\begin{aligned}
(x, y, z) &\mapsto xyz \\
(x, y, z) &\mapsto xzy \\
(x, y, z) &\mapsto yxz \\
(x, y, z) &\mapsto yzx \\
(x, y, z) &\mapsto zxy \\
(x, y, z) &\mapsto zyx
\end{aligned}$$

Second, there's an operad Comm, whose algebras are commutative algebras. This is just like Assoc except we impose one extra relation:

$$xy = yx$$

As a result, all the ways of multiplying n things in different orders become equal, and we get

$$\dim(\mathrm{Comm}_n) = 1.$$

Third, there's an operad Lie, whose algebras are Lie algebras. This is generated by one binary operation

$$(x, y) \mapsto [x, y]$$

satisfying the relations

$$\begin{aligned}
&= -[y, x] \\
[x, [y, z]] &= [[x, y], z] + [y, [x, z]]
\end{aligned}$$

It's harder to work out the dimension of the space of n-ary operations in the Lie operad, but the answer is beautiful:

$$\dim(\mathrm{Lie}_n) = (n-1)!$$

Why is this true? I'll give a proof later on!

Fourth, there's an operad $\mathrm{Poisson}$, whose algebras are Poisson algebras. This is generated by two binary operations and one nullary operation:

$$\begin{aligned}
(x, y) &\mapsto xy \\
(x, y) &\mapsto \{x, y\} \\
() &\mapsto 1
\end{aligned}$$

which obey the relations we've already seen: the commutative algebra relations for xy, the Lie algebra relations for $[x, y]$, and the product rule

$$\{x, yz\} = \{x, y\}z + y\{x, z\}$$

What's the dimension of the space of n-ary operations now? I'll leave this as puzzle. It will be very easy if you pay close attention to what I'm saying.

Okay. Now, you'll notice that we got the operad Comm from the operad Assoc by adding an extra relation. So, every operation in Assoc maps to one in Comm. This map is linear, and it preserves composition. So, we say there's a homomorphism of linear operads

$$\mathrm{Assoc} \to \mathrm{Comm}$$

Quite generally, whenever we have an operad homomorphism

$$\mathcal{O} \to \mathcal{O}'$$

we get a way to turn $\mathcal{O}'$-algebras into $\mathcal{O}$-algebras, since every operation in $\mathcal{O}$ can be reinterpreted as one in $\mathcal{O}'$. So, we get a functor

$$\mathcal{O}'\mathsf{Alg} \to \mathcal{O}\mathsf{Alg}$$

In particular, the homomorphism

$$\mathrm{Assoc} \to \mathrm{Comm}$$

gives the forgetful functor we've already seen:

$$\mathrm{CommAlg} \to \mathrm{AssocAlg}$$

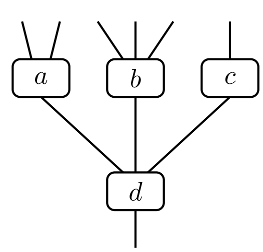
It's really just another way of talking about this functor!

With the main characters introduced, now our tale begins in earnest. Let's use the homomorphism

$$\mathrm{Assoc} \to \mathrm{Comm}$$

to construct the Poisson operad.

To do this, first note that linear operads are a lot like rings. In particular, we can talk about the "kernel" of an operad homomorphism, and this is always an "ideal". The "kernel" consists of operations that go to zero under the homomorphism. Saying it's an "ideal" means that if you compose any operation with one in the ideal, you get one in the ideal. For example, in a composite like this:



if any one of the operations a, b, c, d is in the ideal, the whole composite is in the ideal. So if you think of operations as apples and the operations in the ideal as rotten apples, the rule for ideals is "one rotten apple spoils the whole tree".

Let's take the operad homomorphism

$$\mathrm{Assoc} \to \mathrm{Comm}$$

and call its kernel $\mathcal{I}$. $\mathcal{I}_n$ is the space of n-ary operations for associative algebras that go to zero when we think of them as operations for *commutative* algebras. Let's see what it's like! The first interesting case is $\mathcal{I}_2$. Assoc_2 is 2-dimensional, with this basis:

$$(x, y) \mapsto xy$$
$$(x, y) \mapsto yx$$

and $\mathcal{I}_2$ is 1-dimensional, with this basis:

$$(x, y) \mapsto xy - yx$$

since this is what's zero for commutative algebras. The quotient $\mathrm{Assoc}_2/\mathcal{I}_2$ is the same as Comm_2: it's a 1-dimensional space, and in this space we have identified the operations

$$(x, y) \mapsto xy$$

and

$$(x, y) \mapsto yx$$

Indeed, if you're used to rings, you shouldn't be surprised that the quotient of a linear operad by an ideal is always another operad, and since the homomorphism $\mathrm{Assoc} \to \mathrm{Comm}$ is onto, we have

$$\mathrm{Comm} = \mathrm{Assoc}/\mathcal{I}$$

where I is the kernel of this homomorphism.

Let me quickly say how we use this to get the Poisson operad, and then work through the details a bit more slowly.

As with rings, we can take products of operad ideals. Given ideals $\mathcal{J}$ and $\mathcal{K}$, their product $\mathcal{J}\mathcal{K}$ consists of all linear combinations of composites $f \circ (g_1, \ldots, g_n)$ where f is in $\mathcal{J}$ and at least one of the g_i's is in $\mathcal{K}$. So, given our ideal $\mathcal{I}$, we get a sequence of ideals

$$\mathcal{I}^0, \mathcal{I}^1, \mathcal{I}^2, \mathcal{I}^3, \ldots$$

each containing the next. Here we set $\mathcal{I}^0 = \mathrm{Assoc}$ and $\mathcal{I}^1 = \mathcal{I}$ to get things going. We say the operad Assoc is "filtered" by this sequence of operad ideals. In highbrow terms, this means it's an operad in the category of filtered vector spaces. In lowbrow terms: each vector space in the list above contains the next, and

$$\mathcal{I}^m \mathcal{I}^n \subseteq \mathcal{I}^{m+n}$$

As with rings, this lets us form the "associated graded" operad $\mathrm{gr}(\mathrm{Assoc})$, which is this direct sum:

$$\mathrm{gr}(\mathrm{Assoc}) = \mathcal{I}^0/\mathcal{I}^1 + \mathcal{I}^1/\mathcal{I}^2 + \mathcal{I}^2/\mathcal{I}^3 + \ldots$$

And this is the Poisson operad!

I won't prove this; I'll just sketch the idea, and I'm afraid what I say will only make sense if you have a good intuition for the difference between "filtered" and "graded" things, and how the "associated graded" construction converts the former to the latter.

Operations in $\mathcal{I}$ are those that contain at least one appearance of the bracket $[x, y] = xy - yx$: these are precisely the operations that vanish in a commutative algebra. For example:

$$(x, y, z) \mapsto [xy, z]$$

or

$$(x, y, z) \mapsto z[x, y]$$

Operations in $\mathcal{I}^2$ contain at least two appearances of the bracket like this:

$$(x, y, z) \mapsto [x, [y, z]]$$

And so on. But the operad Assoc is just "filtered", not "graded", because there's no way to say *exactly* how many appearances of the bracket a given operation contains — at least, no way that's compatible with composition and taking linear combinations. For example, you might say these operations contain 0 appearances of the bracket:

$$(x, y) \mapsto xy$$

and

$$(x, y) \mapsto yx$$

But their difference *is* the bracket!

The "associated graded" construction is designed precisely to cure this sort of problem: operations in $\mathcal{I}^k/\mathcal{I}^{k+1}$ contain exactly k appearances of the bracket. And if we look at our example again, we'll see what this achieves. In $\mathrm{gr}(\mathrm{Assoc})$, the operations

$$(x, y) \mapsto xy$$

and

$$(x, y) \mapsto yx$$

live in $\mathcal{I}^0/\mathcal{I}^1$, but now they're equal, because they differ by the commutator, which lives in $\mathcal{I}^1$. So, multiplication becomes commutative! Meanwhile, the operation

$$(x, y) \mapsto xy - yx$$

lives in $\mathcal{I}^1/\mathcal{I}^2 \ldots$ but now we can call it the Poisson bracket:

$$(x, y) \mapsto \{x, y\}$$

And it's easy to check that these rules hold in $\mathrm{gr}(\mathrm{Assoc})$:

$$\{x, y\} = -\{y, x\}$$
$$\{x, \{y, z\}\} = \{\{x, y\}, z\} + \{y, \{x, z\}\}\{x, yz\} = \{x, y\}z + y\{x, z\}$$

So — waving my hands rapidly here — we see that

$$\mathrm{gr}(\mathrm{Assoc}) = \mathrm{Poisson}$$

But the fun isn't done! All this abstract nonsense is just the warmup to a very nice concrete calculation of how the n-ary operations in $\mathrm{gr(Assoc)}$ break up into grades $\mathcal{I}^k/\mathcal{I}^{k+1}$. And here is where the Stirling numbers show up.

Let's look at $n = 3$. The space of 3-ary operations in Assoc has dimension 6. There's a 2d subspace of operations that live in $\mathcal{I}^2$ — that is, where the bracket shows up at least twice:

$$(x, y, z) \mapsto [x, [y, z]]$$
$$(x, y, z) \mapsto [y, [x, z]]$$

You might think it was a 3d subspace, but don't forget the Jacobi identity! There's a 5d subspace of operations that live in $\mathcal{I}$ — that is, where the bracket shows up at least once. For example, we can take the above two together with these three:

$$(x, y, z) \mapsto [x, y]z$$
$$(x, y, z) \mapsto [y, z]x$$
$$(x, y, z) \mapsto [x, z]y$$

And that leaves one more, for a total of 6:

$$(x, y, z) \mapsto xyz$$

A lot of nice patterns show up if you work out more examples. Here's the dimension of the space of n-ary operations in the Poisson operad that lie in $\mathcal{I}^k/\mathcal{I}^{k+1}$:

$_n\backslash^k$	5	4	3	2	1	0
1						1
2					1	1
3				2	3	1
4			6	11	6	1
5		24	50	35	10	1
6	120	274	225	85	15	1

If you're a true expert on combinatories, you'll instantly recognize these as "Stirling numbers of the first kind":

6) Wikipedia, "Stirling numbers of the first kind", `http://en.wikipedia.org/wiki/Stirling_numbers_of_the_first_kind`

But even if you're like me, you'll still see some nice patterns!

First of all, when $k = 0$ we just get 1. This is the dimension of space of n-ary operations in the Poisson operad that don't use the bracket at all. Or in other words, operations in Comm:

$$\mathcal{I}^0/\mathcal{I}^1 = \mathrm{Assoc}/\mathcal{I} = \mathrm{Comm}$$

And we know this space is 1-dimensional. For example, for $n = 4$ it has this basis vector:

$$(w, x, y, z) \mapsto wxyz$$

Second, when $k = 1$ we get the triangle numbers $1, 3, 6, 10, \ldots$. This is the dimension of the space of n-ary operations in the Poisson operad that use the bracket exactly once. This makes sense if you think about it: for $n = 4$ here's a basis:

$$(w,x,y,z) \mapsto \{w,x\}yz$$
$$(w,x,y,z) \mapsto \{w,y\}xz$$
$$(w,x,y,z) \mapsto \{w,z\}xy$$
$$(w,x,y,z) \mapsto \{x,y\}wz$$
$$(w,x,y,z) \mapsto \{x,z\}wy$$
$$(w,x,y,z) \mapsto \{y,z\}wx$$

We're getting $\binom{4}{2}$ different operations.

Third, the numbers in the nth row add to $n!$. That's because the dimension of a filtered vector space equals that of the associated graded vector space. So, the total dimension of $\mathrm{Poisson}_n$ equals the dimension of Assoc_n, which is n factorial.

Fourth, the nth number along the diagonal is $(n-1)!$. This is the dimension of the space of n-ary operations that use the bracket the maximum number of times: namely, $n-1$ times. For example, when $n = 3$ this is a 2d space with basis

$$(x,y,z) \mapsto \{x,\{y,z\}\}$$
$$(x,y,z) \mapsto \{y,\{x,z\}\}$$

These are precisely the operations in the Lie operad! So now we're seeing the operad inclusion

$$\mathrm{Lie} \to \mathrm{Assoc}$$

which gives the forgetful functor

$$\mathsf{AssocAlg} \to \mathsf{LieAlg}$$

Indeed, quite generally, you can check that any operad $\mathcal{O}$ with an ideal $\mathcal{I}$ has a suboperad whose n-ary operations are those lying in $\mathcal{I}^{n-1}$.

Finally, when you learn about Stirling numbers, you see the general pattern. Stirling numbers count the number of permutations of n elements that have a fixed number of disjoint cycles. For example, these permutations of 4 elements have 3 disjoint cycles:

$$(wx)(y)(z)$$
$$(wy)(x)(z)$$
$$(wz)(x)(y)$$
$$(xy)(w)(z)$$
$$(xz)(w)(y)$$
$$(yz)(w)(x)$$

These correspond to the following 4-ary operations in the Poisson operad:

$$(w,x,y,z) \mapsto \{w,x\}yz$$
$$(w,x,y,z) \mapsto \{w,y\}xz$$
$$(w,x,y,z) \mapsto \{w,z\}xy$$
$$(w,x,y,z) \mapsto \{x,y\}wz$$
$$(w,x,y,z) \mapsto \{x,z\}wy$$
$$(w,x,y,z) \mapsto \{y,z\}wx$$

As you can see, there's a lot of fun and mysterious stuff going on here. Todd Trimble wrote a legendary paper "Notes on the Lie operad" which would probably shed a lot of light on this stuff. But unfortunately, the reason I call it "legendary" is that it's almost impossible to find! If I ever get a copy I'll let you know.

For now, I'll wrap the story by proving that the Stirling numbers are really related to the Poisson operad as claimed.

The first step is to show that

$$\dim(\mathrm{Lie}_n) = (n-1)!$$

For this we can use a famous argument, which is probably in Trimble's paper. First consider the forgetful functors:

$$\mathsf{AssocAlg} \to \mathsf{LieAlg} \to \mathsf{Vect}$$

where Vect is the category of vector spaces. These forgetful functors have left adjoints. The first forms the free Lie algebra on a vector space V. Let's call this $\mathrm{Lie}(V)$:

$$\mathrm{Lie}\colon \mathsf{Vect} \to \mathsf{LieAlg}$$

The second forms the free associative algebra on a Lie algebra L. This is called its "universal enveloping algebra", $U(L)$:

$$U\colon \mathsf{LieAlg} \to \mathsf{AssocAlg}$$

If we compose these two functors, we get a functor that forms the free associative algebra on a vector space V. This is usually called its "tensor algebra", but let's write it as $\mathrm{Assoc}(V)$, for reasons soon to become clear:

$$\mathrm{Assoc}\colon \mathrm{Vect} \to \mathrm{AssocAlg}$$

So, we have an canonical isomorphism

$$\mathrm{Assoc}(V) \cong U(\mathrm{Lie}(V))$$

But the Poincar-Birkhoff-Witt theorem gives a canonical isomorphism of vector spaces between the universal enveloping algebra $U(L)$ of a Lie algebra L and its "symmetric algebra" — that is, the free commutative algebra on its underlying vector space. Let's write this symmetric algebra as $\mathrm{Comm}(L)$. So, we get a vector space isomorphism

$$\mathrm{Assoc}(V) \cong \mathrm{Comm}(\mathrm{Lie}(V))$$

(Admittedly, the standard ugly proof of the PBW theorem does not give a *canonical* isomorphism. But the good proof does — see "Week 212".)

Next, let's use some well-known black magic to describe the above functors using operads. The free Lie algebra on a vector space V is given by

$$\mathrm{Lie}(V) \cong \bigoplus_n \mathrm{Lie}_n \otimes V^{\otimes n}$$

where we tensor over the action of the symmetric group. Similarly, the free associative algebra on a vector space V is given by

$$\mathrm{Assoc}(V) \cong \bigoplus_n \mathrm{Assoc}_n \otimes V^{\otimes n}$$

Likewise, the free commutative algebra on V is given by

$$\mathrm{Comm}(V) \cong \bigoplus_n \mathrm{Comm}_n \otimes V^{\otimes n}$$

These are categorified versions of formal power series. That's because linear operads are a special case of linear "species", or "structure types". So, we can decategorify them and get formal power series called their generating functions. I explained this in "Week 185", "Week 190", and "Week 102", but not in the linear case. It's no big deal: where we used cardinalities before, now we use dimensions! We get these generating functions:

$$\begin{aligned} |\mathrm{Lie}|(x) &= \sum_n \dim(\mathrm{Lie}_n) \frac{x^n}{n!} \\ |\mathrm{Assoc}|(x) &= \sum_n \dim(\mathrm{Assoc}_n) \frac{x^n}{n!} \\ &= \sum_n x^n \\ &= \frac{1}{1-x} \\ |\mathrm{Comm}|(x) = \sum_n \dim(\mathrm{Comm}_n) \frac{x^n}{n!} \\ &= \sum_n \frac{x^n}{n!} \\ &= \exp(x) \end{aligned}$$

Now, by general abstract nonsense our isomorphism

$$\mathrm{Assoc}(V) \cong \mathrm{Comm}(\mathrm{Lie}(V))$$

gives an equation

$$|\mathrm{Assoc}|(x) = |\mathrm{Comm}|(|\mathrm{Lie}|(x))$$

or

$$\frac{1}{1-x} = \exp(|\mathrm{Lie}|(x))$$

so

$$|\mathrm{Lie}|(x) = \ln(11 - x) = \sum_n \frac{x^n}{n}$$

but we saw

$$|\mathrm{Lie}|(x) = \sum_n \dim(\mathrm{Lie}_n) \frac{x^n}{n!}$$

so

$$\dim(\mathrm{Lie}_n) = (n-1)!$$

This is a beautiful way of counting the number of n-ary operations in the Lie operad.

Note also that $(n-1)!$ is also the number of permutations of n things with a single cycle. So, the Stirling numbers are already showing up.

Next let's use the fact that for any Lie algebra L, the symmetric algebra $\mathrm{Comm}(L)$ is not just a commutative algebra: it's a Poisson algebra! It has a Poisson bracket, called the Kostant-Kirillov Poisson structure. Indeed, it's the free Poisson algebra on the Lie algebra L.

This implies that $\mathrm{Comm}(\mathrm{Lie}(V))$ is the free Poisson algebra on the vector space V:

$$\mathrm{Comm}(\mathrm{Lie}(V)) \cong \bigoplus_n \mathrm{Poisson}_n \otimes V^{\otimes n}$$

To get a basis of $\mathrm{Poisson}_n$, it's therefore enough to consider commuting products of terms built using Poisson brackets, like this:

$$(a, b, c, d, e, f, g, h, i, j) \mapsto \{\{a, b\}, c\}\{d, e\}\{f, g\}hij$$

Any expression like this can be reinterpreted as a permutation:

$$(abc)(de)(fg)(h)(i)(j)$$

So, by what we've already seen, the dimension of the space of n-ary operations that involve a product of j terms is the same as the number of permutations of n things with j cycles. That's a Stirling number! And this dimension is also the dimension of the space of n-ary operations that live in $\mathcal{I}^k/\mathcal{I}^{k+1}$, where $j + k = n$.

That last fact was not supposed to be instantly obvious. But if you look at the example above, you'll see it works:

- $n = 10$, since there are 10 letters
- $j = 6$, since we've got a product of 6 terms built using Poisson brackets
- $k = 4$, since we're using Poisson brackets 4 times

If you think a while, you'll see it always works like this.

To summarize: the dimension of the space of n-ary operations in $\mathcal{I}^k/\mathcal{I}^{k+1}$ is the same as the number of permutations of n things with $n - k$ disjoint cycles.

Even if you didn't follow this argument, I hope you see that associative, commutative, Lie and Poisson algebras are involved in a beautiful web of relationships.

I didn't get to say much about what all this means for quantization. Indeed, I haven't really figured it all out yet! For example, it must be important that the universal enveloping algebra of a Lie algebra is a deformation quantization of its symmetric algebra. This should be a central part of the story I'm telling, especially because it's crucial to the proof of the Poincar-Birkoff-Witt theorem mentioned in "Week 212". But I didn't fully integrate this stuff into the story.

I also didn't talk about the relation between the Lie operad and the homology of the poset of partitions of a finite set, described at the beginning of this paper:

7) Benoit Fresse, "Koszul duality of operads and homology of partition posets", in *Homotopy Theory: Relations with Algebraic Geometry, Group Cohomology, and Algebraic K-theory*, eds. Paul Gregory Goerss and Stewart Priddy, Contemp. Math 346, 2004, AMS, Providence, Rhode Island, pp. 115–215. Also available at `http://math.univ-lille1.fr/~fresse/PartitionHomology.html`

and first discovered by Joyal:

8) Andr Joyal, "Foncteurs analytiques et especes de structures", in *Combinatoire Enumerative*, Springer Lecture Notes in Mathematics **1234**, Springer, Berlin (1986), 126–159.

Nor did I bring the homology of the little k-cubes operad into the game — the relation of this to the Poisson operad was described in "Week 220", but you'll notice that this only talks about $k > 1$. The story I'm discussing now concerns the case $k = 1$, because the algebra Assoc is the homology of the little 1-cubes operad. For higher k, I especially recommend this paper:

9) Dev Sinha, "The homology of the little disks operad", available as `arXiv:math/0610236`.

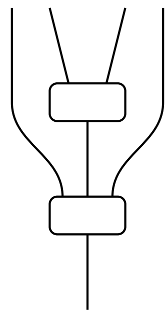
Finally, here are three side remarks that would have been too distracting earlier:

When I said "linear operads are a lot like rings", I could have been more precise. Linear operads are a lot like associative algebras — and indeed, an associative algebra is the same as a linear operad with only unary operations! But since we were talking about the linear operad *for* associative algebras, I didn't want to blow your mind by pointing out that an associative algebra also *is* a linear operad. We could also consider operads whose spaces of operations are abelian groups, with composition being a group homomorphism in each argument. An operad like this with only unary operations is the same as a ring. And this is the precise sense in which operad theory generalizes ring theory.

When I said "one rotten apple spoils the whole tree", I felt like saying "cherry" instead of "apple", since Boardman and Vogt talked about "cherry trees" in their work on operads. Unfortunately, the proverb "one rotten apple spoils the whole barrel" requires apples! For more, see this nice historical survey:

10) James Stasheff, "Grafting Boardman's cherry trees to quantum field theory", in *Homotopy Invariant Algebraic Structures: A Conference in Honor of J. Michael Boardman*, eds. Jean-Pierre Meyer, Jack Morava, and W. Stephen Wilson, AMS, Providence, Rhode Island, 1999. Also available as `http://www.math.unc.edu/Faculty/jds/boardman.ps`

When I said "Indeed, quite generally, you can check that any operad O with an ideal I has a suboperad whose n-ary operations are those lying in $\mathcal{I}^{n-1}$", you might have been puzzled by the "-1". Here's the point. All ways of composing operations can be built up from ways like this:



where we compose an m-ary operation and an n-ary operation (together with some identity operations). The result is an $(m+n-1)$-ary operation! For example, above I'm composing a 3-ary operation and a 2-ary operation and getting a 4-ary operation.

So, if we take an m-ary operation in $\mathcal{I}^{m-1}$ and compose it with an n-ary operation in $\mathcal{I}^{n-1}$, we get an $(n+m)$-ary operation which lies in $\mathcal{I}^{n+m-2} \subseteq \mathcal{I}^{n+m-1}$. So we get a suboperad whose n-ary operations are those lying in $\mathcal{I}^{n-1}$.

You might enjoy working out what other ways there are to get suboperads from an operad with an ideal. Take all n-ary operations lying in $\mathcal{I}^{f(n)}$. For what functions f do these form a suboperad?

Also, you might enjoy answering these questions, most of which I haven't tried:

- If $\mathcal{I}$ is the ideal in Assoc for which $\mathrm{Assoc}/\mathcal{I} = \mathrm{Comm}$, what sort of algebras are described by the operad $\mathcal{I}^k$?
- What about the operad $\mathcal{I}^k/\mathcal{I}^{k+1}$?
- What about the operad $\mathcal{I}^k/\mathcal{I}^{k+2}$, and so on?
- And what about the suboperads of Assoc concocted as in the previous paragraph?

Addenda: I thank James Dolan and Urs Schreiber for catching some mistakes. Allen Knutson adds to my list of questions:

> *Here's another: if* $\mathrm{gr}(\mathrm{Assoc}) = \mathrm{Poisson}$*, what is the meaning of the Rees and blowup algebras associated to this filtration?*
>
> *(Given a filtration* $R = R_0 \supseteq R_1 \supseteq \ldots$*, e.g. by powers of I, you can look at the subring of* $R[t]$ *that has* $t^n R_n$ *in the* n*th degree piece; that's the blowup algebra. If you include* $t^{-n}R$ *in the negative powers, that's the Rees algebra. If you mod out Rees by* $(t-c)$*, you get* R *for any nonzero* c*, and* $\mathrm{gr}(R)$ *for* $c = 0$*.)*

Of course he means "operad" where he writes "algebra" or "ring" — while the constructions he describes are most familiar for algebras or rings, they work for operads too!

David Corfield points out:

> *Wikipedia wants to tag your Stirling numbers as 'unsigned', and yet notes that "that nearly all the relations and identities given on this page are valid only for unsigned Stirling numbers". Also the link with exponential generating functions goes through the unsigned version.*
>
> *So why deal with the signed version? Is it because*
>
> > *The Stirling numbers of the first and second kind can be understood to be inverses of one-another, when taken as triangular matrices.*

For more discussion visit the n-Category Caf. In particular, Toby Bartels raised an important question: what's the physical meaning of treating Planck's constant as a variable instead of a number in deformation quantization?

> The worthwhile problems are the ones you can really solve or help solve, the ones you can really contribute something to.
>
> — *Richard Feynman*

Week 283

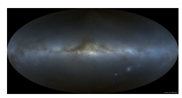
November 10, 2009

We had a great AMS meeting this weekend at UCR, with far too many interesting talks going on simultaneously. For example, there were two sessions on math related to knot theory, one on operator algebras, one on noncommutative geometry, and one on homotopy theory and higher algebraic structures! If I could clone myself, I'd have gone to all of them.

I'd like to discuss some of the talks, and maybe even point you to some videos. But the videos aren't available yet, so for now I'll just summarize my own talk on "Who Discovered the Icosahedron", and some geometry related to that. I'll conclude with a puzzle.

But first — the astronomy pictures of the week!

Galaxies are beautiful things, and there are lots of ways to enjoy them. Here's the Milky Way in visible light — a detailed panorama created from over 3000 individual pictures, carefully calibrated to show large dust clouds:



1) Axel Mellinger, "All-sky Milky Way panorama 2.0", `http://home.arcor.de/axel.mellinger/`

You can see even more structure in this infrared panorama of the Milky Way, created by the Spitzer Space Telescope:

2) Astronomy Picture of the Day, "GLIMPSE the Milky Way", `http://apod.nasa.gov/apod/ap051216.html`

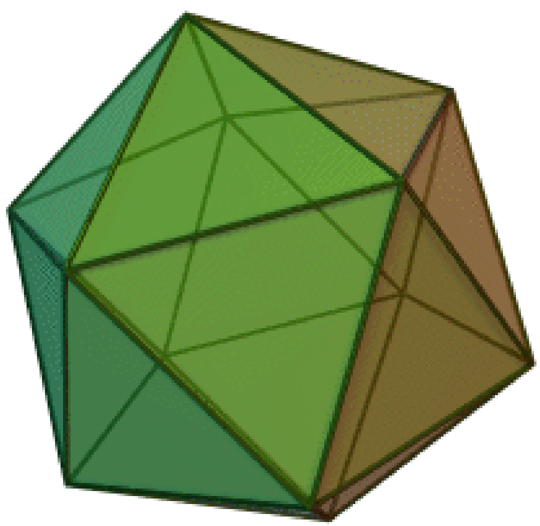
The bright white splotches are star-forming regions. The greenish wisps are hot interstellar gas. The red clouds are dust and organic molecules like polycyclic aromatic hydrocarbons (see "Week 258"). The darkest patches are regions of cool dust too thick for Spitzer to see through.

But here's my favorite: the Andromeda Galaxy in viewed in ultraviolet light:

3) Astronomy Picture of the Day, "Ultraviolet Andromeda", `http://apod.nasa.gov/apod/ap090917.html`

This was taken by Swift, NASA's ultraviolet satellite telescope. At this frequency, young hot stars and dense star clusters dominate the view. It's sort of ghostly looking, no?

Now for my talk on the early history of the icosahedron.



This continues the tale begun in "Week 236" and "Week 241". Someday it'll get folded into a paper on special properties of the number 5, and 5-fold symmetry:

4) John Baez, "Who discovered the icosahedron?", talk at the Special Session on History and Philosophy of Mathematics, 2009 Fall Western Section Meeting of the AMS, November 7, 2009. Available at `http://math.ucr.edu/home/baez/icosahedron/`

The dodecahedron and icosahedron are the most exotic of the Platonic solids, because they have 5-fold rotational symmetry — a possibility that only exists for regular polytopes in 2, 3 or 4 dimensions. The dodecahedron and icosahedron have the same symmetry group, because they are Poincar duals: the vertices of one correspond to faces of the other. But the icosahedron was probably discovered later. As Benno Artmann wrote:

> *The original knowledge of the dodecahedron may have come from crystals of pyrite, but in contrast the icosahedron is a pure mathematical creation.... It is the first realization of an entity that existed before only in abstract thought. (Well, apart from the statues of gods!)*

I'm not sure it's really anything close to the first "realization of an entity that existed before only in abstract thought". But it may have been the first "exceptional" object in mathematics — roughly speaking, an entity that doesn't fit into any easy pattern, which is discovered as part of proving a classification theorem!

Other exceptional objects include the simple Lie group E_8, and the finite simple group M_{12}. Intriguingly, many of these "exceptional objects" are related. For example, the icosahedron can be used to construct both E_8 and M_{12}. But the first interesting classification theorem was the classification of regular polyhedra: convex polyhedra with equilateral polygons as faces, and the same number of faces meeting at each vertex. This theorem appears almost at the end of the last book of Euclid's Elements — Book XIII. It shows that the only possibilities are the Platonic solids: the tetrahedron, the cube, the octahedron, the dodecahedron and the icosahedron. And according to traditional wisdom, the results in this book were proved by Theatetus, who also discovered the icosahedron!

Indeed, Artmann cites an "an ancient note written in the margins of the manuscript" of Book XIII, which says:

> *In this book, the 13th, are constructed the five so-called Platonic figures which, however, do not belong to Plato, three of the five being due to the Pythagoreans, namely the cube, the pyramid, and the dodecahedron, while the octahedron and the icosahedron are due to Theaetetus.*

You may know Theaetetus through Plato's dialog of the same name, where he's described as a mathematical genius. He's also mentioned in Plato's dialogue called the Sophist. In the Republic, written around 380 BC, Plato complained that not enough is known about solid geometry:

> *... and for two reasons: in the first place, no government places value on it; this leads to a lack of energy in the pursuit of it, and it is difficult. In the second place, students cannot learn it unless they have a teacher. But then a teacher can hardly be found....*

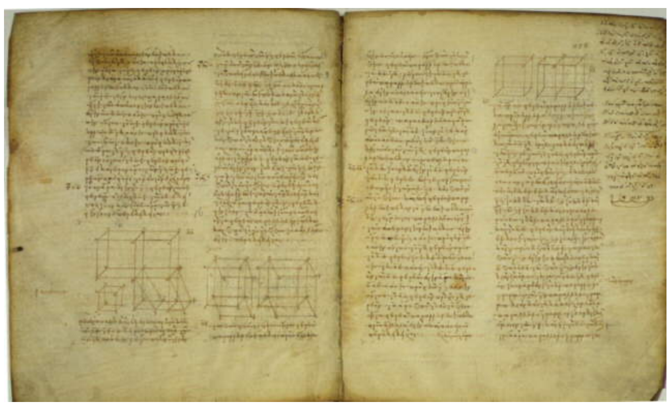
Theaetetus seems to have filled the gap: he worked on solid geometry between 380 and 370 BC, perhaps inspired by Plato's interest in the subject. He died from battle wounds and dysentery in 369 after Athens fought a battle with Corinth.

But how certain are we that Theatetus discovered — or at least studied — the icosahedron? The only hard evidence seems to be this "ancient note" in the margins of the Elements. But who wrote it, and when?

First of all, if you hope to see an ancient manuscript by Euclid with a scribbled note in the margin, prepare to be disappointed! All we have are copies of copies of copies. The oldest remaining fragments of the Elements date to centuries after Euclid's death: some from a library in Herculaneum roasted by the eruption of Mount Vesuvius in 79 AD, a couple from the Fayum region near the Nile, and some from a garbage dump in the Egyptian town of Oxyrhynchus.

There are various lines of copies of Euclid's Elements. Comparing these to guess the contents of the *original* Elements is a difficult and fascinating task. Unfortunately, in the fourth century AD, the Greek mathematician Theon of Alexandria — Hypatia's dad — made a copy that became extremely popular. So popular, in fact, that for many centuries European scholars knew no line of copies that hadn't passed through Theon! And Theon wasn't a faithful copyist: he added extra propositions, lengthened some proofs, and omitted a few things too. It seems he wanted to standardize the language and make it easier to follow. This may have helped people trying to learn geometry — but certainly not scholars trying to understand Euclid.

In 1808, Francois Peyrard made a marvelous discovery. He found that the Vatican library had a copy of Euclid's Elements that hadn't descended through Theon!



This copy is now called "P". It dates back to about 850 AD. I would love to know how Peyrard got his hands on it. One imagines him rooting around in a dusty basement and opening a trunk. . . but it seems that Napoleon somehow took this manuscript from the Vatican to Paris.

In the 1880s, the great Danish scholar Johan Heiberg used "P" together with various "Theonine" copies of the Elements to prepare what's still considered the definitive Greek edition of this book. The all-important English translation by Thomas Heath is based on this. As far as I can tell, "P" is the only known non-Theonine copy of Euclid except for the fragments I mentioned. Heath also used these fragments to prepare his translation.

This is just a quick overview of a complicated detective story. As always, the fractal texture of history reveals more complexity the more closely you look.

Anyway, Heath thinks that Geminus of Rhodes wrote the "ancient note" in the Elements crediting Theatetus. I'm not sure why Heath thinks this, but Geminus of Rhodes was a Greek astronomer and mathematician who worked during the 1st century BC.

In his charming article "The discovery of the regular solids", William Waterhouse writes:

Once upon a time there was no problem in the history of the regular solids. According to Proclus, the discoveries of Pythagoras include "the construction of the cosmic solids," and early historians could only assume that the subject sprang full-grown from his head. But a better-developed picture of the growth of Greek geometry made such an early date seem questionable, and evidence was uncovered suggesting a different attribution. A thorough study of the testimony was made by E. Sachs, and her conclusion is now generally accepted: the attribution to Pythagoras is a later misunderstanding and/or invention.

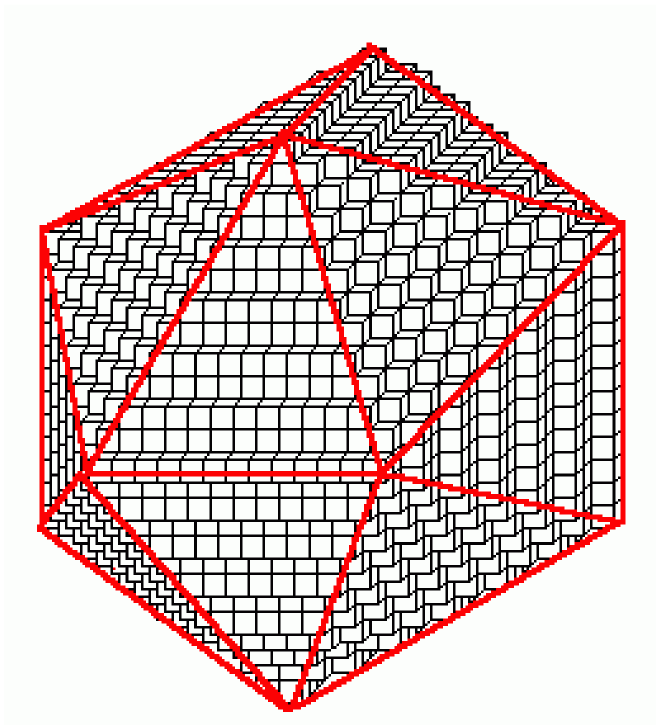
The history of the regular solids thus rests almost entirely on a scholium to Euclid which reads as follows:

"In this book, the 13th, are constructed the 5 figures called Platonic, which however do not belong to Plato. Three of these 5 figures, the cube, pyramid, and dodecahedron, belong to the Pythagoreans; while the octahedron and icosahedron belong to Theaetetus."

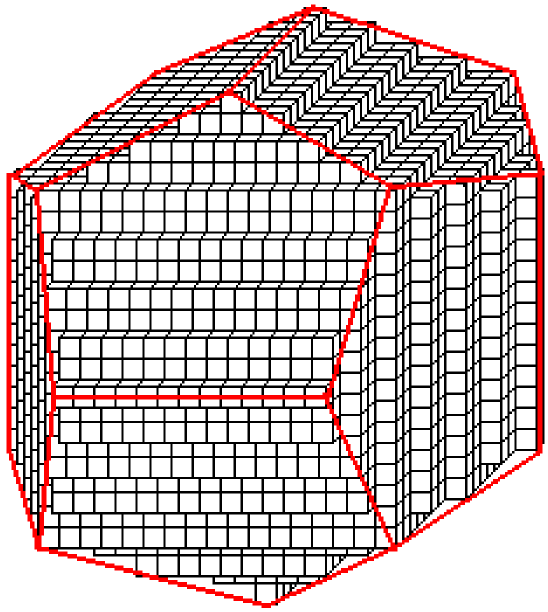
Theaetetus lived c. 415–369 B.C., so this version gives a moderately late date; and it has the considerable advantage of seeming unlikely. That is, the details in the scholium are not the sort of history one would naively conjecture, and hence it is probably not one of the stories invented in late antiquity. As van der Waerden says, the scholium is now widely accepted "precisely because [it] directly contradicts the tradition which used to ascribe to Pythagoras anything that came along."

But probability arguments can cut both ways, and those scholars who hesitate to accept the scholium do so primarily because it seems too unlikely. There have been two main sticking places: first, the earliness of the dodecahedron in comparison with the icosahedron; and second, the surprising lateness of the octahedron. The first objection, however, has been fairly well disposed of. The mineral pyrite (FeS_2) crystallizes most often in cubes and almost-regular dodecahedra; it is quite widespread, being the most common sulphide, and outstanding crystals are found at a number of spots in Italy. Moreover it regularly occurs mixed with the sulphide ores, and underlying the oxidized ores, of copper; these deposits have been worked since earliest antiquity. Thus natural dodecahedra were conspicuous, and in fact they did attract attention: artificial dodecahedra have been found in Italy dating from before 500 BC. Icosahedral crystals, in contrast, are much less common. Hence there is no real difficulty in supposing that early Pythagorean geometers in Italy were familiar with dodecahedra but had not yet thought of the icosahedron.

Indeed, while I've heard that iron pyrite forms "pseudoicosahedra":



I've never seen one, while the "pyritohedra" resembling regular dodecahedra are pretty common:



The puzzle of why the octahedron showed up so late seems to have this answer: it was known earlier, but it was no big deal until the concept of regular polyhedron was discovered! As Waterhouse says, the discovery of the octahedron would be like the discovery of the 4rd perfect number. Only the surrounding conceptual framework makes the discovery meaningful.

So far, so good. But maybe the Greeks were not the first to discover the icosahedron! In 2003, the mathematicians Michael Atiyah and Paul Sutcliffe wrote:

> *Although they are termed Platonic solids there is convincing evidence that they were known to the Neolithic people of Scotland at least a thousand years before Plato, as demonstrated by the stone models pictured in Fig. 1 which date from this period and are kept in the Ashmolean Museum in Oxford.*

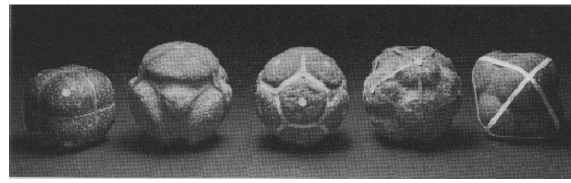
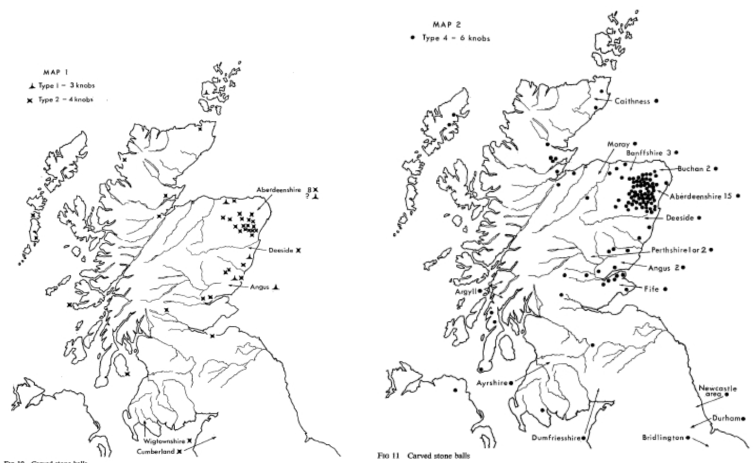


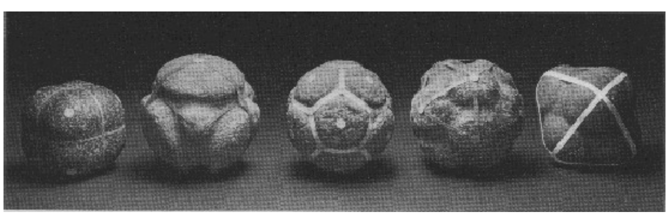
Figure 1. Stone models of the cube, tetrahedron, dodecahedron, icosahedron and octahedron. They date from abo

Various people including John McKay and myself spread this story without examining it very critically. I did read Dorothy Marshall's excellent paper "Carved stone balls", which catalogues 387 carved stone balls found in Scotland, dating from the Late Neolithic to Early Bronze Age. It has pictures showing a wide variety of interesting geometric patterns carved on them, and maps showing where people have found balls with various numbers of bumps on them. But it doesn't say anything about Platonic solids.



Maps by Dorothy Marshall. Left: balls with 3 or 4 knobs. Right: balls with 6 knobs.

In March of 2009, Lieven le Bruyn posted a skeptical investigation of Atiyah and Sutcliffe's claim. For starters, he looked hard at the photo in their paper:



> *... where's the icosahedron? The fourth ball sure looks like one but only because someone added ribbons, connecting the centers of the different knobs. If this ribbon-figure is an icosahedron, the ball itself should be another dodecahedron and the ribbons illustrate the fact that icosa- and dodecahedron are dual*

> *polyhedra. Similarly for the last ball, if the ribbon-figure is an octahedron, the ball itself should be another cube, having exactly 6 knobs. Who did adorn these artifacts with ribbons, thereby multiplying the number of "found" regular solids by two (the tetrahedron is self-dual)?*

Who put on the ribbons? Lieven le Bruyn traced back the photo to Robert Lawlor's 1982 book Sacred Geometry. In this book, Lawlor wrote:

> *The five regular polyhedra or Platonic solids were known and worked with well before Plato's time. Keith Critchlow in his book Time Stands Still presents convincing evidence that they were known to the Neolithic peoples of Britain at least 1000 years before Plato. This is founded on the existence of a number of spherical stones kept in the Ashmolean Museum at Oxford. Of a size one can carry in the hand, these stones were carved into the precise geometric spherical versions of the cube, tetrahedron, octahedron, icosahedron and dodecahedron, as well as some additional compound and semi-regular solids...*

But is this really true? Le Bruyn discovered that the Ashmolean owns only 5 Scottish stone balls — and their webpage shows a photo of them, which looks quite different than the photo in Lawlor's book!

They have no ribbons on them. More importantly, they're different shapes! The Ashmolean lists their 5 balls as having 7, 6, 6, 4 and 14 knobs, respectively — nothing like an icosahedron.

And here is where I did a little research of my own. The library at UC Riverside has a copy of Keith Critchlow's 1979 book Time Stands Still. In this book, we see the same photo of stones with ribbons that appears in Lawlor's book — the photo that Atiyah and Suttcliffe use. In Critchlow's book, these stones are called "a full set of Neolithic 'Platonic solids' ". He says they were photographed by one Graham Challifour — but he gives no information as to where they came from!

And Critchlow explicitly denies that the Ashmolean has an icosahedral stone! He writes:

> *... the author has, during the day, handled five of these remarkable objects in the Ashmolean museum.... I was rapt in admiration as I turned over these remarkable stone objects when another was handed to me which I took to be an icosahedron.... On careful scrutiny, after establishing apparent fivefold symmetry on a number of the axes, a count-up of the projections revealed 14! So it was not an icosahedron.*

It seems the myth of Scottish balls shaped like Platonic solids gradually grew with each telling. Could there be any truth to it? Dorothy Marshall records Scottish stone balls with various numbers of knobs, from 3 to 135 — but just two with 20, one at the National Museum in Edinburgh, and one at the Kelvingrove Art Gallery and Museum in Glasgow. Do these look like icosahedra? I'd like to know. But even if they do, should we credit Scots with "discovering the icosahedron"? Perhaps not.

So, it seems the ball is in Theaetetus' court.

Here are some references:

The quote from Benno Artmann appeared in a copy of the AMS Bulletin where the cover illustrates a construction of the icosahedron:

5) Benno Artmann, "About the cover: the mathematical conquest of the third dimension", *Bulletin of the AMS* **43** (2006), 231–235. Also available at `http://www.ams.org/bull/2006-43-02/S0273-0979-06-01111-6/`

For more, try this wonderfully entertaining book:

6) Benno Artmann, *Euclid — The Creation of Mathematics*, Springer, New York, 2nd ed., 2001. (The material on the icosahedron is not in the first edition.)

It's not a scholarly tome: instead, it's a fun and intelligent introduction to Euclid's Elements with lots of interesting digressions. A great book for anyone interested in math!

I should also get ahold of this someday:

7) Benno Artmann, "Antike Darstellungen des Ikosaeders", *Mitt. DMV* **13** (2005), 45–50.

Heath's translation of and commentary on Euclid's Elements is available online thanks to the Perseus Project. The scholium crediting Theatetus for the octahedron and icosahedron is discussed here:

8) Euclid, *Elements*, trans. Thomas L. Heath, Book XIII, "Historical Note", p. 438. Also available at `http://old.perseus.tufts.edu/cgi-bin/ptext?doc=Perseus%3Atext%3A1999.01.0086&query=head%3D%23566`

while the textual history of the Elements is discussed here:

9) Euclid, *Elements*, trans. Thomas L. Heath, Chapter 5: "The Text", p. 46. Also available at `http://old.perseus.tufts.edu/cgi-bin/ptext?lookup=Euc.+5`

Anyone interested in Greek mathematics also needs these books by Heath, now available cheap from Dover:

10) Thomas L. Heath, *A History of Greek Mathematics*. Vol. 1: *From Thales to Euclid*. Vol. 2: *From Aristarchus to Diophantus*. Dover Publications, 1981.

The long quote by Waterhouse comes from here:

11) William C. Waterhouse, "The discovery of the regular solids", *Arch. Hist. Exact Sci.* **9** (1972–1973), 212–221.

I haven't yet gotten my hold on this "thorough study" mentioned by Waterhouse — but I will soon:

12) Eva Sachs, *Die Fnf Platonischen Krper, zur Geschichte der Mathematik und der Elementenlehre Platons und der Pythagoreer*, Berlin, Weidmann, 1917.

I also want to find this discussion of how Peyrard got ahold of the non-Theonine copy of Euclid's Elements:

13) N. M. Swerlow, "The Recovery of the exact sciences of antiquity: mathematics, astronomy, geography", in *Rome Reborn: The Vatican Library and Renaissance Culture*, ed. Grafton, 1993.

Here is Atiyah and Sutcliffe's paper claiming that the Ashmolean has Scottish stone balls shaped like Platonic solids:

14) Michael Atiyah and Paul Sutcliffe, "Polyhedra in physics, chemistry and geometry", available as `arXiv:math-ph/0303071`.

Here is le Bruyn's critical examination of that claim:

15) Lieven le Bruyn, "The Scottish solids hoax", March 25, 2009, `http://www.neverendingbooks.org/index.php/the-scottish-solids-hoax.html`

Here are the books by Critchlow and Lawlor — speculative books from the "sacred geometry" tradition:

16) Keith Critchlow, *Time Stands Still*, Gordon Fraser, London, 1979.

17) Robert Lawlor, *Sacred Geometry: Philosophy and Practice*, Thames and Hudson, London, 1982. Available at `http://www.scribd.com/doc/13155707/robert-lawlor-sacred-geometry-phi`

Here's the Ashmolean website:

18) British Archaeology at the Ashmolean Museum, "Highlights of the British collections: stone balls", `http://ashweb2.ashmus.ox.ac.uk/ash/britarch/highlights/stone-balls.html`

and here's Dorothy Marshall's paper on stone balls:

19) Dorothy N. Marshall, "Carved stone balls", *Proc. Soc. Antiq. Scotland* **108** (1976/77), 40-72. Available at `http://www.tarbat-discovery.co.uk/Learning%20Files/Carved%20stone%20balls.pdf`

Finally, a bit of math.

In the process of researching my talk, I learned a lot about Euclid's Elements, where the construction of the icosahedron — supposedly due to Theaetetus — is described. This construction is Proposition XIII.16, in the final book of the Elements, which is largely about the Platonic solids. This book also has some fascinating results about the golden ratio and polygons with 5-fold symmetry!

The coolest one is Proposition XIII.10. It goes like this.

Take a circle and inscribe a regular pentagon, a regular hexagon, and a regular decagon. Take the edges of these shapes, and use them as the sides of a triangle. Then this is a right triangle!

In other words, if

$$P$$

is the side of the pentagon,

$$H$$

is the side of the hexagon, and

$$D$$

is the side of the decagon, then

$$P^2 = H^2 + D^2$$

We can prove this using algebra — but Euclid gave a much cooler proof, which actually find this right triangle hiding inside an icosahedron.

First let's give a completely uninspired algebraic proof.

Start with a unit circle. If we inscribe a regular hexagon in it, then obviously

$$H = 1$$

So we just need to compute P and D. If we think of the unit circle as living in the complex plane, then the solutions of

$$z^5 = 1$$

are the corners of a regular pentagon. So let's solve this equation. We've got

$$0 = z^5 - 1 = (z-1)(z^4 + z^3 + z^2 + z + 1)$$

so ignoring the dull solution $z = 1$, we must solve

$$z^4 + z^3 + z^2 + z + 1 = 0$$

This says that the center of mass of the pentagon's corners lies right in the middle of the pentagon.

Now, quartic equations can always be solved using radicals, but it's a lot of work. Luckily, we can solve this one by repeatedly using the quadratic equation! And that's why the Greeks could construct the regular pentagon using a ruler and compass.

The trick is to rewrite our equation like this:

$$z^2 + z + 1 + z^{-1} + z^{-2} = 0$$

and then like this:

$$(z + z^{-1})^2 + (z + z^{-1}) - 1 = 0$$

Now it's a quadratic equation in a new variable. So while I said this proof would be uninspired, it did require a tiny glimmer of inspiration. But that's all! Let's write

$$z + z^{-1} = x$$

so our equation becomes

$$x^2 + x - 1 = 0$$

Solving this, we get two solutions. The one I like is the golden ratio:

$$x = \varphi = -1 + \sqrt{5}2 \sim 0.6180339\ldots$$

Next we need to solve

$$z + z^{-1} = \varphi$$

This is another quadratic equation:

$$z^2 - \varphi z + 1 = 0$$

with two conjugate solutions, one being

$$z = \frac{\varphi + (\varphi^2 - 4)^{\frac{1}{2}}}{2}$$

I've sneakily chosen the solution that's my favorite 5th root of unity:

$$z = \exp\left(\frac{2\pi i}{5}\right) = \cos\left(\frac{2\pi}{5}\right) + i \sin\left(\frac{2\pi}{5}\right)$$

So, we're getting

$$\cos\left(\frac{2\pi}{5}\right) = \frac{\varphi}{2}$$

A fact we should have learned in high school, but probably never did.

Now we're ready to compute P, the length of the side of a pentagon inscribed in the unit circle:

$$\begin{aligned} P^2 &= |1 - z|^2 \\ &= \left(1 - \cos\left(\frac{2\pi}{5}\right)\right)^2 + \left(\sin\left(\frac{2\pi}{5}\right)\right)^2 \\ &= 2 - 2\cos\left(\frac{2\pi}{5}\right) \\ &= 2 - \varphi \end{aligned}$$

Next let's compute D, the length of the side of a decagon inscribed in the unit circle! We can mimic the last stage of the above calculation, but with an angle half as big:

$$D^2 = 2 - 2\cos\left(\frac{\pi}{5}\right)$$

To go further, we can use a half-angle formula:

$$\cos\left(\frac{\pi}{5}\right) = \left(\frac{1+\cos(2\pi/5)}{2}\right)^{\frac{1}{2}} = (\frac{1}{2} + \varphi/4)^{\frac{1}{2}}$$

This gives

$$D^2 = 2 - (2+\varphi)^{\frac{1}{2}}$$

But we can simplify this a bit more. As any lover of the golden ratio should know,

$$2 + \varphi = 2.6180339\ldots$$

is the square of

$$1 + \varphi = 1.6180339\ldots$$

So we really have

$$D^2 = 1 - \varphi$$

Okay. Your eyes have glazed over by now — unless you've secretly been waiting all along for This Week's Finds to cover high-school algebra and trigonometry. But we're done. We see that

$$P^2 = H^2 + D^2$$

simply says

$$2 - \varphi = 1 + (1 - \varphi)$$

That wasn't so bad, but imagine discovering it and proving it using axiomatic geometry back around 300 BC! How did they do it?

For this, let's turn to

20) Ian Mueller, *Philosophy of Mathematics and Deductive Structure in Euclid's Elements*, MIT Press, Cambridge Massachusetts, 1981.

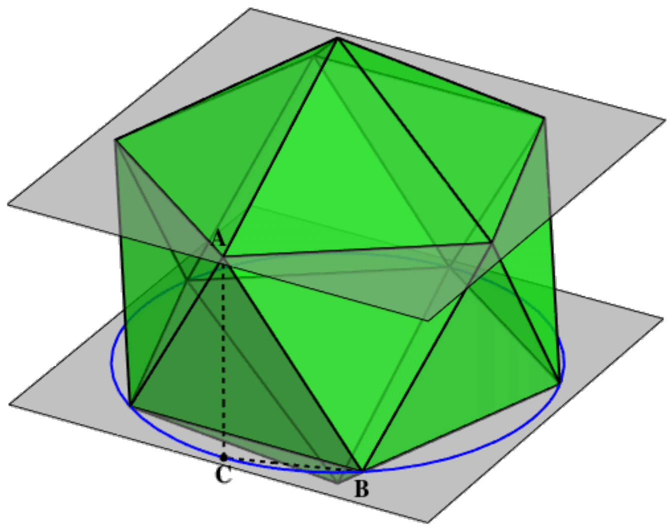
This is reputed to be be the most thorough investigation of the logical structure of Euclid's Elements! And starting on page 257 he discusses how people could have discovered $P^2 = H^2 + D^2$ by staring at an icosahedron!

This should not be too surprising. After all, there are pentagons, hexagons and decagons visible in the icosahedron. But I was completely stuck until I cheated and read Mueller's explanation.

If you hold an icosahedron so that one vertex is on top and one is on bottom, you'll see that its vertices are arranged in 4 horizontal layers. From top to bottom, these are:

- 1 vertex on top
- 5 vertices forming a pentagon: the "upper pentagon"
- 5 vertices forming a pentagon: the "lower pentagon"
- 1 vertex on bottom

Pick a vertex from the upper pentagon: call this A. Pick a vertex as close as possible from the lower pentagon: call this B. A is not directly above B. Drop a vertical line down from A until it hits the horizontal plane on which B lies. Call the resulting point C. If you think about this, you'll see that ABC is a right triangle. Greg Egan drew a picture of it:



And if we apply the Pythagorean theorem to this triangle we'll get the equation

$$P^2 = H^2 + D^2$$

To see this, we only need to check that:

- the length AB equals the edge of a pentagon inscribed in a circle;
- the length AC equals the edge of a hexagon inscribed in a circle;
- the length BC equals the edge of a decagon inscribed in a circle.

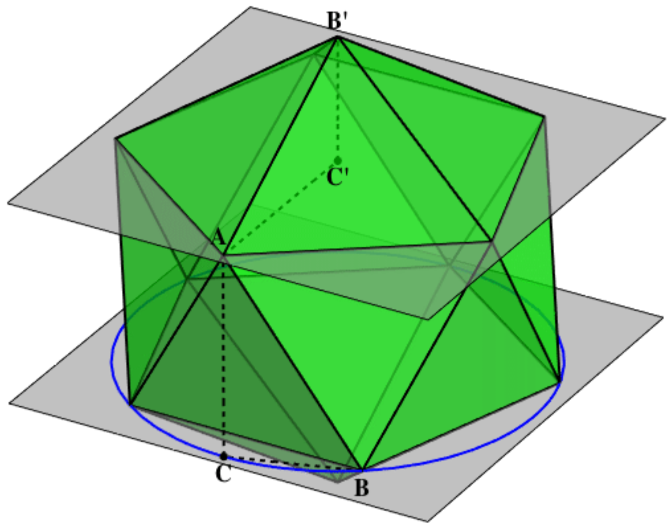
Different circles, but of the same radius! What's this radius? The 5 vertices of the lower pentagon lie on the circle shown in blue. This circle has the right radius.

Using this idea, it's easy to see that the length AB equals the edge of a pentagon inscribed in a circle. It's also easy to see that BC equals the edge of a decagon inscribed in a circle of the same radius. The hard part, at least for me, is seeing that AC equals the edge of a hexagon inscribed in a circle of the same radius... or in other words, the radius of that circle! (The hexagon seems to be a red herring.)

To prove this, it would suffice to show the following marvelous fact: the distance between the "upper pentagon" and the "lower pentagon" equals the radius of the circle containing the vertices of the upper pentagon!

Can you prove this?

In Mueller's book, he suggests various ideas the Greeks could have had about this. Here's one:



The right triangle ABC is shown here. The trick is to construct another right triangle $AB'C'$. Here B' is the top vertex, and C' is where a line going straight down from B' hits the plane containing the upper pentagon.

Remember, we're trying to show the distance between the upper pentagon and lower pentagon equals the radius of the circle containing the vertices of the upper pentagon.

But that's equivalent to showing that AC' is congruent to AC.

To do this, it suffices to show that the right triangles ABC and $AB'C'$ are congruent! Can you do it?

In the references to Mueller's book, he says the historians Dijksterhuis (in 1929) and Neuenschwander (in 1975) claimed this is "intuitively evident". But I don't know if that means it's easy to prove!

I thank Toby Bartels and Greg Egan for help with this stuff. I also thank Jim Stasheff for passing on an email from Joe Neisendorfer pointing out Mellinger's picture of the Milky Way.

Addendum: Kevin Buzzard explained some of the Galois theory behind why the pentagon can be constructed with ruler and compass — or in other words, why the quartic

$$z^4 + z^3 + z^2 + z + 1 = 0$$

can be solved by solving first one quadratic and then another.

He wrote:

> *Now, quartic equations can always be solved using radicals*
>
> *That's because S_4 is a solvable group, and all Galois groups of quartics will live in S_4 (and will usually be S_4). . .*

> *Luckily, we can solve this one by repeatedly using the quadratic equation!*

("this one" being $z^4 + z^3 + z^2 + z + 1 = 0$.)

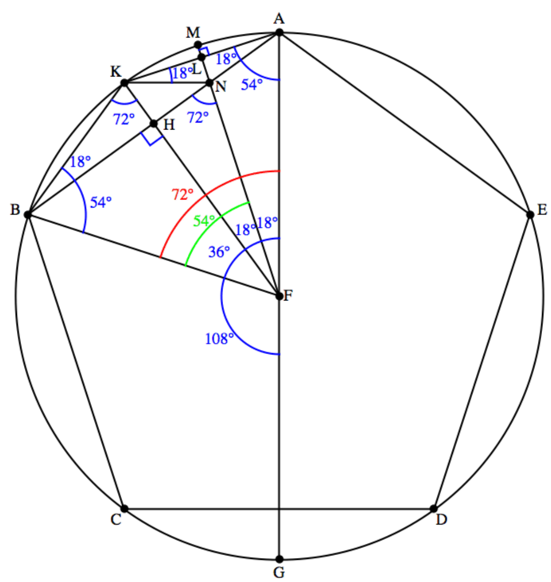
. . . and that's *because the Galois group of that* specific *irreducible polynomial is "only" cyclic of order 4. The splitting field is $\mathbb{Q}(\zeta_5)$, which is a cyclotomic field, so has Galois group $(\mathbb{Z}/5\mathbb{Z})^*$. No $\mathbb{Z}/3\mathbb{Z}$ factors so no messing around with cube roots, for example. . .*

> *So while I said this proof would be uninspired, it did require a tiny glimmer of inspiration.*

With this observation above, I'm trying to convince you that the proof really is *completely uninspired ! To solve the quartic by solving two quadratics, you need to locate the degree 2 subfield of $\mathbb{Q}(z)$ ($z = \zeta_5$) and aim towards it (because it's your route to the solution). This subfield is clearly the real numbers in $\mathbb{Q}(z)$, and the real numbers in $\mathbb{Q}(z)$ contains $z + z^* = z + z^{-1}$. So that's sort of a completely conceptual explanation of why the trick works and why it's crucial to introduce $z + z^{-1}$.*

Here ζ_5 is number-theorist's jargon for a "primitive 5th root of unity", which in turn is number-theorist's jargon for any 5th root of 1 except for 1 itself.

Greg Egan gave a nice modern version of Euclid's original proof of Prop. XIII.10, which states that if you take take a circle and inscribe a regular pentagon, a regular hexagon, and a regular decagon, and make a triangle out of their sides, it's a right triangle!

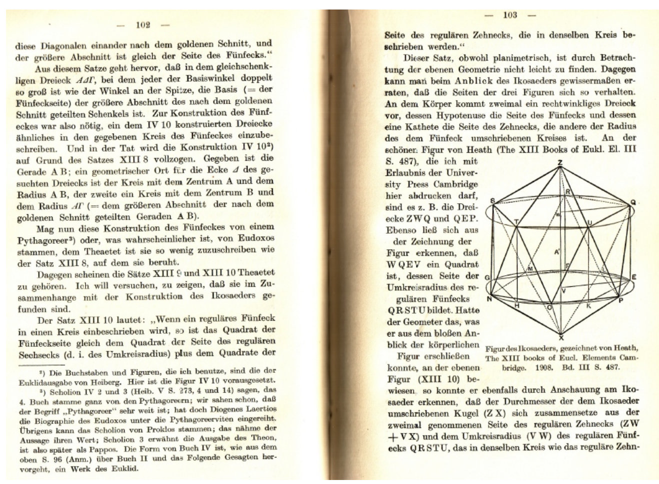


Here's a version of the proof Euclid gave, adapted from the version JB cited. Rather than proving that various angles here are identical, I've just written

> *in the (easily established) numerical values; there's nothing tricky here, so we might as well take them as given.*
>
> *Triangle ABF is similar to triangle BFN. So $AB/BF = BF/FN = BF/BN$, with the last equality true because the triangles are isosceles with $FN = BN$. Thus $BF^2 = AB \cdot BN$*
>
> *Triangle BAK is similar to triangle KAN. So $BA/AK = KA/AN$. Thus $AK^2 = AB \cdot AN$.*
>
> *Adding our two results, we have: $BF^2 + AK^2 = AB \cdot (AN + BN) = AB^2$.*
>
> *BF is our radius, AK is a decagon side, and AB is a pentagon side. Well done Euclid.*

On Thanksgiving of 2009, I got ahold of Eva Sachs' 1917 book *Die Fnf Platonischen Krper*, mentioned above. It's supposed to be the authoritative tome on the early history of the Platonic solids.

If anyone out there reads German, I'd love a translation of what she says about the icosahedron and the pentagon-hexagon-decagon identity. Click on the pictures for larger views:



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diese Diagonalen einander nach dem goldenen Schnitt, und der größere Abschnitt ist gleich der Seite des Fünfecks."

Aus diesem Satze geht hervor, daß in dem gleichschenkligen Dreieck $A\Delta\Gamma$, bei dem jeder der Basiswinkel doppelt so groß ist wie der Winkel an der Spitze, die Basis (= der Fünfeckseite) der größere Abschnitt des nach dem goldenen Schnitt geteilten Schenkels ist. Zur Konstruktion des Fünfeckes war also nötig, ein dem IV 10 konstruierten Dreiecke ähnliches in den gegebenen Kreis des Fünfeckes einzubeschreiben. Und in der Tat wird die Konstruktion IV 10[2]) auf Grund des Satzes XIII 8 vollzogen. Gegeben ist die Gerade A B; ein geometrischer Ort für die Ecke Δ des gesuchten Dreiecks ist der Kreis mit dem Zentrum A und dem Radius A B, der zweite ein Kreis mit dem Zentrum B und dem Radius $\Delta\Gamma$ (= dem größeren Abschnitt der nach dem goldenen Schnitt geteilten Geraden A B).

Mag nun diese Konstruktion des Fünfeckes von einem Pythagoreer[3]) oder, was wahrscheinlicher ist, von Eudoxos stammen, dem Theaetet ist sie so wenig zuzuschreiben wie der Satz XIII 8, auf dem sie beruht.

Dagegen scheinen die Sätze XIII 9 und XIII 10 Theaetet zu gehören. Ich will versuchen, zu zeigen, daß sie im Zusammenhange mit der Konstruktion des Ikosaeders gefunden sind.

Der Satz XIII 10 lautet: „Wenn ein reguläres Fünfeck in einen Kreis einbeschrieben wird, so ist das Quadrat der Fünfeckseite gleich dem Quadrat der Seite des regulären Sechsecks (d. i. des Umkreisradius) plus dem Quadrate der

[2]) Die Buchstaben und Figuren, die ich benutze, sind die der Euklidausgabe von Heiberg. Hier ist die Figur IV 10 vorausgesetzt.

[3]) Scholion IV 2 und 3 (Heib. V S. 273, 4 und 14) sagen, das 4. Buch stamme ganz von den Pythagoreern; wir sahen schon, daß der Begriff „Pythagoreer" sehr weit ist; hat doch Diogenes Laertios die Biographie des Eudoxos unter die Pythagoreerviten eingereiht. Übrigens kann das Scholion von Proklos stammen; das nähme der Aussage ihren Wert; Scholion 3 erwähnt die Ausgabe des Theon, ist also später als Pappos. Die Form von Buch IV ist, wie aus dem oben S. 96 (Anm.) über Buch II und das Folgende Gesagten hervorgeht, ein Werk des Euklid.

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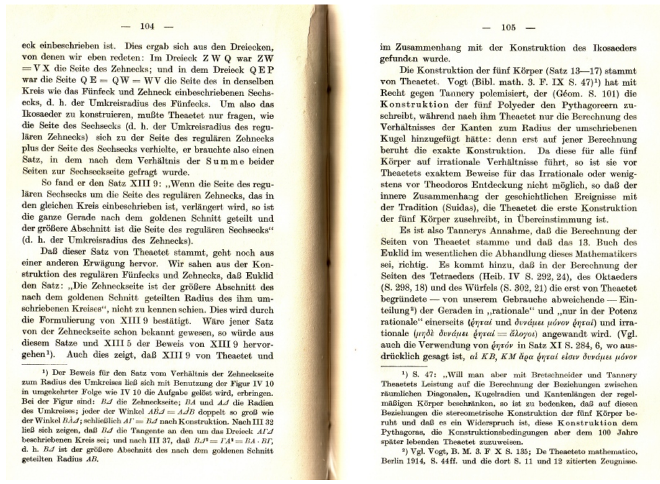
Seite des regulären Zehnecks, die in denselben Kreis beschrieben werden."

Dieser Satz, obwohl planimetrisch, ist durch Betrachtung der ebenen Geometrie nicht leicht zu finden. Dagegen kann man beim Anblick des Ikosaeders gewissermaßen erraten, daß die Seiten der drei Figuren sich so verhalten. An dem Körper kommt zweimal ein rechtwinkliges Dreieck vor, dessen Hypotenuse die Seite des Fünfecks und dessen eine Kathete die Seite des Zehnecks, die andere der Radius des dem Fünfeck umschriebenen Kreises ist. An der schöner Figur von Heath (The XIII Books of Eukl. El. III S. 487), die ich mit Erlaubnis der University Press Cambridge hier abdrucken darf, sind es z. B. die Dreiecke ZWQ und QEP. Ebenso ließ sich aus der Zeichnung der Figur erkennen, daß WQEV ein Quadrat ist, dessen Seite der Umkreisradius des regulären Fünfecks QRSTU bildet. Hatte der Geometer das, was er aus dem bloßen Anblick der körperlichen Figur erschließen konnte, an der ebenen Figur (XIII 10) bewiesen, so konnte er ebenfalls durch Anschauung am Ikosaeder erkennen, daß der Durchmesser der dem Ikosaeder umschriebenen Kugel (Z X) sich zusammensetze aus der zweimal genommenen Seite des regulären Zehnecks (ZW + VX) und dem Umkreisradius (V W) des regulären Fünfecks QRSTU, das in denselben Kreis wie das reguläre Zehn-

Figur des Ikosaeders, gezeichnet von Heath, The XIII books of Eucl. Elements Cambridge. 1908. Bd. III S. 487.



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eck einbeschrieben ist. Dies ergab sich aus den Dreiecken, von denen wir eben redeten: Im Dreieck Z W Q war ZW = V X die Seite des Zehnecks; und in dem Dreieck Q E P war die Seite Q E = QW = WV die Seite des in denselben Kreis wie das Fünfeck und Zehneck einbeschriebenen Sechsecks, d. h. der Umkreisradius des Fünfecks. Um also das Ikosaeder zu konstruieren, mußte Theaetet nur fragen, wie die Seite des Sechsecks (d. h. der Umkreisradius des regulären Zehnecks) sich zu der Seite des regulären Zehnecks plus der Seite des Sechsecks verhielte, er brauchte also einen Satz, in dem nach dem Verhältnis der Summe beider Seiten zur Sechseckseite gefragt wurde.

So fand er den Satz XIII 9: „Wenn die Seite des regulären Sechsecks um die Seite des regulären Zehnecks, das in den gleichen Kreis einbeschrieben ist, verlängert wird, so ist die ganze Gerade nach dem goldenen Schnitt geteilt und der größere Abschnitt ist die Seite des regulären Sechsecks“ (d. h. der Umkreisradius des Zehnecks).

Daß dieser Satz von Theaetet stammt, geht noch aus einer anderen Erwägung hervor. Wir sahen aus der Konstruktion des regulären Fünfecks und Zehnecks, daß Euklid den Satz: „Die Zehneckseite ist der größere Abschnitt des nach dem goldenen Schnitt geteilten Radius des ihm umschriebenen Kreises“, nicht zu kennen schien. Dies wird durch die Formulierung von XIII 9 bestätigt. Wäre jener Satz von der Zehneckseite schon bekannt gewesen, so würde aus diesem Satze und XIII 5 der Beweis von XIII 9 hervorgehen[1]). Auch dies zeigt, daß XIII 9 von Theaetet und

[1]) Der Beweis für den Satz vom Verhältnis der Zehneckseite zum Radius des Umkreises ließ sich mit Benutzung der Figur IV 10 in umgekehrter Folge wie IV 10 die Aufgabe gelöst wird, erbringen. Bei der Figur sind: $B\Delta$ die Zehneckseite; BA und $A\Delta$ die Radien des Umkreises; jeder der Winkel $AB\Delta = A\Delta B$ doppelt so groß wie der Winkel $BA\Delta$; schließlich $A\Gamma = B\Delta$ nach Konstruktion. Nach III 32 ließ sich zeigen, daß $B\Delta$ die Tangente an den um das Dreieck $A\Gamma\Delta$ beschriebenen Kreis sei; und nach III 37, daß $B\Delta^2 = \Gamma A^2 = BA \cdot B\Gamma$, d. h. $B\Delta$ ist der größere Abschnitt des nach dem goldenen Schnitt geteilten Radius AB.

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im Zusammenhang mit der Konstruktion des Ikosaeders gefunden wurde.

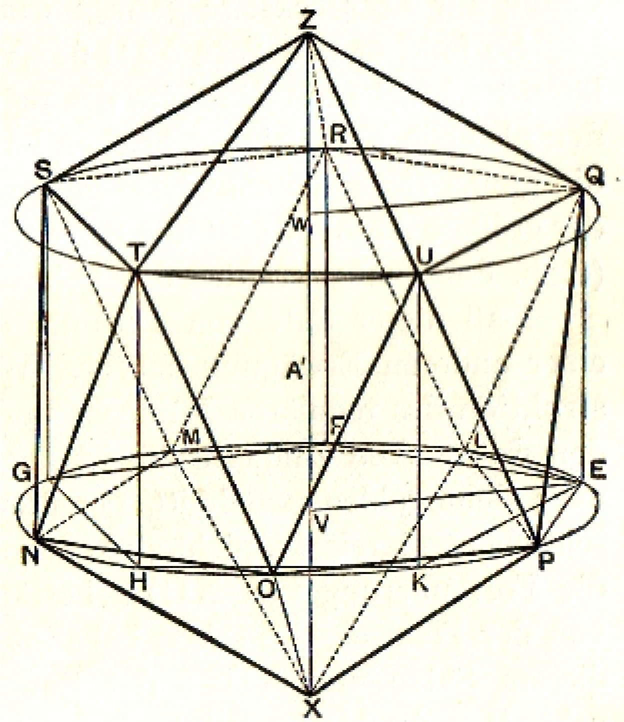
Die Konstruktion der fünf Körper (Satz 13—17) stammt von Theaetet. Vogt (Bibl. math. 3. F. IX S. 47)[1]) hat mit Recht gegen Tannery polemisiert, der (Géom. S. 101) die Konstruktion der fünf Polyeder den Pythagoreern zuschreibt, während nach ihm Theaetet nur die Berechnung des Verhältnisses der Kanten zum Radius der umschriebenen Kugel hinzugefügt hätte: denn erst auf jener Berechnung beruht die exakte Konstruktion. Da diese für alle fünf Körper auf irrationale Verhältnisse führt, so ist sie vor Theaetets exaktem Beweise für das Irrationale oder wenigstens vor Theodoros Entdeckung nicht möglich, so daß der innere Zusammenhang der geschichtlichen Ereignisse mit der Tradition (Suidas), die Theaetet die erste Konstruktion der fünf Körper zuschreibt, in Übereinstimmung ist.

Es ist also Tannerys Annahme, daß die Berechnung der Seiten von Theaetet stamme und daß das 13. Buch des Euklid im wesentlichen die Abhandlung dieses Mathematikers sei, richtig. Es kommt hinzu, daß in der Berechnung der Seiten des Tetraeders (Heib. IV S. 292, 24), des Oktaeders (S. 298, 18) und des Würfels (S. 302, 21) die erst von Theaetet begründete — von unserem Gebrauche abweichende — Einteilung[2]) der Geraden in „rationale“ und „nur in der Potenz rationale“ einerseits (*ῥηταί* und *δυνάμει μόνον ῥηταί*) und irrationale (*μηδὲ δυνάμει ῥηταί = ἄλογοι*) angewandt wird. (Vgl. auch die Verwendung von *ῥητόν* in Satz XI S. 284, 6, wo ausdrücklich gesagt ist, *αἱ KB, KM ἄρα ῥηταί εἰσιν δυνάμει μόνον*

[1]) S. 47: „Will man aber mit Bretschneider und Tannery Theaetets Leistung auf die Berechnung der Beziehungen zwischen räumlichen Diagonalen, Kugelradien und Kantenlängen der regelmäßigen Körper beschränken, so ist zu bedenken, daß auf diesen Beziehungen die stereometrische Konstruktion der fünf Körper beruht und daß es ein Widerspruch ist, diese Konstruktion dem Pythagoras, die Konstruktionsbedingungen aber dem 100 Jahre später lebenden Theaetet zuzuweisen.

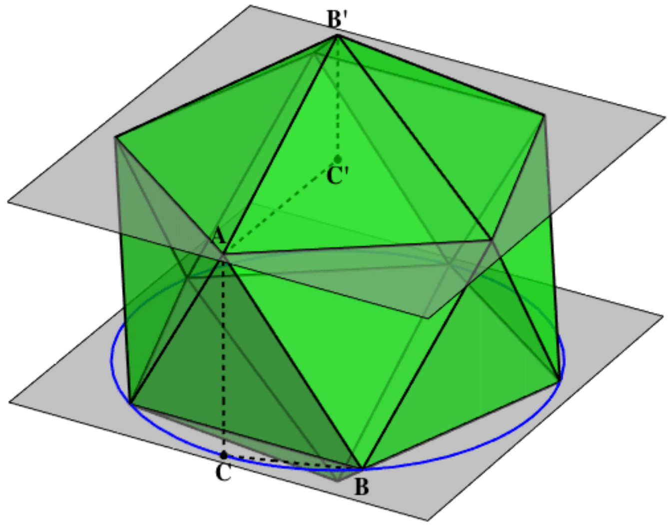
[2]) Vgl. Vogt, B. M. 3. F X S. 135; De Theaeteto mathematico, Berlin 1914, S. 44ff. und die dort S. 11 und 12 zitierten Zeugnisse.

I think the first page gets interesting around “Der Satz XIII 10 lautet. . . ” (“Proposition XIII.10 says. . . ”) and more interesting after “Dieser Satz, obwohl planimetrisch, ist durch Betrachtung der ebenen Geometrie nicht leicht zu finden”. (“This proposition, though planar in character, is not so easy to find through considerations of plane geometry.")



Figur des Ikosaeders, gezeichnet von Heath, The XIII books of Eucl. Elements Cambridge. 1908. Bd. III S. 487.

You'll notice that she focuses our attention on the right triangles ZWQ and QEP, which are the triangles ABC and $AB'C'$ that I mentioned above:



Proving that these are congruent is the key to the pentagon-decagon-hexagon identity!

I'm especially curious about the footnote on page 104, and also the remark further up this page saying "So fand er den Satz XIII 9. . . " ("This is how he found Proposition XII.9").

To see Greg Egan's beautiful proof of the pentagon-decagon-hexagon identity, which meets my challenge above, see "Week 284".

For more discussion visit the n-Category Caf.

> Geometry enlightens the intellect and sets one's mind right. All its proofs are very clear and orderly. It is hardly possible for errors to enter into geometric reasoning, because it is well arranged and orderly. Thus the mind that constantly applies itself to geometry is unlikely to fall into error.
>
> — *Ibn Khaldun*

Week 284

November 24, 2009

A couple of weeks ago there was a meeting of the American Mathematical Society here at UC Riverside. Mathematicians flooded in from across the western US and even further. They gave hundreds of 20-minute talks, drank lots of coffee, ate a few too many pastries, and chatted with each other. Julie Bergner and I ran a session at this conference. My student Chris Rogers took videos of the talks in our session, and you can see them here:

1) "Special session on homotopy theory and higher algebraic structures", AMS Western Section Meeting, November 7–8, 2009. Talks available as Quicktime videos at `http://math.ucr.edu/~jbergner/amsriverside09.htm`

These talks add up to a nice look at recent work on homotopy theory, n-categories, and categorification — some of my favorite subjects. So, I'd like to quickly summarize each talk and give some links to related papers.

But first: something a bit less technical!

Last week I asked you to provide a nice proof of Proposition 10 from the last book of Euclid — the one where he constructs the Platonic solids. Euclid uses this proposition to construct the icosahedron, but it's appealing in its own right. In modern language it says:

> *Inscribe a regular pentagon, hexagon and decagon in a circle, and call their side lengths P, H and D. Then*
> $$P^2 = H^2 + D^2.$$

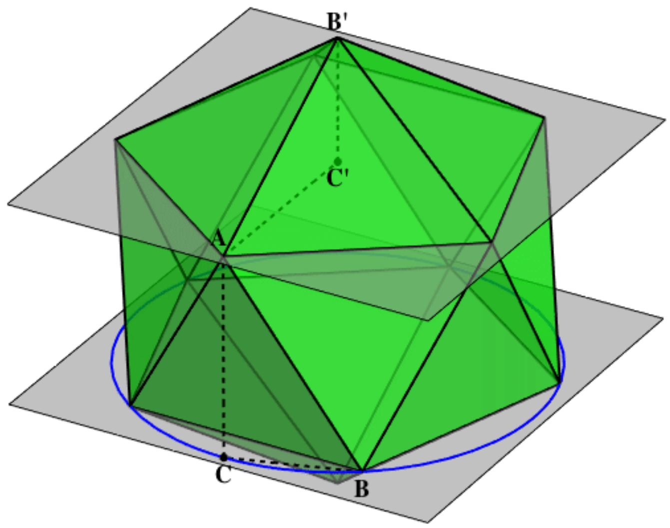
I find this fascinating. One reason is that it's simple but far from obvious. Another is that it's secretly all about the golden ratio and its role in 5-fold symmetry. And another is that Euclid's proof is ingenious but not very intuitive — so it seems there should be a better proof. For example, a proof that uses the icosahedron!

(Last week I gave a proof using algebra and trigonometry, but it wasn't terribly interesting.)

The science fiction writer Greg Egan said that Euclid's proof was "really dazzling, but it made me feel like he'd pulled a coin out from behind my ear." Egan wrote a modernized version of this proof, which you can see in the Addenda to "Week 283". But he then went on to give a number of other proofs, including two that I like a lot better:

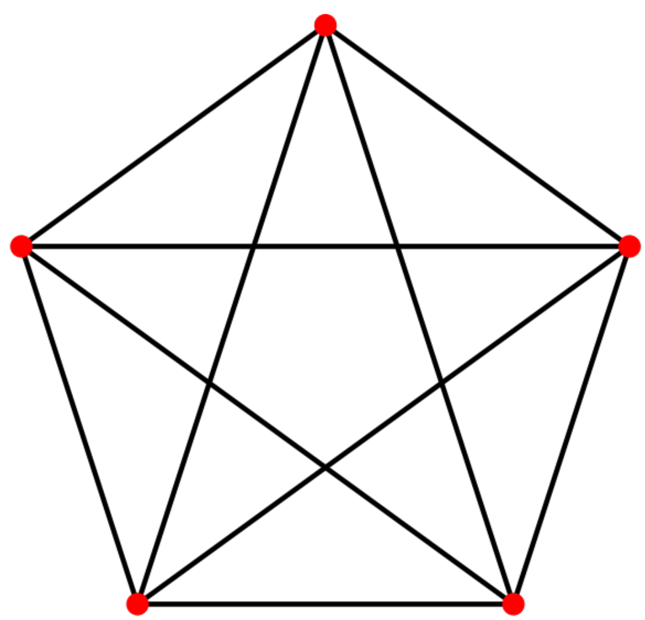
2) nLab, "Pentagon-decagon-hexagon identity", `http://www.ncatlab.org/nlab/show/pentagon+decagon+hexagon+identity`

One of these proofs uses the icosahedron. As I'd dreamt in "Week 283", it proceeds by showing that two right triangles hiding in the icosahedron are congruent. Namely, the triangles ABC and $AB'C'$ shown here:



The other proof is purely 2-dimensional. For this, Egan starts by recalling Proposition 9 from the same book of Euclid. This result states the main property of the "golden triangle".

What's a golden triangle? Well, if you draw a regular pentagon and connect each vertex to every other, you'll get a pentagram in a pentagon — but you'll also see lots of tall skinny isosceles triangles:



These are "golden triangles". They have angles of 36 degrees, 72 degrees, and 72 degrees. How many are there in this picture?

In Proposition 9, Euclid shows that for a golden triangle, the ratio of the edge lengths is the golden ratio. Actually he shows something equivalent, roughly this:

> *Inscribe a regular hexagon and decagon in a circle. Then the ratio of their side lengths is the golden ratio:*
>
> $$\frac{D}{H} = \varphi = \frac{-1+\sqrt{5}}{2}$$

Why is this equivalent? Well, if you inscribe a regular decagon in a circle and draw lines from its center to its vertices, you get ten golden triangles. The long edge of each triangle is H, since the radius of the circle equals the edge of an inscribed circle. The short edge is D.

Oddly, Euclid does not use Proposition 9 to prove Proposition 10, even though it's relevant and it comes right before! But Egan's proof uses it. Check out the nLab entry for details and pretty pictures. I think it's great that 21st-century technology is being used to improve a proof dating back to 300 BC.

Now... on to the talks on homotopy theory and higher algebraic structures! I'm afraid the length of my summaries will be proportional to how much I understood. You can click on the talk titles to see the videos.

Bright and early at 8 am on Saturday morning, Aaron Lauda kicked off the special session with a talk on "Categorifying quantum groups". Luckily he'd come from the east coast, so he was wide awake, and his energy was contagious.

From the very beginning of This Week's Finds you can see that I was interested in Crane and Frenkel's dream of categorified quantum groups. Now this dream is coming true! Aaron gave a great series of talks on this subject in Kyoto this February, and you can see them all here:

3) Aaron Lauda, Kyoto lectures:

- Categorification of quantum $\mathfrak{sl}(2)$,
- Categorification of one half of the quantum group,
- Categorification of quantum $\mathfrak{sl}(n)$,
- Cyclotomic quotients of the rings $R(\nu)$.

Available at `http://www.math.columbia.edu/~lauda/talks/kyoto/`

At Riverside he explained what people often call the "Khovanov-Lauda algebra" R associated to a simply laced Dynkin diagram. This gives a way of categorifying the "positive part" of the corresponding quantum group.

4) Mikhail Khovanov and Aaron Lauda, "A diagrammatic approach to categorification of quantum groups I-III", available as `arXiv:0803.4121`, `arXiv:0804.2080`, and `arXiv:0803.3250`.

Huh? Well, just as the group of matrices has a subgroup consisting of upper triangular matrices and a subgroup consisting of lower triangular matrices, so any quantum group breaks into a "positive part" and a "negative part", with a bit of overlap. It's easier to categorify either of these parts than the whole thing, because when you deal with the whole thing you get formulas involving negative numbers, which are harder to categorify.

How does the algebra R help us categorify the positive part of a quantum group? Or, for that matter, the negative part? — the two parts look alike, so we randomly choose to work with the positive part.

The answer is simple: we just form the category of representations of R!

But how do we get back from this category to the positive part of the quantum group? In other words, how do we "decategorify"? Again, the answer is simple: just take its Grothendieck group! A bit more precisely: we take the category of finitely generated projective R-modules, and look at isomorphism classes of these, and let these generate an abelian group with relations saying that direct sums should act like sums:

$$[M \oplus N] = [M] + [N]$$

This gives a certain "integral form" of the positive part of the quantum group. If we tensor this with the complex numbers, we get the more familiar complex version of the quantum group.

One of the great virtues of the Khovanov-Lauda algebra is that it has a nice presentation, with generators and relations given in terms of pretty pictures. This is great for computations. However, the presentation is a bit complicated, so I can't help but wonder where it came from. Maybe there's some nice geometry underlying the whole story?

Indeed, Aaron has also worked on more geometrical approaches to categorifying quantum groups, at least for the simplest of simple Lie algebras, namely $\mathfrak{sl}(2)$:

5) Aaron Lauda, "A categorification of quantum $\mathfrak{sl}(2)$", available as `arXiv:0803.3652`.

6) Aaron Lauda, "Categorified quantum $\mathfrak{sl}(2)$ and equivariant cohomology of iterated flag varieties", available as `arXiv:0803.3848`.

But there are also lots of other people tackling the geometrical side of the story. One of these is Anthony Licata of Stanford University! Right after Aaron, he gave a talk on "Categorification via quiver varieties", based on these papers:

6) Sabin Cautis, Joel Kamnitzer, and Anthony Licata, "Coherent sheaves and categorical $\mathfrak{sl}(2)$ actions", available as `arXiv:0902.1796`.

7) Sabin Cautis, Joel Kamnitzer, and Anthony Licata, "Derived equivalences for cotangent bundles of Grassmannians via categorical $\mathfrak{sl}(2)$ actions", available as `arXiv:0902.1797`.

The first paper studies various notions of a categorified representation of $\mathfrak{sl}(2)$. The second studies an example coming from cotangent bundles of Grassmannians. That's a lot of math to compress into a 20-minute talk! Luckily Licata was able to do it, by leaving out all but the most fundamental concepts.

His work follows the philosophy that "geometrization leads to categorification". This is based on a branch of math called "geometric representation theory".

The name here is a bit misleading, since a lot of group representation theory is geometrical in nature. For example, if we have a group G acting as symmetries of a space X, we get a representation of G on the vector space of functions on X. And there are many sophisticated refinements of this idea. But "geometric representation theory" is different. It gets representations in unexpected new ways, often starting from the cohomology of a space X on which G does *not* act!

I think this is the best place to start learning geometric representation theory:

8) Neil Chriss and Victor Ginzburg, *Representation Theory and Complex Geometry*, Birkhauser, Boston, 1997.

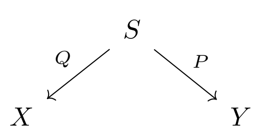
I've spent some time on this book, but not enough. The results still seem strange to me. They're like an outcropping of unfamiliar rocks poking through the strata of mathematics that make sense to me. I'd need to dig deeper to get a sense of what's going on down there. Just thinking about this makes me itch to understand geometric representation theory better. I know specific results, but not the overall pattern!

You expect, for example, to get representations of $\mathfrak{sl}(2)$ whenever you build vector spaces starting from $\mathbb{C}^2$. Why? Because the group $\mathrm{SL}(2)$ acts as symmetries of $\mathbb{C}^2$, and thus on any vector space functorially constructed from it. But Ginzburg found some unexpected new ways of getting representations of $\mathfrak{sl}(2)$... and Licata sketched how this lets you categorify these representations.

Here's the example Licata explained. The group $\mathrm{SL}(2)$ acts on $\mathbb{C}^2$ and thus on its nth tensor power. Everyone knows that. But we can also get this representation in an unexpected way. Start with the space of all k-dimensional subspaces of $\mathbb{C}^n$. This is called the "Grassmannian" $\mathrm{Gr}(k,n)$. Form a vector space by taking the cohomology of the cotangent bundle $T^*\mathrm{Gr}(k,n)$. Then take the direct sum of these vector spaces as k goes from 0 to n.

We get a big fat vector space. But here's the cool part: Ginzburg figured out how to make this big fat space into a representation of $\mathfrak{sl}(2)$! And this representation is isomorphic to the nth tensor power of $\mathbb{C}^2$.

The trick is to get operators on cohomology groups that satisfy the relations for $\mathfrak{sl}(2)$. As usual in geometric representations theory, we build these using "spans". These are setups where you have three spaces and two maps like this:



We can pull back cohomology classes along P, and then if we're lucky we can push them forward along Q, getting an operator from the cohomology of X to the cohomology of Y. I explained why spans are geometrically interesting back in "Week 254".

Anyway, so Ginzburg got a representation of $\mathfrak{sl}(2)$ using this trick. To categorify this representation, Licata replaced the cohomology of $T^*\mathrm{Gr}(k,n)$ by a category called the "bounded derived category of coherent sheaves" on this space. That's a plausible strategy, because it's known quite generally that for any smooth variety X you can take the Grothendieck group of this category and get back the cohomology of X.

In fact, if you have no idea what a "bounded derived category of coherent sheaves" is, this should make you want to know! It's a categorification of cohomology. Here's a good place to start learning more:

9) Andrei Caldararu, "Derived categories of sheaves: a skimming". Available at `http://www.math.wisc.edu/~andreic/publications/lnPoland.pdf`

Next came two talks on another approach to categorification, called "groupoidification". This involves replacing vector spaces by groupoids and linear operators by spans of groupoids. The reverse process, "degroupoidification", is an entirely systematic procedure for squashing groupoids into vector spaces and spans of groupoids into linear operators. I explained how this works back in "Week 256".

First Alex Hoffnung spoke about "A categorification of the Hecke algebra". The idea here is to see the Hecke algebras associated to Dynkin diagrams as a special case of a much more general construction: the Hecke bicategory.

Given a finite group G, the Hecke bicategory $\mathsf{Hecke}(G)$ is a gadget where:

- objects are finite G-sets;
- the groupoid of morphisms from X to Y is the weak quotient $(X \times Y)//G$.

Here the "weak quotient" is a bit like the ordinary quotient of a set by a group action — but instead making elements *equal* when there's a group element mapping one to another, we make them *isomorphic*. So, it's a groupoid. (For more details, see "Week 249".)

Using a systematic procedure for turning groupoids into vector spaces, we can squash $\mathsf{Hecke}(G)$ down into a category that has a mere vector space of morphisms from X to Y.

Now, a category where the set of morphisms between any two objects is a *vector space*, and composition is linear in each argument, is sometimes called an "algebroid". Why? Because an algebroid with one object is an algebra — in the same way that a groupoid with an object is a group.

So, the Hecke bicategory gets squashed down into something that deserves to be called the "Hecke algebroid" of G.

Now pick a finite field and a Dynkin diagram. This gives a simple algebraic group G and a very important G-set X, called the "flag variety" of G. Take the Hecke algebroid of G and concentrate your attention on the morphisms from X to X. By what I've said, these form an algebra. And this is the famous "Hecke algebra" associated to our Dynkin diagram! The usual parameter q that appears in the definition of a Hecke algebra is just the number of elements in our finite field.

Alex Hoffnung illustrated his talk with a picture of a cow jumping over the moon, wearing a bowtie, and getting killed by a lightning bolt. You'll have to watch his talk to see how this is relevant. The otherwise excellent slides do not explain this joke:

8) Alex Hoffnung, "A categorification of the Hecke algebra", `http://math.ucr.edu/~alex/hecke.pdf`

Then Christopher Walker gave a talk on "A categorification of Hall algebras". Unfortunately, the cameraman showed up a little late, so the video of his talk starts after a couple of minutes have gone by. Fortunately, the next week he passed his oral exam at UCR with a longer version of the same talk! So, check out the slides for that:

9) Christopher Walker, "A categorification of Hall algebras", http://math.ucr.edu/~cwalker66/Oral_Exam_talk_11_10.pdf

But here's the idea in a nutshell. Take a simply-laced Dynkin diagram. Draw arrows on the edges to get a directed graph. Let this graph freely generate a category, say Q. There's a groupoid of "quiver representations", where:

- objects are functors from Q to the category of vector spaces over some fixed finite field;
- morphisms are natural isomorphisms.

Next, apply our systematic procedure for turning groupoids into vector spaces! In the case at hand, we get the positive part of the quantum group associated to our Dynkin diagram. The usual parameter q that appears in the definition of a quantum group is just the number of elements in our finite field.

(Here we see a difference from the Khovanov-Lauda approach, where q is a formal variable.)

So far, this is actually an old theorem of Ringel. The trick is to use it to systematically "groupoidify" quantum groups — or at least their positive parts — and then work with them at the groupoidified level. And that's what Christopher is doing now!

His talk explains more, and you can learn more about groupoidification and its applications to Hecke and Hall algebras here:

10) John Baez, Alex Hoffnung and Christopher Walker, "Higher-dimensional algebra VII: groupoidification". arXiv:0908.4305.

Next came three talks on homotopy theory.

Jonathan Lee of Stanford University spoke on "Homotopy colimits and the space of square-zero upper-triangular matrices". You can see slides of his talk here:

11) Jonathan Lee, "Homotopy colimits and the space of square-zero upper-triangular matrices", http://math.stanford.edu/~jlee/homotopy-talk.pdf

He talked about his work on a conjecture of Halperin and Carlsson. There are different ways to formulate it, but here's a nice topological way. Suppose the torus T^n acts freely on a finite CW complex X. Then the sum of the Betti numbers of X is at least 2^n. There's also a nice purely algebraic way!

Nitu Kitchloo of UC San Diego spoke on "Universal Bott-Samelson resolutions". As a warmup for this, I should just tell you what a Bott-Samelson resolution is.

I spoke quite a bit about Schubert cells in "Week 184" and subsequent Weeks. The idea is that if you have a Grassmannian, or more generally any space of the form G/P where G is a simple Lie group and P is a parabolic subgroup, it comes equipped with a decomposition into cells. These are the "Schubert cells". They're packed with fascinating algebra, geometry, and combinatorics. They are, in fact, algebraic varieties! But, they're not smooth — they're singular.

And so, if you were an algebraic geometer, you might be tempted to "resolve" their singularities: that is, find a smooth variety that maps onto them in a nice way. Bott and Samelson figured out a way to do this... but not just one way. So, you might want to find a "best" — or more technically, a "universal" — Bott-Samelson resolution. And that's what Nitu Kitchloo talked about.

After lunch, Maia Averett of Mills College started the show with a talk on "Real Johnson-Wilson theories", based on work with Nitu Kitchloo and Steve Wilson. This was heavy-duty homotopy theory of the sort I can only gape at in awe. It's part of a big network of ideas which include elliptic cohomology and higher steps in the "chromatic filtration" — topics I discussed back in "Week 197" and "Week 255".

You can see some slides here:

12) Maia Averett, "Real Johnson-Wilson theories", `http://www.math.uchicago.edu/~fiore/1/Averett.pdf`

Real Johnson-Wilson theories are certain generalized cohomology theories (see "Week 149"). They can be thought of as "higher" versions of real K-theory. Thanks to complex conjugation, the group $\mathbb{Z}/2$ acts on the complex K-theory spectrum KU, and if we take the homotopy fixed points we get the real K-theory spectrum KO. But complex K-theory is just the first of the Johnson-Wilson theories!

To get the others, you do something roughly like this. (I'm reading some stuff to figure this out, and I could be getting it wrong.) The spectrum for complex cobordism theory is called MU. If you localize this at 2 you get something called the Brown-Peterson spectrum, BP. The generalized cohomology for this, applied to a one-point space, is a ring on infinitely many generators. If you do some trick to kill off all the generators above the nth, you get the nth Johnson-Wilson theory. And since this was built starting from complex cobordism theory, complex conjugation acts on it. So, we can take the homotopy fixed points, you get the nth "real" Johnson-Wilson theory.

Emin Tatar of Florida State University spoke on "Abelian sheaves and Picard stacks":

13) A. Emin Tatar, "Abelian sheaves and Picard stacks", `http://www.math.ucr.edu/~jbergner/tatar_slides.pdf`

This talk assumed a fair amount of background, so let me just sketch a bit of that background. For more details, try this:

14) A. Emin Tatar, "Length 3 complexes of abelian sheaves and Picard 2-stacks", available as arXiv:0906.2393.

You've probably heard me talk about 2-groups. These are categorified groups. More precisely, they're categories with a tensor product, where every morphism has an inverse and every object x has an inverse with respect to the tensor product: that is, an object x^* such that

$$x \otimes x^* \cong 1$$

and

$$x^* \otimes x \cong 1$$

2-groups are a great way to dip your toe in vast ocean of n-category theory. They're one step to the right of groups in the n-groupoid version of the periodic table:

Table 17: k-tuply groupal n-groupoids

	$n = 0$	$n = 1$	$n = 2$
$k = 0$	sets	groupoids	2-groupoids
$k = 1$	groups	2-groups	3-groups
$k = 2$	abelian groups	braided 2-groups	braided 3-groups
$k = 3$	" "	symmetric 2-groups	sylleptic 3-groups
$k = 4$	" "	" "	symmetric 3-groups
$k = 5$	" "	" "	" "

Just as abelian groups are especially simple and nice, so are symmetric 2-groups. Where an abelian group obeys the equation

$$xy = yx$$

a symmetric 2-group instead has an isomorphism

$$S_{x,y} : x \otimes y \to y \otimes x$$

with the property that doing it twice gives the identity:

$$S_{y,x} S_{x,y} = 1$$

Lately people have been generalizing a lot of math from abelian groups to symmetric 2-groups. See "Week 266" for more, and especially this:

15) Mathieu Dupont, *Abelian categories in dimension 2*, Ph.D thesis, l'Universite Catholique de Louvain, 2008. Available as arXiv:0809.1760. Original available in French at http://hdl.handle.net/2078.1/12735

But the simplest symmetric 2-groups are those with this extra property:

$$S_{x,x} = 1$$

Emin Tatar calls these "Picard categories", following Deligne.

(I would like to call these "Picard 2-groups", but that might be confusing, since "Picard group" already means something quite different. To add to the confusion, it seems that Dupont and others use "Picard category" as a synonym for symmetric 2-group!)

Anyway, there's a nice description of Picard categories. They're all equivalent to the 2-groups that you get from 2-term chain complexes of abelian groups!

It's nice to see how this works. Take a 2-term chain complex of abelian groups:

$$A \stackrel{d}{\leftarrow} B$$

Then there's a category where the objects are elements of A, and the morphisms from a to a' are elements b of B with

$$a' = a + db$$

Addition lets you compose morphisms — but it also lets you add objects, making this category into a 2-group. And the abelianness makes this not just a symmetric 2-group, but even a Picard category!

But the cool fact is that every Picard category is equivalent to one arising this way.

In fact, Deligne went a lot further. There's a general principle that anything really important that you can do with sets, you can also do with sheaves of sets. So, you might guess that anything really important you can do with categories, you can do with sheaves of categories.

That's morally correct — but not quite technically correct, because we need to take the definition of "sheaf" and replace some equations by isomorphisms to make it applicable to categories. If we do this, we get the concept of a "stack".

Then everything works great. Just as we can talk about sheaves of abelian groups, we can talk about stacks of Picard categories — or "Picard stacks", for short. And the cool fact I mentioned generalizes to these! Every Picard stack is equivalent to one that comes from a 2-term complex of sheaves of abelian groups. This was proved by Deligne quite a while ago — it's Lemma 1.4.13 here:

15) Pierre Deligne, "La formule de dualit globale", *Sem. Geom. Algbrique Bois-Marie 1963/64, SGA 4 III*, No. XVIII, Springer Lecture Notes in Mathematics **305**, 1973, pp. 481–587. Also available at `http://www.math.polytechnique.fr/~laszlo/sga4/SGA4-3/sga43.pdf`

But you can also see a different proof in Proposition 8.3.2 of this paper by Tatar's advisor and Behrang Noohi:

16) Ettore Aldrovandi and Behrang Noohi, "Butterflies I: morphisms of 2-group stacks", *Adv. Math.* **221** (2009), 687–773. Also available as `arXiv:0808.3627`.

Now, what did Tatar do? He categorified all this stuff once more! In other words, he defined Picard 2-stacks, and proved that every Picard 2-stack is equivalent to one coming from a 3-term chain complex of sheaves of abelian groups!

Next, David Spivak of the University of Oregon spoke on "Mapping spaces in quasi-categories". Quasicategories are a nice way to formalize the idea of an $(\infty, 1)$-category — that is, an ∞-category where all the morphisms above the 1-morphisms are weakly invertible. Technically, quasicategories are just simplicial sets with a special property. So, one can study them using all the simplicial machinery that homotopy theorists have been developing over the years.

However, there are many other ways to formalize $(\infty, 1)$-categories. A classic one is "simplicial categories". These are just categories "enriched over simplicial sets". In other words, they have a simplicial set of morphisms from any object to any other object, and composition is a map of simplicial sets.

(If I'd been willing to use this jargon earlier, I could have defined an algebroid to be a category "enriched over vector spaces". However, I didn't want to scare away all my readers — at least, not so soon! By this point I figure all the wimps are gone.)

A while back, Jacob Lurie described a way to turn any quasicategory into a simplicial category — see for example Remark 1.1.5.18 here:

16) Jacob Lurie, *Higher Topos Theory*, Annals of Mathematics Studies **170**, Princeton University Press, Princeton, NJ, 2009. Also available as `arXiv:math/0608040`.

This involves taking two vertices of our quasicategory — which, remember, is just a simplicial set with some properties — and cooking up a simplicial set of "morphisms" from one to the other. Recently Daniel Dugger and David Spivak have come up with another way:

17) Daniel Dugger and David I. Spivak, "Rigidification of quasi-categories", available as `arXiv:0910.0814`.

18) Daniel Dugger and David I. Spivak, "Mapping spaces in quasi-categories", available as `arXiv:0911.0469`.

And that's what David explained in his talk!

The day concluded with two talks of a somewhat more concrete nature. Ben Williams of Stanford University spoke on "An application of A^1-homotopy theory to problems in commutative algebra". Like Jonathan Lee, the problems he was considering included the conjecture of Halperin and Carlsson that I mentioned before. But, he used ideas from A^1-homotopy theory. So, let me say a word about that.

I actually tried my hand at explaining A^1-homotopy theory near the end of "Week 255". It's an attempt to do homotopy theory for algebraic varieties, where homotopies are parametrized not by the interval but by the line — since the line is an algebraic variety. Algebraic geometers call the line A^1, just to make the rest of us feel dumb.

In his work on A^1-homotopy theory, Voevodsky studied certain cohomology groups for a variety X, called "motivic cohomology groups". The curious thing is that they're bigraded instead of just graded. Instead of getting cohomology groups $H^p(X, A)$ with coefficients in an abelian group A, we get cohomology groups $H^{p,q}(X, A)$.

Why is this? I wish I understood it better... but I think it's basically because we could already define cohomology groups for varieties without this extra notion of homotopies parametrized by the line... but now we can also define them *with* that notion, as well. The old cohomology groups were defined using sheaves; the new one is defined using simplicial sheaves, and the *simplicial* aspect of these sheaves gives a new grading.

And indeed, Voevodsky was able to relate motivic cohomology to another bigraded gadget: the "higher Chow groups" of the variety X. These are a lot easier to define, so let me describe those. Consider the free abelian group generated by irreducible subvarieties of codimension k in

$$X \times \Delta^n$$

where Δ^n is the n-simplex. (Actually, we should only use subvarieties that hit the faces of the simplex "properly".) As we let n vary, we get a simplicial abelian group. But a simplicial abelian group is just a chain complex in disguise! — I explained how in item H of "Week 116".

So, define the higher Chow groups to be the homology groups of this chain complex. They depend on two parameters: the "simplicial" dimension n, but also the "geometrical" codimension k.

Obviously it would take me a few years of hard work to get from this to the point of actually understanding Ben William's talk!

Finally, Christian Haesemeyer of UCLA wrapped up the day with a talk "On the K-theory of toric varieties". For quite a while I've been meaning to explain toric varieties, which are a marvelous playground for exploring algebraic geometry. Roughly: just as an algebraic variety looks locally like the solution set of a bunch of polynomial equations, a toric variety looks like the solution set of a bunch of polynomial equations *where you're not allowed to add, only multiply!*

This restriction makes them marvelously tractable — you can easily describe them using pictures called "fans". Here's a nice informal explanation of how this works:

19) David Speyer, "Toric varieties and polytopes", `http://sbseminar.wordpress.com/2009/02/09/toric-varieties-and-polytopes/`

"Toric varieties and fans", `http://sbseminar.wordpress.com/2009/02/18/toric-varieties-and-fans/`

Once you become a fan of fans — and it's easy to do — you can't resist wanting to take all your favorite invariants of algebraic varieties and see what they look like for toric varieties. Like K-theory!

Hmm. I'm only described the first day's worth of talks, and it's taken more than one day. And I'm left with a lot of questions. For example:

- Aaron Lauda wrote: "It turns out, at least in the simply-laced case, that our algebras are also isomorphic to the Ext algebras between simple perverse sheaves on the Lusztig quiver variety. Lusztig's bilinear form can be seen as taking the graded dimension of this Ext algebra, so it is natural that there is a relationship between the two constructions." Can someone say more about what's going on here? Please *don't* assume I understand what Aaron told me!

- How does the representation Licata describes, involving the cohomology of the cotangent bundle of the Grassmannians $\mathrm{Gr}(n, k)$ for k between 0 and n, fit into a more general story? I think the disjoint union of these Grassmannians should be thought of as the space of 1-stage "Springer flags" in n dimensions — where an m-stage Springer flag is a chain of m subspaces of $\mathbb{C}^n$. I vaguely recall that it's interesting to generalize by letting m be arbitrary. And I think that an even more general story — where we pass from $\mathfrak{sl}(2)$ to $\mathfrak{sl}(N)$ — involves Springer flags in the category of quiver representations. Is this right? What's the big picture?

- Is my account of Johnson-Wilson theories accurate? What are the most important things that I left out here?

- What's "motivic" about Voevodsky's motivic cohomology? Does he propose a definition of motives? How is it related to Grothendieck's conception of motives? How, from this viewpoint, can we see that motivic cohomology should be bigraded?

- What other things should I have said, but didn't?

If you have answers, or just other questions, please visit the n-Category Caf.

Happy Thanksgiving!

Addenda: I thank Toby Bartels yet again for catching a messed-up link, and David Corfield for catching some typos.

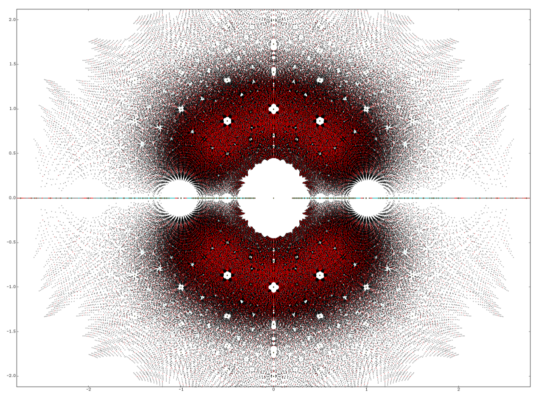
For more discussion visit the n-Category Caf. Please try to answer my questions above!

> There are two fundamental and completely different examples in group theory: the "symmetric group" of permutations of n objects, and the "linear group" of n by n matrices over a field. Lusztig says the linear group is a quantum version of the symmetric group, with the value of Planck's constant telling you which field you're looking at. He has made that idea precise in a thousand beautiful ways for the past 30 years.
>
> — *David Vogan*

Week 285

December 5, 2009

A while back, my friend Dan Christensen drew a picture of all the roots of all the polynomials of degree at most 5 with integer coefficients ranging from -4 to 4:

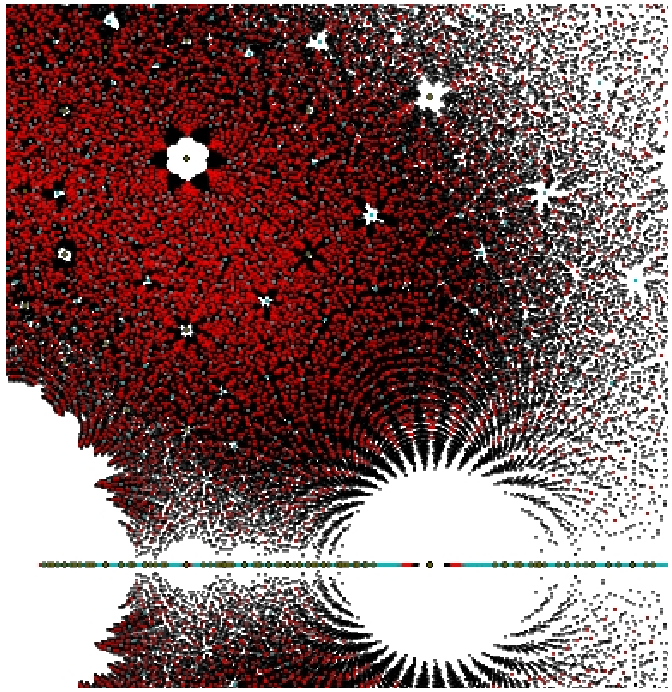


1) Dan Christensen, 'Plots of roots of polynomials with integer coefficients', `http://jdc.math.uwo.ca/roots/`

2) John Baez, "The beauty of roots", `http://math.ucr.edu/home/baez/roots/`

Click on the picture for bigger view. Roots of quadratic polynomials are in grey; roots of cubics are in cyan; roots of quartics are in red and roots of quintics are in black. The horizontal axis of symmetry is the real axis; the vertical axis of symmetry is the imaginary axis. The big hole in the middle is centered at 0; the next biggest holes are at ± 1, and there are also holes at $\pm i$ and all the cube roots of 1.

You can see lots of fascinating patterns here, like how the roots of polynomials with integer coefficients tend to avoid integers and roots of unity — except when they land *right on* these points! You can see more patterns if you zoom in:

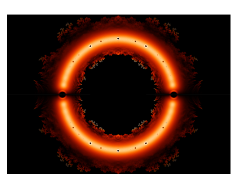


Now you see beautiful feathers surrounding the blank area around the point 1 on the real axis, a hexagonal star around $\exp(i\pi/6)$, a strange red curve from this point to 1, smaller stars around other points, and more. . . .

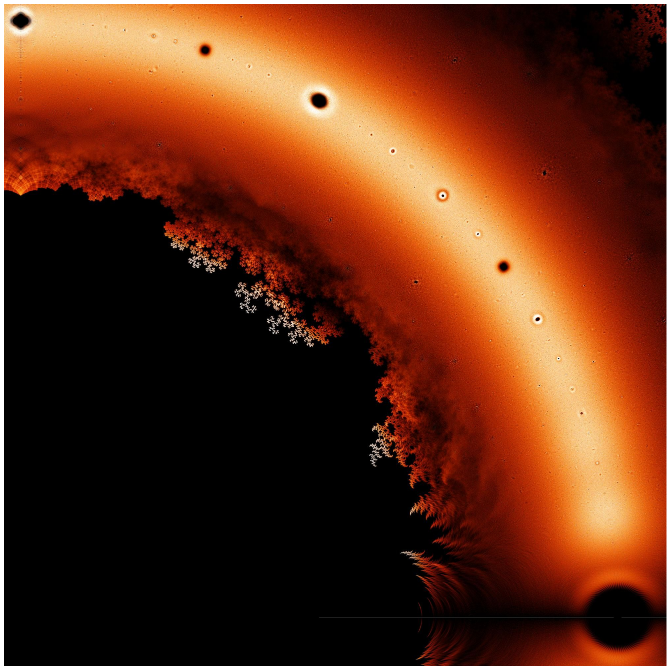
People should study this sort of thing! Let's define the set $\mathbb{C}(d,n)$ to be the set of all roots of all polynomials of degree d with integer coefficients ranging from $-n$ to n. Clearly $\mathbb{C}(d,n)$ gets bigger as we make either d or n bigger. It becomes dense in the complex plane as $n \to \infty$, as long as $d \geqslant 1$. We get all the rational complex numbers if we fix $d \geqslant 1$ and let $n \to \infty$, and all the algebraic complex numbers if let both $d, n \to \infty$.

But based on the above picture, there seem to be a lot of interesting conjectures to make about this set as $d \to \infty$ for fixed n.

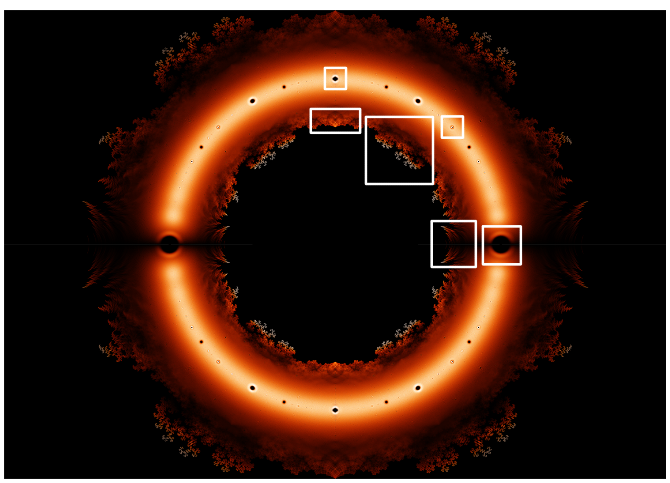
Inspired by the pictures above, Sam Derbyshire decided to to make a high resolution plot of some roots of polynomials. After some experimentation, he decided that his favorite were polynomials whose coefficients were all 1 or -1 (not 0). He made a high-resolution plot by computing all the roots of all polynomials of this sort having degree 24. That's 2^{24} polynomials, and about 24×2^{24} roots — or about 400 million roots! It took Mathematica 4 days to generate the coordinates of the roots, producing about 5 gigabytes of data. He then used some Java programs to create this amazing image:



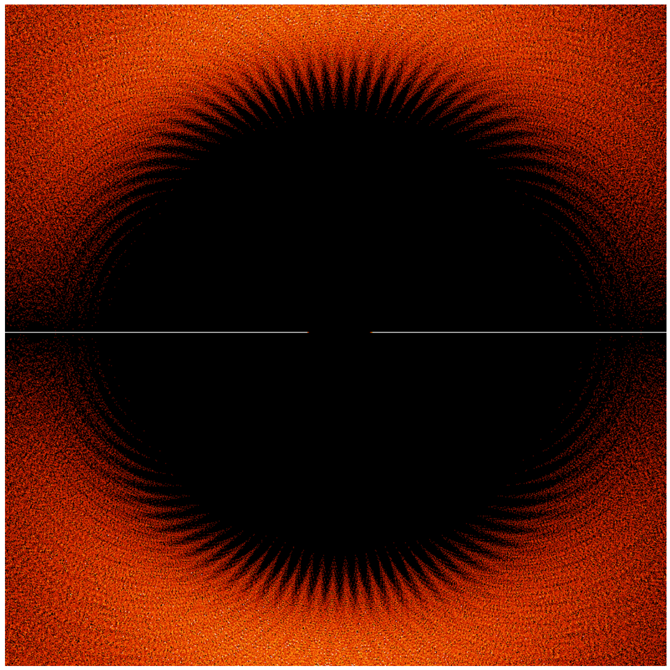
The coloring shows the density of roots, from black to dark red to yellow to white. The picture above is a low-resolution version of the original image, which is available as a 90-megabyte file on Dan's website. We can zoom in to get more detail:



Note the holes at certain roots of unity, and wondrously intricate patterns as we move inside the unit circle. To make all this clearer, Sam Derbyshire zoomed in on certain regions, marked here:

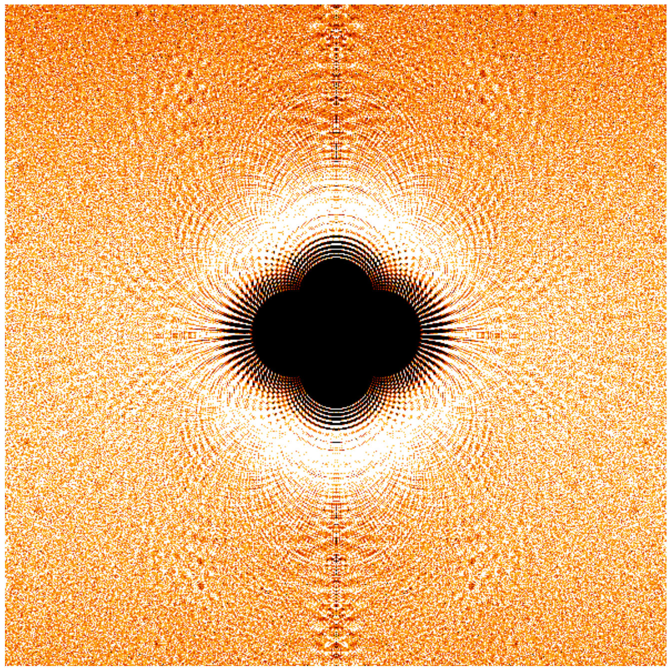


Here's a closeup of the hole at 1:

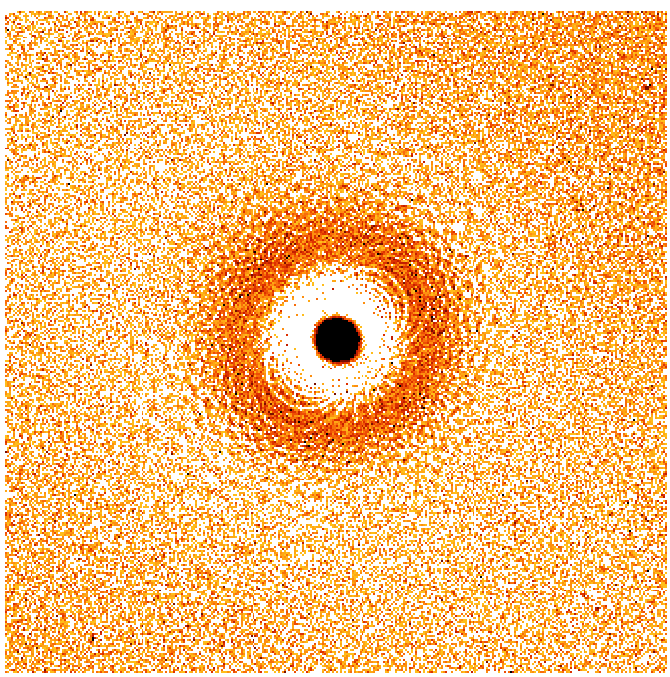


Note the white line along the real axis. That's because lots more of these polynomials have real roots than *nearly* real roots.

Next, here's the hole at i:

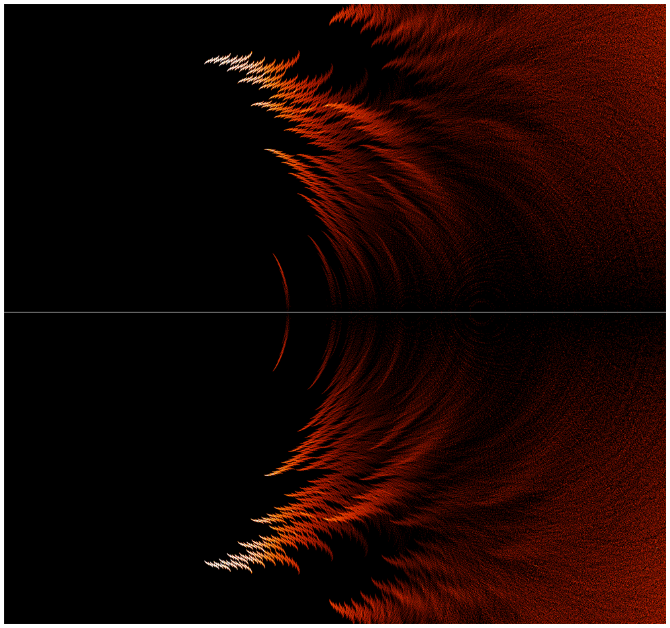


And here's the hole at $\exp(i\pi/4) = (1+i)/\sqrt{2}$:

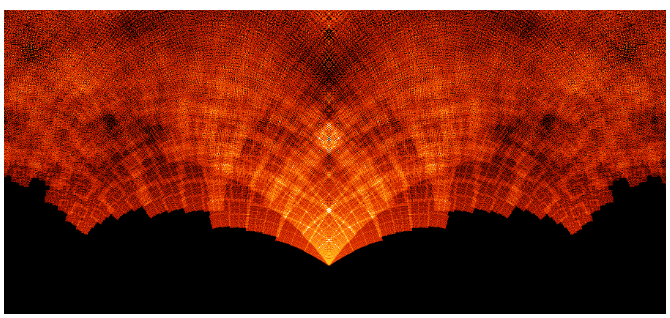


Note how the density of roots increases as we get closer to this point, but then suddenly drops off right next to it. Note also the subtle patterns in the density of roots.

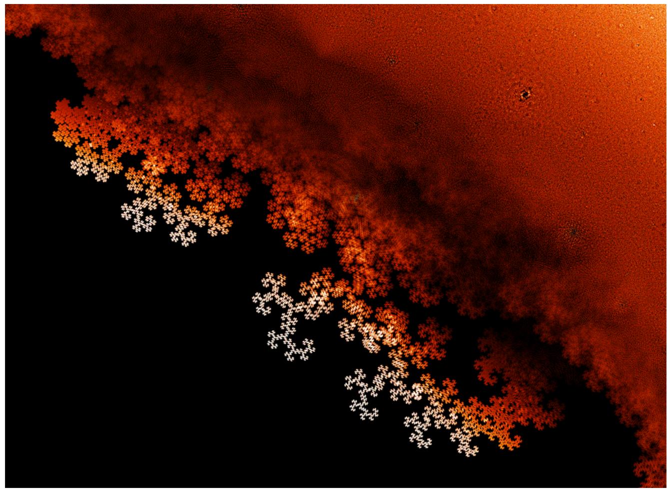
But the feathery structures as move inside the unit circle are even more beautiful! Here is what they look near the real axis — this plot is centered at the point $4/5$:



They have a very different character near the point $(4/5)i$:



But I think my favorite is the region near the point $(1/2)\exp(i/5)$. This image is almost a metaphor of how mathematical patterns emerge from confusion like sharply defined figures looming from the mist:



Dan and Sam were not the first to explore these issues, but there's a lot left to do: conjectures to make, theorems to prove, and pictures to draw! If you come up with some pretty pictures, I'd love to include them on my webpage — and cite you. For previous research, see:

3) Loki Joergenson, "Zeros of polynomials with constrained coefficients", `http://www.cecm.sfu.ca/~loki/Projects/Roots/`

4) Eric W. Weisstein, *MathWorld*, "Polynomial roots", `http://mathworld.wolfram.com/PolynomialRoots.html`

My colleague the knot theorist Xiao-Song Lin — sadly no longer with us — plotted the zeros of the Jones polynomial for prime alternating knots with up to 13 crossings, and you can see his pictures here:

5) Xiao-Song Lin, "Zeros of the Jones polynomial", `http://math.ucr.edu/~xl/abs-jk.pdf`

You'll see that *some* of the patterns in his pictures just come from the patterns we see in the roots of polynomials with integer coefficients... since the Jones polynomial has integer coefficients.

This paper is also interesting:

6) Andrew M. Odlyzko and B. Poonen, "Zeros of polynomials with $0, 1$ coefficients", *L'Enseignement Math.* **39** (1993), 317–348. Also available at `http://dx.doi.org/10.5169/seals-60430`

Odlyzko and Poonen proved some interesting things about the set of all roots of all polynomials with coefficients 0 or 1. If we define the set $\mathbb{C}(d, p, q)$ to be the set of roots of all polynomials of degree d with coefficients ranging from p to q, Odlyzko and Poonen are studying $\mathbb{C}(d, 0, 1)$ in the limit $d \to \infty$. They mention some known results and prove some new ones: this set is contained in the half-plane $\Re(z) < 3/2$ and contained in the annulus

$$\frac{1}{\Phi} < |z| < \Phi$$

where Φ is the golden ratio, $(\sqrt{5} + 1)/2$. In fact they trap it, not just between these circles, but between two subtler curves. They also show that the closure of this set is path connected but not simply connected.

But from the pictures above, these ideas just scratch the surface of the wealth of patterns to be found and theorems to be proved!

Next, let me say a bit about the talks from the second day of the AMS session on homotopy theory and higher algebraic structures at UC Riverside. You can see videos of these talks here, or by clicking on the talk titles below:

7) "Special session on homotopy theory and higher algebraic structures", *AMS Western Section Meeting*, November 7–8, 2009. Talks available as Quicktime videos at `http://math.ucr.edu/~jbergner/amsriverside09.htm`

It's been good for me trying to discuss all these talks — it's forced me to think about them a lot harder. I'm not sure how good it is for you, though: lots of ideas are flashing past without adequate explanation. Each talk could be the basis for a whole This Week's Finds. But I'm happy to get a chance to at least *mention* all sorts of ideas that would be fun to explore more deeply someday.

Eric Malm of Stanford University started the show bright and early 8:30 am on Sunday with a talk on "String topology and the based loop space". You can see the slides here:

8) Eric Malm, "String topology and the based loop space", `http://math.ucr.edu/~jbergner/ucr-st-present.pdf`

In its original form, string topology studies the *unbased* loop space of an oriented d-dimensional manifold M. This is the space LM of all maps from a circle into M. In their fundamental paper on the subject:

9) Moira Chas and Dennis Sullivan, "String topology", available as `arXiv:math/9911159`.

Chas and Sullivan showed that the homology groups of LM with degrees shifted by d:

$$A_i = H_{i+d}(LM)$$

are equipped with a graded-commutative product:

$$\circ\colon A_i \otimes A_j \to A_{i+j}$$

together with an operator

$$D\colon A_i \to A_{i+1}$$

with $D^2 = 0$. These satisfy a bunch of equations, which makes the degree-shifted homology of LM into a gadget called a "Gerstenhaber algebra". I explained such gadgets in "Week 220": they're precisely algebras of the homology of the little 2-cubes operad.

But the homology of the loop space has even more structure: it's a "Batalin-Vilkovisky algebra". That means that in addition to the above stuff, it has a Lie bracket of degree 1:

$$[-,-]\colon A_i \otimes A_j \to A_{i+j+1}$$

which gets along with the rest in a nice way. I also talked about these in "Week 220": they're precisely algebras of homology of the framed little 2-discs operad!

This is just the beginning of a big story. Malm's talk surveys this story and adapts the ideas of string topology to the *based* loop space of a manifold, using its relations to Hochschild homology. For some useful background here, try this book — or at least the very informative review:

10) Ralph L. Cohen, Kathryn Hess, and Alexander A. Voronov, *String Topology and Cyclic Homology*, Birkhauser, Boston, 2006. Review by Janko Latschev at `http://www.ams.org/bull/0000-000-00/S0273-0979-09-01265-8/`

Next, Laura Scull of Fort Lewis College spoke on "Orbifolds and equivariant homotopy theory". This is joint work she's doing with Dorette Pronk. Laura is an expert on equivariant homotopy theory: that's the kind of homotopy theory you do for spaces on which a group acts. Dorette is an expert on category theory. So it was natural for them to team up and tackle orbifolds!

Why? And what's an orbifold? Well, just as a manifold is built up from patches that look like $\mathbb{R}^n$, an orbifold is built up from patches that look like $\mathbb{R}^n$ modulo the linear action of a finite group. So, most places it looks like a manifold, but it can have singularities of a mild sort here and there.

When people tried to define maps of orbifolds, they ran into a lot of trouble. Naive approaches led to a mess. It turns out there's a good reason for this. There's not really a nice category of orbifolds. But there's a nice 2-*category* of orbifolds!

The reason is that we shouldn't think of an orbifold as a set with extra structure. We should think of it as a *groupoid* with extra structure. The points of the orbifold are the objects of this groupoid. For a plain old manifold, we'd only have identity morphisms — so it's basically just a set. But for a more interesting orbifold, the singular points have extra automorphisms.

Everything likes to live in something bigger and fancier than itself. Groupoids, being categories, want to be objects of a 2-category. The same is true for orbifolds. However, there are extra subtleties due to the *smooth structure* on our orbifold. To deal with these, it's nice to treat orbifolds as "Lie groupoids" or "stacks". I could say a lot more, but instead I'll just refer you to this very readable paper:

11) Eugene Lerman, "Orbifolds as stacks?". Available as `arXiv:0806.4160`.

It begins by discussing easy approaches, then describes their flaws, and so on, leading up to the current state of the art. After this warmup, try:

12) Dorette Pronk and Laura Scull, "Translation groupoids and orbifold Bredon cohomology", `arXiv:0705.3249`.

Then Anssi Lahtinen of Stanford University spoke about "The Atiyah-Segal completion theorem in twisted K-theory".

Twisted K-theory is fascinating to folks who like categorification, because it involves "U(1) gerbes", which are categorified U(1) bundles. Just as a U(1) bundle over a space can be defined by chopping a space into open sets U_i and giving U(1)-valued "transition functions" on the intersections $U_i \cap U_j$, a U(1) gerbe over a space can be defined by chopping a space into open sets and giving transition functions

$$h_{ijk} \colon U_i \cap U_j \cap U_k \to \mathrm{U}(1)$$

If you have a U(1) gerbe, you can define "twisted vector bundles". These are like vector bundles, but where the transition functions g_{ij} satisfy the usual cocycle conditions only up to a phase, given by h_{ijk}. In other words, instead of the famous formula

$$g_{ij} g_{jk} = g_{ik}$$

we just have

$$g_{ij} g_{jk} h_{ijk} = g_{ik}$$

Given a space X, we can form its K-theory $K(X)$ by taking the category of vector bundles and forming its Grothendieck group. We saw some Grothendieck group constructions last time! Here's how it goes this time. Take the category of vector bundles over X, say $\mathsf{Vect}(X)$. Then take the set of isomorphism classes of vector bundles. Then take formal linear combinations of these to get an abelian group, and then impose the equivalence relation

$$[M \oplus N] = [M] + [N]$$

The result is an abelian group $K(X)$ called the "K-theory" of X. And in fact it's a ring, since we can also take tensor products of vector bundles!

The Atiyah-Segal completion theorem concerns $K(X)$ when X is the classifying space of a topological group G. As explained in "Week 151", this is a space BG with a principal G-bundle over it:

$$EG \to BG$$

with the property that every other principal G-bundle over every other space is a pullback of this one. Given any representation of G, we can use the "associated bundle" trick to create a vector bundle over BG. So, we get a functor from the category of representations of G to the category of vector bundles over BG:

$$\mathsf{Rep}(G) \to \mathsf{Vect}(BG)$$

Applying the Grothendieck group construction, this functor in turn gives a ring homomomorphism

$$R(G) \to K(BG)$$

where $R(G)$, the so-called "representation ring" of G, is the Grothendieck group of $\mathsf{Rep}(G)$.

The Atiyah-Segal theorem explains how this map from $R(G)$ to $K(BG)$ is almost — though not quite — an isomorphism. It's tempting to generalize this from K-theory to twisted K-theory. . . and that's what Anssi Lahtinen spoke about!

Next, Konrad Waldorf of UC Berkeley spoke on "String connections and supersymmetric sigma models":

13) Konrad Waldorf, "String connections and supersymmetric sigma models", http://www.konradwaldorf.de/docs/riverside.pdf

14) Konrad Waldorf, "String connections and Chern-Simons theory", available as arxiv:0906.0117.

His talk was a great introduction to some things I know and love, and some others that I'd never quite understood before... but loved at first sight now.

$\mathrm{U}(1)$ bundles over a space are classified by elements of its second cohomology with integer coefficients. $\mathrm{U}(1)$ gerbes are similarly classified by the third integral cohomology group. This story keeps on going! $\mathrm{U}(1)$ 2-gerbes are classified by the fourth cohomology, and so on. If you don't know what a 2-gerbe is, don't panic: just go back to my description of bundles and gerbes, and you can guess how the story continues.

But when M is a manifold, there's a nice way to get an element of its fourth integral cohomology group! If it's an oriented manifold, its oriented frame bundle is a principal $\mathrm{SO}(n)$ bundle. This has "characteristic classes"; the first interesting one is the "first Pontryagin class", which is an element in the fourth integral cohomology group of M. You can get a representative of this in deRham cohomology by picking a connection, taking its curvature 2-form F and multiplying the closed 2-form

$$\mathrm{tr}(F \wedge F)$$

by the right number. But in fact the first Pontryagin class lives in integral cohomology. So, any oriented 4-manifold automatically gives a 2-gerbe... but that's not the one we need here!

If M is equipped with a spin structure, its oriented frame bundle is equipped with a double cover that's a principal $\mathrm{Spin}(n)$ bundle. This too has characteristic classes. The first interesting one lives in the fourth integral cohomology group of M, and it has the property that when you multiply it by 2 you get the first Pontryagin class. (In integral cohomology there can be various different classes with this property, coming from different spin structures.)

So: every spin structure on M gives an element of the fourth integral cohomology group of M, and thus a 2-gerbe. This is called the "Chern-Simons 2-gerbe". The reason for this term is explained here:

15) Urs Schreiber, "States of Chern-Simons theory", http://golem.ph.utexas.edu/category/2008/02/states_of_chernsimons_theory.html

There are lots of ways to think about "string structures" on a spin manifold M, but Waldorf advocated thinking of them as *choices of trivialization* of its Chern-Simons 2-gerbe. There may of course be none, or many. But the really nice thing about this viewpoint is that it gives a nice approach to "string connections".

Next, Sren Galatius of Stanford University gave a talk on "Monoids of moduli spaces of manifolds", explaining a paper with the same title:

16) Sren Galatius and Oscar Randal-Williams, "Monoids of moduli spaces of manifolds", available as arXiv:0905.2855.

The goal of their work was to create a title with as many words beginning with "M" as possible... no, not really. In fact, it's a kind of continuation of this famous paper:

17) Sren Galatius, Ib Madsen, Ulrike Tillmann, and Michael Weiss, "The homotopy type of the cobordism category", available as arXiv:math/0605249.

In item K of "Week 117" I explained how to turn any category into a topological space called its "classifying space". This construction has a nice generalization to "topological categories" — that is, categories where the set of morphisms from any object to any other is a topological space, and composition is continuous.

For example, a topological group G is the same as a topological category with one object and all morphisms being invertible. If we apply the construction to this example, we get the classifying space BG that I mentioned a while back.

The Galatius-Madsen-Tillmann-Weiss paper determined the homotopy type of the classifying space of the topological category of n-dimensional oriented cobordisms! The new work constructs a topological monoid that has the same classifying space — a nice simplification.

After lunch, Alissa Crans of Loyola Marymount University spoke on "2-Quandles: categorified quandles". A "quandle" is the sort of algebraic gadget when you axiomatize the properties of conjugation in a group. So, start with a group and define an operation of "left conjugation":

$$g > h = ghg^{-1}$$

and an operation of "right conjugation":

$$h < g = g^{-1}hg$$

Then, figure out all the equations these obey, regardless of what group you've got! Clearly these operations are inverses of each other:

$$g > (h < g) = h = (g > h) < g$$

If you conjugate anything by itself, nothing happens:

$$g > g = g = g < g$$

But more interestingly, we also have

$$\begin{aligned} g > (h > k) &= (g > h) > (g > k) \\ (k < h) < g &= (k < g) < (h < g) \end{aligned}$$

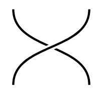
Conjugation distributes over itself! Do the calculation yourself and see! As far as I know, all equations obeyed by these operations follow from the ones I've listed... though I've never seen a proof, and I'd like to. These equations form the definition of a "quandle".

So, we may define a quandle in a very conceptual way as an algebraic structure where each element acts as a symmetry of that structure, and every element acts trivially on itself. Think about it.

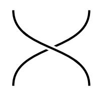
But the magical thing about quandles is that they give invariants of tangles! The easiest way to start seeing this is by pondering braids. Given a quandle Q there's a way to turn any n-strand braid into a function

$$Q^n \to Q^n$$

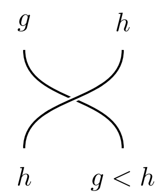
Here's how. In braids we can have two kinds of crossings:



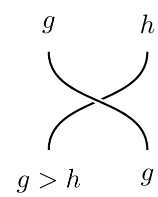
and



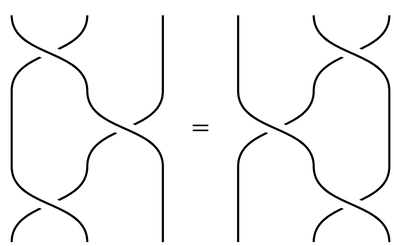
Let's think of each as giving a function from Q^2 to itself. To do this, we let the quandle element labelling one strand act on the quandle element labelling the other, using our two kinds of conjugation:



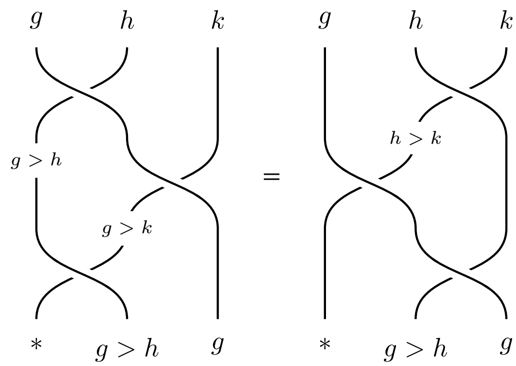
and



The strand above acts on the strand below, following the general principle that the people higher up cause trouble for the people below them. Now, look at the third Reidemeister move, which says:



If we feed in three quandle elements on top, look what happens:



Look at the strand marked with an asterisk! On the left it should be labelled by

$$(g > h) > (g > k)$$

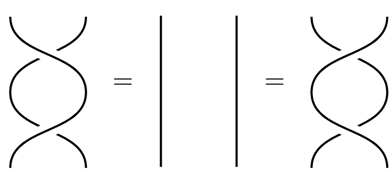
On the right it should be labelled by

$$g > (h > k)$$

But thanks to the self-distributive law, these are equal! Similarly, the equation

$$g > (h < g) = h = (g > h) < g$$

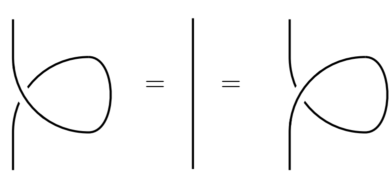
handles the second Reidemeister move:



while the equation

$$g > g = g = g < g$$

handles the first Reidemeister move. The first Reidemeister move is not really about about braids — it's about tangles:



So, there's a deep relation between crossings in tangles and conjugation in groups, captured by the quandle axioms. And the quandle axioms also cover *Lie algebras*, with self-distributivity corresponding to the Jacobi identity:

16) J. Scott Carter, Alissa Crans, Mohamed Elhamdadi and Masahico Saito, "Cohomology of categorical self-distributivity", available as arXiv:math/0607417.

It's possible to explain this relation a lot more deeply than I just did... but anyway, what Alissa did is start *categorifying* this relation. Together with the topologists Carter and Saito, she's studying "2-quandles", which should relate 2-tangles to conjugation in 2-groups.

Next, Chad Giusti of the University of Oregon spoke on "Unstable Vassiliev theory":

17) Chad Giusti, "Unstable Vassiliev theory", http://math.ucr.edu/~jbergner/RiversideTalk.pdf

The goal here is to understand the space of "long knots". A long knot is a curve in $\mathbb{R}^3$ that goes on forever and is a vertical straight line outside some compact set. So, it can get knotted around in the middle. One nice thing about long knots is that there's a multiplication defined on them, by sticking them end-to-end.

If you know about tangles, a long knot is just another way of thinking about a one-strand tangle with a strand coming in at top and going out at bottom. Then the multiplication on long knots is a special case of the composition of tangles.

(We can define even more operations if we work with "thickened" long knots. In fact, the space of these forms an algebra of the little 2-cubes operad! This gives a mystical relation between thickened long knots and Gerstenhaber algebras. I explained this near the end of "Week 220".)

Anyway, the part of Giusti's talk that I understood best, and therefore liked the most, was a neat combinatorial description of the space of long knots. He calls them "plumbers' knots", because they're like pipes that move only along the x, y, or z directions... for details, see his slides!

Then Robin Koytcheff of Stanford University gave a somewhat related talk on "A homotopy-theoretic view of Bott-Taubes integrals and knot spaces":

18) Robin Koytcheff, "A homotopy-theoretic view of Bott-Taubes integrals and knot spaces", http://math.ucr.edu/~jbergner/RKslidesUCR.pdf

He began with a nice introduction to the Bott-Taubes approach to Vassiliev theory. Then he gave a great description of how the little 2-cubes operad acts on the space of thickened long knots, and how one can use this to underand the homology of this space. Then he discussed how to combine these ideas. For more details, see:

19) Robin Koytcheff, "A homotopy-theoretic view of Bott-Taubes integrals and knot spaces", *Alg. Geom. Top.* **9** (2009), 1467–1501. Also available as arXiv:0810.1785.

Next, Chris Douglas of U.C. Berkeley gave talk charmingly entitled "3-categories for the working mathematician" — unfortunately no video for this one. It's great to see how weak 3-categories are making their way into applications. Douglas is working with Arthur Bartels and Andre Henriques on their applications to "conformal nets" — that is, algebras of local observables in conformal field theory. The bulk of Douglas' talk involved a kind of hieroglyphic notation for operations and equations in a definition of weak 3-category. This definition is close to the existing definitions of "tricategory", but not exactly the same — at least, not superficially. It's probably equivalent.

Finally, Scott Morrison and Kevin Walker gave a 2-part talk on "blob homology" — a great introduction to their big paper in progress:

20) Scott Morrison and Kevin Walker, "Blob homology slides": `http://tqft.net/UCR-blobs1` and `http://tqft.net/UCR-blobs2`

21) Scott Morrison and Kevin Walker, "The blob complex". Draft available at `http://tqft.net/papers/blobs.pdf`

The clever idea here is to use manifolds to provide a quick and practical definition of "n-categories with duals" — thus short-circuiting, at least temporarily, the need to prove some big conjectures linking this algebraic concept to topology. With this definition, they're able to define and study "blob homology": that is, a kind of homology for manifolds with coefficients in a linear n-category with duals!

This includes ordinary TQFTs and also Hochschild homology as special cases. So, it's a big deal, and I'm sure we'll be seeing more of it in the years to come.

Next week I'll start a series of This Week's Finds on rational homotopy theory. This is a great subject with connections to pretty much everything: deformation theory, Lie n-groups and Lie n-algebras, classical mechanics, supergravity and more! So stay tuned. . . .

Addenda: I thank Toby Bartels for some improvements, and Ralf Bader for a link to Odlyzko and Poonen's paper.

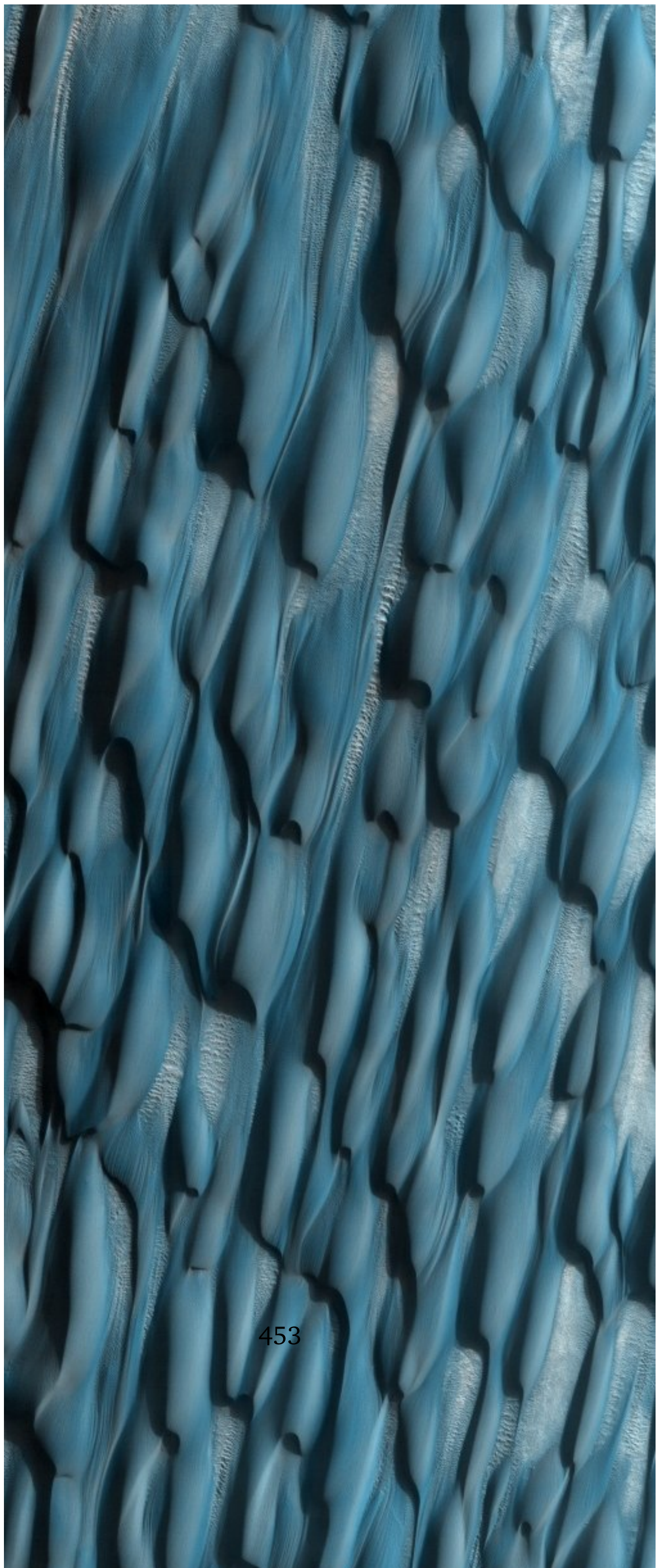
For more discussion visit the n-Category Caf.

> The author feels that this technique of deliberately lying will actually make it easier for you to learn the ideas.
>
> — *Donald Knuth*

Week 286

December 8, 2009

This Week I'd like to start telling you about "rational homotopy theory". But first: can you guess what this is a picture of?



I'll explain it at the end.

So, what's "rational homotopy theory"? One might naively define it as the study of spaces whose homotopy groups are vector spaces over the rational numbers.

But if you think about it, that's pretty weird!

For example, the first homotopy group of a space X, usually called the "fundamental group" of X and denoted $\pi_1(X)$, consists of equivalence classes of loops in X that start and end at our favorite point. Two loops count as equivalent if you can continuously deform one until it looks like the other. If you can do this, we say these loops are "homotopic".

The fundamental group of the circle is $\mathbb{Z}$, the group of integers. The reason is that two loops drawn on the circle are homotopic if and only if they wind around the same number of times — and that number must be an integer! You can walk around the block once and get back home. You can walk around the block twice and get back home. You can even walk around the block -5 times and get back home: the negative number just means you walk around the other way. But you can't walk *halfway* around the block and be back home!

But suppose you had a space whose fundamental group was $\mathbb{Q}$, the rational numbers. Then you *could* walk halfway around the block and get back home. That sounds pretty weird — nay, downright impossible!

But part of why it sounds so weird is that it's not right. We really need some other "block" such that walking around *that* block *twice* is homotopic to walking around the original block *once*. This sounds more complicated... but also more possible.

Later in this post I'll describe a space called "the rational circle", whose fundamental group is indeed $\mathbb{Q}$. Then you can see how it actually works.

Anyway: spaces whose homotopy groups are rational vector spaces are weird. Why should we care about them?

We shouldn't! In fact, the real point of rational homotopy theory lies elsewhere.

It's better not to think of rational homotopy theory as the study of weird spaces whose homotopy groups are rational vector spaces. It's better to think of it as the study of *ordinary* spaces — but viewed in a way that doesn't let us see their homotopy groups, only their homotopy groups tensored with $\mathbb{Q}$. This process turns their homotopy groups into rational vector spaces!

This is a common theme in algebraic topology. We can think of various kinds of homotopy theory either as the completely precise study of rather strange spaces, or as the study of ordinary spaces as seen through a blurry lens. A blurry lens can be a good thing, because it simplifies a complicated picture.

However, even *this* way of thinking about rational homotopy theory misses the point. The real point is that rational vector spaces come from the land of linear algebra, so rational homotopy blends topology and linear algebra. So does rational *homology* theory, but rational homotopy theory is deeper. When we get into it, we'll take lots of important concepts from linear algebra — like commutative algebras, and Lie algebras, and Hopf algebras — and study very interesting "homotopy versions" of these concepts.

By doing this, we'll vastly generalize linear algebra. We'll wind up with a whole new perspective... and we'll see applications to physics ranging from classical field theory, to quantization, to supergravity!

And you should not be surprised that we're doing here is really *categorifying* linear algebra.

But more on that later. Today, I want to start with the naive viewpoint that rational homotopy theory is about spaces whose homotopy groups are rational vector spaces.

In algebraic topology, the really hard part is *torsion*. A group element is "torsion" if you can add it to itself a bunch of times and get zero. So, for example, every element of a finite group is torsion, but the group of integers is "torsion-free".

Look at some homotopy groups of spheres and you'll see what I mean:

$$\pi_3(S^2) = \mathbb{Z}\pi_5(S^3) \quad = \mathbb{Z}/2\pi_7(S^4) = \mathbb{Z} \times \mathbb{Z}/12\pi_9(S^5) \quad = \mathbb{Z}/2\pi_{11}(S^6) = \mathbb{Z}\pi_{13}(S^7) \quad = \mathbb{Z}/2\pi_{15}(S^8) = \mathbb{Z} \times \mathbb{Z}/120\pi_{17}($$

These are the homotopy groups $\pi_{2n-1}(S^n)$. If you were asked to make a guess about the torsion-free part of these groups, you could easily formulate a conjecture: it's $\mathbb{Z}$ when n is even, and trivial when n is odd. And this is true.

But if you were asked to make a guess about the torsion part of these groups, you'd find it a lot harder. And indeed, nobody knows the full story here.

This suggests trying to do a version of algebraic topology where we systematically get rid of torsion. We'll lose a lot of important information, but things will get easy and fun — and still far from trivial!

This is "rational homotopy theory".

How can we get rid of torsion?

Well, the nth homotopy group of a compact manifold, like a sphere, is always finitely generated — and abelian when $n > 1$. A finitely generated abelian group always looks like $\mathbb{Z}^n \times T$ where T is finite. All the torsion is in T, so to get rid of torsion we can just throw out T.

But that doesn't work in general. In general, the nth homotopy group of a space can be *any* group when $n = 1$ — and any abelian group when $n > 1$.

For an arbitrary abelian group, the torsion elements always form a subgroup, called the "torsion subgroup". It's not true in general that an abelian group is the product of its torsion subgroup and some other group! But, we can still kill off the torsion by modding out by the torsion subgroup.

For a nonabelian group, the torsion elements don't necessarily form a subgroup! For example, take the free group generated by x and y, and mod out by the relations $x^2 = y^2 = 1$. Then x and y are torsion elements, but xy is not.

I don't know any good way to kill off the torsion for an arbitrary nonabelian group. A lot of work on rational homotopy theory sidesteps this issue by working only with "1-connected" spaces. These are spaces that are path-connected and also simply connected. That means the fundamental group is trivial — and the higher homotopy groups are always abelian, so we don't have to worry about nonabelian groups.

Now, I've made it sound like the right way to "kill off torsion" in an abelian group is to mod out by its torsion subgroup. This makes me wonder if there's a systematic way to take a space X and turn it into a space X' such that $\pi_n(X')$ is $\pi_n(X)$ mod its torsion subgroup. Does anyone know?

But anyway, this is *not* how we kill off torsion in rational homotopy theory!

Instead, here's what we do. Abelian groups are the same as $\mathbb{Z}$-modules where $\mathbb{Z}$ is the ring of integers. Since $\mathbb{Z}$ is commutative, we can take tensor products of $\mathbb{Z}$-modules. In other words, we can take tensor products of abelian groups. And to kill off the torsion in an abelian group, we just tensor it with the rational numbers!

I hope you see what this accomplishes. Tensoring an abelian group G with the rational numbers gives a new abelian group $\mathbb{Q} \otimes G$ that includes elements like

$$q \otimes g$$

where $g \in G$ and q is a rational number. Any element g of G gives an element of $\mathbb{Q} \otimes G$, namely

$$1 \otimes g$$

But we also get elements like

$$\frac{1}{2} \otimes g$$

which acts like "half of g". More generally, given any element of $\mathbb{Q} \otimes G$, we're allowed to multiply it by any fraction.

Now, suppose g is a torsion element of G. Then $ng = 0$ for some n, so

$$1 \otimes ng = 0,$$

If we multiply both sides by $1/n$, we get

$$1 \otimes g = 0$$

So, torsion elements of G get sent to zero in $\mathbb{Q} \otimes G$. We've killed the torsion.

But the great thing about this trick is that $\mathbb{Q} \otimes G$ is even better than a torsion-free abelian group. It's a vector space over the rational numbers! So, we're not just killing off torsion. We're going from the world of abelian groups to the world of *linear algebra*, which is notoriously well-behaved.

Next let me sketch how we can take a 1-connected space X and "rationalize" it, obtaining a new space $X_{\mathbb{Q}}$ with

$$\pi_n(X_{\mathbb{Q}}) = \pi_n(X) \otimes \mathbb{Q}$$

for all n.

Since we're doing homotopy theory, we can assume X is a "CW complex". A space of this sort is built from balls. To build a CW complex, we start with some 0-balls — that is, points. Then we take some 1-balls — that is, intervals — and glue their boundaries to the 0-balls. We get a space that's just a graph. Then we take some 2-balls — that is, disks — and glue their boundaries to the space we've got so far. Then we take some 3-balls and glue their boundaries to what we've got so far. And so on, ad infinitum. Any space is "weakly homotopy equivalent" to a space of this sort, and that's good enough for us.

So, to rationalize X we should rationalize this whole procedure! This procedure relies on balls — and also spheres, since the boundary of a ball is a sphere. So, we should define a "rational n-ball" and a "rational n-sphere", and then make sure that given a CW complex, we can build a new space where each ball or sphere we used has been replaced by a "rational" one!

I'll describe the rational n-sphere, since that's the fun part. Even though we don't need it here, let's start with the case $n = 1$: the "rational circle". As mentioned earlier, this is a space whose fundamental group is $\mathbb{Q}$. Here's one way to build it.

First, take an ordinary circle, and take a cylinder, and glue your circle to the bottom of that cylinder. But: make sure the circle goes around the bottom of the cylinder *twice!*

See what this accomplishes? It means that walking around your original circle *once* is homotopic to walking around the top of the cylinder *2 times*.

This solves our problem of how walking once around the block can be the same as walking twice around some other block.

Then take another cylinder, and glue the top of your first cylinder to that. But: make sure the top of your first cylinder winds around the bottom of this new one *3 times*.

Then take yet another cylinder. Glue the top of your second cylinder to the bottom of that — but make sure it wraps around the bottom *4 times*.

And then do this forever. . .

. . . and then take a little rest, since it's very tiring to do an infinite amount of work. Sit back and admire your handiwork. The space you've built has $\mathbb{Q}$ as its fundamental group, because for any loop g and any integer n, we've created a new loop h such that $g = nh$.

Mathematicians call this general type of space a "telescope". An ordinary hand telescope — the kind that pirates use — is built from cylinders of metal that fit into each other:

A mathematician's telescope is similar — but it's built from infinitely many cylinders, and you can't collapse it, because they're attached to each other in a complicated way. This makes it really easy to spot a mathematician in a roomful of pirates.

We can easily generalize this telescope idea to construct the "rational n-sphere". The point is that for each integer k, there's a way to wrap the n-sphere around itself k times. So, we can use these to build an infinite telescope, just as we did for the rational circle. This telescope is a space whose homotopy groups are those of the n-sphere, but tensored with the rational numbers.

A similar trick produces a rational n-ball, but this is less exciting, since all the homotopy groups of the n-ball were trivial already — it's contractible, after all. The rational n-ball is still contractible, but it's been modified so that its "boundary" is a rational n-sphere.

Having rationalized our spheres and balls, we also need to check that the maps we used to build our CW complex extend in a canonical way from the spheres to the rational spheres. But let's skip this: in This Week's Finds we only do the fun part!

As you can see, the rationalized version of a nice little CW complex is usually a huge nightmarish space. This is a familiar tradeoff in algebra topology: spaces with comprehensible homotopy groups almost always look big and scary when we try to build them by gluing balls together. But it's a tradeoff algebraic topologists have learned to accept. There's more to life than whether a space *looks* nice.

In particular, this rationalization process has a very nice abstract characterization. Suppose X is any 1-connected pointed space. Then we can define "a rationalization" of X to be any 1-connected pointed space X' equipped with a map

$$X \to X'$$

satisfying two properties. First, X' is a "rational space": a 1-connected pointed space whose homotopy groups are rational vector spaces. Second, the induced map

$$\mathbb{Q} \otimes \pi_n(X) \to \mathbb{Q} \otimes \pi_n(X')$$

is isomorphism for all n.

It turns out that the rationalization of a space is unique up to weak homotopy equivalence. And we can construct it for CW complexes as I just explained.

Okay. So far I've been treating rational homotopy theory as the study of weird "rational" spaces. And I've showed you how to turn any space into one of these. But as I already admitted, this misses the point.

To come closer to the point, we need to recall an amazing old theorem due to J. H. C. Whitehead, which says a map

$$f\colon X \to Y$$

between connected CW complexes is a homotopy equivalence if and only if the induced maps

$$\pi_n(f)\colon \pi_n(X) \to \pi_n(Y)$$

are isomorphisms for all n. This is why for more general connected spaces we define any map that induces isomorphisms on homotopy groups to be a "weak homotopy equivalence". Even better, every space is weakly homotopy equivalent to a CW complex! So, if we formally throw in inverses to all weak homotopy equivalences, we get a category called where every space is *isomorphic* to a CW complex. This is called the "homotopy category".

These ideas are very powerful, so it's good to generalize them to rational homotopy theory. So now suppose X and Y are 1-connected pointed spaces. And let's say a map

$$f\colon X \to Y$$

is a "rational homotopy equivalence" if the induced maps on rational homotopy groups

$$\mathbb{Q} \otimes \pi_n(f)\colon \mathbb{Q} \otimes \pi_n(X) \to \mathbb{Q} \otimes \pi_n(Y)$$

are isomorphisms for all n. There's a nice category where we formally throw in inverses to all rational homotopy equivalences. This is called the "rational homotopy category".

In the rational homotopy category, we're studying ordinary spaces through a blurry lens, because two spaces that have a rational homotopy equivalence between them look the same.

But the rational homotopy category is equivalent to a subcategory of the usual homotopy category: namely, the subcategory consisting of rational spaces and all morphisms between those! So, we can also say we're studying strange spaces, but with complete precision — or at least, the usual level of precision in homotopy theory.

To learn more, I urge you to grab this and take a look:

2) Kathryn Hess, "Rational homotopy theory: a brief introduction", in *Interactions Between Homotopy Theory and Algebra*, ed. Luchezar L. Avramov, Contemp. Math **436**, AMS, Providence, Rhode Island, 2007. Also available as arXiv:math.AT/0604626.

For even more detail, I recommend:

3) Yves Felix, Stephen Halperin and Jean-Claude Thomas, *Rational Homotopy Theory*, Springer, Berlin, 2005.

I'll give more references later. In the weeks to come, we'll see two very different descriptions of the rational homotopy category, which were both discovered by Daniel Quillen back in 1969:

4) Daniel Quillen, "Rational homotopy theory", *Ann. Math.* **90** (1969), 205–295.

It's these other descriptions that make the subject really interesting. One is based on a homotopy version of Lie algebras. The other is based on a homotopy version of commutative algebras!

In the first sentence of his paper, Quillen defines the rational homotopy category. But he does it a bit differently than I just did. This is worth understanding. He says it's "the category obtained from the category of 1-connected pointed spaces by localizing with respect to the family of maps which are isomorphisms modulo the class in the sense of Serre of torsion abelian groups".

Let me say this with less jargon. Start with the category of 1-connected pointed spaces. Throw in formal inverses of all maps

$$f\colon X \to Y$$

for which the induced maps

$$\pi_n(f)\colon \pi_n(X) \to \pi_n(Y)$$

have kernels and cokernels where all elements are torsion. This gives the rational homotopy category!

I'll conclude with a few remarks that would have been a bit too distracting earlier.

First: I discussed rational homotopy theory only for 1-connected spaces. This is great as far as the connection to algebra goes. But in terms of topology it's a bit sad. Sometimes people go a step further and work with "nilpotent" spaces. These are spaces where the fundamental group is nilpotent and acts nilpotently on the higher homotopy groups.

Second: the rational circle is an interesting space. As we've seen, it's a space with the rational numbers as its fundamental group. All its other homotopy groups are trivial, since that's already true for the circle.

Any space with G as its nth homotopy group and every other homotopy group being trivial is called "the Eilenberg-Mac Lane space $K(G, n)$". We're allowed use the word "the", since this space is unique up to weak homotopy equivalence. So, the rational 1-sphere is $K(\mathbb{Q}, 1)$.

I've talked about lots of different Eilenberg-Mac Lane spaces in This Week's Finds, and they're all collected here:

3) John Baez, "Generalized cohomology theories, Eilenberg-Mac Lane spaces, classifying spaces and $K(\mathbb{Z}, n)$, and other examples of classifying spaces". Available at `http://math.ucr.edu/home/baez/calgary/BG.html`

Now you can add $K(\mathbb{Q}, 1)$ to your collection!

Third: in case you're wondering about Quillen's jargon: by "localizing" he means the process of taking a category and throwing in formal inverses to a bunch of morphisms. This is an important way of simplifying categories. It lets us count slightly different objects as the same.

A "Serre class" of abelian groups is a bunch of abelian groups such that whenever A and C are in this class, and the sequence

$$0 \to A \to B \to C \to 0$$

is exact, then B is in this class too. The idea is that we think of abelian groups in the Serre class as "small", or "negligible". For example: the class of finite abelian groups, or the class of torsion abelian groups. We can localize the category of abelian groups by throwing in an inverse for any morphism whose kernel and cokernel are in the Serre class.

If you like abelian categories, you can generalize this "Serre class" idea from the category of abelian groups to other abelian categories.

There's also much more to say about localization! Try this for starters:

5) nLab, "Localization", `http://ncatlab.org/nlab/show/localization`

Besides doing rational homotopy theory, we can use localization to take homotopy theory and "localize at the prime p". This is a way to focus special attention on the "p-torsion" in our homotopy groups: that is, the elements that give zero when you multiply them by a power of p.

Finally, what about the picture at the beginning of this Week's Finds? It shows sand dunes in a region called Abalos Undae near the north pole of Mars:

5) HiRISE (High Resolution Imaging Science Experiments), "Dunes in Abalos Undae", `http://hirise.lpl.arizona.edu/PSP_010219_2785`

The photo covers a strip about 1.2 kilometers across. As the HiRISE satellite sweeps over Mars it takes incredibly detailed photos like this. Here's the description on the HiRISE website:

> *The Abalos Undae dune field stretches westward, away from a portion (Abalos Colles) of the ice-rich north polar layered deposits that is separated from the*

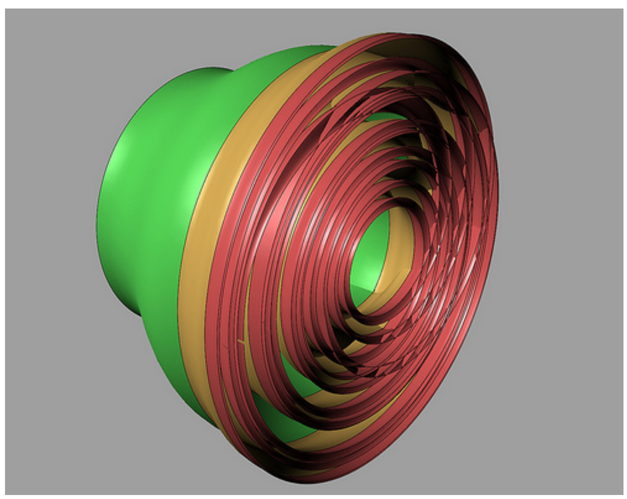
main Planum Boreum dome by two large chasms. These dunes are special because their sands may have been derived from erosion of the Rupes Tenuis unit (the lowest stratigraphic unit in Planum Boreum, beneath the icier layers) during formation of the chasms. Some researches have argued that these chasms were formed partially by melting of the polar ice.

The enhanced color data illuminate differences in composition. The dunes appear blueish because of their basaltic composition, while the reddish-white areas are probably covered in dust. Upon close inspection, tiny ripples and grooves are visible on the surface of the dunes; these features are formed by wind action, as are the dunes themselves.

It is possible that the dunes are no longer migrating (the process of dune formation forces dunes to move in the direction of the main winds) and that the tiny ripples are the only active parts of the dunes today.

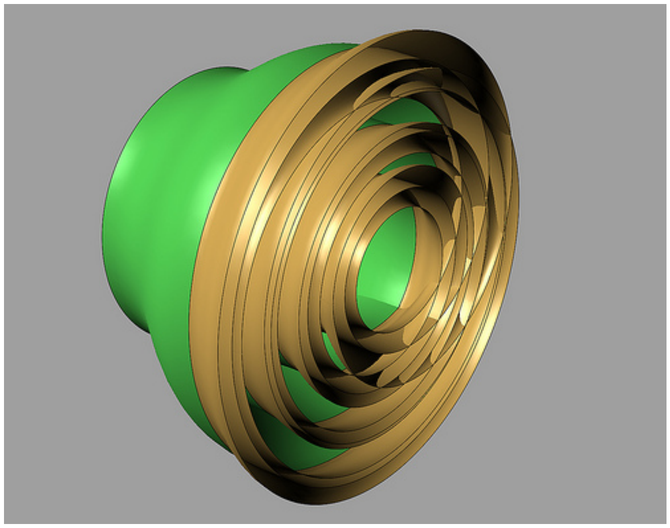
Addenda: The rational circle is pretty hard to draw, but Kenneth Baker did a great job of illustrating some early stages of its construction:

1) Kenneth Baker, "A (reverse) rational circle?", on his blog *Sketches of Topology* at `http://sketchesoftopology.wordpress.com/2009/12/10/a-rational-circle/`

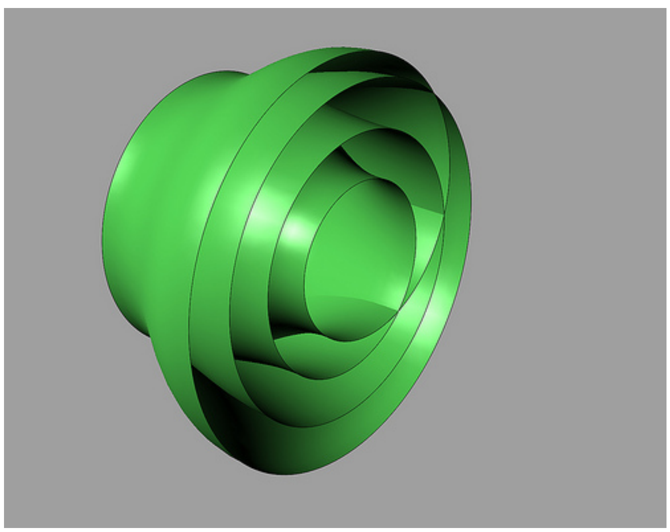


The right edge of the red band is our original circle, drawn in a tricky way to make the whole picture more manageable. The left edge of the red band is homotopic to 2 times the loop traced out by this original circle. The left of the orange band is homotopic to 6 times it, and the left edge of the green band is homotopic to 24 times it!

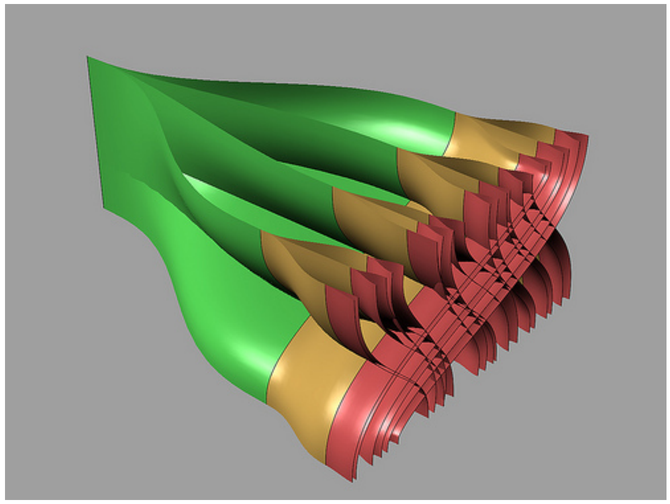
If we remove the red band we see how the orange one wrapped around it 3 times:



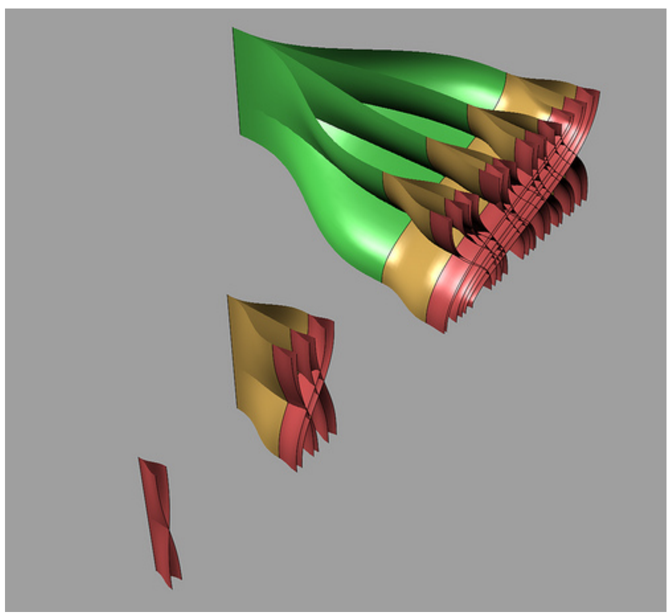
and if we remove the yellow band we see how the green one wrapped around it 4 times:



Here's a kind of cross-section that reveals more about what's going on:



Or in stages:



You're probably curious about how Kenneth Baker drew these pictures. Here's how:

> *These pictures are done using Rhino 3D. Actually I'm using the beta version of their port to OS X. There's a function (called Flow) that lets you map a "spine" of an object to another curve to tell it how to deform the object. This is how I went from the chopped open version to the round one. It's also how I managed to make the orange wrap around the green and the red wrap around the orange.*

On the n-Category Caf, Tom Leinster raised a useful point:

> *Something that bothered me for a while about rational homotopy, as an outsider, was this phrase "the homotopy groups are rational vector spaces". A priori the (higher) homotopy groups are abelian groups. So does this mean that there exists a rational vector space structure? That there exists a unique one? That one is somehow specified?*
>
> *In fact, these questions are unnecessary, for the following reason. (I think this was explained to me by the James who sometimes comments here.) Fact:*
>
>> *Let A be an abelian group. Then A has the structure of a rational vector space in at most one way.*
>
> *So, despite appearances, being a rational vector space is a property of abelian groups, not extra structure.*
>
> *The proof is fairly straightforward, I think. If A admits a rational vector space structure then*
>
>> *for all $a \in A$ and all positive integers n, there exists a unique $b \in A$ such that $nb = a$.*
>
> *And this condition clearly determines what the scalar multiplication over $\mathbb{Q}$ must be. (In fact, it's an 'iff': an abelian group admits the structure of a rational vector space if and only if it satisfies this condition.)*

Todd Trimble added:

> *Yes. A rational vector space is the same as a divisible torsionfree abelian group. Incidentally, an abelian group is divisible if and only if it is injective in the category of abelian groups, and is torsionfree if and only if it is flat in the category of abelian groups.*

For more discussion visit the n-Category Caf.

> . . . the pursuit of science is more than the pursuit of understanding. It is driven by the creative urge, the urge to construct a vision, a map, a picture of the world that gives the world a little more beauty and coherence than it had before. Somewhere in the child that urge is born.
>
> — *John Archibald Wheeler*

Week 287

December 19, 2009

This week: a fascinating history of categorical logic, and more about rational homotopy theory. But first, guess what this is a picture of:

If you give up, go to the bottom of this article.

Next, here's an incredibly readable introduction to the revolution that happened in logic starting in the 1960s:

1) Jean-Pierre Marquis and Gonzalo Reyes, "The history of categorical logic", 1963–1977. Available at `https://www.webdepot.umontreal.ca/Usagers/marquisj/MonDepotPublic/HistofCatLog.pdf`

It's a meaty but still bite-sized 116 pages. It starts with the definitions of categories, functors, and adjoint functors. But it really takes off in 1963 with Bill Lawvere's thesis, which revolutionized universal algebra using category theory. It then moves on through Lawvere and Tierney's introduction of the modern concept of topos, and it ends in 1977, when Makkai and Reyes published their book on categorical logic, and Johnstone published his book on topos theory. The world has never been the same since!

One great thing about this paper is that it discusses the history in a blow-by-blow way, including conferences and unpublished but influential writings. It also gives a great summary of the key ideas in Lawvere's thesis. I'll quote it, since everyone should know or at least *have seen* these ideas:

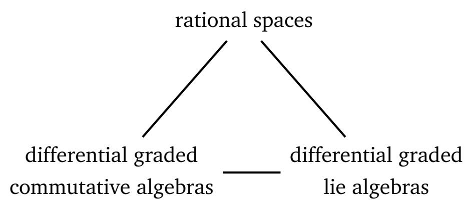
> 1. *To use the category of categories as a framework for mathematics, i.e. the category of categories should be the foundations of mathematics;*
> 2. *Every aspect of mathematics should be representable in one way or another in that framework; in other words, categories constitute the background to mathematical thinking in the sense that, in this framework, essential features of that thinking are revealed;*
> 3. *Mathematical objects and mathematical constructions should be thought of as functors in that framework;*
> 4. *In particular, sets always appear in a category, there are no such thing as sets by themselves, in fact there is no such thing as a mathematical concept by itself;*
> 5. *But sets form categories and the latter categories play a key role in the category of categories, i.e. in mathematics;*
> 6. *Adjoint functors occupy a key position in mathematics and in the development of mathematics; one of the guiding principles of the development of mathematics should be "look for adjoints to given functors"; in that way foundational studies are directly linked to mathematical practice and the distinction between foundational studies and mathematical studies is a matter of degree and direction, it is not a qualitative distinction;*
> 7. *As the foregoing quote clearly indicates, Lawvere is going back to the claim made by Eilenberg and Mac Lane that the "invariant character of a mathematical discipline can be formulated in these terms" [i.e. in terms of functoriality]. But now, in order to reveal this invariant character, extensive use of adjoint functors is made.*
> 8. *The invariant content of a mathematical theory is the "objective" content of that theory; this is expressed at various moments throughout his publications. To wit:*

As posets often need to be deepened to categories to accurately reflect the content of thought, so should inverses, in the sense of group theory, often be replaced by adjoints. Adjoints retain the virtue of being uniquely determined reversal attempts, and very often exist when inverses do not.

9. *Not only sets should be treated in a categorical framework, but also logical aspects of the foundations of mathematics should be treated categorically, in as much as they have an objective content. In particular, the logical and the foundational are directly revealed by adjoint functors.*

If this sounds mysterious, well, read the paper!

Now I want to dig a little deeper into rational homotopy theory, and start explaining this chart:



Last time I described rational spaces and the category they form: the "rational homotopy category". I actually described this category in three ways. But there are two other ways to think about this category that are much cooler!

That's what the other corners of this triangle are. The lower left corner involves a drastic generalization of differential forms on smooth manifolds. The lower right corner involves a drastic generalization of Lie algebras coming from Lie groups.

Today I'll explain the road to the lower left corner. Very roughly, it goes like this. If you give me a space, I can replace it with a space made of simplices that has the same homotopy type, and then take the differential forms on this replacement. Voil! A differential graded commutative algebra!

But I want to avoid talking to just the experts. Among other things, I want to use rational homotopy theory as an excuse to explain lots of good basic math. So, first I'll remind you about differential forms on a manifold, and why they're a "differential graded commutative algebra", or "DGCA" for short. Then I'll show you how to define something like differential forms starting from any topological space! Again, they're a DGCA. And it turns out that for rational spaces, this DGCA knows everything there is to know about our space — at least as far as homotopy theory is concerned.

So: what are differential forms? Differential forms are a basic tool for doing calculus on manifolds. We use them throughout physics: they're the grownup version of the "gradient", "divergence" and "curl" that we learn about as kids. There are lots of ways to define them, but the most rapid is this.

The smooth real-valued functions

$$f: X \to R$$

on a manifold X form an algebra over the real numbers. In other words: you can add and multiply them, and multiply them by real numbers, and a bunch of familiar identities hold, which are the axioms for an algebra. Moreover, this algebra is commutative:

$$fg = gf$$

Starting from this commutative algebra, or any commutative algebra A, we can define differential forms as follows. First let's define vector fields in a purely algebraic way. Since the job of a vector field is to differentiate functions, people call them derivations in this algebraic approach. A "derivation of A" is a linear map

$$v\colon A \to A$$

which obeys the product rule

$$v(ab) = v(a)b + av(b)$$

Let $\mathrm{Der}(A)$ be the set of derivations of A. This is a "module" of the algebra A, since we can multiply a derivation by a guy in A and get a new derivation. (This part works only because A is commutative.)

Next let's define 1-forms. Since the job of these is to eat vector fields and spit out functions, let's define a "1-form" to be a linear map

$$\omega\colon \mathrm{Der}(A) \to A$$

which is actually a module homomorphism, meaning

$$\omega(fv) = f\omega(v)$$

whenever f is in A. Let $\Omega^1(A)$ be the set of 1-forms. Again, this is a module of A.

Just as you'd expect, there's a map

$$d\colon A \to \Omega^1(A)$$

defined by

$$(df)(v) = v(f)$$

and you can check that

$$d(fg) = (df)g + fdg$$

So, we've got vector fields and 1-forms! It's a bit tricky, but you can prove that when A is the algebra of smooth real-valued functions on a manifold, the definitions I just gave are equivalent to all the usual ways of defining vector fields and 1-forms. One advantage of working algebraically is that we can generalize. For example, we can take A to consist of *polynomial* functions. We'll use this feature in a minute.

But what about other differential forms? There's more to life than 1-forms: there are p-forms for $p = 0, 1, 2, \ldots$.

To get these, we just form the exterior algebra of the module $\Omega^1(A)$. You may have seen the exterior algebra of a vector space — if not, it may be hard understanding the stuff I'm explaining now. The exterior algebra of a module over a commutative algebra works the same way! To build it, we run around adding and multiplying guys in A and

$\Omega^1(A)$, all the while making sure to impose the axioms of an associative unital algebra, together with these rules:

$$f(dg) = (dg)f$$
$$(df)(dg) = -(dg)(df)$$

The stuff we get forms an algebra: the algebra of "differential forms" for A, which I'll call $\Omega(A)$. And when A is the smooth functions on a manifold, these are the usual differential forms that everyone talks about!

Now, thanks to the funny rule

$$(df)(dg) = -(dg)(df)$$

the algebra $\Omega(A)$ is not commutative. However, it's "graded commutative", meaning roughly that it's commutative except for some systematically chosen minus signs.

A bit more precisely: every differential form can be written as a linear combination of guys like this:

$$v = f dg_1 dg_2 \ldots dg_p$$

where p ranges over all natural numbers. Linear combinations of guys of this sort for a particular fixed p are called "p-forms". We also say they're "of degree p". And the algebra of differential forms obeys

$$\nu\omega = (-1)^{pq}\omega\nu$$

whenever ν is of degree p and ω is of degree q. This is what we mean by saying $\Omega(A)$ is "graded commutative".

But the algebra of differential forms is better than a mere graded commutative algebra! We've already introduced df when f is an element of our original algebra. But we can define "d" for *all* differential forms simply by saying that d is linear and saying that d of

$$f dg_1 dg_2 \ldots dg_p$$

is

$$df dg_1 dg_2 \ldots dg_p$$

This definition implies three facts. First, it implies that d of a p-form is a $(p+1)$-form. That's pretty obvious. Second, it implies that

$$d(d\omega) = 0$$

for any differential form ω. Why? Well, I'll let you check it, but I'll give you a hint: the key step is to show that $d1 = 0$. And third, it implies this version of the product rule:

$$d(\nu\omega) = (d\nu)\omega + (-1)^p \nu d\omega$$

for any p-form ν and q-form ω. Again the proof is a little calculation.

We can summarize these three facts, together with the linearity of d, by saying that differential forms are a "differential graded commutative algebra", or "DGCA".

You can do lots of wonderful stuff with differential forms. After you learn a bunch of this stuff, it becomes obvious that you should generalize them to apply to spaces of many kinds.

It's easy to generalize them from manifolds to spaces X where you have a reasonable idea of when a real-valued function

$$f: X \to R$$

counts as "smooth". Just take the commutative algebra A of smooth real-valued functions on X and construct $\Omega(A)$ following my instructions!

There are many examples of such spaces, including manifolds with boundary, manifolds with corners, and infinite-dimensional manifolds. In fact, there are general theories of "smooth spaces" that systematically handle lots of these examples:

2) Andrew Stacey, "Comparative smootheology", available as arXiv:0802.2225.

3) Patrick Iglesias-Zemmour, *Diffeology*. Available at http://math.huji.ac.il/~piz/Site/The%20Book/The%20Book.html

4) John Baez and Alexander Hoffnung, "Convenient categories of smooth spaces", to appear in *Trans. Amer. Math. Soc.*. Also available as arXiv:0807.1704.

But here's a question that sounds harder: how can we generalize differential forms to an arbitrary *topological* space X?

You could take A to be the algebra of *continuous* functions on X and form $\Omega(A)$. There's no law against it... go ahead... but I bet no good will come of it. (What goes wrong?)

But there's a better approach, invented by Dennis Sullivan in this famous paper:

5) Dennis Sullivan, "Infinitesimal computations in topology", *Publications Mathmatiques de l'IHES* **47** (1977), 269–331. Available at http://www.numdam.org/item?id=PMIHES_1977__47__269_0

We start by turning our topological space into a simplicial set. Remember, a simplicial set is a bunch of

- 0-simplices (vertices)
- 1-simplices (edges)
- 2-simplices (triangles)
- 3-simplices (tetrahedra)

and so on, all stuck together. Given a topological space X, we can form an enormous simplicial set whose n-simplices are all possible maps

$$f: \Delta^n \to X$$

where Δ^n is the standard n-simplex, that is, the intersection of the hyperplane

$$x_0 + x_1 + \ldots + x_n = 1$$

with the set where all the coordinates x_i are nonnegative.

This enormous simplicial set is called the "singular nerve" of X, $\mathrm{Sing}(X)$. Like any simplicial set, we can think of $\mathrm{Sing}(X)$ as a purely combinatorial gadget, but we can also "geometrically realize" it and think of it as a topological space in its own right. The resulting space is called $|\mathrm{Sing}(X)|$.

(For more details on the singular nerve and its geometric realization, see items E and F of "Week 116".)

This space $|\mathrm{Sing}(X)|$ is made of a bunch of simplices stuck together along their faces. So, we can say a real-valued function on $|\mathrm{Sing}(X)|$ is "simplex-wise smooth" if it's continuous and smooth on each simplex. And this is enough to set up a theory of differential forms! We just take the algebra A of simplex-wise smooth functions on $|\mathrm{Sing}(X)|$, and use this to build our algebra of differential forms $\Omega(A)$ as I've described!

But Sullivan noted that we can go even further. Thanks to how we've defined the standard n-simplex, it makes sense to talk about polynomial functions on this simplex. We can even sidestep the need for real numbers, by looking at polynomial functions with *rational* coefficients. And that's just right for rational homotopy theory.

So, let's focus our attention on functions on $|\mathrm{Sing}(X)|$ that when restricted to any simplex give polynomials with rational coefficients. This is a commutative algebra over the *rational* numbers. Call it A. We can copy our previous construction of $\Omega(A)$ but now working with rational numbers instead of reals. Let's call guys in here "rational differential forms".

Now, you may complain that we're not really studying differential forms on X: we're studying them on this other space $|\mathrm{Sing}(X)|$. At one point in my life this really annoyed me. It seemed like a cheat. But for the purposes of homotopy theory it's perfectly fine, since $|\mathrm{Sing}(X)|$ has the same homotopy type as X.

(By this, I really mean they're isomorphic in the "homotopy category", which I defined last week. So: they're the same, as far as homotopy theory is concerned.)

And even better, when X is a rational space, the rational differential forms on $|\mathrm{Sing}(X)|$ will know *everything* about the homotopy type of X. This is amazing! It means that for rational spaces, we can reduce homotopy theory to a souped-up version of the theory of differential forms!

In particular, Sullivan was able to use this trick to compute the *homotopy groups* of a rational space X, starting from the rational differential forms on $|\mathrm{Sing}(X)|$.

Since X and $|\mathrm{Sing}(X)|$ have the same homotopy type, they have the same homotopy groups, and cohomology groups, and so on. And it's not surprising that we can read off the *cohomology groups* of $|\mathrm{Sing}(X)|$ starting from the rational differential forms on this space — this is just a slight twist on the usual idea of deRham cohomology. But it's surprising that we can compute the *homotopy groups*, which are usually a lot harder. This is the magic of rational homotopy theory.

I won't explain this magic, at least not today. For that read Sullivan's paper, or this paper I recommended last time:

6) Kathryn Hess, "Rational homotopy theory: a brief introduction", in *Interactions Between Homotopy Theory and Algebra*, ed. Luchezar L. Avramov, Contemp. Math **436**, AMS, Providence, Rhode Island, 2007. Also available as `math.AT/0604626`.

For more detail, try this book:

7) Phillip A. Griffiths and John W. Morgan, *Rational Homotopy Theory and Differential Forms*, Birkhuser, Boston, 1981.

Someday I should explain exactly the sense in which (certain) DGCAs are "the same" as rational homotopy types. But not today!

Instead, I want to go over what I just said in a slightly more formal way. This will give me an excuse to introduce a bunch of beautiful concepts that everyone should know... and maybe demonstrate a tiny bit of what Lawvere was talking about: the power of categories.

First of all, what's a DGCA, really? It's a commutative monoid in the symmetric monoidal category of cochain complexes!

Let me explain.

A "cochain complex", for us, will be a list of vector spaces and linear maps

$$C_0 \xrightarrow{d} C_1 \xrightarrow{d} C_2 \xrightarrow{d} \ldots$$

with $d^2 = 0$. We can use vector spaces over any field we like; let's use the rational numbers to be specific.

Just as you can tensor vector spaces, you can tensor cochain complexes. The tensor product of cochain complexes C and C' will have

$$(C \otimes C')_n = \oplus_{p+q=n} C_p \otimes C'_q$$

and we define

$$d(c \otimes c') = dc \otimes c' + (-1)^p c \otimes dc'$$

when c is in C_p and c' is in C'_q.

(You've seen a similar "product rule" earlier in this article. There's a general principle at work here. Physicists know that whenever you exchange two fermions, their phase gets multiplied by -1. In math, we should stick in a minus sign whenever we switch two "odd" things. The map d counts as odd since it sends guys in our cochain complex to guys whose degree is 1 more, and the number 1 is odd. The element c in C_p counts as "odd" whenever p is odd. In the equation above, we're switching d past c and getting a minus sign whenever c is odd.)

Just as you can define a commutative algebra to be a vector space V with a product

$$V \otimes V \to V$$

that's associative and commutative, you can define a "differential graded commutative algebra", or DGCA, to be a cochain complex C with a product

$$C \otimes C \to C$$

that's associative and graded-commutative. By "graded-commutative", I mean you need to remember to put in a sign $(-1)^{pq}$ whenever you switch a guy in C^p and a guy in C^q.

We can systematize all this by checking that, just like the category of vector spaces with its usual tensor product, the category of cochain complexes with its tensor product is a "symmetric monoidal category":

8) nLab, "Symmetric monoidal category", `http://ncatlab.org/nlab/show/symmetric+monoidal+category`

So is the category of sets with its cartesian product. We can define a "commutative monoid" in any symmetric monoidal category. In the category of sets, this is just a commutative monoid in the traditional sense. In the category of vector spaces, it's a commutative algebra. And in the category of cochain complexes, it's a DGCA!

Notice: a DGCA where only C_0 is nonzero is just a plain old commutative algebra. So, DGCAs really are a generalization of commutative algebras. So whenever anyone tells you something about DGCAs, you should check to see what it says about commutative algebras. And whenever anyone tells you something about commutative algebras, you should try to generalize it to DGCAs!

This should keep you pretty busy, since commutative algebras are the playground of the simplest kind of algebraic geometry: the kind where you look at solutions of polynomial equations in a bunch of variables, like this:

$$x^2 + y^2 + z^2 = 0$$
$$xyz - 1 = 0$$

If you take your polynomials and count them as zero when they satisfy your equations, you get a commutative algebra. Even better, you can get any sufficiently small commutative algebra this way — the technical term is "finitely presented". And if you allow infinitely many variables and infinitely many equations, you can drop that technical fine print.

So, the study of commutative algebras is really just the study of polynomial equations. And if we think about their solutions as forming curves or surfaces or the like, we're doing algebraic geometry - so-called "affine" algebraic geometry.

This means that we can — and in fact should! — generalize all of affine algebraic geometry from commutative algebras to DGCAs. I'd like to say more about this someday. . . but not today. This is just a digression. I got distracted from my real goal.

Before I got distracted, I was telling you how commutative algebras are the same as DGCA's with only C_0 being nonzero. And here's why I mentioned this. We can take *any* DGCA and violently kill C_p for all $p > 0$, leaving a commutative algebra C_0. We can think of this as a forgetful functor

$$\text{[DGCAs]} \rightarrow \text{[commutative algebras]}$$

And this functor has a left adjoint, which freely generates a DGCA starting from a commutative algebra:

$$\text{[commutative algebras]} \rightarrow \text{[DGCAs]}$$

Now, I've already told you about process that takes a commutative algebra and creates the DGCA. Namely, the process that takes a commutative algebra A and gives the DGCA of differential forms, $\Omega(A)$. So, you might think this left adjoint is just that!

I thought so too, when I was first writing this. But it turns out not to be true — at least not always! The left adjoint gives a slightly *different* kind of differential forms for our commutative algebra A. Let's call these the "Khler forms" $\Omega_K(A)$.

The Khler 1-forms are usually called "Khler differentials". We can can build them as follows: take the A-module generated by symbols

$$df$$

which obey the 3 basic relations we expect in calculus:

$$d(cf) = cdf$$
$$d(f+g) = df + dg$$
$$d(fg) = fdg + (df)g$$

where f, g are in A and c is in our field. This gives the A-module of Khler differentials — let's call this $\Omega^1_K(A)$. The Khler forms $\Omega_K(A)$ are then the exterior algebra on $\Omega^1_K(A)$.

By how we've set things up, the Khler differentials are blessed with a map

$$d\colon A \to \Omega^1_K(A)$$

And this map is a "derivation", meaning it satisfies the 3 rules listed above. But here's the cool part: the Khler differentials are the *universal* A-module with a derivation. In other words, suppose M is any A-module equipped with a map

$$v\colon A \to M$$

that's a derivation in the above sense. Then there's a unique A-module homomorphism

$$j\colon \Omega^1_K(A) \to M$$

such that

$$v = jd$$

The proof is easy: just define $j(df) = v(f)$ and check that everything works!

Thanks to this universal property, Khler differentials are much beloved by algebraists. So, it's natural to wonder if they're the same as the 1-forms $\Omega^1(A)$ that I explained above!

As it turns out, these 1-forms are the double dual of the Khler differentials:

$$\Omega^1(A) = \Omega^1_K(A)^{**}$$

Sometimes we get

$$\Omega^1(A) = \Omega^1_K(A)$$

and this case it's easy to check that

$$\Omega(A) = \Omega_K(A)$$

But sometimes the 1-forms and the Khler differentials are *different*. Let me explain why. It's technical, but fun if you're already familiar with some of these ideas.

For starters, let me explain what I mean! We've got a commutative algebra A. If we have an A-module M, its "dual" M^* is the set of all A-module maps

$$w\colon M \to A$$

The dual becomes a module in its own right by

$$(gw)(f) = gw(f)$$

So, we can take the dual of the dual, M^{**}. And then there's always a module homomorphism

$$j\colon M \to M^{**}$$

given by

$$j(f)(w) = w(f)$$

for f in M, w in M^*. Sometimes j is an isomorphism: for example, when M is finitely generated and projective. But often it's not. And that's where the subleties arise.

If you look back at my definition of 1-forms, it amounted to this:

$$\Omega^1(A) = \mathrm{Der}(A)^*$$

And the universal property of Khler differentials gives us this:

$$\mathrm{Der}(A) \cong \Omega^1_K(A)^*$$

Putting these facts together, we get

$$\Omega^1(A) \cong \Omega^1_K(A)^{**}$$

So, we always have a module homomorphism

$$j\colon \Omega^1_K(A) \to \Omega^1(A)$$

This is *both* the map we always get from a module to its double dual, *and* the map we get from the universal property of Khler differentials.

Now, here's the tricky part. This map j is always a surjection. And it will be an *isomorphism* when the Khler differentials are a finitely generated projective module. But it won't *always* be an isomorphism!

For example, when A is the algebra of rational polynomials on a simplex, $\Omega^1_K(A)$ is a finitely generated projective module. In fact it's the free module with one generator dx_i for each independent coordinate. So in this case we actually get an isomorphism

$$\Omega^1(A) \cong \Omega^1_K(A)$$

and thus

$$\Omega(A) \cong \Omega_K(A)$$

More generally, this is true whenever A is the algebraic functions on a smooth affine algebraic variety, by the same sort of argument. So in these cases, you don't need to worry about the niggling nuances I'm rubbing your nose in here.

But when A is the algebra of smooth functions on a manifold, the 1-forms are *not* the same as the Khler differentials!

Indeed, let A be the algebra of smooth functions on the real line. Then one can show

$$j\colon \Omega^1_K(A) \to \Omega^1(A)$$

is not one-to-one. In fact, David Speyer showed this after Maarten Bergvelt noticed I was being overoptimistic in assuming otherwise. You can see Speyer's argument here:

9) David Speyer, "Kahler differentials and ordinary differentials", *Math Overflow*, `http://mathoverflow.net/questions/6074/kahler-differentials-and-ordinary-differentials/9723#9723`

He shows that in $\Omega^1_K(A)$, $d(e^x)$ is not equal to $e^x dx$. The intuition here is simple: showing these guys are equal requires actual calculus, with limits and stuff. But Khler differentials are defined purely algebraically, so they don't know that stuff!

However, turning this idea into a proof takes work. It can't be as easy as I just made it sound! After all, $\Omega^1(A)$ was *also* defined purely algebraically, and in here we *do* have $d(e^x) = e^x dx$. Indeed, this is *why* Speyer's argument shows that

$$j\colon \Omega^1_K(A) \to \Omega^1(A)$$

fails to be one-to-one.

So now you should be wondering: how do we know $d(e^x) = e^x dx$ in $\Omega^1(A)$? Since $\Omega^1(A)$ is the dual of the derivations, to show

$$d(e^x) = e^x dx$$

we just need to check that they agree on all derivations. The hard part is to prove that any derivation of A is of the form

$$v(f) = gf'$$

for some g in A, where f' is the usual derivative of f. Then we have

$$d(e^x)(v) = v(e^x) = ge^x = e^x v(x) = (e^x dx)(v)$$

so we're done!

(Here x is the usual function by that name on the real line — you know, the one that equals x at the point x. Sorry — that sounds really stupid! But anyway, the derivative of x is 1, so $v(x) = g$.)

So here's the hard part. Say we have a derivation v of the algebra A of smooth functions on the real line. Why is there a function g such that

$$v(f) = gf'$$

for all functions f? As you can guess from my parenthetical remark, we should try

$$g = v(x)$$

So, let's prove

$$v(f) = v(x)f'$$

We just need to check they're equal at any point x_0. So, let's use a kind of Taylor series trick:

$$f(x) = f(x_0) + (x - x_0)f'(x_0) + (x - x_0)^2 h(x)$$

Here it's utterly crucial that h is a smooth function on the real line. Check that yourself!!! Then, apply the derivation v and use the three rules that derivations obey:

$$v(f)(x) = v(x)f'(x_0) + 2(x - x_0)v(x)h(x) + (x - x_0)^2 v(h)$$

Then evaluate both sides at $x = x_0$. A bunch of stuff goes away:

$$v(f)(x_0) = v(x)f'(x_0)$$

Since this was true for any point x_0, we indeed have

$$v(f) = v(x)f'$$

as desired.

Sneaky, huh? The argument looked "purely algebraic" — but only because we could pack all the calculus into the utterly crucial bit that I made you check for yourself. By the way, this utterly crucial bit uses the theory of "Hadamard quotients": if f is smooth function on the real line then

$$\frac{f(x) - f(y)}{x - y}$$

extends to a smooth function on the plane if we define it to be the derivative of f when $x = y$.

A fancier version of this argument works for $\mathbb{R}^n$. This in turn gives the usual proof that that derivations of the algebra A of smooth functions on a manifold X are the same as smooth vector fields. And that, in turn, guarantees that $\Omega(A)$ as defined algebraically matches the ordinary concept of differential forms on X. The Khler forms are different, but as we've seen, there's a surjection of DGCAs

$$j\colon \Omega_K(A) \to \Omega(A)$$

sending any function f in Khler land to the same function f in ordinary differential form land.

So that's the story! It's a bit technical, but if we didn't occasionally enjoy being dragged through the mud of technical details, we wouldn't like math, now, would we? I think even more details will become available here:

10) nLab, "Khler differential", `http://ncatlab.org/nlab/show/K%C3%A4hler+differential`

This may be a good place to stop reading, if you don't already love category theory up to and including "weighted colimits". But I can't resist saying a bit more. And if you've never understood weighted colimits, maybe this will make you want to.

I already told you how we turn any topological space X into a simplicial set $\mathrm{Sing}(X)$ and then back into a bigger topological space $|\mathrm{Sing}(X)|$ and then into a DGCA.

But if you know homotopy theory well, you know this subject regards topological spaces and simplicial sets as two different views of "the same thing". So turning a topological space into a simplicial set is no big deal. So in fact, the the core of the above construction is the process that takes a simplicial set and turns it into a DGCA. And I'd like to explain this process a bit more efficiently now.

Here's the point: this process is a lot like "geometric realization". In geometric realization we start with a simplicial set S, which is really a functor

$$S\colon \Delta^{\mathrm{op}} \to \mathsf{Set}$$

where Δ is the category of simplices. And we know how to turn any simplex into a topological space, so we also have a functor

$$F\colon \Delta \to \mathsf{Top}$$

We can then take the "weighted colimit" of F with S as our "weight". This creates a topological space $|S|$, the "geometric realization" of S.

The idea is that we take each simplex in our simplicial set, turn it into a space, and then glue all these spaces together. For this trick to work, all we need is that the category Top has colimits.

Similarly, we know how to turn any simplex into a DGCA, namely the rational differential forms on that simplex! So we also have a functor

$$F'\colon \Delta \to [\text{DGCAs}]^{\text{op}}$$

There's an "op" here because of the usual contravariant relation between algebra and geometry. But never mind: what matters is that $\mathsf{DGCA}^{\text{op}}$ has colimits. So we can copy what we did before, and take the weighted colimit of f' with S as our weight. And now this creates a DGCA: the "rational differential forms" on our simplicial set S.

The idea is that we take each simplex in our simplicial set, turn it into a DGCA, and then glue all these DGCAs together. But perhaps I should say "coglue", because of that "op".

While we're playing these games, I should point out a simpler version. We also have a functor

$$F''\colon \Delta \to [\text{commutative algebras}]^{\text{op}}$$

and we can pull the same stunt to turn our simplicial set into a commutative algebra, which is the algebra of functions that restrict to polynomials with rational coefficients on each simplex!

But in this case, there's a super-famous name for the category

$$[\text{commutative algebras}]^{\text{op}}$$

It's called the category of "affine schemes". And so we can think of this stunt more geometrically as the process of taking an affine scheme for each simplex and gluing them together to get an affine scheme for our simplicial set S! So we're doing a kind of "geometric realization" with affine schemes replacing topological spacs.

This leads up to a question for the experts. Is there a famous name for the category

$$[\text{DGCAs}]^{\text{op}}?$$

It's *related* to the category of "simplicial affine schemes", no? But it's not quite the same. Can we think of this category as consisting of simplicial affine schemes with an extra property? You see, this bears heavily on the idea that rational homotopy theory is a generalization of algebraic geometry, with DGCAs replacing commutative algebras.

Finally: the picture at the start of This Week's Finds shows dry ice — frozen carbon dioxide — on the south pole of Mars:

9) HiRISE (High Resolution Imaging Science Experiments), "South polar residual cap monitoring: rare stratigraphic contacts", `http://hirise.lpl.arizona.edu/ESP_014379_0925`

This dry ice forms quite a variety of baroque patterns. I don't know how it happens! Here are couple more good pictures:

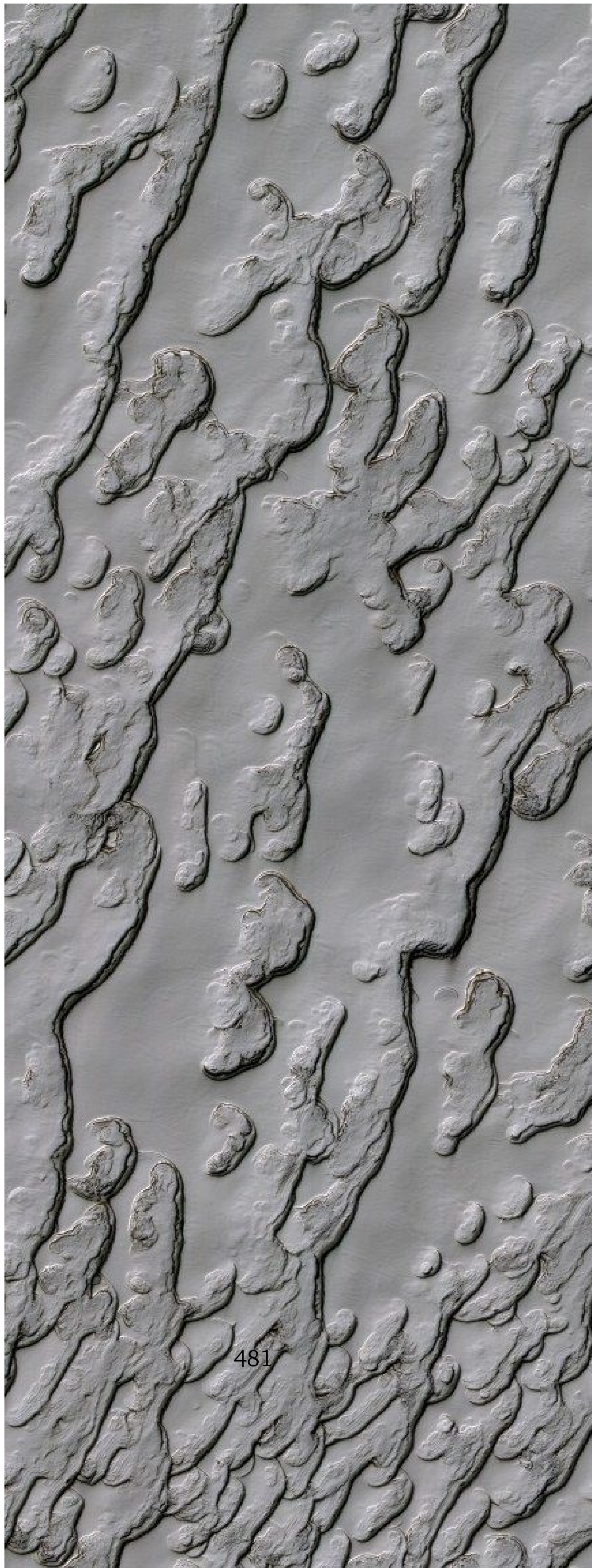
10) HiRISE (High Resolution Imaging Science Experiments), "Evolution of the south polar residual cap", `http://hirise.lpl.arizona.edu/PSP_004687_0930`

11) HiRISE (High Resolution Imaging Science Experiments), "South polar carbon dioxide ice cap", `http://hirise.lpl.arizona.edu/ESP_014261_0930`

Patrick Russell wrote a description of the last one:

> *This HiRISE image is of a portion of Mars' south polar residual ice cap. Like Earth, Mars has concentrations of water ice at both poles.*
>
> *Because Mars is so much colder, however, the seasonal ice that gets deposited at high latitudes in the winter and is removed in the spring (generally analogous to winter-time snow on Earth) is actually carbon dioxide ice. Around the south pole there are areas of this carbon dioxide ice that do not disappear every spring, but rather survive winter after winter. This persistent carbon dioxide ice is called the south polar residual cap, and is what we are looking at in this HiRISE image.*
>
> *Relatively high-standing smooth material is broken up by semi-circular depressions and linear, branching troughs that make a pattern resembling those of your fingerprints. The high-standing areas are thicknesses of several meters of carbon dioxide ice. The depressions and troughs are thought to be caused by the removal of carbon dioxide ice by sublimation (the change of a material from solid directly to gas). HiRISE is observing this carbon dioxide terrain to try to determine how these patterns develop and how fast the depressions and troughs grow.*
>
> *While the south polar residual cap as a whole is present every year, there are certainly changes taking place within it. With the high resolution of HiRISE, we intend to measure the amount of expansion of the depressions over multiple Mars years. Knowing the amount of carbon dioxide removed can give us an idea of the atmospheric, weather, and climate conditions over the course of a year.*
>
> *In addition, looking for where carbon dioxide ice might be being deposited on top of this terrain may help us understand if there is any net loss or accumulation of the carbon-dioxide ice over time, which would be a good indicator of whether Mars' climate is in the process of changing or not.*

Here's what it looks like:



It looks like a white Christmas, just like the one they're having on the east coast of the United States! My mom lives in DC, and I need to call her and find out how she's doing, with all this snow.

Addenda: I wrote:

> *So, let's focus our attention on functions on $|\mathrm{Sing}(X)|$ that when restricted to any simplex give polynomials with rational coefficients. This is a commutative algebra over the* rational *numbers. Call it A.*

Maarten Bergvelt inquired:

> *Is it obvious that there are any such functions?*

And this was a good question. The constant functions obviously work, but we'd really like at least enough functions of this sort to "separate points" on $|\mathrm{Sing}(X)|$. We say a collection of functions on a space "separate points" if for any two points $x \neq y$ in that space, we can find a function f in our collection with $f(x) \neq f(y)$.

And indeed, we'd like this to work for any simplicial set. Given a simplicial set S, we can define an algebra A of real-valued functions on $|S|$ that are rational polynomials when restricted to each simplex. Do the functions in A separate points of $|S|$?

Over at the n-Category Caf we showed the answer is *yes*. The key lemma is this:

> ***Conjecture:*** *Suppose we are given an n-simplex and a continuous function f on its boundary which is a rational polynomial on each face. Then f extends to a rational polynomial on the whole n-simplex.*

David Speyer explained how to prove it.

Maarten Bergvelt also caught a big mistake. I had thought the smooth 1-forms on smooth manifold were the same as the Khler differentials for its algebra of smooth functions. Maarten doubted this — and David Speyer was able to prove it's wrong! (His proof uses the axiom of choice, since it involves a nonprincipal ultrafilter. Do we *need* the axiom of choice here?)

This led to a big discussion, which I've attempted to summarize in the above improved version of "Week 287". To see the discussion we had, and add your comments, visit n-Category Caf.

> We live on an island surrounded by a sea of ignorance. As our island of knowledge grows, so does the shore of our ignorance.
>
> — *John Wheeler*

Week 288

January 1, 2010

Happy New Decade! I hope you're doing well. This week I'll say more about rational homotopy theory, and why the difference between equality and isomorphism is important for understanding the weather in space. But first: electrical circuits!

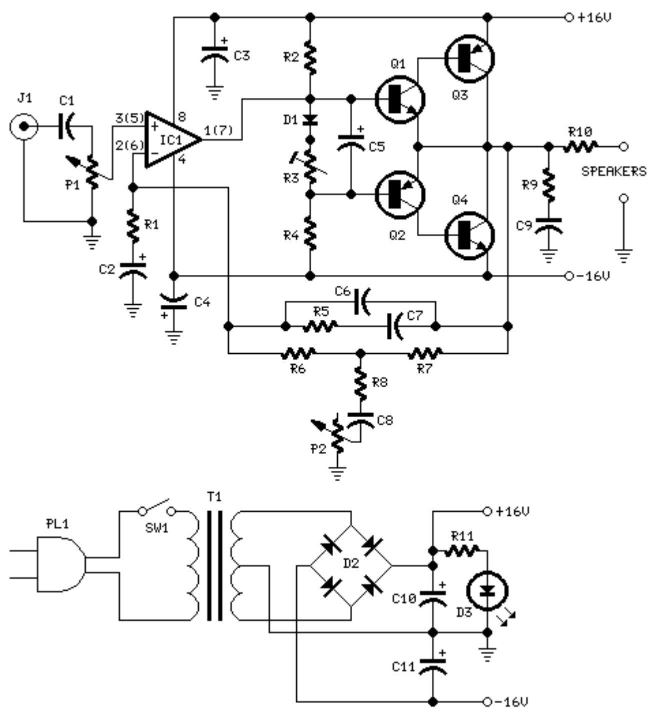
But even before that. . . guess what this is a picture of:

Now, what about electrical circuits?

I've been thinking This Week's Finds has become a bit too far removed from its roots in physics. This problem started when I quit working on quantum gravity and started focusing on n-categories. Overall it's been a big boost to my sanity. But I don't want This Week's Finds to be comprehensible only to an elite coterie of effete mathematicians — the sort who eat simplicial presheaves for breakfast and burp up monoidal bicategories.

So, in an effort to prevent This Week's Finds from drifting off into the stratosphere of abstraction, I've decided to talk a bit about electrical circuits. Admittedly, these are less glamorous than theories of quantum gravity. But: they actually work! And there is a lot of nice math involved.

I rarely dare predict what *future* Week's Finds will discuss, because I know from bitter experience that I change my mind. But lately I've started writing a new way: long stories with lots of episodes, which I can dole out a bit at a time. So I know that for at least a few Weeks I'll talk about electrical circuits — and various related things.



10 watt amplifier with bass boost.

I've been trying to understand electrical circuits using category theory for a long time. Indeed, Peter Selinger and I are very slowly writing a paper on this subject. The basic inspiration is that electrical circuit diagrams look sort of like Feynman diagrams, flow charts, and various other diagrams that have "inputs" and "outputs". I love diagrams like this! All the kinds I've met so far can be nicely formalized using category theory. For an explanation, try this:

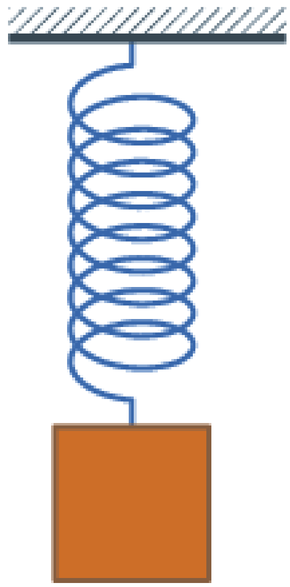
1) John Baez and Mike Stay, "Physics, topology, logic and computation: a Rosetta

Stone", to appear in *New Structures in Physics*, ed. Bob Coecke. Available at arXiv:0903.0340.

And after I spent a while thinking about electrical circuits using category theory, I realized that this perspective might shed light on analogies between circuits and other systems.

For example: mechanical systems made from masses and springs!

Indeed, whenever I teach linear differential equations, I like to explain the basic equation describing a "damped harmonic oscillator": for example, a rock hanging on a spring.



Then I explain how the same equation describes the simplest circuit made of a resistor, an inductor, and a capacitor — the so-called "RLC circuit". It's a nice easy example of how the same math applies to superficially different but secretly isomorphic problems!

Let me explain. I hope this is a chance to help mathematicians review their physics and ask questions about it over on the n-Category Caf.

Let the height of a rock hanging on a spring be $q(t)$ at time t, where $q(t)$ is negative when the end of the spring is down below its equilibrium position. Then making all sort of simplifying assumptions and approximations, we have:

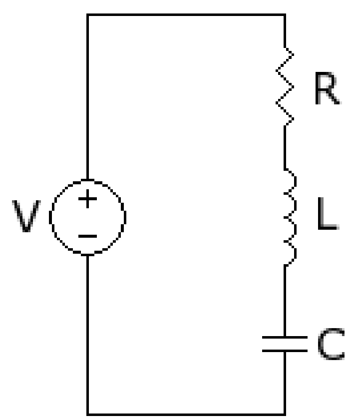
$$mq''(t) = -cq'(t) - kq(t) + F(t)$$

where:

- m is the mass of the rock.
- c is the damping coefficient, which describes the force due to friction: we're assuming this force is proportional to the rock's velocity, but points the other way.
- k is the spring constant, which describes the force due to the spring: we're assuming this force is proportional to how much the spring is stretched from its equilibrium position, but points the other way.
- $F(t)$ is the externally applied force, e.g. if you push on the rock.

This equation is just Newton's second law, force equals mass times acceleration. The left side of the equation is mass times acceleration; the right side is the total force.

Now for the analogy. Everything here is analogous to something in an RLC circuit! An RLC circuit has current flowing around a loop of wire with 4 gizmos on it: a resistor, an inductor, a capacitor, and a voltage source — for example, a battery.



I won't say much about these gizmos. I just want to outline the analogy. The amount of current is analogous to the velocity of the rock, so let's call it $q'(t)$. The resistor acts to slow the current down, just as friction acts to slow down the rock. The inductor is analogous to the mass of the rock. The capacitor is analogous to the spring — but according to the usual conventions, a capacitor with a big "capacitance" acts like a weak spring. Finally, the voltage source is analogous to the external force.

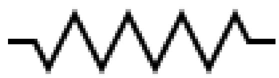
So, here's the equation that governs the RLC circuit:

$$Lq''(t) = -Rq'(t) - \frac{1}{C}q(t) + V(t)$$

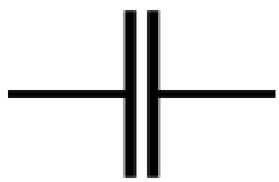
where

- L is the inductance of the inductor.

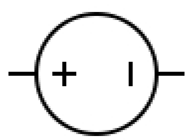
- R is the resistance of the resistor.



- C is the capacitance of the capacitor.



- V is the voltage of the voltage source.



As you can see, the equation governing the RLC circuit is the same as the one that governs a rock on a spring! True, $1/C$ plays the role of k, since a capacitor with a big capacitance acts like a spring with a small spring constant. But this is just a difference in conventions. The systems are isomorphic!

We could have fun solving the above equation and pondering what the solutions mean, but that would be the class I teach. Instead, I want to explain how this famous analogy between mechanics and electronics is just one of many analogies.

When I started thinking seriously about electrical circuits and category theory, I mentioned them my student Mike Stay, and he reminded me of the "hydraulic analogy" where you think of an electrical current flowing like water through pipes. There's a decent introduction to this here:

2) Wikipedia, "Hydraulic analogy", http://en.wikipedia.org/wiki/Hydraulic_analogy

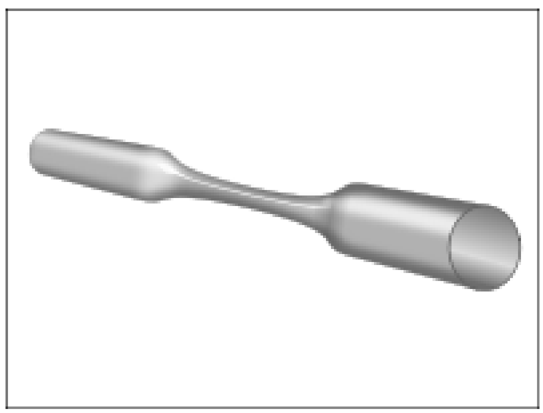
Apparently this analogy goes back to the early days when people were struggling to understand electricity, before electrons had been observed. The famous electrical engineer Oliver Heaviside pooh-poohed this analogy, calling it the “drain-pipe theory”. I think he was making fun of William Henry Preece. Preece was another electrical engineer, who liked the hydraulic analogy and disliked Heaviside’s fancy math. In his inaugural speech as president of the Institution of Electrical Engineers in 1893, Preece proclaimed:

> *True theory does not require the abstruse language of mathematics to make it clear and to render it acceptable. All that is solid and substantial in science and usefully applied in practice, have been made clear by relegating mathematic symbols to their proper store place — the study.*

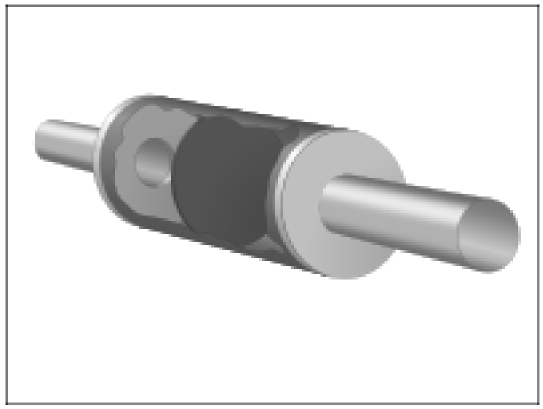
According to the judgement of history, Heaviside made more progress in understanding electromagnetism than Preece. But there’s still a nice analogy between electronics and hydraulics.

In this analogy, a pipe is like a wire. Water is like electrical charge. The flow of water plays the role of “current”. Water pressure plays the role of “voltage”.

A resistor is like a narrowed pipe:



An inductor is like a heavy turbine placed inside a pipe: this makes the water tend to keep flowing at the same rate it’s already flowing! In other words, it provides a kind of “inertia”, analogous to the mass of our rock. Finally, a capacitor is like a tank with pipes coming in from both ends, and a rubber sheet dividing it in two lengthwise:



When studying electrical circuits as a kid, I was shocked when I first learned that capacitors *don't let the electrons through*. Similarly, this gizmo doesn't let the water through.

Okay... by now you're probably wanting to have the analogies laid out more precisely. So that's what I'll do. But I'll throw in one more! I've been talking about the mechanics of a rock on a spring, where the motion of the rock up and down is called *translation*. But we can also study *rotation* in mechanics. And then we get these analogies:

	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Mechanics (translation)	position	velocity	momentum	force
Mechanics (rotation)	angle	angular velocity	angular momentum	torque
Electronics	charge	current	flux linkage	voltage
Hydraulics	volume	flow	pressure momentum	pressure

The top row lists 4 concepts from the theory of general systems, and my favorite symbols for them, where the dot stands for time derivative. The other rows list what these concepts are called in the subjects listed. So, "displacement" is the general concept which people call "position" in the mechanics of translation. Similarly, "flow" and "effort" correspond to "velocity" and "force", while "momentum" is just "momentum".

I found this chart here:

3) Dean C. Karnopp, Donald L. Margolis and Ronald C. Rosenberg, *System Dynamics: a Unified Approach*, Wiley, New York, 1990.

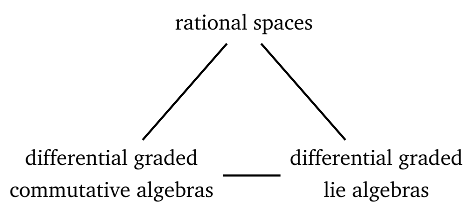
I discovered this wonderful book after an intensive search for stuff that makes the analogies between mechanics, electronics and hydraulics explicit. It turns out there's a whole theory devoted to precisely this! It's sometimes called "systems theory" or "network theory". Engineers use this theory to design and analyze systems made out of mechanical, electronic, and/or hydraulic components: springs, gears, levers, pulleys, pumps, pipes, motors, resistors, capacitors, inductors, transformers, amplifiers, and more!

Engineers often describe such systems using a notation called "bond graphs". Bond graphs are reminscent of Feynman diagrams... so they're simply *begging* to be understood as a branch of applied category theory. In fact, there's an interesting blend of category theory, symplectic geometry and complex analysis at work here. So in the Weeks to come, I'd like to tell you more about bond graphs and analogies between different kinds of systems.

(I'll warn you right now that Karnopp, like most experts on systems theory, use the symbols "f" and "e" for flow and effort, instead of $\dot{q}$ and $\dot{p}$. It's more or less impossible to find a unified notation for general systems that doesn't conflict with some existing notation used in the study of some *particular* kind of system. But since I want to get into symplectic geometry, I want to use some notation that reminds me of that — and for physicists, symplectic geometry is the study of "conjugate variables" like position q and momentum p.)

Okay... enough of this for now.

Last week I introduced the differential graded commutative algebra approach to rational homotopy theory. Next week I'll get into the differential graded Lie algebra approach, filling in another corner of the triangle here:



But I realized there's some important stuff I can tell you before we get to that!

Last time I told you how Sullivan defined "rational differential forms" for any topological space X:

First he converted this space into a simplicial set $\mathrm{Sing}(X)$.

Then he defined an algebra A of functions that are polynomials with rational coefficients on each simplex of $\mathrm{Sing}(X)$.

Then he defined his algebra $\Omega(A)$ of rational differential forms, using a general recipe that takes a commutative algebra A and spits out a differential graded commutative algebra.

But towards the end, I admitted that homotopy theorists feel perfectly fine about studying simplicial sets rather than topological spaces. The reason is that both the category of simplicial sets and the category of topological spaces are "model categories" — contexts where you can do homotopy theory. Moreover, these model categories are "Quillen equivalent" — the same in every way that matters for homotopy theory! Don't worry too much if you don't know about model categories and Quillen equivalence. The point is that we have a functor that converts spaces into simplicial sets:

$$\mathrm{Sing}\colon [\text{topological spaces}] \to [\text{simplicial sets}]$$

and its right adjoint going back the other way, called "geometric realization":

$$|-|\colon [\text{simplicial sets}] \to [\text{topological spaces}]$$

which we also saw last time. And, these let us freely switch viewpoints between topological spaces and simplicial sets.

So, while in "Week 286" I defined rational spaces to be specially nice *topological spaces*, I could equally well have defined them to be specially nice *simplicial sets*. Taking this viewpoint, we can forget about topological spaces, and think of Sullivan's innovation as a recipe for defining rational differential forms on a *simplicial set*.

This is a good idea. Among other things, it helps us see more simply what was so new about rational differential forms when Sullivan first discovered them.

What's new is that they give a functor that takes any simplicial set S and gives a differential graded algebra that's *commutative* and whose cohomology is the rational cohomology of S.

(By which I mean: the rational cohomology of the space $|S|$.)

You see, it's not so hard to achieve this if we drop our insistence that our differential graded algebra be *commutative*. This has been known for a long time. You start with your simplicial set S and define a "rational n-cochain" on it to be a function that eats n-simplices and spits out rational numbers. This gives a cochain complex

$$C^0(S) \xrightarrow{d} C^1(S) \xrightarrow{d} C^2(S) \xrightarrow{d} \dots$$

where $C^n(S)$ is the vector space of rational n-cochains. This cochain complex is usually called $C^*(S)$, where the star stands for the variable n. And there's a standard way to make $C^*(S)$ into a differential graded algebra, using a product called the "cup product". But, it's not a differential graded *commutative* algebra.

Instead, it's only graded commutative "up to chain homotopy". So, we don't have

$$v \smile w = (-1)^{pq} w \smile v$$

when v is in $C^p(S)$ and w is in $C^q(S)$. But, we do have

$$v \smile w = (-1)^{pq} w \smile v + da(v,w)$$

where $a(v,w)$ is something that depends on v and w. This is good enough to imply that when we take the cohomology of our cochain complex, we get a graded commutative algebra. This algebra is called $H^*(S)$, and the product in here is also called the cup product. You can read a lot about it in basic books on algebraic topology. Here's one that's free online:

4) Allen Hatcher, *Algebraic Topology*, Section 3.2: "Cup Product", available at `http://www.math.cornell.edu/~hatcher/AT/ATch3.pdf`

The memorably numbered Theorem 3.14 says the cup product is graded commutative.

So you might say: "So, who cares if the cup product of cochains is graded commutative merely up to chain homotopy? When we go to cohomology, that distinction washes away!"

Well, it turns out there can be lots interesting information in this chain homotopy $a(v,w)$. At least, this is true when we do cohomology using the integers or the integers modulo some prime as our coefficients — instead of rational numbers, as we've been doing.

In fact this chain homotopy is the tip of an enormous iceberg! For starters, it satisfies an interesting equation, but only up chain homotopy... and that chain homotopy satisfies an equation of its own, but only up to homotopy, and so on. So, we get a differential graded algebra that's graded commutative up to an infinite series of chain homotopies. Folks call this sort of gadget an "E_∞-algebra".

And when we work over the integers mod some prime, we can squeeze interesting information out of all these chain homotopies. They're called "Steenrod operations". You can use them to distinguish spaces that would be indistinguishable if you merely used their cohomology as a graded commutative algebra!

At least, that's what they say. I don't *personally* run around using Steenrod operations to distinguish weird spaces that shady characters pull out of their coat pockets on dark streetcorners. Some topologists actually do. But what fascinates me is the subtle distinction between equations that hold "on the nose" and equations that hold only up to homotopy, or up to isomorphism. Sometimes you can "strictify" a gadget where the equations hold only up to homotopy, and get them to hold on the nose. But sometimes you can't.

Once I was giving a talk about n-categories and Roger Penrose was in the audience. I said the most basic fact about n-categories was:

$$\cong \neq =$$

He raised his hand and asked:

$$\cong \cong = \ ?$$

Very good question! And the answer is: sometimes yes, sometimes no. This is where things get interesting!

So, it's a famous puzzle whether you can find some functorial way to turn a simplicial set S into a differential graded commutative algebra $A^*(S)$ whose cohomology is the usual cohomology $H^*(S)$. This is called the "commutative cochain problem".

I haven't said it precisely enough yet, since there's a cheap and easy way to solve the version I just stated: just $A^*(S)$ to be the cohomology $H^*(S)$ itself, with $d = 0$. What a dirty trick!

To rule out such tricks people demand various extra good properties. For example, the usual cochains $C^*(S)$ are "extendible": any cochain on a little simplicial set extends to a cochain on a bigger one. In other words, if

$$S \to T$$

is an inclusion of simplicial sets, then the corresponding map

$$C^*(T) \to C^*(S)$$

is onto. This is definitely not true if we replace C^* by H^*.

This paper gives a bit of history of the commutative cochains problem:

5) Bohumil Cenkl, "Cohomology operations from higher products in the de Rham complex", *Pacific J. Math.* **140** (1989), 21–33. Available at `http://projecteuclid.org/euclid.pjm/1102647247`

It gives a somewhat different statement of the problem, which alas I don't understand, and it proves that this version has no solution if we work over the integers. But over the rationals it *does*, if we take $A^*(S)$ to be the rational differential forms on our simplicial set S.

Just so you don't think this is pie-in-the-sky stuff, I should emphasize that this problem actually matters in electrical engineering, where we might triangulate spacetime and study discrete analogues of famous differential equations on the resulting simplicial complex! My friends Robert Kotiuga and Eric Forgy have thought about this a lot.

Here's a nice excerpt from the website of a conference at Boston University. I bet Robert Kotiuga wrote this. It mentions "Whitney forms", which are simplex-wise linear differential forms on a simplicial set. These are closely related to Sullivan's simplex-wise *polynomial* differential forms.

> *The analysis of electric circuits, using Kirchhoff's Laws, brought topology into electrical engineering over 150 years ago. Hermann Weyl's reformulation of Kirchhoff's laws in terms of homology over 80 years ago is an abstraction which is proving to be essential in the finite element analysis of three-dimensional electromagnetic fields. It enables computers to be programmed to identify an electrical circuit in an electromagnetic field problem — a task once considered the domain of the engineer's intuition. In "control theory" parlance, circuit theory equations are low frequency model reductions of distributed parameter electromagnetic systems, and homology theory yields the key mathematical tools for obtaining robust numerical algorithms. One aspect of the workshop will deal with large scale homology calculations and the realization of cycles representing generators of integral homology groups as embedded manifolds. The underlying homology calculations involve large sparse integer matrices with remarkable structure even when the underlying finite element meshes are "unstructured". One aim of the workshop is to bring together those performing large scale homology calculations in the context of dynamical systems and point cloud data analysis, with those requiring more geometrical applications of homology groups in electromagnetics.*
>
> *Over two decades ago, boundary value problems arising in the analysis of quasistatic electromagnetic fields were reinterpreted in terms of Hodge theory on manifolds with boundary. This observation is quite natural when Maxwell's equations are viewed in the context of differential forms and the problem of defining potentials is phrased in terms of de Rham cohomology. This observation, along with the variational formulation of Hodge theory on manifolds with boundary, created a revolution in the finite element analysis of electromagnetic fields. When phrased this way, the most difficult theoretical problems were actually solved in the 1950's by Andre Weil and Hassler Whitney who were concerned with problems in algebraic topology. They had an explicit interpolation formula for turning simplicial cochains into piecewise linear differential forms. This formula gives a chain homotopy between the algebraic complexes involved, and an isomorphism of cohomology rings. Although it took 30 years for Whitney forms to impact engineering practice, once the genie was out of the bottle, there was no way to put it back in. In the early 1990s, Whitney form techniques solved the problem of "spurious modes" appearing in electromagnetic cavity resonator*

> *calculations and soon after became widely accepted as an essential tool which is only recently being appreciated in the context of nanophotonics.*
>
> *It is important to re-examine this Whitney form revolution in the context of recent attempts to develop "discrete exterior calculus", "mimetic discretizations," "compatible discretizations" etc. For example, in algebraic topology it is well known that simplicial cochains do not admit a graded-commutative, associative product analogous to the wedge product on differential forms. This classical result, known as "the commutative cochain problem," is surprising and unintuitive in light of the fact that simplicial cochains admit a graded-commutative, associative product on the level of cohomology, analogous to the one induced by the wedge product in the de Rham complex. The bottom line is that these types of classical results are often ignored by newcomers trying to develop a discrete approach to calculus. Obviously, there is still some important technology transfer to be performed between algebraic topology and numerical analysis! Much of the mathematical work was done by Patodi, Dodziuk and Muller in the 1970's, has been exploited by electrical engineers, but has been largely ignored by applied mathematicians. Although the multiplicative structure on differential forms does not seem to be very important in the context of linear boundary value problems, it seems to play an important role in magnetohydrodynamics. Magnetohydrodynamics, in turn is an essential tool in space physics, in particular, in the growing field of space weather.*

So you see, everything is related. The difference between equality and isomorphism matters when you're trying to simulate the weather in space! That's the kind of thing that makes math so fun. Here's the conference webpage:

5) *Advanced Computational Electromagnetics 2006 (ACE '06)*, Boston University, `http://www.bu.edu/eng/ace2006/`

You can learn more here:

6) P. W. Gross and P. Robert Kotiuga, *Electromagnetic Theory and Computation: A Topological Approach*, Cambridge University Press, 2004.

Someday my discussion of electrical circuits may expand to include some algebraic topology. But all I want to explain now is the usual cup product on the cochains $C^*(S)$ for a simplicial set S. We'll need this in the Weeks to come!

Actually, it'll be easier if I work with chains instead of cochains. For us a chain complex will be a list of vector spaces and linear maps

$$C_0 \stackrel{d}{\leftarrow} C_1 \stackrel{d}{\leftarrow} C_2 \stackrel{d}{\leftarrow} \dots$$

with $d^2 = 0$. We call the whole thing C_*, where now the star is a subscript. I'll show you the usual way to get a chain complex $C_*(S)$ from a simplicial set, and then show you a way to *comultiply* chains. Then you can get the cochains by taking duals:

$$C^n(S) = C_n(S)^*$$

This will give a way to multiply chains.

Here's how it goes. The idea is that we define the comultiplication directly at the level of simplicial sets and then feed it through a couple of functors. There's a functor

$$F\colon \text{[simplicial sets]} \to \text{[simplicial vector spaces]}$$

and a functor

$$N\colon \text{[simplicial vector spaces]} \to \text{[chain complexes]}$$

Composing these will give the chains $C_*(S)$ for a simplicial set S.

The first functor

$$F\colon \text{[simplicial sets]} \to \text{[simplicial vector spaces]}$$

creates the free simplicial vector space on a simplicial set. This functor is symmetric monoidal: it carries products of simplicial spaces to tensor products of simplicial vector spaces The second functor

$$N\colon \text{[simplicial vector spaces]} \to \text{[chain complexes]}$$

is called the "normalized chain complex" or "normalized Moore complex" functor. This functor is an equivalence of categories! It's *almost* symmetric monoidal, but not quite, and this is where all the subtlety lies.

The category of simplicial sets has finite products. So, every simplicial set has a diagonal map:

$$\Delta\colon S \to S \times S$$

It also has a unique map to the simplicial set called 1, which consists of single 0-simplex:

$$\varepsilon\colon S \to 1$$

These two maps satisfy the usual axioms of a commutative monoid, written out as commutative diagrams, except with the arrows pointing backwards. So, S is a "cocommutative comonoid" in the category of simplicial sets.

Indeed, whenever you have any category with finite products, every object in it becomes a cocommutative comonoid — and in a unique way!

So far, this is a completely bland fact of life. If we feed our simplicial set S through the functor

$$F\colon \text{[simplicial sets]} \to \text{[simplicial vector spaces]}$$

what happens? Well, because this functor is monoidal it sends comonoids to comonoids. And because it's *symmetric* monoidal, it sends *cocommutative* comonoids to *cocommutative* comonoids. And a cocommutative comonoid in simplicial vector spaces is the same as a "simplicial cocommutative coalgebra".

(I love this kind of stuff, but not everyone does: that's why I save it for the very end of each Week's Finds.)

So, we've turned our simplicial set S into a simplicial cocommutative coalgebra $F(S)$. Now feed this gizmo into the next functor:

$$N\colon \text{[simplicial vector spaces]} \to \text{[chain complexes]}$$

By definition, we get the chains on S:

$$N(F(S)) = C_*(S)$$

And thanks to the wonders of functoriality, these chains are blessed with a comultiplication

$$C_*(\Delta)\colon C_*(S) \to C_*(S) \otimes C_*(S)$$

and counit

$$C_*(S) \to \mathbb{Q}$$

where $\mathbb{Q}$ is our ground field, the rationals.

And *if* the functor N were also symmetric monoidal, $C_*(S)$ would also be a cocommutative comonoid, but now in the world of chain complexes. In other words, it would be a "differential graded cocommutative coalgebra". Then, taking duals, the cochains $C^*(S)$ would be a DGCA!

But I warned you: things aren't quite so simple.

I said the functor N is *almost* a symmetric monoidal functor. But not quite.

For starters, it's a "lax monoidal functor", which implies among other things that there's a natural transformation

$$EZ\colon N(X) \otimes N(Y) \to N(X \otimes Y)$$

This is called the Eilenberg-Zilber map. The word "lax" means that this map isn't necessarily an isomorphism. A lax monoidal functor is good enough to send monoids to monoids. That's important — but it's no use to us now, since we've got a comonoid on our hands!

On the other hand, N is also an "oplax monoidal functor", which implies among other things that there's a natural transformation going the other way

$$AW\colon N(X \otimes Y) \to N(X) \otimes N(Y)$$

This is called the Alexander-Whitney map. The word "oplax" means that this map isn't necessarily an isomorphism — but now it's going the opposite way. An oplax monoidal functor is good enough to send comonoids to comonoids. Yay!

So, our cochains $C_*(S)$ do indeed form a comonoid in the world of chain complexes — that is, "differential graded coalgebra".

However, it's not cococommutative!

To dig deeper into this, I'd need to draw lots of pictures or write lots of formulas, and I don't feel in the mood for that. So, I'll just say what I hope you're thinking: the passage from simplicial vector spaces to chain complexes is quite tricky.

For example, it's sort of frustrating that we have these EZ and AW maps going both ways, but they're not inverses! In fact they come very close. Eilenberg-Zilber followed by Alexander-Whitney is the identity on $N(X) \otimes N(Y)$. Alas, Alexander-Whitney followed by Eilenberg-Zilber is not the identity on $N(X \otimes Y)$. But, it's chain homotopic to the identity!

You can read more about the "normalized Moore complex" functor here:

7) nLab, "Moore complex", available at `http://ncatlab.org/nlab/show/Moore+complex`

The fact that it's an equivalence of categories is called the "Dold-Kan correspondence". You can read more about this here:

8) nLab, "Dold-Kan correspondence", available at `http://ncatlab.org/nlab/show/Dold-Kan+correspondence`

And I should point out that while I've been working with vector spaces over the rational numbers, everything I've said about the functors F and N generalize to R-modules for an arbitrary commutative ring R. So, we have

$$F\colon [\text{simplicial sets}] \to [\text{simplicial } R\text{-modules}]$$
$$N\colon [\text{simplicial } R\text{-modules}] \to [\text{chain complexes of } R\text{-modules}]$$

with all the good (or frustrating) properties that I just described. The nLab pages focus somewhat on the case where $R = \mathbb{Z}$, where we get

$$F\colon [\text{simplicial sets}] \to [\text{simplicial abelian groups}]$$
$$N\colon [\text{simplicial abelian groups}] \to [\text{chain complexes of abelian groups}]$$

This is indeed the most fundamental case.

Finally, what about that picture at the beginning? If you're smirking just because you can guess what *planet* it was taken on, wipe that smile off your face! If I showed you a picture of a city and asked you where it is, would you say "Earth"?

These pictures show linear dunes on the north polar region of Mars: latitude 78 degrees north, longitude 209 degrees east.

(Hmm, on Earth there was a big battle between the British and French to say what longitude counts as "0 degrees". How did it work on Mars?)

Anyway, according to Maria Banks:

> *This observation shows linear dunes in the north polar region of Mars. Linear or longitudinal sand dunes are elongated, sharp crested ridges that are typically separated by a sand-free surrounding surface.*
>
> *These features form from bi-directional winds and extend parallel to the net wind direction. In this case, the net wind direction appears to be from the west-southwest. Linear sand dunes are found in many different locations on Earth and commonly occur in long parallel chains with regular spacing.*
>
> *Superimposed on the surface of the linear dunes are smaller secondary dunes or ripples. This is commonly observed on terrestrial dunes of this size as well. Polygons formed by networks of cracks cover the substrate between the linear dunes and may indicate that ice-rich permafrost (permanently frozen ground) is present or has been present geologically recently in this location.*

9) HiRISE (High Resolution Imaging Science Experiments), "Linear dunes in the north polar region", `http://hirise.lpl.arizona.edu/PSP_009739_2580`

Addenda: I thank Richard Lozes and Jonathan vos Post for catching typos. Jesse McKeown pointed out a NASA website that addresses my puzzle about how people settled on a definition of longitude on Mars:

10) NASA, "The Martian Prime Meridian — longitude zero", http://www.nasaimages.org/luna/servlet/detail/nasaNAS_44~16934~120671:The-Martian-Prime-Meridian----Longi

On Earth, the longitude of the Royal Observatory in Greenwich, England is defined as the "prime meridian," or the zero point of longitude. Locations on Earth are measured in degrees east or west from this position. The prime meridian was defined by international agreement in 1884 as the position of the large "transit circle," a telescope in the Observatory's Meridian Building. The transit circle was built by Sir George Biddell Airy, the 7th Astronomer Royal, in 1850. (While visual observations with transits were the basis of navigation until the space age, it is interesting to note that the current definition of the prime meridian is in reference to orbiting satellites and Very Long Baseline Interferometry (VLBI) measurements of distant radio sources such as quasars. This "International Reference Meridian" is now about 100 meters east of the Airy Transit at Greenwich.) For Mars, the prime meridian was first defined by the German astronomers W. Beer and J. H. Mdler in 1830–32. They used a small circular feature, which they designated "a," as a reference point to determine the rotation period of the planet. The Italian astronomer G. V. Schiaparelli, in his 1877 map of Mars, used this feature as the zero point of longitude. It was subsequently named Sinus Meridiani ("Middle Bay") by Camille Flammarion. When Mariner 9 mapped the planet at about 1 kilometer (0.62 mile) resolution in 1972, an extensive "control net" of locations was computed by Merton Davies of the RAND Corporation. Davies designated a 0.5-kilometer-wide crater (0.3 miles wide), subsequently named "Airy-0" (within the large crater Airy in Sinus Meridiani) as the longitude zero point. (Airy, of course, was named to commemorate the builder of the Greenwich transit.) This crater was imaged once by Mariner 9 (the 3rd picture taken on its 533rd orbit, 533B03) and once by the Viking 1 orbiter in 1978 (the 46th image on that spacecraft's 746th orbit, 746A46), and these two images were the basis of the Martian longitude system for the rest of the 20th Century. The Mars Global Surveyor (MGS) Mars Orbiter Camera (MOC) has attempted to take a picture of Airy-0 on every close overflight since the beginning of the MGS mapping mission. It is a measure of the difficulty of hitting such a small target that nine attempts were required, since the spacecraft did not pass directly over Airy-0 until almost the end of the MGS primary mission, on orbit 8280 (January 13, 2001). In the left figure above, the outlines of the Mariner 9, Viking, and Mars Global Surveyor images are shown on a MOC wide angle context image, M23-00924. In the right figure, sections of each of the three images showing the crater Airy-0 are presented. A is a piece of the Mariner 9 image, B is from the Viking image, and C is from the MGS image. Airy-0 is the larger crater toward the top-center in each frame. The MOC observations of Airy-0 not only provide a detailed geological close-up of this historic reference feature, they will be used to improve our knowledge of the locations of all features on Mars, which will in turn enable more precise landings on the Red Planet by future spacecraft and explorers.

For more discussion, visit the friendly and welcoming n-Category Caf.

If you haven't found something strange during the day, it hasn't been much of a day.

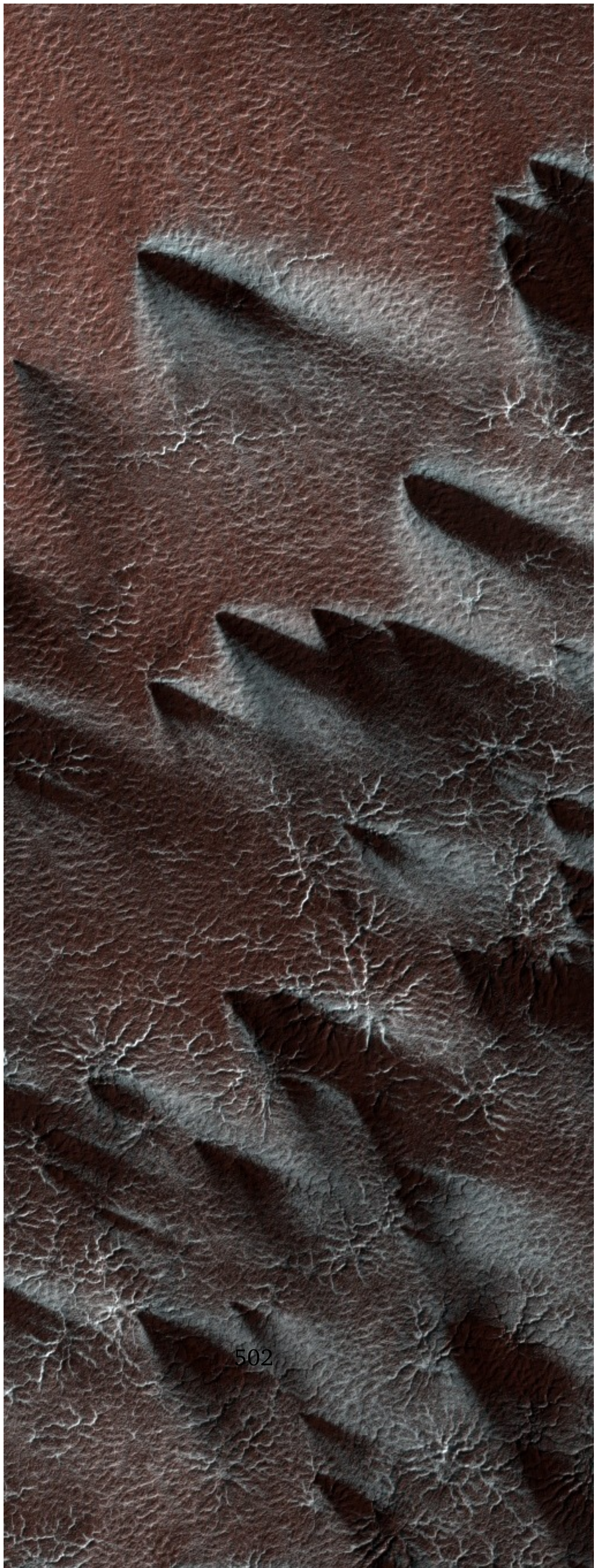
— *John Wheeler*

Week 289

January 8, 2010

This week I'll tell you some news about E_8. Then I'll continue expanding the grand analogy between different kinds of physics. We'll get into a bit of thermodynamics — and chemistry too! And then we'll continue our exploration of rational homotopy theory, this time entering the world of "differential graded Lie algebras".

But first: what's going on here?



As usual, the answer is at the end.

Now for the news:

Hurrah! Yippee-yay! They've discovered the exceptional group E_8 in nature! And they've found the golden ratio lurking in a quantum system! At least, that's what the headlines are blaring:

1) Joe Palca, "Finding 'beautiful' symmetry near absolute zero", *All Things Considered*, National Public Radio, `http://www.npr.org/templates/story/story.php?storyId=122365883`

Here's the paper that started all the fuss:

2) R. Coldea, D. A. Tennant, E. M. Wheeler, E. Wawrzynska, D. Prabhakaran, M. Telling, K. Habicht, P. Smeibidl and K. Kiefer, "Quantum criticality in an Ising chain: experimental evidence for emergent E_8 symmetry", *Science* **327**, 177–180. Available at `http://www.sciencemag.org/cgi/content/abstract/327/5962/177`

Unfortunately you can only see the paper if you subscribe to Science or have magical superpowers. And I don't understand the details, which apparently go back to a theoretical paper by the guy who invented the "Zamolodchikov tetrahedron equation", much beloved by people who study knotted surfaces in 4 dimensions:

3) A. B. Zamolodchikov, "Integrals of motion and S-matrix of the (scaled) $T = T_c$ Ising model with magnetic field", *Int. J. Mod. Phys. A* **4** (1989), 4235.

But the rough idea is this. You can simulate a 1-dimensional magnet using a chemical called cobalt niobate. The molecules form a chain, and each one has a spin, and the spins want to line up: either all pointing up, or all pointing down. And at zero temperature, that's what they'd do.

At any slightly higher temperature, random fluctuations spoil things. This is different than in higher dimensions, where a single atom whose spin points the wrong way will be surrounded by conformists on all sides and eventually fall into line, as long as it's cool enough. In one dimension, once one spin flips, its neighbors don't know which side to go with: the guy on their left or the guy on the right. Rebellious provinces of flipped spins can grow every larger without an ever-lengthening frontier that takes a lot of energy to maintain!

But at temperatures close to zero, we'll see big stretches of spins pointing the same way, like this:

$$\downarrow\uparrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow$$

The points at which the spin flips can move around like particles. There are two kinds of particles like this, which we could call "kinks":

$$\downarrow\uparrow$$

and "antikinks":

$$\uparrow\downarrow$$

When a kink meets an antikink, they can annihilate each other. In the following picture, each line is a moment of time; as time proceeds we march down the page. We see a kink moving right and an antikink moving left. When they collide, they annihilate!

$$\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\uparrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow\downarrow$$
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$$\downarrow$$

Kinks and antikinks can also be created in pairs.

So far this is pretty simple. But next thing this experiment did is turn on a "transverse magnetic field" — in other words, a field at *right angles* to the direction I'm drawing as vertical. If the field is strong enough, most of the spins will line up in this new direction:

$$\rightarrow$$

But at nonzero temperature, random thermal fluctuations will make a few spins flip and point the wrong way. And these wrongheaded spins can again move around like particles:

$$\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\leftarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow$$
$$\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\leftarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow$$
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$$\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\leftarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow\rightarrow$$

So, this is what we see if the transverse magnetic field is strong enough. On the other hand, if the field is small, it won't have much effect. But right in between, at some critical value, some very complicated things can happen. This is called a "critical point".

The people doing the experiment found this critical point. And then they did something really tricky. They turned on an *extra* magnetic field pointing in the direction that I'm drawing as *vertical*. Under these conditions, Zamolodchikov claimed that something amazing should happen. Namely: a kink and an antikink should be able to stick together in 8 different ways!

When two or more particles stick together and form a new one, we call the result a "bound state". So, we should get 8 different kinds of bound states, which can zip along our spin chain like particles! I wish I could draw them, but I don't know how.

If you know particle physics, these bound states should remind you a bit of mesons. A meson is a bound state made of a quark and an antiquark. In fact there are even 8 kinds of mesons made from up, down and strange quarks and their corresponding antiquarks. That's why Gell-Mann called his theory of quarks the "Eightfold Way" when he came up with it back around 1961. In this theory, the number 8 shows up because the relevant symmetry group, called $\mathrm{SU}(3)$, is 8-dimensional.

However, the math surrounding these 8 kink-antikink bound states is a lot more sophisticated. It's related to the exceptional Lie group E_8, which is 248-dimensional!

Well, actually it's not quite that bad. What really matters in this game is not the group E_8 but rather its "root lattice", which is just 8-dimensional. This is the pattern you get when you pack equal-sized balls in 8 dimensions in the unique way such that each ball touches the maximum number of others — namely, 248. I sort of understand this pattern, and I explained it in back in "Week 193". But I don't understand why it shows up when you're studying a chain of spins in a magnetic field!

What did the experiment actually measure? Among other things, they measured the ratio of masses of two of the 8 "particles" formed as kink-antikink bound states — namely, the lightest two. According to Zamolodchikov's calculation, it should be the golden ratio! You know:

$$\Phi = \frac{1 + \sqrt{5}}{2}$$

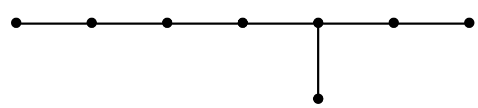
And that's what the experiment saw!

Now, I know plenty of relationships between E_8, and the golden ratio — see "Week 270" — so this connection doesn't shock me. But I sure hope someone explains the details!

If you're an amateur looking for a few clues, I suggest starting with this very readable expository paper:

4) Paul A. Pearce, "Phase transitions, critical phenomena and exactly solvable lattice models". Also available at `http://mac0916.ms.unimelb.edu.au/~pap/Publications_pdf/1997Pearce_VicRoyalSociety.pdf`.

You'll notice he talks about various models of *2-dimensional* magnets, leading up to a model that involves something called the E_8 Dynkin diagram:



which encodes the structure of the E_8 lattice. These 2-dimensional magnets are related to the 1-dimensional magnetic "spin chain" we've been discussing — but in a sneaky way. The 1-dimensional spin chain is 2-dimensional if we think of time as an extra dimension! Indeed I've already been drawing some 2-dimensional pictures, like the picture of a kink colliding with an antikink. So, that's part of the story.

But the story is much deeper — and for this, I really must thank Will Orrick, who also caught some mistakes in an earlier version of my story here! Orrick is a mathematician at Indiana University who works on statistical mechanics and combinatorics.

For starters, at a critical point, a 2-dimensional magnet is related to a kind of quantum field theory called a "conformal field theory". And the particular conformal theory this experiment is studying is a so-called "minimal model" by the name of $M(3,4)$. This conformal field theory can be built from E_8 using something called a "coset construction" — but it does not have 8 bound states. To see those, we really need to turn on that *extra* magnetic field I mentioned: the field pointing in the vertical direction. This gives an "integrable massive perturbation" of the conformal field theory. That's what we need to understand to see those 8 bound states, and compute their masses.

If you know nothing of conformal field theory, minimal models and the coset construction, it can't hurt to look at my outline of Di Francesco, Mathieu, and Senechal's book in "Week 124". To dig a bit deeper, try:

5) Scholarpedia, "A-D-E classification of conformal field theories", `http://www.scholarpedia.org/article/Cappelli-Itzykson-Zuber_A-D-E_Classification`

And for more details on how the extra magnetic field creates those 8 bound states, try these:

6) Giuseppe Mussardo, "Off-critical statistical models: factorized scattering theories and bootstrap program", *Physics Reports* **218** (1992), 215–379.

7) Giuseppe Mussardo, *Statistical Field Theory*, Oxford, 2010.

Okay, back down to earth. Last week I began to sketch an analogy between various kinds of physical systems, based on general concepts of "displacement" and "momentum", and their time derivatives: "flow" and "effort":

	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Mechanics (translation)	position	velocity	momentum	force
Mechanics (rotation)	angle	angular velocity	angular momentum	torque
Electronics	charge	current	flux linkage	voltage
Hydraulics	volume	flow	pressure momentum	pressure

Today I want to make this chart even bigger! There are more systems that fit into this collection of analogies.

For a really good analogy, we want "effort" times "flow" to have dimensions of power — that is, energy per time. Indeed, we want it to be true that:

- pq has dimensions of action (= energy $\times$ time)
- $\dot{p}q$ has dimensions of energy
- $p\dot{q}$ has dimensions of energy
- $\dot{p}\dot{q}$ has dimensions of power (= energy / time)

If any one of these is true, they're all true. And they're true in the four examples I've listed so far.

For example, suppose we have a circuit with one wire coming in and one going out, and a complicated black box in the middle. Then at any given time, the power it takes to run this circuit equals the voltage across the circuit times the current flowing through it. That's effort times flow.

Note the wording here. Engineers say that voltage is an "across" variable, while current is a "through" variable.

I hope the idea of current flowing "through" a circuit is reasonably intuitive: think of water flowing through a pipe. But the idea of voltage "across" a circuit may be a bit less intuitive. Crudely speaking, at any point of spacetime there's a number called the "voltage". And at any given time, the voltage "across" our circuit is the voltage on the wire coming in, minus the voltage on the wire coming out.

To be a bit less crude, it's important to note that only *differences* between voltages are measurable:

4) John Baez, "Torsors made easy", `http://math.ucr.edu/home/baez/torsors.html`

But the voltage across a circuit is precisely such a difference.

Anyway, what are some other examples of physical systems where we have a notion of "effort" and a notion of "flow", such that effort times flow equals power?

Here are two:

	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Thermodynamics	entropy	entropy flow	temperature momentum	temperature
Chemistry	moles	molar flow	chemical momentum	chemical potential

I made up the phrases "temperature momentum" and "chemical momentum" since these quantities don't have standard names, as far as I know. But that's not so important. What really matters is that we've brought two more subjects into our circle of analogies.

The example of thermodynamics works like this. Say you have a physical system in thermal equilibrium and all you can do is heat it up or cool it down "reversibly" — that is, while keeping it in thermal equilibrium all along. For example, imagine a box of gas that you can heat up or cool down. If you put a tiny amount dE of energy into the system in the form of heat, then its entropy increases by a tiny amount dS. And it works like this:

$$dE = TdS$$

where T is the temperature.

Another way to say this is

$$\frac{dE}{dt} = T\frac{dS}{dt}$$

where t is time. On the left we have the power put into the system in the form of heat. But since power should be "effort" times "flow", on the right we should have "effort" times "flow". It makes some sense to call dS/dt the "entropy flow". So temperature, T, must play the role of "effort".

This is a bit weird. I don't usually think of temperature as a form of "effort" analogous to force or torque. Stranger still, our analogy says that "effort" should be the time derivative of some kind of "momentum". So, we need to introduce "temperature momentum": namely, the integral of temperature over time. I've never seen people talk about this concept, so it makes me nervous.

But when we have a more complicated physical system like a piston full of gas in thermal equilibrium, we can see the analogy working. Now we have

$$dE = TdS - PdV$$

The change in energy dE of our gas now has two parts. There's the change in heat energy TdS, which we saw already. But now there's also the change in energy due to compressing the piston! When we change the volume of the gas by a tiny amount dV, we put in energy $-PdV$.

Now look back at the first chart I drew! It says that pressure is a form of "effort", while volume is a form of "displacement". If you believe that, the equation above should help convince you that temperature is also a form of "effort", while entropy is a form of "displacement".

But what about the minus sign? That's no big deal: it's the result of some arbitrary conventions. P is defined to be the *outwards* pressure of the gas on our piston. If this is *positive*, *reducing* the volume of the gas takes a *positive* amount of energy — so we need to stick in a minus sign. I could eliminate this minus sign by changing some conventions — but if I did, the chemistry professors at UCR would haul me away and increase my heat energy by burning me at the stake.

Speaking of chemistry: here's how we can extend our table of analogies to include chemistry! Suppose we have a piston full of gas made of different kinds of molecules, and there can be chemical reactions that change one kind into another. Now our equation gets fancier:

$$dE = TdS - PdV + \sum_i \mu_i dN_i$$

Here N_i is the number of molecules of the ith kind, while μ_i is a quantity called a "chemical potential". The chemical potential simply says how much energy it takes to increase the number of molecules of a given kind:

8) Wikipedia, "Chemical potential", `http://en.wikipedia.org/wiki/Chemical_potential`

So, we see that "chemical potential" is another form of "effort", while "number of molecules" is another form of "displacement".

Chemists are too busy to count molecules one at a time, so they count them in big bunches called "moles". A mole is the number of atoms in 12 grams of carbon-12. That's roughly

$$602,214,150,000,000,000,000,000$$

atoms. This is called Avogadro's number.

So, instead of saying that the displacement in chemistry is called "number of molecules", you'll sound more like an expert if you say "moles". And the corresponding flow is called "molar flow". I don't know a name for the thing whose time derivative is chemical potential, so let's call it "chemical momentum".

For more on this, try the following book on network theory:

9) Francois E. Cellier, *Continuous System Modelling*, Chap. 9: "Modeling chemical reaction kinetics", Springer, Berlin, 1991.

So, we've added two more items to our list of analogies: thermodynamics and chemistry. But, we've seen that they're intimately interlinked.

There are also weaker analogies to subjects where effort times flow doesn't have dimensions of power. The two most popular are these:

	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Heat Flow	heat	heat flow	temperature momentum	temperature
Economics	inventory	flow of product	economic momentum	price of product

The heat flow analogy comes up because people like to think of heat flow as analogous to electrical current, and temperature as analogous to voltage. Why? Because an insulated wall acts a bit like a resistor! The current flowing through a resistor is a function the voltage across it. Similarly, the heat flowing through an insulated wall is about proportional to the difference in temperature between the inside and the outside.

However, at least according to most engineers, there's a big difference. Current times voltage has dimensions of power, which is what we want. Heat flow times temperature does not have dimensions of power. In fact, heat flow by itself already has dimensions of power! So, engineers feel somewhat guilty about this analogy.

Being a mathematical physicist, a possible way out presents itself to me: use units where temperature is dimensionless! In fact such units are pretty popular in some circles. But I don't know if this solution is a real one, or whether it causes some sort of trouble.

In the economic example, "energy" has been replaced by "money". So other words, "inventory" times "price of product" has units of money. And so does "flow of product" times "economic momentum"! I'd never heard of "economic momentum" before, and I have absolutely no intuition for it, but I didn't make it up. It's the thing whose time derivative is "price of product".

I'm suspicious of any attempt to make economics seem like physics. Unlike elementary particles or rocks, people don't seem to be very well modelled by simple differential equations. However, some economists have used the above analogy to model economic systems. And I can't help but find that interesting — even if intellectually dubious when taken too seriously.

Now... what can we do with all these analogies? I'll explain that in detail in the Weeks to come. But maybe you want a quick answer now.

First of all, engineers use these analogies to systematically model all sorts of gadgets using "bond graphs". Bond graphs were invented by an engineer named Henry Paynter. His original book goes way back to 1961:

10) Henry M. Paynter, *Analysis and Design of Engineering Systems*, MIT Press, Cambridge, Massachusetts, 1961.

I haven't gotten ahold of this book yet, but I've learned a bit about Paynter. He got a bachelor's degree in civil engineering, a master's in mathematics, and then a doctorate in hydroelectric engineering, all from MIT. He then became a professor at MIT and taught there until he retired in 1985. I can easily imagine that this diverse background made him the perfect guy to unify lots of different subjects.

I want to explain bond graphs, how they differ from circuit diagrams, and how they're both examples of "string diagrams" in category theory. But it will take me a while to get there — since while abstract generalities are always fun, this is a great opportunity to talk about lots of basic physics.

In particular, you'll note how all these analogies rely on a pair of variables q and p: displacement and momentum. In classical mechanics we call these "conjugate variables". The importance of such pairs is explained in the "Hamiltonian" approach to classical mechanics, which in turn leads to a branch of math called "symplectic geometry". So, I should try to explain a bit of that, though probably just the basics.

One more thing. If you've studied your physics, you've seen how "Legendre transforms" show up in both classical mechanics and thermodynamics. The Legendre transform lets you start with a function of q and $\dot{q}$ and turn it into a function of q and p.

Mathematically, the idea is that given a function on the tangent bundle of a manifold:

$$f\colon TM \to \mathbb{R}$$

you get a map from the tangent bundle to the cotangent bundle:

$$\lambda\colon TM \to T^*M$$

which records the derivative of f in the "vertical directions". In nice cases, this map λ is one-to-one and onto.

In classical mechanics, this lets us pass from the "Lagrangian" formalism, where everything is a function of position and velocity, to the "Hamiltonian" formalism, where everything is a function of position and momentum. The idea is that position and velocity $(q, \dot{q})$ are represented by a point in TM, while position and momentum (q, p) are represented by a point in T^*M. In our discussion of analogies so far, we considering the simplest case, where M is the real line. That's why I've been treating q, p, $\dot{q}$ and $\dot{p}$ as mere *numbers* that depend on time. But it's good to generalize to an arbitrary manifold M.

For an elementary yet insightful introduction to the physics of Legendre transforms, try this:

11) R. K. P. Zia, Edward F. Redish and Susan R. McKay, "Making sense of the Legendre transform", available as arXiv:0806.1147.

I've spent decades thinking about the Legendre transform in the context of classical mechanics, but not so much in thermodynamics. I think its appearance in both subjects should be a big part of the analogy I'm talking about here. But if anyone knows a clear, detailed treatment of the analogy between classical mechanics and thermodynamics, focusing on the Legendre transform, please let me know!

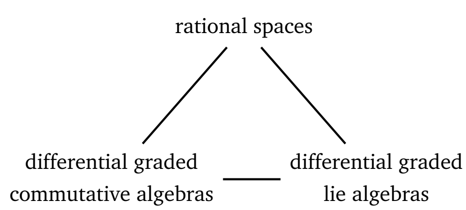
The above article helps a bit. But it seems to be using a slightly different analogy than the one I was just explaining... so my confusion is not eliminated.

I'm also curious about lots of other things. For example: in classical mechanics it's really important that we can define "Poisson brackets" of smooth real-valued functions on the cotangent bundle. So: how about in thermodynamics? Does anyone talk about the Poisson bracket of temperature and entropy, for example?

And Poisson brackets are related to quantization — see "Week 282" for more on that. So: does anyone try to quantize thermodynamics by taking seriously the analogies I've described? I'm not sure it makes physical sense, but it seems mathematically possible.

These are just a few of the strange ways you can try to extend the analogies I've listed.

Anyway, stay tuned for more on this. But for now, let me turn to a different story: rational homotopy theory!



Last time I explained how we can turn a rational space into a differential graded commutative algebra, or DGCA. Now I want to tell you how to turn a rational space into a differential graded Lie algebra, or DGLA.

But first: why should we care?

A differential graded Lie algebra is a generalization of a Lie algebra. Usually we get Lie algebras from Lie groups. But now we'll get one of these generalized Lie algebra from any rational space.

So, we're massively generalizing Lie theory!

This should seem odd at first. It's easy to imagine generalizing Lie theory from Lie groups to other groups, like "infinite-dimensional Lie groups". But how can we generalize it to *spaces*?

The answer is this: there's a way to turn any pointed space X into a topological group called $\Omega(X)$. Roughly, this is the group of "based loops" in X: maps from an interval into X that start and end at the basepoint. There are some technicalities involved in getting an honest group this way. We'll talk about them later. But *roughly*, the idea is that we multiply two loops by forming a new loop that runs first along one and then the other. And *roughly*, the inverse of a loop is the same loop run backwards.

So here's the plan. We're going to generalize Lie theory from Lie groups to topological groups. Just as a Lie group has a Lie algebra, any topological group will have a "differential graded Lie algebra". Whenever we have a pointed space X, we can turn it into a topological group $\Omega(X)$, and then apply this construction.

And when X is a *rational* space, the resulting differential graded algebra will know *everything* about X!

Well, I shouldn't get carried away in my enthusiasm. The differential graded Lie algebra will only know everything about the "homotopy type" of X — a concept I defined last week. But that's still amazing. It means that at least for rational spaces, we can reduce homotopy theory to a souped-up version of the theory of Lie algebras.

It's like a dream come true: reducing a largish chunk of homotopy theory to linear algebra!

But now let's see how it works.

First of all, what's a differential graded Lie algebra? It's a Lie algebra in the world of chain complexes. A "chain complex", for us, will be a list of vector spaces and linear maps

$$C_0 \stackrel{d}{\leftarrow} C_1 \stackrel{d}{\leftarrow} C_2 \stackrel{d}{\leftarrow} \dots$$

with $d^2 = 0$. And a vector space, for us, will be vector space over the rationals.

Just as you can tensor vector spaces, you can tensor chain complexes. And just as you can define a Lie algebra to be a vector space V with a bracket operation

$$[-,-] \colon V \otimes V \to V$$

satisfying antisymmetry and the Jacobi identity, so you can define a "differential graded Lie algebra" to be a chain complex C with a bracket operation

$$[-,-] \colon C \otimes C \to C$$

satisfying graded antisymmetry and the graded Jacobi identity. By "graded", I mean you need to remember to put in a sign $(-1)^{jk}$ whenever you switch a guy in C_j and a guy in C_k.

Differential graded Lie algebras are often called DGLAs for short. A DGLA where only C_0 is nonzero is just a plain old Lie algebra. So, DGLAs really are a generalization of Lie algebras. Whenever anyone tells you something about DGLAs, you should check to see what it says about Lie algebras.

Next let me tell you how to turn our rational homotopy type X into a DGLA. I'll quickly sketch this process, which consists of 3 steps, and then go over the steps more slowly. Don't get scared if none of them make sense yet:

- Let $\Omega(X)$ the space of based loops in X. You should think of this as a topological group, with the group operation being concatenation of loops.
- Let $C_*(\Omega(X))$ be the chain complex of singular chains on $\Omega(X)$ taking values in the rational numbers. This is a differential graded cocommutative Hopf algebra, or "DGCHA" for short.
- Let $P(C_*(\Omega(X)))$ consist of the "primitive" elements of our DGCHA. This is a differential graded Lie algebra, or DGLA!

Each step is interesting in itself. And each step is actually a functor. So I need to explain 3 different functors:

$$\begin{aligned} \Omega\colon & \text{[path-connected pointed spaces]} \to \text{[topological groups]} \\ C_*\colon & \text{[topological groups]} \to \text{[DGCHAs]} \\ P\colon & \text{[DGCHAs]} \to \text{[DGLAs]} \end{aligned}$$

One thing that excites me about this subject is getting to know the last two functors. I've been in love with the first one for years, and also the functor going back:

$$B\colon \text{[topological groups]} \to \text{[path-connected pointed spaces]}$$

which sends any topological group G to its "classifying space" BG.

Indeed, it was a life-changing experience to realize that as far as homotopy theory goes, pointed path-connected spaces are just the same as topological groups, thanks to these functors going back and forth. Both these things seemed fundamental and fascinating: spaces and symmetry groups! To realize they were "the same" was mindblowing.

It's the next two steps that are exciting me now. Let me try to explain what simpler, perhaps more familiar constructions they generalize.

If you have a plain old group G, it has a "group algebra" $\mathbb{Q}[G]$ consisting of formal rational linear combinations of elements of G. Its multiplication comes from the multiplication in G. But it's better than an algebra: it's a "cocommutative Hopf algebra". This means it has a bunch of extra operations that completely encode the group structure on G.

For example, in a Hopf algebra you can "comultiply" as well as multiply. In the group algebra $\mathbb{Q}[G]$, the comultiplication map

$$\Delta\colon \mathbb{Q}[G] \to \mathbb{Q}[G] \otimes \mathbb{Q}[G]$$

is defined on elements g of G by the equation

$$\Delta(g) = g \otimes g$$

We say a Hopf algebra is "cocommutative" if comultiplying is the same as comultiplying and then switching the two outputs. You can see that's true here.

A Hopf algebra also has a "counit" as well as a unit, and the counit in a group algebra is a map

$$\varepsilon \colon \mathbb{Q}[G] \to \mathbb{Q}$$

defined by

$$\varepsilon(g) = 1$$

In fact, given any cocommutative Hopf algebra, the elements satisfying both of the above two equations form a group! These elements are called "grouplike elements". If we take the grouplike elements of $\mathbb{Q}[G]$, we get the group G back.

The functor

$$C \colon \text{[topological groups]} \to \text{[DGCHAs]}$$

generalizes this idea from groups to topological groups. Instead of just taking formal linear combinations of *elements* of G, we now take formal linear combinations of *simplices* in G. The 0-simplices in G are just elements of G. But the higher-dimensional simplices keep track of the topology of G.

Now let's turn to the next functor:

$$P \colon \text{[DGCHAs]} \to \text{[DGLAs]}$$

This generalizes a simpler procedure that takes cocommutative Hopf algebras and gives Lie algebras.

To understand this, it's best to think about the reverse procedure first. If you have a plain old Lie algebra L, it has a "universal enveloping" algebra UL. This is the free associative algebra on L mod relations saying that

$$xy - yx = [x, y]$$

for any x, y in L.

But UL is better than an algebra: it's a cocommutative Hopf algebra! The point is that Lie algebras are a lot like groups, and *both* can be encoded in cocommutative Hopf algebras.

In the universal enveloping algebra UL, comultiplication is a map

$$\Delta \colon UL \to UL \otimes UL$$

defined on elements x of L by the equation

$$\Delta(x) = x \otimes 1 + 1 \otimes x$$

The counit is a map

$$\varepsilon \colon UL \to \mathbb{Q}$$

defined by the equation

$$\varepsilon(x) = 0$$

And conversely, given any cocommutative Hopf algebra, the elements satisfying both these equations form a Lie algebra! These elements are called "primitive elements". If we take the primitive elements of UL, we get the Lie algebra L back.

Let's summarize this using a bit more jargon. There's a "universal enveloping algebra" functor:

$$U\colon \text{[Lie algebras]} \to \text{[cocommutative Hopf algebras]}$$

and this has a right adjoint, the "primitive elements" functor:

$$P\colon \text{[cocommutative Hopf algebras]} \to \text{[Lie algebras]}$$

Even better, if L is any Lie algebra, $P(UL)$ is isomorphic to L.

Today we're generalizing all this to the world of chain complexes! There's a universal enveloping algebra for differential graded Lie algebras:

$$U\colon \text{[DGLAs]} \to \text{[DGCHAs]}$$

and it has a right adjoint, the "primitive elements" functor:

$$P\colon \text{[DGCHAs]} \to \text{[DGLAs]}$$

Even better, if L is any DGLA, $P(UL)$ is isomorphic to L.

So now I hope you understand the strategy for generalizing Lie theory to rational spaces. We can take any path-connected pointed space X and form its group of loops:

$$\Omega\colon \text{[path-connected pointed spaces]} \to \text{[topological groups]}$$

Then we can form a differential graded analogue of its group algebra:

$$C\colon \text{[topological groups]} \to \text{[DGCHAs]}$$

Finally, we can turn that into a differential graded Lie algebra:

$$P\colon \text{[DGCHAs]} \to \text{[DGLAs]}$$

So, just as we could study a Lie group "infinitesimally" by looking at its Lie algebra, we can now study any path-connected pointed space "infinitesimally" by looking at the differential graded algebra Lie algebra of its group of loops! And for *rational* spaces, this "infinitesimal" description knows everything about the homotopy type of our space.

This is probably a good place to stop if you just want the basic idea. But now I want to tell the tale of three functors in a bit more detail. There are some subtleties that are worth knowing if you want to be an expert on algebraic topology. (I'm always hoping someday I'll be one, but it never seems to happen.)

I listed a bunch of fundamental concepts in homotopy theory starting in "Week 115" and going through "Week 119". I listed them with letters A, B, C, and so on up to the letter P. Then I decided to slack off and take a ten-year break. Now I'll continue. . .

Q. *The "based loop space" functor:*

$$\Omega\colon \text{[path-connected pointed spaces]} \to \text{[topological groups]}$$

Suppose X is a path-connected pointed space. Often people define $\Omega(X)$ to be the space of all based loops

$$f\colon [0,1] \to X$$

where $f(0) = f(1)$ is the basepoint of X. There's an obvious way to compose these loops, spending half your time on the first loop and half your time on the second, but it's not associative! It's just associative up to homotopy. So, we don't get a topological monoid, just a topological monoid "up to homotopy". Similarly, the "reverse" of a loop, where we run it backwards in time, is only an inverse up to homotopy.

The concept of a topological monoid "up to homotopy" can be made precise using Stasheff's theory of A_∞ spaces. So, we can learn to love those — and we should. But we can also fight harder to get an honest topological group!

For starters, let's try to make the associative and unit laws hold as equations, instead of just up to homotopy. For this, we can just use "Moore loops", which are maps

$$f\colon [0,T] \to X$$

where $f(0) = f(T)$ is the basepoint of X, and T is any nonnegative real number. Composing a Moore loop of length T and one of length T' naturally gives one of length $T+T'$. This way of composing loops satisfies the associative and unit laws "on the nose", since we don't need to do any reparametrization. So, if we let $\Omega(X)$ be the space of based Moore loops on X, it's a topological monoid!

Even better, the space of based Moore loops is homotopy equivalent to the space of ordinary based loops. They're even equivalent "as A_∞ spaces" — that is, topological spaces with a multiplication that's associative up to a homotopy that satisfies some equation up to homotopy... and so on to infinity.

So, we're not really changing the subject by switching from ordinary loops to Moore loops — at least, not as far as homotopy theory goes.

But what about inverses? Sadly, Moore loops still only have inverses "up to homotopy". But here we can play another trick.

Namely: we can always take a topological monoid, throw in formal inverses, and put on a suitable topology to get a topological group. This process is called "group completion". It's a functor:

$$G\colon [\text{topological monoids}] \to [\text{topological groups}]$$

and it's the left adjoint of the forgetful functor

$$F\colon [\text{topological groups}] \to [\text{topological monoids}]$$

I described group completion in item P of "Week 119", and gave the classic reference.

Now, if we start with a *path-connected* topological monoid M, its group completion GM is homotopy equivalent to M. They're even equivalent as A_∞ spaces, I think. So in this case we're just improving M slightly to make it into a group. But if M has lots of connected components, GM can be drastically different. For example, if we start with the natural numbers, its group completion is the integers!

So, to improve our topological monoid $\Omega(X)$ into a topological group, I think this is what we should do. Take the path component of the identity and group complete that, getting a group G. Then build a topological group with the same group of path components as $\Omega(X)$, but with each component replaced by the group G.

I'm pretty sure this trick lets us turn the monoid of based Moore loops in X into a topological group that's equivalent as an A_∞ space. I'd love to be corrected if I'm wrong here, or doing something suboptimal.

Henceforth, let's use $\Omega(X)$ to stand for the group completion of the monoid of based Moore loops. These are what we naively *want* from our based loops in X: an honest topological group!

R. *The "singular chains" functor from topological groups to differential graded cocommutative Hopf algebras:*

$$C_*\colon \text{[topological groups]} \to \text{[DGCHAs]}$$

To get this, let's line up some functors I mentioned last week:

$$\begin{aligned} Sing\colon &\text{[topological spaces]} \to \text{[simplicial sets]} \\ F\colon &\text{[simplicial sets]} \to \text{[simplicial vector spaces]} \\ N\colon &\text{[simplicial vector spaces]} \to \text{[chain complexes]} \end{aligned}$$

Composing these is how we take any space and get a chain complex!

$$C_*\colon \text{[topological spaces]} \to \text{[chain complexes]}$$

Namely, the chain complex whose homology is the rational homology of that space. This is often called the "singular chain complex" of our space.

And now we want to tackle this puzzle: if our topological space is a topological *group*, why does its chain complex become a DGCHA?

The argument is an easy downhill slide... but alas, there's a big bump near the end that throws me off.

You see, all the categories above have a tensor product that makes them symmetric monoidal. For topological spaces this is the usual cartesian product; for simplicial sets it's also the cartesian product, and for chain complexes it's the tensor product I already mentioned.

And, *almost* all the functors listed above are symmetric monoidal functors. The first two actually are. The third one:

$$N\colon \text{[simplicial vector spaces]} \to \text{[chain complexes]}$$

is not quite. I talked about this problem last week.

If all three functors *were* symmetric monoidal, they would send cocommutative Hopf monoids to cocommutative Hopf monoids. And every topological group G is a cocommutative Hopf monoid. So, if we didn't have this slight problem, we would instantly know that $C_*(G)$ is a cocommutative Hopf monoid in [chain complexes]. And that's precisely a DGCHA!

But alas, it's not quite so easy. We get stuck at the second stage: our group G becomes a cocommutative Hopf monoid in simplicial abelian groups, and then we get stuck.

Let me remind you a bit about the annoying properties of the third functor on my list:

$$N\colon \text{[simplicial vector spaces]} \to \text{[chain complexes]}$$

It's called the "normalized chain complex" or "normalized Moore complex" functor.

As I said last time, this functor is not monoidal. But it's "lax monoidal". So, there's a natural transformation

$$EZ\colon N(X) \otimes N(Y) \to N(X \times Y)$$

And it's also "oplax monoidal". So, there's also a natural transformation going back:

$$AW\colon N(X \times Y) \to N(X) \otimes N(Y)$$

But they're not inverses.

These natural transformations are called the Eilenberg-Zilber and Alexander-Whitney maps — it took 4 great mathematicians to invent them. Maybe too many cooks spoil the broth: it's really annoying that these maps aren't inverses! As I said last time, they come very close. EZ followed by AW is the identity. AW followed by EZ is not. But, it's chain homotopic to the identity!

Let's see how far we can get with just this.

In any monoidal category, we can define "monoids". I explained how back in "Week 89", so let's pretend you know this. The great thing about a lax monoidal functor is that it sends monoids to monoids.

A monoid object in topological spaces is called a "topological monoid" — an example is a topological group. On the other hand, a monoid object in chain complexes is called a "differential graded algebra". Since C is a composite of functors that are either monoidal or (ahem) just lax monoidal, pure abstract nonsense tells us that C sends topological groups to differential graded algebras!

In any monoidal category, we can also define "comonoids". The great thing about an oplax monoidal functor is that it sends comonoids to comonoids.

As I mentioned last week, in a category with finite products, every object is a comonoid in exactly one way! The comultiplication

$$\Delta\colon X \to X \times X$$

is the diagonal map, and the counit

$$\varepsilon\colon X \to 1$$

is the unique map to the terminal object. This, by the way, is why people don't talk about comonoids in the category of sets: every set is a comonoid in exactly one way.

The category of topological spaces has finite products, so every topological space is a comonoid in just one way. On the other hand, a comonoid object is chain complexes is called a "differential graded coalgebra".

Since C is a composite of functors that are either monoidal or (ahem) just oplax monoidal, pure abstract nonsense tells us that C sends topological spaces to differential graded coalgebras!

So, without breaking a sweat, we have seen that for a topological group G, the chain complex $C_*(G)$ is both a differential graded algebra and a differential graded coalgebra.

But why do these fit together neatly to make a differential graded Hopf algebra? I don't know. Somehow we just luck out.

I also don't know why $C_*(G)$ gets to be *cocommutative*. It would be automatic all 3 functors on my list were symmetric monoidal. But again, the third one is not. Somehow we just luck out.

So, there are some formal properties of the normalized chain complex functor

$$N\colon [\text{simplicial vector spaces}] \to [\text{chain complexes}]$$

that I still need to understand!

I'll conclude with some wisdom from Kathryn Hess, just so you can get an expert's take on this situation. Note that she says "lax comonoidal" instead of "oplax monoidal":

> *The normalized chains functor from simplicial sets to chain complexes (with any coefficients) is both lax monoidal and lax comonoidal. The Eilenberg-Zilber equivalence, from the tensor product of the chains on X and on Y to the chains on the cartesian product of X and Y, provides the natural transformation that shows that the chain functor is lax monoidal. The Alexander-Whitney equivalence goes in the opposite direction and shows that the chain functor is lax comonoidal.*
>
> *Since the chain functor is lax comonoidal, the normalized chains on any simplicial set is a dg coalgebra, where the comultiplication is given by the composite of the chain functor applied to the diagonal map, followed be the Alexander-Whitney transformation. It turns out that the Eilenberg-Zilber equivalence is actually itself a morphism of coalgebras with respect to this comultiplication. On the other hand, the Alexander-Whitney map is a morphism of coalgebras up to strong homotopy.*
>
> *The A-W/E-Z equivalences for the normalized chains functor are a special case of the strong deformation retract of chain complexes that was constructed by Eilenberg and MacLane in their 1954 Annals paper "On the groups $H(\pi, n)$. II". For any commutative ring R, they defined chain equivalences between the tensor product of the normalized chains on two simplicial R-modules and the normalized chains on their levelwise tensor product.*
>
> *Steve Lack and I observed recently that the normalized chains functor is actually even Frobenius monoidal. We then discovered that Aguiar and Mahajan already had a proof of this fact in their recent monograph. :-)*

Finally: what about the picture at the top of the page? It was taken in spring, near the south pole of Mars:

6) HiRISE (High Resolution Imaging Science Experiments), "Cryptic terrain on Mars", `http://hirise.lpl.arizona.edu/PSP_003179_0945`

Candy Hansen writes:

> *There is an enigmatic region near the south pole of Mars known as the "cryptic" terrain. It stays cold in the spring, even as its albedo darkens and the sun rises in the sky.*
>
> *This region is covered by a layer of translucent seasonal carbon dioxide ice that warms and evaporates from below. As carbon dioxide gas escapes from below the slab of seasonal ice it scours dust from the surface. The gas vents to the surface, where the dust is carried downwind by the prevailing wind.*
>
> *The channels carved by the escaping gas are often radially organized and are known informally as "spiders."*

Sounds spooky! I love how these photos of Mars are revealing it to be a complex and varied place. They dispel the common impression that it's uniformly red, dusty and dull. I thank Jim Stasheff for pointing them out!

Addenda: I thank John Armstrong and Tim Silverman for catching small mistakes, and Kathryn Hess and Will Orrick for catching big ones.

Forrest W. Doss reassured me somewhat about the thing I called "temperature momentum" — the thing whose time derivative is temperature. He wrote:

> *Hello, I am a grad student who reads your 'weekly' posts. I research shock waves in radiation-hydrodynamic regimes where the usual models fail, and amuse myself by studying QFT and other things on the side. I just wanted to reply to your statement that you were 'nervous' that nobody seemed to talk about temperature as the time-derivative of a quantity.*
>
> *I actually once ran into this concept while looking for work on extremal-action formalisms of thermodynamics/gas dynamics. I found it in A. Taub, "On Hamilton's principle for perfect compressible fluids", in the Proceedings of the First Symposium in Applied Mathematics of the American Mathematical Society, 1947. He references the idea from Helmholtz's* Wissenschaftliche Abhandlungen*, I don't know in what context it appeared there. He also says it is written as "a" in Von Laue's* Relativittstheorie*, a convention which he follows. So the concept does exist out there!*

For more discussion, visit the n-Category Caf.

> If to any homogeneous mass... we suppose an infinitesimal quantity of any substance to be added, the mass remaining homogeneous and its entropy and volume remaining unchanged, the increase of the energy of the mass divided by the quantity of the substance added is the potential for that substance in the mass considered.
>
> *— J. Willard Gibbs*

A vague discomfort at the thought of the chemical potential is still characteristic of a physics education. This intellectual gap is due to the obscurity of the writings of J. Willard Gibbs who discovered and understood the matter 100 years ago.

— *Charles Kitell,* Introduction to Solid State Physics

A nightmare... The prose is both laconic and imprecise — a combination that spells very poor readability.

— *J. Zrake, review of Kitell's* Introduction to Solid State Physics

Week 290

January 15, 2010

This week we'll start with a math puzzle, then a paper about categorification in analysis. Then we'll continue learning about electrical circuits and their analogues in other branches of physics. We'll wrap up with a bit more rational homotopy theory.

But first: here's an image that's been making the rounds lately. What's going on here?

Next: a math puzzle! This was created by a correspondent who wishes to remain anonymous. Here are some numbers. Each one is the number of elements in some famous mathematical gadget. What are these numbers — and more importantly, what are these gadgets?

- *How many minutes are in an hour?*
- *How many hours are in a week?*
- *How many hours are in 3 weeks?*
- *How many feet are in 1.5 miles?*
- *How many minutes are in 2 weeks?*
- *How many inches are in 1.5 miles?*
- *How many seconds are in a week?*
- *How many seconds are in 3 weeks?*

The answers are at the end.

The wave of categorification overtaking mathematics is finally hitting analysis! I spoke a tiny bit about this in "Week 274", right after I'd finished a paper with Baratin, Freidel and Wise on infinite-dimensional representations of 2-groups. I thought it would take a long time for more people to get interested in the blend of 2-categories and measure theory that we were exploring. After all, there's a common stereotype that says mathematicians who like categories hate analysis, and vice versa. But I was wrong:

1) Goncalo Rodrigues, "Categorifying measure theory: a roadmap", available as arXiv:0912.4914.

Read both papers together and you'll get a sense of how much there is to do in this area! A lot of basic definitions remain up for grabs. For example, Rodrigues' paper defines "2-Banach spaces", but will his definition catch on? It's too soon to tell. There are already lots of theorems. And there's no shortage of interesting examples and applications to guide us. But finding the best framework will take a while. I urge anyone who likes analysis and category theory to jump into this game while it's still fresh.

But my own work is taking me towards mathematics of a more applied sort. My excuse is that I'll be spending a year in Singapore at the Centre for Quantum Technologies, starting in July. This will give me a chance to think about computation, and condensed matter physics, and quantum information processing, and diagrams for physical systems built from pieces. Such systems range from the humble electrical circuits that I built as a kid, to integrated circuits, to fancy quantum versions of these things.

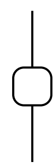
So, lately I've been talking about a set of analogies relating various types of physical systems. I listed 6 cases where the analogies are quite precise:

	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Mechanics (translation)	position	velocity	momentum	force
Mechanics (rotation)	angle	angular velocity	angular momentum	torque

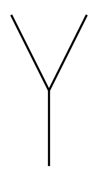
	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Electronics	charge	current	flux linkage	voltage
Hydraulics	volume	flow	pressure momentum	pressure
Thermodynamics	entropy	entropy flow	temperature momentum	temperature
Chemistry	moles	molar flow	chemical momentum	chemical potential

This week I'd like to talk about five circuit elements that we can use to build more complicated electrical circuits: resistors, inductors, capacitors, voltage sources, and current sources. I'll tell you the basic equations they obey, and say a bit about their analogues in the mechanics of systems with translational degrees of freedom. They also have analogues in the other rows.

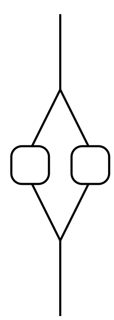
Engineers call these five circuit elements "1-ports". A 1-port can be visualized as a black box with 2 places where you can stick in a wire:



More generally, an "n-port" has $2n$ places where you can attach a wire. This numbering system may seem peculiar. Indeed, it overlooks circuits that have an odd number of wires coming out, like this one made of just wires:

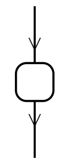


You can use gizmos like this to stick together 1-ports "in parallel":



However, if you've ever looked at the back of a TV or stereo, you'll see that place where you can plug in cables tend to come in pairs! Each pair is called a "port". So, electrical engineers often — though not always — focus on n-ports, where the wires coming out are grouped in pairs. And there's probably even a good *mathematical* reason for paying special attention to these — something related to symplectic geometry. That's one of the things I'm trying to understand better.

Later I'll tell you about some famous 2-ports and 3-ports, but today let's do 1-ports. If we have a 1-port with wires coming out of it, we can arbitrarily choose one wire and call it the the "input", with the other being the "output":



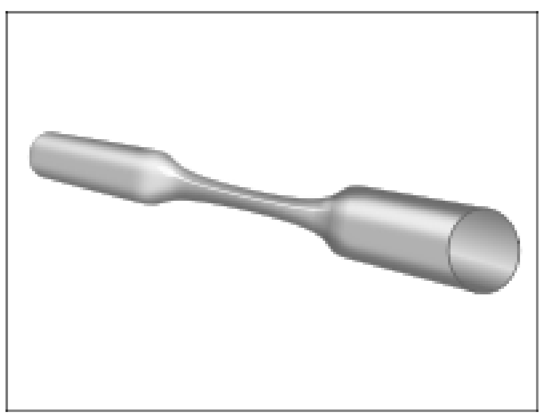
If you know a little category theory, this should seem suspiciously similar to a "morphism". And if you know a bit more, this should remind you of other situations where it takes an arbitrary choice to distinguish between the "input" and the "output" of a morphism.

Any 1-port has a "flow through it" and an "effort across it", which which are functions of time. Remember, "flow" is the general concept that reduces to current in the special case of electronics. "Effort" is the concept that reduces to voltage.

The time integral of flow is called the "displacement" and denoted q, and the time integral of effort is called the "momentum" and denoted p. So, flow is $\dot{q} = dq/dt$ and effort is $\dot{p} = dp/dt$.

To mathematically specify a 1-port, we give one equation involving p, q, $\dot{p}$, $\dot{q}$, and the time variable t. Here's how it works for the five most popular types of 1-ports:

1. A "resistance". This is the general term for what we call a "resistor" in the case of electrical circuits, and "friction" in mechanics. In hydraulics, you can make a resistance using a narrowed pipe:

   In all cases, the effort is some function of the flow:

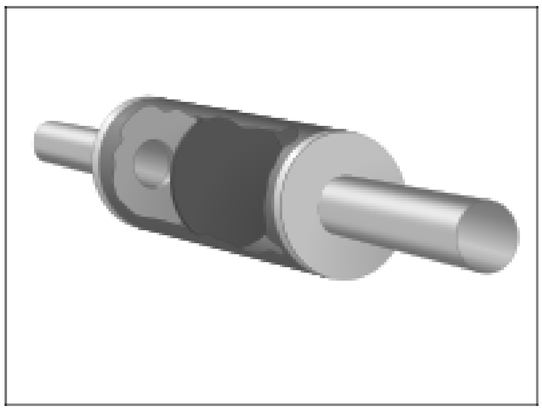
   $$\dot{p} = f(\dot{q})$$

   An easy special case is a linear resistance, for which the effort is proportional to the flow:

   $$\dot{p} = R\dot{q}$$

Here R is some constant, also called the "resistance". In electric circuit theory this equation is called Ohm's law, and people write it using different symbols. Note we need to be careful about our sign conventions: in mechanics we usually think of friction as giving $\mathrm{force} = R\mathrm{velocity}$ with R *negative*, while in electric circuit theory we usually think of an ordinary resistor as giving $\mathrm{voltage} = R\mathrm{current}$ with R *positive*. The two cases are not fundamentally different: it's just an artifact of differing sign conventions!

2. A "capacitance". This is the general term for what we call a "capacitor" in the case of electrical circuits, or a "spring" in mechanics. In hydraulics, you can make a capacitance out of a tank with pipes coming in from both ends and a rubber sheet dividing it in two:



In all cases, the displacement is some function of effort:

$$q = f(\dot{p})$$

An easy special case is a linear capacitance, for which the displacement is proportional to the effort:

$$q = C\dot{p}$$

Here C is some constant, also called the "capacitance". Again we need to be careful with our conventions: in mechanics we usually think of a spring as being stretched by an amount equal to $1/k$ times the force applied. Here k, the *reciprocal* of C, is called the spring constant. But some engineers work with C and call it the "compliance" of the spring. An easily stretched spring has big C, small k.

3. An "inertance". This is the general term for what we call an "inductor" in the case of electrical circuits, or a "mass" in mechanics. The weird word "inertance" hints at how mass gives a particle inertia. In hydraulics, you can build an inertance by putting a heavy turbine inside a pipe: this makes the water want to keep flowing at the same rate.

In all cases, the momentum is some function of flow:

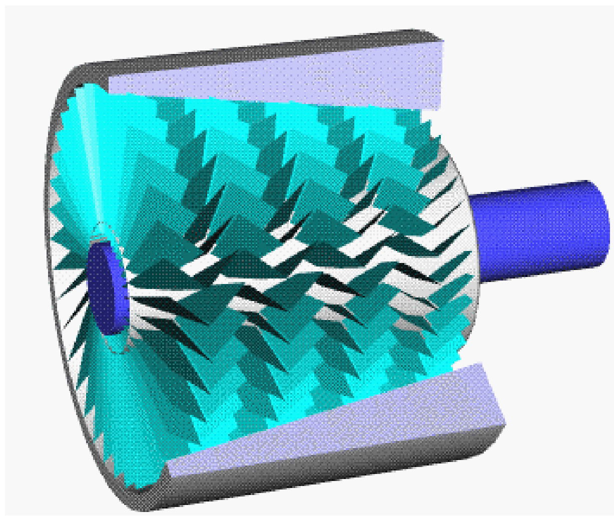
$$p = f(\dot{q})$$

An easy special case is a linear inertance, for which the momentum is proportional to the flow:

$$p = L\dot{q}$$

Here L is some constant, also called the "inertance". In the case of mechanics, this would be the mass.

4. An "effort source". This is the general term for what we call a "voltage source" in the case of electrical circuits, or an "external force" in mechanics. In hydraulics, an effort source is a compressor set up to maintain a specified pressure difference between the input and output:



Here the equation is of different type than before! It can involve the time variable t:

$$\dot{p} = f(t)$$

5. A "flow source". This is the general term for what we call a "current source" in the case of electrical circuits. In hydraulics, an flow source is a pump set up to maintain a specified flow. Here the equation is

$$\dot{q} = f(t)$$

It's interesting to ponder these five 1-ports and how they form families.

The voltage and current sources form a family, since only these involve the variable t in an explicit way. Also, only these can be used to add energy to a circuit. So, these two are called "active" circuit elements.

The other three are called "passive". Among these, the capacitance and inertance form a family because they both conserve energy. The resistance is different: it dissipates energy — or more precisely, turns it into heat energy, which is not part of our simple model. If you're more used to mechanics than electrical circuits, let me translate what I'm saying into the language of mechanics: a machine made out of masses and springs will conserve energy, but friction dissipates energy.

Let's try to make this "energy conservation" idea a bit more precise. I've already said that $\dot{p}\dot{q}$, effort times flow, has dimensions of power — that is, energy per time. Indeed, for any 1-port, the physical meaning of $\dot{p}\dot{q}$ is the rate at which energy is being put in. So, in electrical circuit theory, people sometimes say energy is "conserved" if we can find some function $H(p, q)$ with the property that

$$\frac{dH(p,q)}{dt} = \dot{p}\dot{q}$$

This function H, called the "Hamiltonian", describes the energy stored in the 1-port. And this equation says that the energy stored in the system changes at a rate equal to the rate at which energy is put in! So energy doesn't get lost, or appear out of nowhere.

Now, when I said "energy conservation", you may have been expecting something like $dH/dt = 0$. But we only get that kind of energy conservation for "closed" systems — systems that aren't interacting with the outside world. We'll indeed get $dH/dt = 0$ when we build a big circuit with no inputs and no outputs out of circuit elements that conserve energy in the above sense. The energy of the overall system will be conserved, but of course it can flow in and out of the various parts.

But of course it's really important to think about circuits with inputs and outputs — the kind of gizmo you actually plug into the wall, or hook up to other gizmos! So we need to generalize classical mechanics to "open" systems: systems that can interact with their environment. This will let us study how big systems are made of parts.

But right now we're just studying the building blocks — and only the simplest ones, the 1-ports.

Let's see how energy conservation works for all five 1-ports. For simplicity I'll only do the linear 1-ports when those are available, but the results generalize to the nonlinear case:

1. The "resistance". For a linear resistance we have

   $$\dot{p} = R\dot{q}$$

   so the power is

   $$\dot{p}\dot{q} = R(\dot{q})^2$$

   In the physically realistic case $R > 0$ so this is nonnegative, meaning that we can only put energy *into* the resistor. And note that $\dot{p}\dot{q}$ is *not* the time derivative of some function of p and q, so energy is not conserved. We say the resistance "dissipates" energy.

2. The "capacitance". For a linear capacitance we have

   $$q = C\dot{p}$$

   so the power is

   $$\dot{p}\dot{q} = \frac{q\dot{q}}{C}$$

   Note that unlike the resistor this can take either sign, even in the physically realistic case $C > 0$. More importantly, in this case $\dot{p}\dot{q}$ is the time derivative of a function of p and q, namely

   $$H(p,q) = \frac{q^2}{2C}$$

So in this case energy is conserved. If you're comfortable with mechanics you'll remember that a spring is an example of a capacitance, and $H(p, q)$ is the usual "potential energy" of a spring when $C4 is the reciprocal of the spring constant.

3. The "inertance". For a linear inertance

$$p = L\dot{q}$$

so the power is

$$\dot{p}\dot{q} = \frac{p\dot{p}}{L}$$

Again this can take either sign, even in the physically realistic case $L > 0$. And again, $\dot{p}\dot{q}$ is the time derivative of a function of p and q, namely

$$H(p, q) = \frac{p^2}{2L}$$

So energy is also conserved in this case. If you're comfortable with mechanics you'll remember that a mass is an example of a inertance, and $H(p, q)$ is the usual "kinetic energy" of a mass when L equals the mass.

4. The "effort source". For an effort source

$$\dot{p} = f(t)$$

for some function f, so the power is

$$\dot{p}\dot{q} = f(t)\dot{q}$$

This is typically not the time derivative of some function of p and q, so energy is not usually conserved. I leave it as a puzzle to give the correct explanation of what's going on when $f(t)$ is a constant.

5. The "flow source". For a flow source

$$\dot{q} = f(t)$$

for some function f, so the power is

$$\dot{p}\dot{q} = f(t)\dot{p}$$

This is typically not the time derivative of some function of p and q, so energy is not usually conserved. Again, I leave it as a puzzle to understand what's going on when $f(t)$ is constant.

So, everything works as promised. But if your background in classical mechanics is anything like mine, you should still be puzzled by the equation

$$\frac{dH(p, q)}{dt} = \dot{p}\dot{q}$$

This is sometimes called the "power balance equation". But you mainly see it in books on electrical engineering, not classical mechanics. And I think there's a reason. I don't see how to derive it from a general formalism for classical mechanics, the way I can derive $dH/dt = 0$ in Hamiltonian mechanics. At least, I don't see how when we write the equation this way. I think we need to write it a bit differently!

In fact, I was quite confused until Tim van Beek pointed me to a nice discussion of this issue here:

2) Bernard Brogliato, Rogelio Lozano, Bernhard Maschke and Olav Egeland, *Dissipative Systems Analysis and Control: Theory and Applications*, 2nd edition, Springer, Berlin, 2007.

I'll say more about this later. For now let me just explain two buzzwords here: "control theory" and "dissipative systems".

Traditional physics books focus on closed systems. "Control theory" is the branch of physics that focuses on open systems — and how to make them do what you want!

For example, suppose you want to balance a pole on your finger. How should you move your finger to keep the pole from falling over? That's a control theory problem. You probably don't need to read a book to solve this particular problem: we're pretty good at learning to do tricks like this without thinking about math. But if you wanted to build a robot that could do this — or do just about anything — control theory might help.

What about "dissipative systems"? I already gave an example: a circuit containing a resistor. I talked about another in "Week 288": a mass on a spring with friction. In general, a dissipative system is one that loses energy, or more precisely converts it to heat. We often don't want to model the molecular wiggling that describes heat. If we leave this out, dissipative systems are not covered by ordinary Hamiltonian mechanics — since that framework has energy conservation built in. But there are generalizations of Hamiltonian mechanics that include dissipation! And these are pretty important in practical subjects like control theory... since life is full of friction, as you've probably noticed.

So, this book covers everything that *my* classical mechanics education downplayed or left out: open systems, dissipation and control theory! And in the chapter on "dissipative physical systems", it derives power balance equations for "input-output Hamiltonian systems" and "port-controlled Hamiltonian systems". Apparently it's the latter that describes physical systems built from n-ports.

For more on port-controlled Hamiltonian systems, this book recommends:

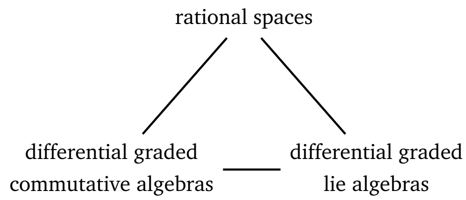
3) B. M. Maschke and A. J. van der Schaft, "Port controlled Hamiltonian systems: modeling origins and system theoretic properties", in *Proceedings of the 2nd IFAC Symp. on Nonlinear Control Systems Design, NOLCOS'92* (1992), pp. 282–288.

4) B. M. Maschke and A. J. van der Schaft, "The Hamiltonian formulation of energy conserving physical systems with ports", *Archiv fur Elektronik und Ubertragungstechnik* **49** (1995), 362–371.

5) A. J. van der Schaft, "L^2-gain and Passivity Techniques in Nonlinear Control", 2nd edition, Springer, Berlin, 2000.

So, I need I learn more about this stuff, and then explain it to you. But let's stop here for now, and turn to... rational homotopy theory!

Nothing big this week: I just want to take stock of where we are. I've been trying to explain a triangle of concepts:



In "Week 287" I explained a functor going down the left side of this triangle. In fact I explained how we can get a differential graded commutative algebra, or DGCA, from *any* topological space. This involved a grand generalization of differential forms.

In "Week 289" I explained a functor going down the right side. In fact I explained how we can get a differential graded Lie algebra, or DGLA, from *any* topological space with a chosen basepoint. This involved a grand generalization of Lie groups, and their Lie algebras.

Today I'd like to explain a sense in which all three concepts in this triangle are "the same". I won't give you the best possible theorem along these lines — just Quillen's original result, which is pretty easy to understand. It says that three categories are equivalent: one for each corner of our triangle!

I explained the first category back in "Week 286". I called it the "rational homotopy category", and I described it in several ways. Here's one. Start with the category where:

- the objects are 1-connected pointed spaces;
- the morphisms are basepoint-preserving maps.

Then, throw in formal inverses to all "rational homotopy equivalences" — that is, maps

$$f \colon X \to X'$$

that give isomorphisms between rational homotopy groups:

$$\mathbb{Q} \otimes \pi_n(f) \colon \mathbb{Q} \otimes \pi_n(X) \to \mathbb{Q} \otimes \pi_n(X')$$

This gives the rational homotopy category.

The second category involves DGCAs. Well — actually not. To get the nicest results, it seems we should work dually and use differential graded *co*commutative *co*algebras, or DGCCs. I'm sorry to switch gears on you like this, but that's life. The difference is "purely technical", but I want to state a theorem that I'm sure is true!

In "Week 287" we saw how Sullivan took any space and built a DGCA whose cohomology was the rational cohomology of that space. But today let's follow Quillen and instead work with a DGCC whose homology is the rational homology of our space.

So, let's start with the category of DGCC's over the rational numbers — but not *all* of them, only those that are trivial in the bottom two dimensions:

$$0 \stackrel{d}{\leftarrow} 0 \stackrel{d}{\leftarrow} C_2 \stackrel{d}{\leftarrow} C_3 \stackrel{d}{\leftarrow} \dots$$

Why? Because our spaces are 1-connected, so their bottom two homology groups are boring. Then, let's throw in formal inverses to "quasi-isomorphisms" — that is, maps between DGCCs

$$f\colon C \to C'$$

that give isomorphisms between homology groups:

$$H_n(f)\colon H_n(C) \to H_n(C')$$

The resulting category is *equivalent* to the rational homotopy category!

The third category involves DGLAs. We start with the category of DGLAs over the rational numbers — but not *all* of them, only those that are trivial in the bottom dimension:

$$0 \stackrel{d}{\leftarrow} L_1 \stackrel{d}{\leftarrow} L_2 \stackrel{d}{\leftarrow} L_3 \stackrel{d}{\leftarrow} \dots$$

Just the very bottom dimension, not the bottom two! Why? Because we get a DGLA from the group of *loops* in our rational space, and looping pushes down dimensions by one. Then, we throw in formal inverses to "quasi-isomorphisms" — that is, maps between DGLAs:

$$f\colon L \to L'$$

that give isomorphisms between homology groups:

$$H_n(f)\colon H_n(L) \to H_n(L')$$

Again, the resulting category is *equivalent* to the rational homotopy category!

So, we have a nice unified picture. We could certainly improve it in various ways. For example, I haven't discussed the bottom edge of the triangle. Doing this quickly brings in L_∞-algebras, which are like DGLAs where all the laws hold only "up to chain homotopy". It also brings in gadgets that are like DGCAs or DGCCs, but where all the laws hold only up to chain homotopy. This outlook eventually leads us to realize that we have something much better than three equivalent categories. We have three equivalent $(\infty, 1)$-categories!

But there's also the question of what we can *do* with this triangle of concepts. There are lots of classic applications to topology, and lots of new applications to mathematical physics.

So, there's more to come.

As for the number puzzle at the beginning, all the numbers I listed are the sizes of various "finite simple groups". These are the building blocks from which all finite groups can be built. You can see a list of them here:

6) Wikipedia, "Finite simple groups", `http://en.wikipedia.org/wiki/List_of_finite_simple_groups`

There are 16 infinite families and 26 exceptions, called "sporadic" finite simple groups. Anyway, here we go:

- *How many minutes are in an hour?*

  60, which is the number of elements in the smallest nonabelian finite simple group, namely A_5. Here A_n is an an "alternating group": the group of even permutations of the set with n elements. By some wonderful freak of nature, A_5 is isomorphic to both $\mathrm{PSL}(2,4)$ and $\mathrm{PSL}(2,5)$. Here $\mathrm{PSL}(n,q)$ is a "projective special linear group": the group of determinant-1 linear transformations of an n-dimensional vector space over the field with q elements, modulo its center.

- *How many hours are in a week?*

  168, which is the number of elements — or "order" — of the second smallest nonabelian finite simple group, namely $\mathrm{PSL}(2,7)$. Thanks to another marvelous coincidence, this is isomorphic to $\mathrm{PSL}(3,2)$. See "Week 214" for a lot more about this group and its relation to Klein's quartic curve and the Fano plane.

- *How many hours are in 3 weeks?*

  504, which is the order of the finite simple group $\mathrm{PSL}(2,8)$.

- *How many feet are in 1.5 miles?*

  7,920, which is the order of the finite simple group M_{11} — the smallest of the finite simple groups called Mathieu groups. See "Week 234" for more about this.

- *How many minutes are in 2 weeks?*

  20,160, which is the order of the finite simple group A_8. Thanks to another marvelous coincidence, this is isomorphic to $\mathrm{PSL}(4,2)$. And there's also another nonisomorphic finite simple group of the same size, namely $\mathrm{PSL}(3,4)$!

- *How many inches are in 1.5 miles?*

  95,040, which is the order of the finite simple group M_{12} — the second smallest of the Mathieu groups. See "Week 234" for more about this one, too.

- *How many seconds are in a week?*

  604,800, which is the order of the finite simple group J_2 — the second Janko group, also called the Hall-Janko group. I don't know anything about the Janko groups. They don't seem to have much in common except being sporadic finite simple groups that were discovered by Janko.

  I like what the Wikipedia says about the third Janko group: it "seems unrelated to any other sporadic groups (or to anything else)". Unrelated to anything else? Zounds!

- *How many seconds are in 3 weeks?*

  1,814,400, which is the order of the finite simple group A_{10}.

If you like this sort of stuff, you might enjoy this essay:

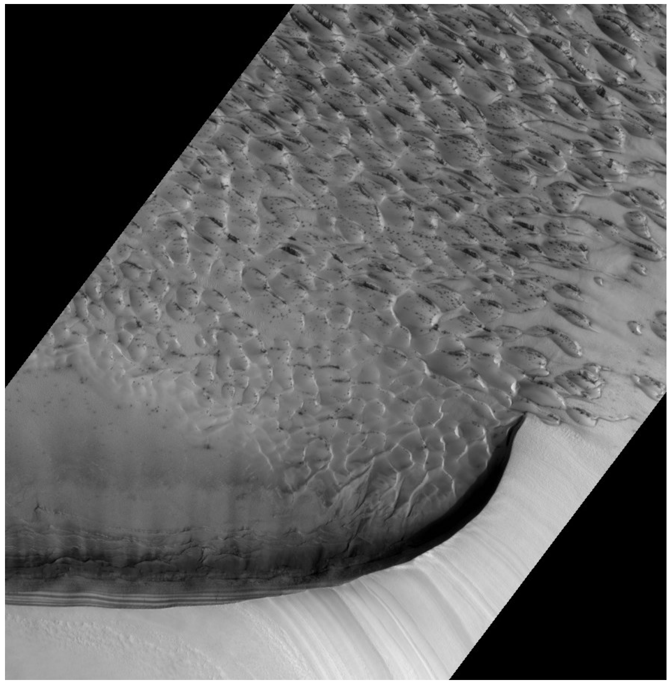
7) John Baez, "Why there are 63360 inches in a mile?", `http://math.ucr.edu/home/baez/inches.html`

It's a curious number:

$$63360 = 2^7 \times 3^2 \times 5 \times 11$$

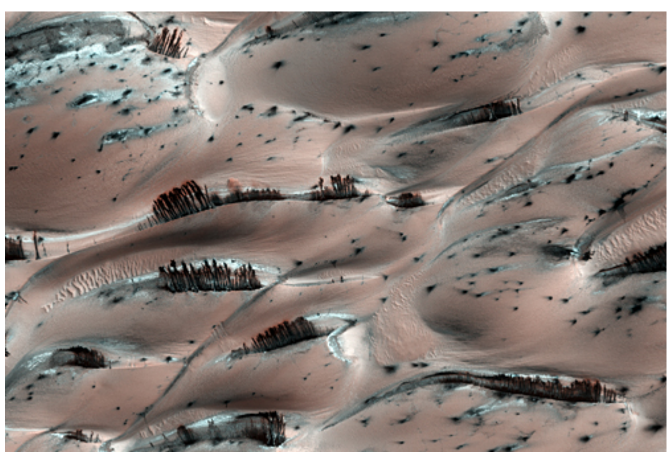
It seems rather odd that this number is divisible by 11. Find out why it is!

Finally, what about that image? Unsurprisingly, it's from Mars. It shows a dune field less than 400 kilometers from the north pole, bordered on both sides by flat regions — but also a big cliff on one side:



8) HiRISE (High Resolution Imaging Science Experiments), "Falling material kicks up cloud of dust on dunes", `http://hirise.lpl.arizona.edu/PSP_007962_2635`

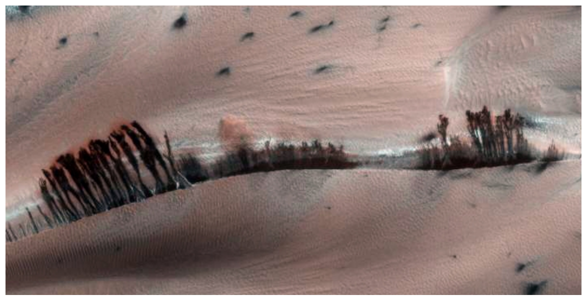
Some streaks on the dunes look like stands of trees lined up on hilltops:



It would be great if there were trees on Mars, but it's not true. In fact what you're seeing are steep slopes with dark stuff slowly sliding down them! Here's a description written by

Candy Hansen, a member of NASA's Mars Reconnaissance Orbiter team at the University of Arizona:

> *There is a vast region of sand dunes at high northern latitudes on Mars. In the winter, a layer of carbon dioxide ice covers the dunes, and in the spring as the sun warms the ice it evaporates. This is a very active process, and sand dislodged from the crests of the dunes cascades down, forming dark streaks.*



> *In the subimage falling material has kicked up a small cloud of dust. The color of the ice surrounding adjacent streaks of material suggests that dust has settled on the ice at the bottom after similar events.*
>
> *Also discernible in this subimage are polygonal cracks in the ice on the dunes (the cracks disappear when the ice is gone).*

Addenda: I thank Toby Bartels, Bruce Smith, and Don Davis of Boston for some corrections.

In particular, the number of inches in a mile is divisible by 11 because there are $33/2$ feet in a rod. For the explanation of *that*, see my webpage. But Don Davis pointed out that this is not the only reason why the number 11 appears in the American system of units. A US liquid gallon is $231 = 3 \times 7 \times 11$ cubic inches!

Why? According to Don Davis and the Wikipedia article on gallons) the reason is that once upon a time, a British wine gallon was 7 inches across and 6 inches deep — for some untold reason that deserves further investigation. If we approximate π by $22/7$, the volume then comes out to $3 \times 7 \times 11$ cubic inches!

This 11-ness of the gallon then infects other units of volume. For example, a US liquid ounce is

$$\frac{3 \times 7 \times 11}{2^7}$$

cubic inches!

My friend Bruce Smith says that his young son Peter offered the following correction to the quote of the week: it should really be *"The most important thing is to keep the 2nd most important thing the 2nd most important thing"* — because the first most important thing is the topic of the sentence!

John McKay writes:

> *You say you don't know anything about the Janko groups. Let me help you...*
>
> *The first Janko group is a subgroup of $G_2(11)$. It is called J_1 and has order $= 11 \times (11+1) \times (11^3-1)$ suggesting incorrectly it may be one of a family. This is the first of the modern sporadics. Then came J_2 and J_3 both having isomorphic involution centralizers. The first was constructed by Marshall Hall and the second by Graham Higman and me.*
>
> *David Wales and I decided on the names so that J_k has a Schur multiplier (=second cohomology group) of order k. J_2 is the Hall-Janko group. Janko finally produced his fourth group J_4 (which unfortunately does not have a Schur multiplier of order 4)! J_1, J_3, and J_4 are among the Pariahs (as are O'Nan, Rud, Ly-Sims). They are those sporadics that have no involvement with $M =$ the Monster group (see Mark Ronan's book).*
>
> *This group, M, appears to have incredible connections with many areas of mathematics and of physics. Its real nature has yet to be revealed.*
>
> *Best,*
> *John*

Here $G_2(11)$ is like the exceptional Lie group G_2 except it's defined over the field with 11 elements. So, the number 11 raises its ugly head yet again!

For more discussion, visit the n-Category Caf.

> The most important thing is to keep the most important thing the most important thing.
>
> — *Donald P. Coduto*

Week 291

January 22, 2010

This week I want to ask for references — references on a cool relationship between Julia sets and the Mandelbrot set. Then, we'll delve further into electrical circuits and analogous systems. No more rational homotopy theory, I'm afraid! There's a lot more to say, but I've been thinking about other things. These days I'm trying to crank out one This Week's Finds every week. I may give up on that soon... but I want to finish this one today, and it's 9 pm, and I haven't had dinner.

First: if you're into n-categories, you have to check out Carlos Simpson's new book:

1) Carlos Simpson, *Homotopy theory of higher categories*, draft available as `http://hal.archives-ouvertes.fr/hal-00449826/fr/`

It's very readable, with a long historical introduction that'll help you understand the motivations behind current work, and a warmup section on strict n-categories — which are relatively easy — before diving into the subtleties of weak ones. It compares many approaches to weak n-categories before explaining his own.

This could be the book the world has been waiting for! And he's asking for comments and corrections, so you can help make it better.

Next: a little music. Mike Stay pointed me to a great video illustrating the first piece from Bach's Musical Offering. Jos Leys did the animation, while a physics blogger with the monicker "Xantox" played the music.

2) Jos Leys, `http://www.josleys.com/Canon/Canon.html`

3) Xantox, Canon 1 a 2, at his blog Strange Paths, `http://strangepaths.com/canon-1-a-2/2009/01/18/en/`

This is a "crab canon", meaning roughly a melody that sounds good when you play it both forwards and backwards, simultaneously. Bach wrote it after Frederick the Great invited him to the Prussian court in Berlin. When Bach arrived, he was asked to test the king's new pianos. The king proposed a musical theme and asked Bach to improvise a fugue based on it.

Legend has it that Bach immediately improvised two: one for three voices, and one for six! And later, after returning to his home in Leipzig, Bach composed a set of canons and a trio sonata featuring the king's theme, and sent the whole lot to Frederick as a "Musiche Opfer", or musical offering.

The whole Musical Offering is a *tour de force* — the sort of highly patterned thing you'd expect mathematicians to like. It consists mainly of "strict canons". In a strict canon, first you start playing one melody, called the "leader". Then, while that melody is going on, you start playing another, the "follower", which is an exact copy of the leader — except perhaps transposed to a different pitch.

The hard part is to make the leader and follower fit beautifully when they're both going on. If you need to bend the rules to make your canon sound better, that's okay — but then it's not "strict".

A crab canon, which is very rare, bends the rules by letting the follower be an upside-down version of the leader. This style is *not* for wimps who can't write a good strict canon: it's for people like Bach who find strict canons insufficiently challenging.

The crab canon is not the only tricky feat in the Musical Offering. For example, the fifth piece is a "spiral canon", designed to sound good if you play it over and over, but going up a whole step each time. And the eighth piece is a "mirror canon" Here the follower is an upside-down version of the leader!

I first learned this stuff here, back when I was a teenager:

4) Douglas Hofstadter, *Gdel, Escher, Bach: an Eternal Golden Braid*, Basic Books, 1979.

I feel sort of silly recommending this book. You must have already read it! But maybe not. I can imagine various good excuses. Maybe you were just recently born, or something. Anyway: if you like logic, self-reference, goofiness, puzzles and puns, and you haven't read this book yet, do it now! But if you hate such things, you're excused. Hofstadter's humor might grate on some people's nerves.

While it's fun to read about crab canons, and fun to listen to them, you may have trouble fully appreciating them unless you see the score while you're listening. And that's one reason the video by Jos Leys and Xantox is so great.

For more on the Musical Offering, try these:

5) Timothy A. Smith, "Canons of the Musical Offering", `http://jan.ucc.nau.edu/~tas3/musoffcanons.html`

6) Tony Phillips, "Math and the Musical Offering", `http://www.ams.org/featurecolumn/archive/canons.html`

Next: there's an incredibly cool relationship between the Mandelbrot set and all the Julia sets. Somehow somebody neglected to tell me about it when I was first learning about fractals. They ought to be sued! I just learned about it from Jesse McKeown over at the n-Category Caf, and I want some good references on it. I don't understand it as well as I'd like! But I can show it to you.

Consider this function of two complex variables:

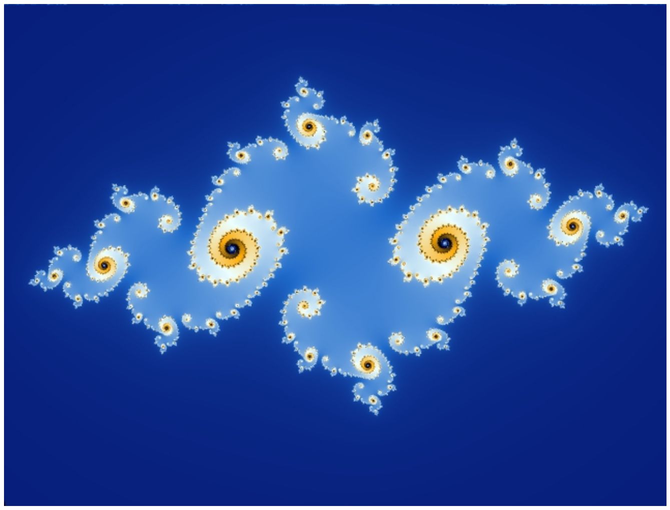
$$z \mapsto z^2 + c$$

If we fix a number c, this function defines a map from the complex plane to itself. We can start with any number z and keep applying this map over and over. We get a sequence of numbers. Sometimes this sequence shoots off to infinity and sometimes it doesn't. The boundary of the set where it doesn't is called the "Julia set" for this number c.

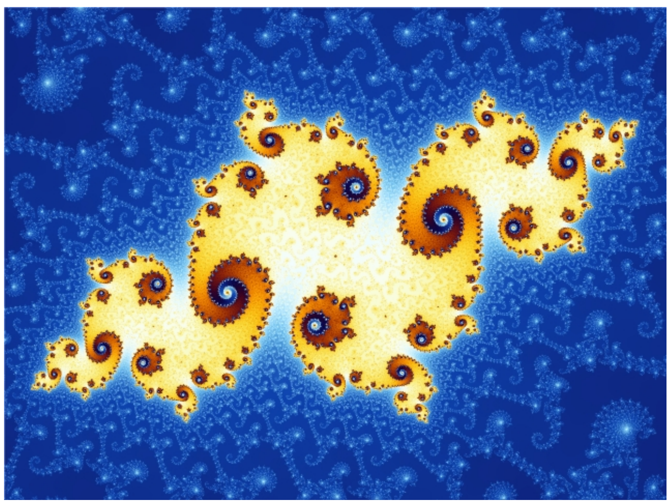
On the other hand, we can start with $z = 0$, and draw the set of numbers c for which the resulting sequence doesn't shoot off to infinity. That's called the "Mandelbrot set".

Here's the cool relationship: in the vicinity of the number c, the Mandelbrot set tends to look like the Julia set for that number c. This is especially true right at the boundary of the Mandelbrot set.

For example, this is the Julia set for $c = -0.743643887037151 + 0.131825904205330i$:



while this:

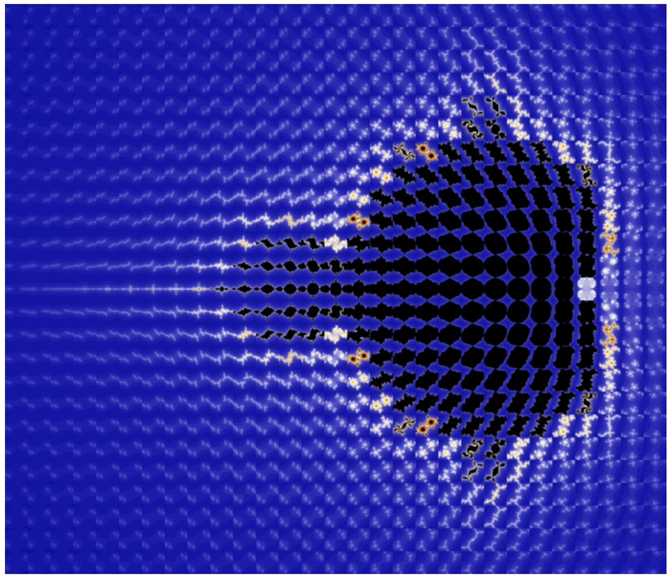


is a tiny patch of the Mandelbrot set centered at the same value of c. They're shockingly similar!

7) Wikipedia, "Mandelbrot sets: relationship with Julia sets", `http://en.wikipedia.org/wiki/Mandelbrot_set#Relationship_with_Julia_sets`

This is why the Mandelbrot set is so complicated. Julia sets are already very complicated. But the Mandelbrot set looks like *a lot* of Julia sets!

Here's a great picture illustrating this fact. Click on it for a bigger view:



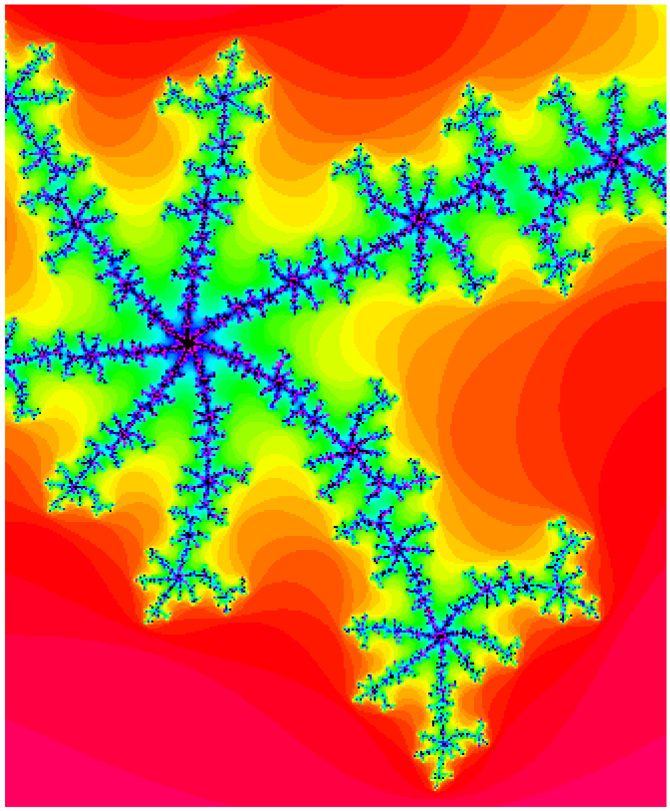
8) Wikimedia Commons, "725 Julia sets", `http://commons.wikimedia.org/wiki/File:725_Julia_sets.png`

It's a big picture made of lots of little pictures of Julia sets for various values of c... but it mimics the Mandelbrot set. You'll notice that the Mandelbrot set is the set of numbers c whose Julia sets are connected. Those Julia sets are the black blobs. When c leaves the Mandelbrot set, its Julia set falls apart into dust: that's the white stuff.

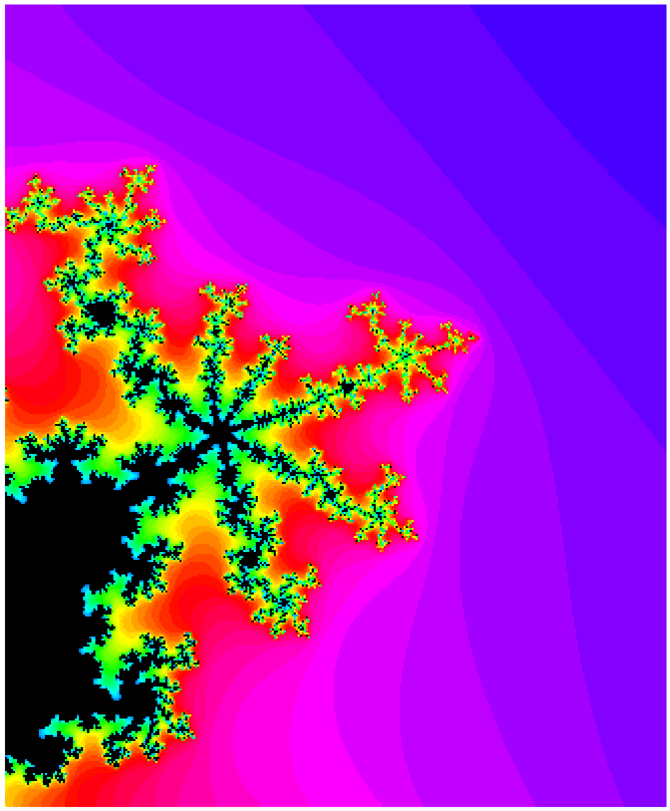
For an even better view of this phenomenon, try this:

9) David Joyce, "Mandelbrot and Julia set explorer", `http://aleph0.clarku.edu/~djoyce/julia/explorer.html`

You can zoom into the Mandelbrot set and see the corresponding Julia set at various values of c. For example, here's the Julia set at $c = -0.689494949 - -- 0.462323232i$:

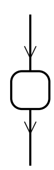


and here's a tiny piece of Mandelbrot set near that point:



Does anyone know a good introduction to this phenomenon? Apparently it's the key to all deep work on the Mandelbrot set.

Last week I explained five kinds of circuit elements: resistances, capacitances, inertances, effort sources and flow sources. All these are "1-ports", meaning they have one wire coming in and one going out:



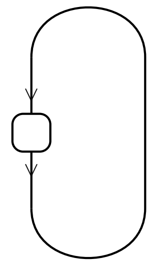
Today I want to talk about 2-ports and 3-ports. From these, we can build all the more complicated circuits we'll be wanting to study. But first, just for fun, here's some very basic stuff about one of the 1-ports I just listed. Namely: effort sources.

We see plenty of effort sources in everyday life. Indeed, all the technology in a modern home relies on them!

For starters, batteries try to act like constant voltage sources. For example, a 9-volt battery tries to provide

$$V(t) = 9$$

Why do I say "tries"? Because this is an idealization. If you take a perfect constant voltage source and connect its input and output with a perfectly conductive wire:



you'll get an infinite current! In reality, if you connect the two terminals of battery with a highly conductive copper wire, you'll get a short circuit: a large amount of current which winds up destroying the battery.

(Particle physicists should look at the above diagram and think about how Feynman diagrams with closed loops in them lead to infinities. Category theorists should think about "traces" and how sometimes traces diverge. It is my job to make these analogies precise. But not today.)

Electrical outlets also do their best to act like voltage sources. But they put out alternating current, so the voltage wiggles like a sine wave. In America, from Canada down to Ecuador, outlets mostly try to produce this voltage:

$$V(t) = \sqrt{2} 120 \sin(2\pi 60 t + c)$$

where c is some undetermined constant. People say they put out 120 volts at a frequency of 60 hertz. But this 120 volts is the "root-mean-square" voltage. To get the "peak" voltage we need to multiply by the square root of 2, for reasons explained here:

10) Wikipedia, "Root mean square: average electrical power", at `http://en.wikipedia.org/wiki/Root_mean_square#Average_electrical_power`

That's where the square root of 2 comes from. Also, in electrical engineering, a frequency of 60 hertz means you've got a wave that makes 60 full cycles per second, so we need a 2π in the above formula. Physicists often define frequency a different way, that doesn't require the 2π. This causes violent fistfights when engineers meet physicists.

In most of the rest of the world, outlets try to produce 240 volts at a frequency of 50 hertz, so

$$V(t) = \sqrt{2} 240 \sin(2\pi 50 t + c)$$

But humans can never agree on anything. So, there are also countries that do lots of other things — and countries like Brazil that do a mixture of things: 115 volts, 127 volts or 220 volts at 60 hertz, depending on where you are!

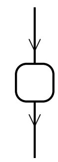
Why does Brazil use three voltages? Why did Australia convert from 240 volts to 230 in the year 2000? Why do some parts of Japan use 50 hertz current while others use 60 hertz, forcing Japanese appliances to have a switch that lets you pick which one you're using? I don't know. . . but now I want to. I have an endless capacity to find these puzzles electrifying, once I let go of a certain mental resistance, which impedes me.

And let's not even get *started* on the various types of plugs used in different countries!

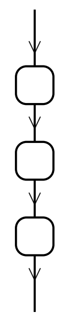
11) Wikipedia, "Mains electricity", http://en.wikipedia.org/wiki/Mains_electricity

12) Wikipedia, "Mains power around the world", http://en.wikipedia.org/wiki/Mains_power_around_the_world

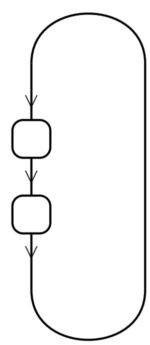
Okay, now let's talk about 2-ports and 3-ports. Remember, a 1-port looks like this:



If all we have is 1-ports, we can only build circuits by stringing them together in series:

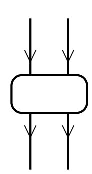


or perhaps forming a closed loop:



This is sort of dull, though still worth understanding. To have more fun, we need some 2-ports or 3-ports!

A 2-port looks like this:



The current flowing in the left wire on top must equal the current flowing out the left wire on bottom — that's just a rule in this game. And similarly for the wires on the right. So, a 2-port has just two flows, say $\dot{q}_1$ and $\dot{q}_2$. Similarly, it has two efforts $\dot{p}_1$ and $\dot{p}_2$.

Mathematically, we specify a 2-port by giving 2 equations involving these two efforts and flows, the corresponding momenta and displacements, and perhaps the time variable t.

The most popular 2-ports are very simple. They are:

1. A "transformer". A transformer multiplies effort and divides flow:

$$\dot{p}_2 = m\dot{p}_1$$
$$\dot{q}_2 = \frac{1}{m}\dot{q}_1$$

   If you bought some electrical equipment in Europe and you try to use it in the US, you need a transformer — although your equipment may have one built in. The transformer multiplies the voltage by the right number. But thanks to some sad fact of life, it must also divide the current by that same number.

   In mechanics, a lever acts as a transformer. If you push on the long end, the short end pushes with a force that's been multiplied by some number. But thanks to some sad fact of life, the short end moves at a velocity that's been divided by that very same number!

2. A "gyrator". A gyrator trades effort for flow:

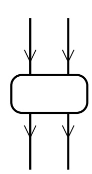
$$\dot{p}_2 = r\dot{q}_1$$
$$\dot{q}_2 = \frac{1}{r}\dot{p}_1$$

   An example is a spinning gyroscope that's leaning completely horizontally. If you push it down slightly, its axis turns at a rate proportional to your push. So, it's trading angular velocity for torque!

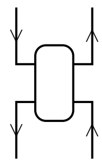
Both these 2-ports "conserve energy" in the sense I described last week. Of course we need to generalize that notion a bit, since we've got more ports now! But it's easy. In the conventions we're using right now, the power absorbed by a 2-port equals

$$\dot{p}_1\dot{q}_1 - \dot{p}_2\dot{q}_2$$

The minus sign here is one of many that plague this subject, like flies in an impoverished, unsanitary tropical village. I would like to exterminate them all by a better choice of conventions, but I haven't figured out the best way. Luckily the signs don't really matter much. Here they seem to arise from treating the first port as an "input" and the second as an "output". In other words, instead of this:



people sometimes think of the 2-port this way:



Anyway, if we use vectors and write

$$p = (p_1, p_2)$$
$$q = (q_1, q_2)$$

then the power is some funny dot product of these vectors, namely

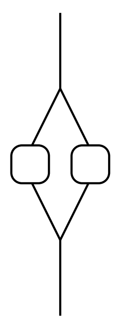
$$p' \cdot q' = \dot{p}_1\dot{q}_1 - \dot{p}_2\dot{q}_2$$

for short. And we say the 2-port "conserves energy" if we can find some function $H(p, q)$ such that

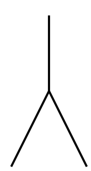
$$\frac{dH(p,q)}{dt} = p' \cdot q'$$

Remember, H is the energy or "Hamiltonian". So, this equation means that when you pour power into the 2-port, its energy rises at exactly the rate you'd expect. And, you can check that both the transformer and gyrator conserve energy according to this definition.

Next: 3-ports! To build interesting circuits, we need the ability to hook up two 1-ports in parallel, like this:



But this gizmo, made of just wire:

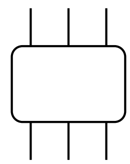


is not an n-port of any kind, since it has an odd number of wires coming out.

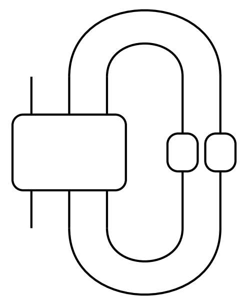
So, how can we connect 1-ports in parallel using just n-ports?

This puzzle had me stumped for a while. But the answer is simple. To connect 1-ports in parallel, we need *two* gizmos of the above sort! And taken together, they can be viewed as a 3-port!

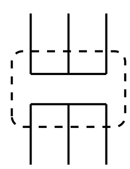
In other words, there's a 3-port like this:



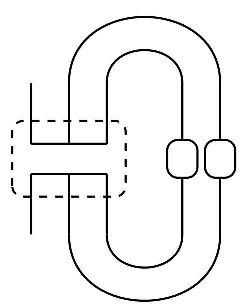
which you can use to connect two 1-ports in parallel. You just attach them like this:



What's in this 3-port? Nothing but wires:



The border around the wires doesn't actually do anything here — it's just the "packaging" that makes our 3-port seem impressive. Inside, it's just two three-pronged gizmos made of wire. But if the customer can't see inside, we can sell it for a lot of money! See how it works?



Current flows in at the upper left. It gets split, goes through our two 1-ports at right, gets rejoined, and exits at the lower left!

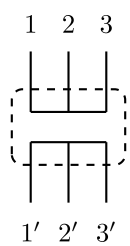
This 3-port is called a "parallel junction". Henry Paynter, who invented bond graphs — which we're gradually getting ready to discuss - also called this 3-port a "0-junction".

And it's also called a "flow junction", which makes some sense, since this 3-port takes the flow coming in and divides it in two.

Just as the mathematical description of a 1-port requires 1 equation, while a 2-port requires 2, the description of a 3-port requires 3. For the parallel junction they are:

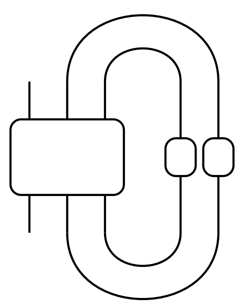
$$\dot{q}_1 + \dot{q}_2 + \dot{q}_3 = 0$$
$$\dot{p}_1 = \dot{p}_2 = \dot{p}_3$$

The first equation says that the total flow through is zero. That's obvious from the design: current can't flow from the top to the bottom. The other equations say that the voltage difference between points 1 and $1'$ equals the voltage difference between points 2 and $2'$, and also that between points 3 and $3'$:

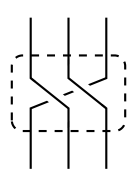


This is clear if you know a tiny bit about electrical circuits: the voltage on each connected component of wire is constant, at least in the idealization we're using. That's because our wires have zero electrical resistance. They're like resistors with resistance 0, and we've seen that the voltage difference across a resistor is the current times the resistance.

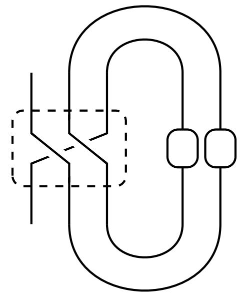
Our second kind of 3-port is called a "series junction". It's a different sort of black box, which you can use to connect two 1-ports in series. You just attach them like this:



What's in this 3-port? Just wires, but now arranged a different way:



See how it works?



The series junction is also called a "1-junction" or "effort junction". This makes sense, since the equations defining this 3-port are exactly like the equations for the previous one, but with effort and flow switched!

$$\dot{p}_1 + \dot{p}_2 + \dot{p}_3 = 0$$
$$\dot{q}_1 = \dot{q}_2 = \dot{q}_3$$

I'll let you figure out why these are true.

By the way: this "duality" between the series junction and parallel junction — the way they're the same, but with the roles of effort and flow switched — is actually the tip of a big iceberg! There's a duality between effort and flow. This duality is related to Fourier duality, since in quantum physics the Fourier transform interchanges momentum and position — the quantities whose time derivatives are the effort and flow variables in classical mechanics. But this duality is also related to Poincar duality. For any circuit whose underlying graph is planar, there's a "Poincar dual" circuit where we replace edges by vertices, vertices by edges — and also switch efforts and flows!

I hope to say more about this duality when I reach the more cosmic, grandiose aspects of the long story I'm telling. But if I forget, you'll have to read this:

13) Istvan Vago, *Graph Theory: Application to the Calculation of Electrical Networks*, Elsevier, 1985.

See the section called "The Principal of Duality", on page 77. Also, look on the web for stuff about the "Δ-Y transformation", which is a special case.

If you want to learn more about the 1-ports, 2-ports and 3-ports I've been discussing, let me again recommend this book:

14) Dean C. Karnopp, Donald L. Margolis and Ronald C. Rosenberg, *System Dynamics: a Unified Approach*, Wiley, New York, 1990.

It's good on the abstract concepts, it clearly lays out most of the basic analogies, and it's not very long. It seems to be a modernized version of this earlier book, which has its own homegrown charm:

15) Dean C. Karnopp and Ronald C. Rosenberg, *Analysis and Simulation of Multiport Systems*, MIT Press, Cambridge, Massachusetts, 1968.

For something vastly more detailed, try:

16) Forbes T. Brown, *Engineering System Dynamics: a Unified Graph-Centered Approach*, Taylor and Francis, 2007.

This mammoth tome is 1058 pages long, mainly because it's packed with examples. So, some of the big ideas are a bit hard to spot. But it proves these ideas are useful in many different fields!

Addenda: I thank Tim van Beek for correcting my German spelling. David Roberts says it's questionable whether Bach really composed a six-part fugue on the spot in Frederick's court: contemporary reports say so, but it may be an exaggeration. Theo pointed out that a Mbius strip is not really perfectly suited to a crab canon:

> *Mbius strips are cool, and the Crab Canon is cool, but they're essentially different. Notice that in the video, the two players are still going around the Mbius strip in opposite directions, and each is keeping to its own side of the strip. Moreover, in spite of visually putting in a twist, the "backwards" player is really playing the sound in a mirror, not upside-down. There's a reason Bach calls it "crab": it can be played forward and backward.*
>
> *Thus, the correct visualization is not a Mbius strip at all, but the orbifold with boundary formed by reflecting the rectangle in half. Making this is easy: take a piece of paper with the music written on one side, and fold it so that the music is on the outside. In this way, each side of the orbifold has half the music on it. Now start at the non-mirror end, but play both sides, reflecting through the orbifold boundary and continuing until you're back where you started.*

Someone with the monicker Mixo Lydian sent me an email answering my question about why Japan has currents of two different frequencies — 50 and 60 cycles per second. As expected, there's some history involved:

> *The 50Hz/60Hz divide in Japan is due to historic reasons. Towards the end of the Meiji era, Japan made the switch from DC to AC. Tokyo Dento (Japan's first electric power company) adopted 50Hz German AEG generators while its rival Osaka Dento decided to adopt 60Hz American GE generators to power their respective electric grids.*
>
> *Neighboring regions built their electric infrastructure adopting either Tokyo or Osaka standards which has led to a east-west / Tokyo-Osaka divide which continues to the present day, the exact border being the Fuji river which runs thru Shizuoka prefecture: east of the river the frequency is 50Hz, west of river the frequency is 60Hz.*
>
> *This has hilarious consequences for the town of Shibakawa-cho, Shizuoka. The Fuji river runs directly thru Shibakawa-cho: some parts of town use 50Hz while others use 60Hz! All you have to do is cross a bridge to alternate between (intentional pun)! Hope this has been helpful.*

For more discussion, visit the n-Category Caf.

> Mathematics is not a careful march down a well-cleared highway, but a journey into a strange wilderness, where the explorers often get lost. Rigour should be a signal to the historian that the maps have been made, and the real explorers have gone elsewhere.
>
> — *W. S. Anglin*

Week 292

January 29, 2010

I've been telling a long tale about analogies between different physical systems. Now I finally want to tell you about "bond graphs" — a technique engineers use to exploit these analogies. I'll just say a bit, but hopefully enough so you get the basic idea.

Then I'll sketch a rough classification of physical systems, and discuss the different kinds of math used to study these different kinds of systems. I'll only talk about systems from the realm of classical mechanics! To people who love classical field theory, quantum mechanics and quantum field theory, this may seem odd. Isn't classical mechanics completely understood?

Well, nothing is ever completely understood. But there are some reasons that mathematical physicists like myself tend to slip into thinking classical mechanics is better understood than it actually is. We love conservation of energy! Taking this seriously led to a wonderful framework called Hamiltonian mechanics, which we have been studying for over a century. We know a lot about that. We all studied it in school.

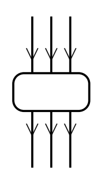
But the Hamiltonian mechanics we learn in school needs to be generalized a bit to nicely handle "dissipative systems", with friction — or more general "open systems", where energy can flow in and out of the boundary between the system and its environment.

(A dissipative system is really a special sort of open system, since energy lost to friction is really energy that goes into the environment. But the study of dissipative systems has not been fully integrated into the study of open systems! So, people often treat them separately, and I may do that too, now and then — even though it's probably dumb.)

Anyway, while lovers of beauty have the freedom to neglect dissipative systems and open systems if they want, engineers don't! Every machine interacts with its environment, and loses energy to its environment thanks to friction. Furthermore, machines are made of pieces, or "components". Each piece is an open system! Each component needs to be understood on its own... but then engineers need to understand how the components fit together and interact.

A lot of engineers do this with the help of "bond graphs". These are diagrams that describe systems made of various kinds of components: electrical, mechanical, hydraulic, chemical, and so on. The one thing all these components have in common is *power*. Energy can flow from one component to another. The rate of energy flow is called "power", and bond graphs are designed to make this easy to keep track of.

The idea behind bond graphs is very simple. I've been describing various "n-ports" lately, and I've drawn pictures of them. In my pictures, a 3-port looked like this:



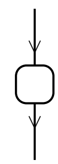
In the case of an electrical system, this means 3 wires coming in and 3 going out. More generally, an n-port is a gadget with n inputs and n outputs, where the flow into each input equals the flow out of the corresponding output.

The idea of bond graphs is to draw these pictures differently. Don't draw individual wires! Instead, draw each pair of wires — input and output — as a single edge!

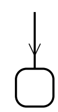
Such an edge is called a "bond". So, an n-port has n "bonds" coming out of it.

Take an electrical resistor, for example. This is a kind of 1-port — an example of what bond graph experts call a "resistance". Mathematically, a resistance is specified by a function relating effort to flow. In the example of an electrical resistor, effort is "voltage" and "flow" is current.

It's pretty natural to draw a resistor like this:

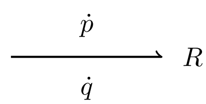


But in the world of bond graphs, people draw it more like this:



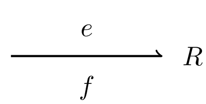
One "bond" for two "wires"!

Actually, to be a bit more honest, they draw it a bit more like this:



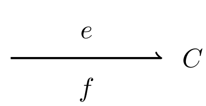
Now the arrow is pointing across instead of down. There's a bond coming in at the left, but nothing coming out at right. The $\dot{p}$ and $\dot{q}$ let us know that the resistance is relating effort to flow. The R stands for resistance.

To be even more honest, I should admit that most bond graph people use "e" for effort and "f" for flow. So, they really draw something like this:



But I want to stick with $\dot{p}$ and $\dot{q}$.

Another famous 1-port is a capacitor. Bond graph people draw it like this:



A nice example of a 2-port is a transformer. I explained this back in "Week 292". Bond graph people draw it like this:

$$\xrightarrow[f_1]{e_1} TF \xrightarrow[f_2]{e_2}$$

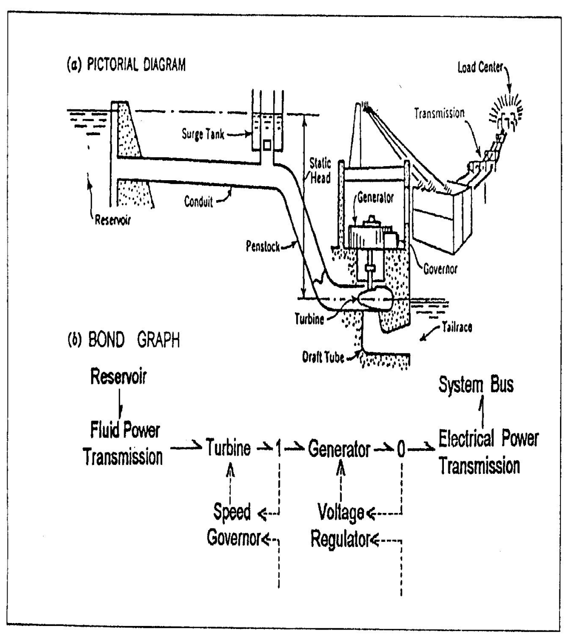
There's a bond coming in at left and a bond coming out at right: 2 bonds for a 2-port. Similarly, a 3-port has 3 bonds coming out of it, and so on. You'll see some 3-ports soon!

Bond graphs were invented by Henry Paynter. You can read his story here:

1) Henry M. Paynter, *The gestation and birth of bond graphs*, `http://www.me.utexas.edu/~longoria/paynter/hmp/Bondgraphs.html`

It reminds me slightly of Hamilton's story about inventing quaternions while walking down the river with his wife to a meeting of the Royal Irish Academy. Just slightly... but you can tell that bond graphs thrilled Paynter as much as quaternions thrilled Hamilton. I want to quote a bit, and comment on it. He begins:

> *Bond graphs were born in their present form on April 24, 1959. They were the direct outgrowth of my academic and professional experience during two previous decades. My MIT undergraduate and graduate training was centered on hydroelectric engineering, as was my work at Puget Power and my 8 years teaching in the Civil Engineering Department at MIT. This involved all aspects of the typical power plant indicated below.*



Hydroelectric plant.

Here he shows a picture of a hydroelectric power plant, and the bond graph that abstractly describes it. A reservoir full of water acts as an "effort source", since water pressure is a form of "effort". Water flows down a conduit and turns a turbine. Here hydraulic power gets converted to mechanical power. Then the turbine turns a generator, which produces electricity — so here, mechanical power is getting converted to electrical power.

There are also some feedback loops, shown with dotted arrows. Solid arrows represent power, while dotted arrows represent "signals". For example, the turbine sends a signal about how fast it's turning to a a gadget called a "speed governor". If the turbine starts turning too fast or too slow, this gadget reduces or increases the flow of water to the turbine. There's a similar feedback loop involving the generator.

I haven't said anything about "signals" yet. The idea here is that information can be transmitted using negligible power. For example, you don't slow a turbine down much by attaching a little gadget that measures how fast it's turning. So, we can often get away with pretending that a signal carries *no* power. But this idealization breaks down in quantum mechanics — so if we ever get to talking about "quantum bond graphs", we'll have to rethink things. In fact, the idealization often breaks down long before quantum effects kick in! I think this aspect of bond graphs deserves more mathematical study.

You can see in Paynter's picture that the reservoir is a 1-port. It's an example of an "effort source" — a kind of 1-port I explained back in "Week 290". The turbine and generator are 2-ports, since they have an input and output. These are both "transformers" — a kind of 2-port I explained last week. You'll also see that the feedback loops involve some 3-ports. I explained these too last week. The 0 stands for a "parallel junction", and the 1 stands for a "series junction".

Paynter continues:

> *This training and experience in hydroelectric power actually forced certain insights upon me, most particularly an awareness of the strong analogies existing between:*
>
> *TRANSMISSION (fluid pipes & electric lines);*
> *TRANSDUCTION (turbines & generators);*
> *CONTROL (speed governors & voltage regulators).*
>
> *When these analogous devices were reduced to equations for computer simulation, distinctions became completely blurred.*
>
> *Even before 1957 it was obvious that the above hydroelectric plant necessarily involved two energy-converting transduction multiports: the hydraulic turbine converting fluid power to rotary shaft power and the electrical generator converting this shaft power into polyphase AC power. Moreover, the strict analogy between these two devices holds right down to the local field-continuum level. Thus the fluid vorticity corresponds precisely to the current density and the fluid circulation to the magnetizing current, so that even the turbine blades correspond to the generator pole pieces! In dynamic consequence, both these highly efficient components become 2-port gyrators, with parasitic losses. Common sense dictated that such compelling analogies implied some underlying common generalization from which other beneficial specializations might ensue. My efforts were also strongly motivated by a preoccupation with the logical philosophy un-*

derlying analogies in general. Such concerns were much earlier formalized by the mathematician, Eliakim Hastings Moore, in the following dictum:

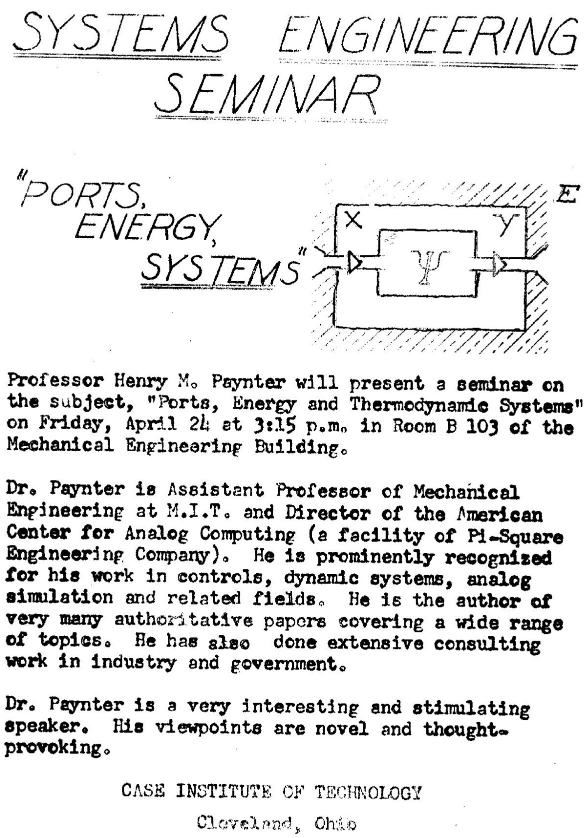
> *"We lay down a fundamental principle of generalization by abstraction: The existence of analogies between central features of various theories implies the existence of a general theory which underlies the particular theories and unifies them with respect to those central features. . . "*

But Paynter only got the idea of bond graphs when he moved from the civil engineering department to the mechanical engineering department at MIT. Then comes the part that reminds me of Hamilton's famous description of inventing the quaternions. In a letter to his pal Tait, Hamilton wrote:

> *To-morrow will be the 15th birthday of the Quaternions. They started into life, or light, full grown, on the 16th of October, 1843, as I was walking with Lady Hamilton to Dublin, and came up to Brougham Bridge, which my boys have since called the Quaternion Bridge. That is to say, I then and there felt the galvanic circuit of thought* close*; and the sparks which fell from it were the* fundamental equations between i, j, k*; exactly such as I have used them ever since. I pulled out on the spot a pocket-book, which still exists, and made an entry, on which,* at the very moment*, I felt that it might be worth my while to expend the labour of at least ten (or it might be fifteen) years to come. But then it is fair to say that this was because I felt a problem to have been at that moment solved — an intellectual want relieved — which had haunted me for at least fifteen years before.*

Paynter writes:

> *In 1954, I moved over to the MIT mechanical engineering department to establish the first systems engineering subjects at MIT. It was this specific task which 5 years later produced bond graphs, drawing naturally upon all the attitudes and experience indicated above. So it was on April 24, 1959, when I was to deliver the lecture as posted below, I awoke that morning with the idea of the* $0,1$*-junctions somehow planted in my head overnight! Moreover the very symbols* $(0,1)$ *for Kirchoff's current law and Kirchoff's voltage law, respectively, made direct the correspondence between circuit duality and logical duality. (The limited use of these* 3*-ports in the hydro plant bond graph above hardly does justice to their role in rendering bond graphs a complete and formal discipline.)*


SYSTEMS ENGINEERING SEMINAR

"PORTS, ENERGY, SYSTEMS"

Professor Henry M. Paynter will present a seminar on the subject, "Ports, Energy and Thermodynamic Systems" on Friday, April 24 at 3:15 p.m. in Room B 103 of the Mechanical Engineering Building.

Dr. Paynter is Assistant Professor of Mechanical Engineering at M.I.T. and Director of the American Center for Analog Computing (a facility of Pi-Square Engineering Company). He is prominently recognized for his work in controls, dynamic systems, analog simulation and related fields. He is the author of very many authoritative papers covering a wide range of topics. He has also done extensive consulting work in industry and government.

Dr. Paynter is a very interesting and stimulating speaker. His viewpoints are novel and thought-provoking.

CASE INSTITUTE OF TECHNOLOGY

Cleveland, Ohio

The picture on the talk poster makes it clear that even without knowing it, Henry Paynter was helping invent a branch of applied *category theory* — a branch where physical systems that interact with their neighbors are treated as *morphisms*.

(If you don't understand what Paynter means by Kirchoff's current law and Kirchoff's voltage law, and "the correspondence between circuit duality and logical duality", you can see a bit of explanation in the Addenda.)

Paynter's book on bond graphs came out in 1961:

2) Henry M. Paynter, *Analysis and Design of Engineering Systems*, MIT Press, Cambridge, Massachusetts, 1961.

About a decade later, bond graphs were taken up by many others authors, notably Jean Thoma:

3) Jean U. Thoma, *Introduction to Bond Graphs and Their Applications*, Pergamon Press, Oxford, 1975.

By now there is a vast literature on bond graphs. This website is a bit broken, but you can use it to get a huge bibliography:

4) bondgraph.info, Journal articles: http://www.bondgraph.info/journal.html, Books: http://www.bondgraph.info/books.html

I've listed some of my favorite books in previous Weeks. But if you want an online introduction to bond graphs, start here:

5) Wikipedia, "Bond graph", http://en.wikipedia.org/wiki/Bond_graph

It covers a topic I haven't even mentioned, the "causal stroke". And it gives some examples of how to convert bond graphs into differential equations. If you read the talk page for this article, you'll see that various people have found it confusing at various times. But it's gotten a lot clearer since then, and I hope people keep improving it. I'll probably work on it myself a bit.

Then, watch some of these:

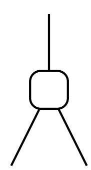
6) Soumitro Banerjee, *Dynamics of physical systems*, lectures on YouTube. Lectures 13–19: "The bond graph approach". Available at http://www.youtube.com/view_play_list?p=D074EEC1EBEFAE

These lectures are very thoughtful and nice. I thank C. J. Fearnley for pointing them out.

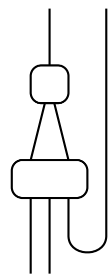
Now I'd like to veer off in a slightly different direction, and ponder the various n-ports we've seen, and how they fit into different branches of mathematical physics. My goal is to dig a bit deeper into the mathematics behind this big analogy chart:

	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Mechanics (translation)	position	velocity	momentum	force
Mechanics (rotation)	angle	angular velocity	angular momentum	torque
Electronics	charge	current	flux linkage	voltage
Hydraulics	volume	flow	pressure momentum	pressure
Thermodynamics	entropy	entropy flow	temperature momentum	temperature
Chemistry	moles	molar flow	chemical momentum	chemical potential

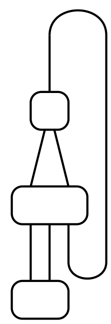
But I won't be using the language of bond graphs! The reason is that I want to talk about gizmos where the total number of inputs and outputs can be either even or odd, like this:



Even though I'm talking about all sorts of physical systems, I'll use the language of electronics, and call these gizmos "circuit elements". We can stick these together to form "circuits", like this:



Category theorists will instantly see that circuits are morphisms in something like a compact closed symmetric monoidal category! But the rest of you shouldn't worry your pretty heads about that yet. The main thing to note is that we have "open" circuits that have wires coming in and out, as above, but also "closed" ones that don't, like this:



I will also call circuits "systems", since that's what physicists call them. And indeed, they often speak of "closed" systems, which don't interact with their environment, or "open" ones, which do.

We've seen different kinds of circuit elements. First there are "active" circuit elements, which can absorb and emit energy, and for which we cannot define a Hamiltonian that makes energy conserved. Then there are "passive" ones, which come in two kinds:

- "conservative" circuit elements, which can absorb and emit energy, but for which we can define a Hamiltonian that makes energy conserved.
- "dissipative" circuit elements, which only absorb energy, and for which we cannot define a Hamiltonian that makes energy conserved.

Not surprisingly, circuits made of different kinds of circuit elements want to be studied in different ways! We get pulled into all sorts of nice mathematics this way — especially symplectic geometry, Hodge theory, and complex analysis. Here's a quick survey:

1. If we have a circuit made of conservative circuit elements, we can study it using the **principle of least action**. So, we can define a Lagrangian for such a circuit, which is a function $L(q, \dot{q})$ of the displacements (q) and flows ($\dot{q}$) of all its wires. This function is a sum of terms, also called Lagrangians, one for each circuit element. The "action" for the circuit is the integral over time of the total Lagrangian. The

circuit will roughly do whatever minimizes this action. It's lazy! (Experts will know I'm lying slightly here.)

By using a Legendre transform, we can compute p as a function of $\dot{q}$. Then we can work out the "Hamiltonian" of our circuit, as follows:

$$H(p, q) = L(q, \dot{q}) - p \cdot \dot{q}$$

Like the Lagrangian, this Hamiltonian will be the sum of Hamiltonians for each piece — and I've told you what those Hamiltonians are for all the conservative circuit elements I've mentioned.

If the overall circuit is closed, no wires coming in or going out, its Hamiltonian will be conserved in the strongest sense:

$$\frac{dH}{dt} = 0$$

There are elegant ways to study closed systems using Hamiltonian mechanics — or in other words, symplectic geometry. This is something mathematical physicists know well.

We can also examine the special case of a conservative closed system in a static state, meaning that p and q don't depend on time. The behavior of such systems is governed by the **principle of least energy**: it will choose p and q that minimize the Hamiltonian $H(q, p)$.

If the circuit is open, we need a slight generalization of Hamiltonian mechanics that can handle systems that interact with their environment. Open systems are less familiar in mathematical physics — but as I explained in "Week 290", they're studied in "control theory". Open systems obey a weaker form of energy conservation, called the "power balance equation".

2. If we have a circuit made of only dissipative circuit elements, we can study it using the **principle of least power**. At least in a stationary state, where the efforts $\dot{p}$ and flows $\dot{q}$ don't depend on time, the system will act to minimize the power

$$\dot{p} \cdot \dot{q}$$

Using this we can often solve for $\dot{q}$ as a function of $\dot{p}$ or vice versa.

The principal of least power is closely related to other minimum principles in physics. For example, if we build a network of resistors and fix the voltages on the wires coming in and out, the voltages on the network will obey a discretized version of the Laplace equation. This is the equation a function f satisfies when it minimizes

$$\int (\nabla f)^2$$

So, circuits of this second kind are closely related to the Laplace equation, differential forms, Hodge theory and the like. In fact this is why Raoul Bott switched from electrical engineering to differential topology!

3. If we have a circuit made of only conservative and dissipative circuit elements, it's called "passive". In a linear passive circuit we can multiply the vector of efforts $\dot{p}$ by an operator called the "admittance matrix" to get the vector of flows $\dot{q}$:

$$\dot{q} = A\dot{p}$$

Or, we can take the inverse of this operator and get the "impedance matrix", which tells us the flows as a function of the efforts:

$$\dot{p} = Z\dot{q}$$

Here both efforts and flows are functions of time. Taking a Fourier transform in the time variable we get a version of the impedance matrix that's a function of the dual variable: "frequency". And if the circuit is built from linear resistances and inertances, we'll get a *rational* function of frequency! The poles of this function contain juicy information. So, we can use complex analysis to study such circuits. This is very standard stuff in electrical engineering.

4. If we have a circuit made of linear passive circuit elements together with effort and flow sources, we can still use the ideas that worked in case 3, but now we get an *inhomogeneous* linear equation like

$$\dot{p} = A\dot{q} + e$$

where e comes from the effort sources. This is called Norton's theorem. Alternatively we can write

$$\dot{q} = Z\dot{p} + f$$

where f comes from the flow sources. This is called Thvenin's theorem. Again, these are standard results that electrical engineers learn — but don't forget, they apply to *all* the systems in our chart of analogies!

5. If we drop the linearity assumption and consider fully general circuits, things get more complicated.

I hope in future Weeks to say more about this stuff. I hope you see there are some strange and interesting patterns here — like this trio:

1. **the principle of least action**
2. **the principle of least energy**
3. **the principle of least power**

We've seen the trio of action, energy and power before, back in "Week 289". Action has units of energy $\times$ time; power has units of energy/time. How do these three minimum principles fit together in a unified whole? I know how to derive the principal of least energy from the principle of least action by starting with a conservative system and imposing the assumption that it's static. But how about the principle of least power? Where does this come from?

I don't know. If you know, tell me!

I'll tell you a bit about linear dissipative circuits and Hodge theory next week. But if you're impatient to learn circuit theory — or at least know what books are lying next to my bed — let me give some references!

This book is quite good:

7) Brian D. O. Anderson and Sumeth Vongpanitlerd, *Network Analysis and Synthesis: A Modern Systems Theory Approach*, Dover Publications, Mineola, New York, 2006.

There's a lot of complex analysis in here! Some is familiar, but there's also a lot we mathematicians don't usually learn: the Positive Real Lemma, the Bounded Real Lemma, and more.

Speaking of Norton's and Thvenin's theorems, these articles demystify those:

8) Wikipedia, "Norton's theorem", `http://en.wikipedia.org/wiki/Norton%27s_theorem`

9) Wikipedia, "Thvenin's theorem", `http://en.wikipedia.org/wiki/Th%C3%A9venin%27s_theorem`

These articles cover only circuits with one input and one output which are made from flow sources, effort sources and linear resistances. I know the results generalize to circuits where we also allow capacticances and inertances, and above I was willing to wager that they apply to circuits with as many inputs and outputs as you like.

This book of classic papers is also good:

10) M. E. van Valkenburg, ed., *Circuit Theory: Foundations and Classical Contributions*, Dowden, Hutchington and Ross, Stroudsburg, Pennsylvania, 1974.

I mentioned Raoul Bott — mathematicians will be pleased and perhaps surprised to see a 1948 paper by him here! It's 5 paragraphs long, and it solved a basic problem.

Addenda: Joris Vankerschaver writes:

> *I've been following TWF the past few weeks extra carefully since I'm also interested in a systematic approach of electrical circuits, mechanical systems, and the like. For this issue of TWF, I was wondering if you know whether there is a link between the* Hamilton-Pontryagin *(HP) variational principle and the action principles you mentioned. I hope you don't mind me asking a direct question like this. . .*
>
> *The HP principle consists of taking a Lagrangian $L(q, v)$, and thinking of v as an extra coordinate. We then add the condition that $\dot{q} = v$ as an extra constraint with a Lagrange multiplier p, to get a functional of the form*
>
> $$S(q, v, p) = \int p(\dot{q} - v) + L(q, v)dt$$
>
> *where q, v, p are varied independently. The result is the Euler-Lagrange equations in implicit form, together with Hamilton's equations and the Legendre transformation.*

> *I've added a PDF draft where these calculations are done in more detail.*
>
> *H. Yoshimura (who is a classical bond grapher) and J. Marsden have been working on this variational principle and apparently used it to great effect in circuit theory as well. Circuits typically have degenerate Lagrangians and nonholonomic constraints, and the HP principle handles these very well. But the HP variational principle has been re-discovered many times before.*
>
> *The equations of motion obtained from the HP principle can also be incorporated into a Dirac structure, which (according to van der Schaft and Maschke) is very well suited for interconnection purposes (where power is conserved). So again, I was wondering if there was a link between the HP principle and what you are considering.*
>
> *I would be very interested in hearing your thoughts about this. I am really looking forward to the next few issues of TWF!*

Unfortunately I had to tell him that I've never heard of the Hamilton-Pontryagin variational principle. More to learn!

In "Week 292" I briefly mentioned the "dual" of a planar electrical circuit, where we switch series junctions and parallel junctions, switch efforts (voltages) and flows (currents), and so on. You'll note that in my quote of Paynter he was drawing a perhaps slightly obscure analogy between this sort of duality and what he called "logical" duality. This is usually called "De Morgan duality": it's a symmetry of classical logic, which consists of switching true and false, AND and OR, and so on. In binary notation it consists of switching 0 and 1. This is why Paynter called a parallel junction a "0-junction" and the series junction a "1-junction". I didn't really understand the connection until Chris Weed explained it:

> *John,*
>
> *The point is pretty trivial, but it's perhaps worth reminding the reader of the immediate connection to the dualities of Boolean algebra.*
>
> *More precisely, a series connection of two switches can be considered to implement the function* AND(x, y) *— defined by the usual truth table — if one encodes 'True' as a closed connection and 'False' as an open connection. Of course, this can be considered a convention. If 'True' is encoded by an open connection and 'False' is encoded by a closed connection, then a series connection of the switches implements* OR(x, y).
>
> *Of course, the "dual" of this little exposition applies to a parallel connection.*
>
> *I have a continuing interest in these simple observations in connection with an idea that I attempted to present in a post on Math Overflow. For understandable reasons, it didn't generate much of a response. Perhaps a few people were motivated to chew on it for a while.*
>
> *— Chris*

Francesco La Tella writes:

> *Greetings John,*

> *I am having fun reading about bond graphs (in an attempt to stay awake during the graveyard shift at work) on your site.*
>
> *With regard to the principle of least power (PLP). I remember writing an assignment for the subject of Optimization II, in my senior year of an undergraduate maths degree. Basically we were asked to use mathematical optimization techniques to model an appropriate physical, industrial, financial, etc. system in order to determine optimal operational parameters or values. Most of my classmates chose typical, classic, textbook problems from one of the many fine textbooks available to us. However, having had a vague recollection, at the time, to a reference in a 1989 (circa?) issue of Electronics & Wireless World which touched on this very subject, I got to digging.*
>
> *In brief, it turns out that, the distributions of voltages and currents in electrical networks (circuits), containing both active and passive circuit elements, can be solved for by using a stationary-power-condition dictated by the principle of least power. Using this idea an objective function is formulated in which each term describes the power dissipated in each of the circuit elements comprising the network. Since the objective is bivariate, one only needs to find the stationary point in this "power manifold" to determine the real, physical values of currents and voltages in and around all circuit elements.*
>
> *The situation is only slightly complicated by the presence of active and reactive circuit elements, but is covered sufficiently by a generalized version of this concept.*
>
> *Over the years I've had occasion to mention this alternative technique for circuit analysis to many of my electrical engineer colleagues and friends, only to be surprised that ALL were blissfully unaware of this very elegant yet very useful solution. It's unfortunate that engineers today are taught nodal analysis (Kirchhoff's current law & Kirchoff's voltage law), a little linear algebra and perhaps some physics, certainly lots of experience using circuit-CAD packages, but no time exploring alternative possibilities. In contrast ALL my physicist friends are intuitively, if not explicitly attuned to the existence of the unifying power of the three principles PLA, PLE and PLP, and all their possibilities.*
>
> *Thank you for helping to keep my brain cells active.*
>
> *Kind regards,*
>
> *Francesco La Tella*

For more discussion, visit the n-Category Caf.

> I was born not knowing and have had only a little time to change that here and there
>
> — *Richard Feynman*

Week 293

February 5, 2010

This week I want to list a bunch of recent papers and books on n-categories. Then I'll tell you about a conference on the math of environmental sustainability and green technology. And then I'll continue my story about electrical circuits. But first...

This column started with some vague dreams about n-categories and physics. Thanks to a lot of smart youngsters — and a few smart oldsters — these dreams are now well on their way to becoming reality. They don't need my help anymore! I need to find some new dreams. So, "Week 300" will be the last issue of This Week's Finds in Mathematical Physics.

I still like learning things by explaining them. When I start work at the Centre for Quantum Technologies this summer, I'll want to tell you about that. And I've realized that our little planet needs my help a lot more than the abstract structure of the universe does! The deep secrets of math and physics are endlessly engrossing — but they can wait, and other things can't. So, I'm trying to learn more about ecology, economics, and technology. And I'd like to talk more about those.

So, I plan to start a new column. Not completely new, just a bit different from this. I'll call it This Week's Finds, and drop the "in Mathematical Physics". That should be sufficiently vague that I can talk about whatever I want.

I'll make some changes in format, too. For example, I won't keep writing each issue in ASCII and putting it on the usenet newsgroups. Sorry, but that's too much work.

I also want to start a new blog, since the n-Category Cafe is not the optimal place for talking about things like the melting of Arctic ice. But I don't know what to call this new blog — or where it should reside. Any suggestions?

I may still talk about fancy math and physics now and then. Or even a lot. We'll see. But if you want to learn about n-categories, you don't need me. There's a *lot* to read these days. I mentioned Carlos Simpson's book in "Week 291" — that's one good place to start. Here's another introduction:

1) John Baez and Peter May, *Towards Higher Categories*, Springer, 2009. Also available at `http://ncatlab.org/johnbaez/show/Towards+Higher+Categories`

This has a bunch of papers in it, namely:

- John Baez and Michael Shulman, Lectures on n-categories and cohomology.
- Julia Bergner, A survey of $(\infty, 1)$-categories.
- Simona Paoli, Internal categorical structures in homotopical algebra.
- Stephen Lack, A 2-categories companion.
- Lawrence Breen, Notes on 1- and 2-gerbes.
- Ross Street, An Australian conspectus of higher categories.

After browsing these, you should probably start studying $(\infty, 1)$-categories, which are ∞-categories where all the n-morphisms for $n > 1$ are invertible. There are a few different approaches, but luckily they're nicely connected by some results described in Julia Bergner's paper. Two of the most important approaches are "Segal spaces" and "quasicategories". For the latter, start here:

2) Andre Joyal, "The Theory of Quasicategories and Its Applications", `http://www.crm.cat/HigherCategories/hc2.pdf`

and then go here:

3) Jacob Lurie, *Higher Topos Theory*, Princeton U. Press, 2009. Also available at `http://www.math.harvard.edu/~lurie/papers/highertopoi.pdf`

This book is 925 pages long! Luckily, Lurie writes well. After setting up the machinery, he went on to use $(\infty, 1)$-categories to revolutionize algebraic geometry:

4) Jacob Lurie, "Derived algebraic geometry I: stable infinity-categories", available as `arXiv:math/0608228`.
"Derived algebraic geometry II: noncommutative algebra", available as `arXiv:math/0702299`.
"Derived algebraic geometry III: commutative algebra", available as `arXiv:math/0703204`.
"Derived algebraic geometry IV: deformation theory", available as `arXiv:0709.3091`.
"Derived algebraic geometry V: structured spaces", available as `arXiv:0905.0459`.
"Derived algebraic geometry VI: E_k algebras", available as `arXiv:0911.0018`.

For related work, try these:

5) David Ben-Zvi, John Francis and David Nadler, "Integral transforms and Drinfeld centers in derived algebraic geometry", available as `arXiv:0805.0157`.

6) David Ben-Zvi and David Nadler, "The character theory of a complex group", available as `arXiv:0904.1247`.

Lurie is now using (∞, n)-categories to study topological quantum field theory. He's making precise and proving some old conjectures that James Dolan and I made:

7) Jacob Lurie, "On the classification of topological field theories", available as `arXiv:0905.0465`.

Jonathan Woolf is doing it in a somewhat different way, which I hope will be unified with Lurie's work eventually:

8) Jonathan Woolf, "Transversal homotopy theory", available as `arXiv:0910.3322`.

All this stuff is starting to transform math in amazing ways. And I hope physics, too — though so far, it's mainly helping us understand the physics we already have.

Meanwhile, I've been trying to figure out something else to do. Like a lot of academics who think about beautiful abstractions and soar happily from one conference to another, I'm always feeling a bit guilty, wondering what I could do to help "save the planet". Yes, we recycle and turn off the lights when we're not in the room. If we all do just a little bit... a little will get done. But surely mathematicians have the skills to do more!

But what?

I'm sure lots of you have had such thoughts. That's probably why Rachel Levy ran this conference last weekend:

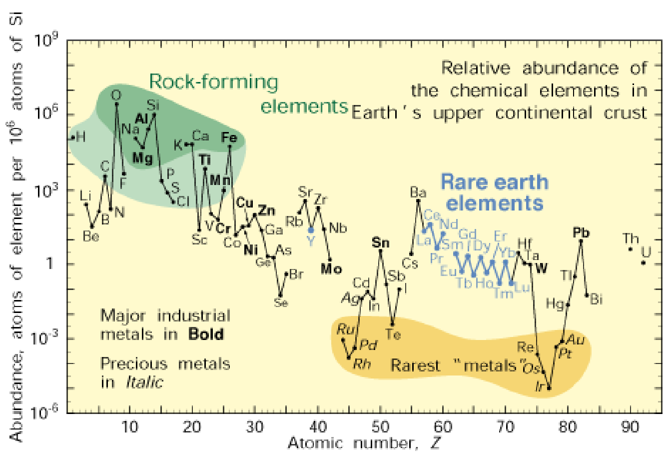
9) *Conference on the Mathematics of Environmental Sustainability and Green Technology*, Harvey Mudd College, Claremont, California, Friday–Saturday, January 29–30, 2010. Organized by Rachel Levy.

Here's a quick brain dump of what I learned.

First, Harry Atwater of Caltech gave a talk on photovoltaic solar power:

10) Atwater Research Group, `http://daedalus.caltech.edu/`

The efficiency of silicon crystal solar cells peaked out at 24% in 2000. Fancy "multijunctions" get up to 40% and are still improving. But they use fancy materials like gallium arsenide, gallium indium phosphide, and rare earth metals like tellurium. The world currently uses 13 terawatts of power. The US uses 3. But building just 1 terawatt of these fancy photovoltaics would use up more rare substances than we can get our hands on:



11) Gordon B. Haxel, James B. Hedrick, and Greta J. Orris, "Rare earth elements — critical resources for high technology", *US Geological Survey Fact Sheet* **087-02**, available at `http://pubs.usgs.gov/fs/2002/fs087-02/`

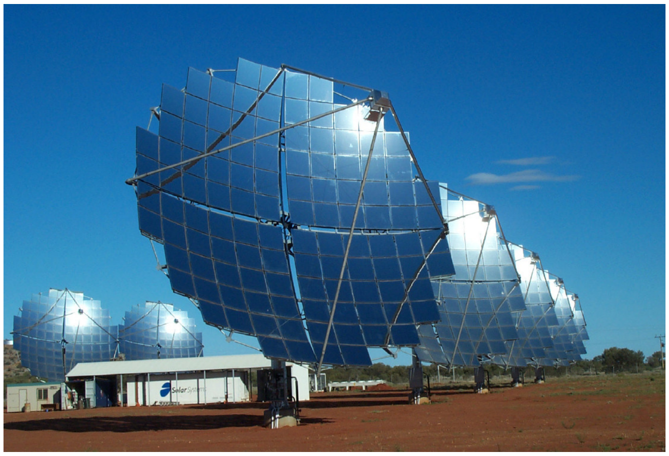
So, if we want solar power, we need to keep thinking about silicon and use as many tricks as possible to boost its efficiency.

There are some limits. In 1961, Shockley and Quiesser wrote a paper on the limiting efficiency of a solar cell. It's limited by thermodynamical reasons! Since anything that can absorb energy can also emit it, any solar cell also acts as a light-emitting diode, turning electric power back into light:

12) W. Shockley and H. J. Queisser, "Detailed balance limit of efficiency of p-n junction solar cells", *J. Appl. Phys.* **32** (1961) 510–519.

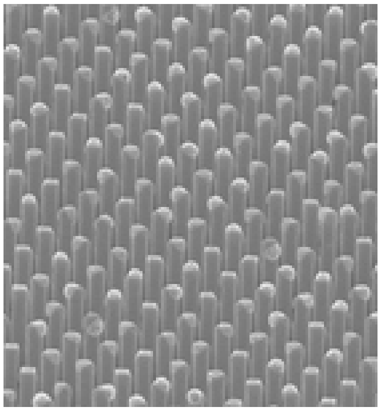
13) Wikipedia, "Schockley-Quiesser limit", `http://en.wikipedia.org/wiki/Shockley%E2%80%93Queisser_limi`

What are the tricks used to approach this theoretical efficiency? Multijunctions use layers of different materials to catch photons of different frequencies. The materials are expensive, so people use a lens to focus more sunlight on the photovoltaic cell. The same is true even for silicon — see the Umuwa Solar Power Station in Australia. But then the cells get hot and need to be cooled.



Roughening the surface of a solar cell promotes light trapping, by large factors! Light bounces around ergodically and has more chances to get absorbed and turned into useful power. There are theoretical limits on how well this trick works. But those limits were derived using ray optics, where we assume light moves in straight lines. So, we can beat those limits by leaving the regime where the ray-optics approximation holds good. In other words, make the surface complicated at length scales comparable to the wavelength at light.

For example: we can grow silicon wires from vapor! They can form densely packed structures that absorb more light:



14) B. M. Kayes, H. A. Atwater, and N. S. Lewis, "Comparison of the device physics principles of planar and radial p-n junction nanorod solar cells", *J. Appl. Phys.* **97** (2005), 114302.

James R. Maiolo III, Brendan M. Kayes, Michael A. Filler, Morgan C. Putnam, Michael D. Kelzenberg, Harry A. Atwater and Nathan S. Lewis, "High aspect ratio silicon wire array photoelectrochemical cells", *J. Am. Chem. Soc.* **129** (2007), 12346–12347.

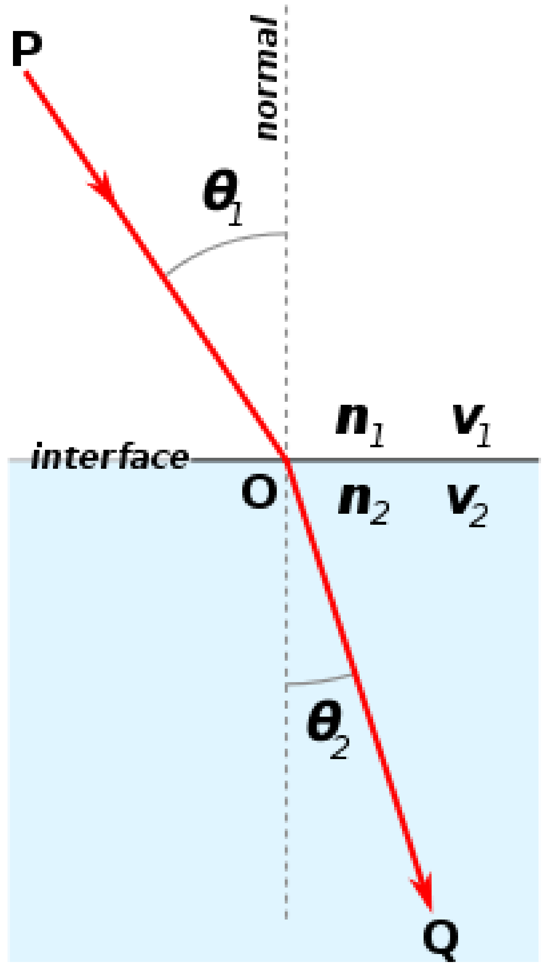
Also, with such structures the charge carriers don't need to travel so far to get from the n-type material to the p-type material. This also boosts efficiency.

There are other tricks, still just under development. Using quasiparticles called "surface plasmons" we can adjust the dispersion relations to create materials with really low group velocity. Slow light has more time to get absorbed! We can also create "metamaterials" whose refractive index is really wacky — like $n = -5$!

I should explain this a bit, in case you don't understand. Remember, the refractive index of a substance is the inverse of the speed of light in that substance — in units where the speed of light in vacuum equals 1. When light passes from material 1 to material 2, it takes the path of least time — at least in the ray-optics approximation. Using this you can show Snell's law:

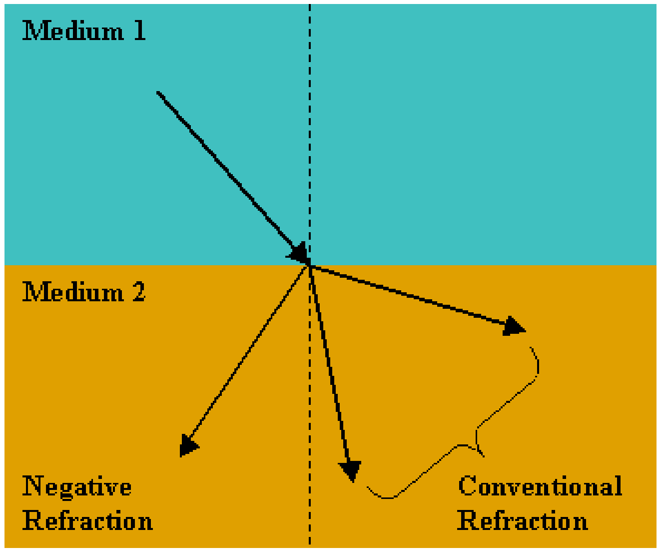
$$\frac{\sin(\theta_1)}{\sin(\theta_2)} = \frac{n_2}{n_1}$$

where n_i is the index of refraction in the ith material and θ_i is the angle between the light's path and the line normal to the interface between materials:



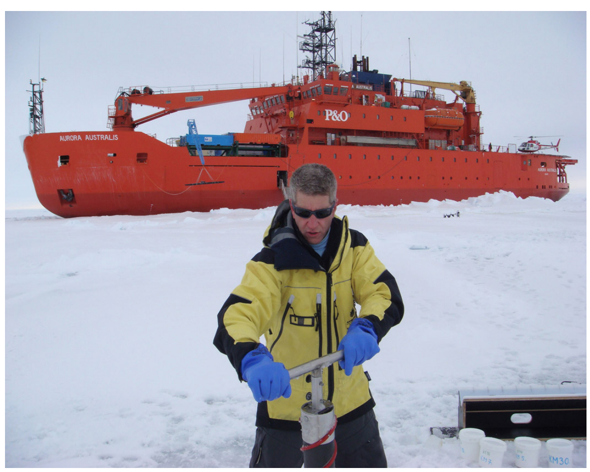
Air has an index of refraction close to 1. Glass has an index of refraction greater than 1. So, when light passes from air to glass, it "straightens out": its path becomes closer to perpendicular to the air-glass interface. When light passes from glass to air, the reverse happens: the light bends more. But the sine of an angle can never exceed 1 — so sometimes Snell's law has no solution. Then the light gets stuck! More precisely, it's forced to bounce back into the glass. This is called "total internal reflection", and the easiest way to see it is not with glass, but water. Dive into a swimming pool and look up from below. You'll only see the sky in a limited disk. Outside that, you'll see total internal reflection.

Okay, that's stuff everyone learns in optics. But *negative* indices of refraction are much weirder! The light entering such a material will bend *backwards*.



Materials with a negative index of refraction also exhibit a reversed version of the ordinary Goos-Hnchen effect. In the ordinary version, light "slips" a little before reflecting during total internal reflection. The "slip" is actually a slight displacement of the light's wave crests from their expected location — a "phase slip". But for a material of negative refractive index, the light slips *backwards*. This allows for resonant states where light gets trapped in thin films. Maybe this can be used to make better solar cells.

Next, Kenneth Golden gave a talk on sea ice, which covers 7–10% of the ocean's surface and is a great detector of global warming. He's a mathematician at the University of Utah who also does measurements in the Arctic and Antarctic. If you want to go to math grad school without becoming a nerd — if you want to brave 70-foot swells, dig trenches in the snow and see emperor penguins — you want Golden as your advisor:

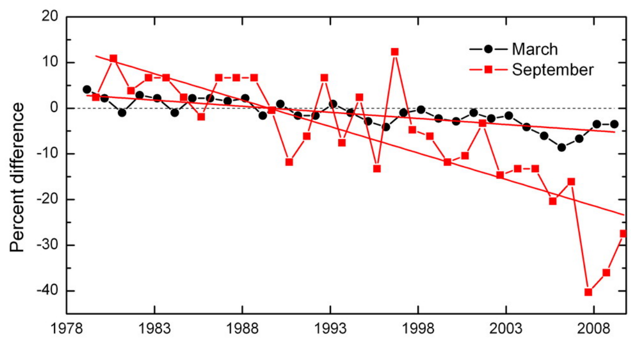


15) Ken Golden's website, `http://www.math.utah.edu/~golden/`

Salt gets incorporated into sea ice via millimeter-scale brine inclusions between ice platelets, forming a "dendritic platelet structure". Melting sea ice forms fresh water in melt ponds atop the ice, while the brine sinks down to form "bottom water" driving the global thermohaline conveyor belt. You've heard of the Gulf Stream, right? Well, that's just part of this story.

When it gets hotter, the Earth's poles get less white, so they absorb more light, making it hotter: this is "ice albedo feedback". Ice albedo feedback is *largely controlled by melt ponds*. So if you're interested in climate change, questions like the following become important: when do melt ponds get larger, and when do they drain out?

Sea ice is diminishing rapidly in the Arctic — much faster than all the existing climate models had predicted. In the Arctic, winter sea ice diminished in area by about 10% from 1978 to 2008. But summer sea ice diminished by about 40%! It took a huge plunge in 2007, leading to a big increase in solar heat input due to the ice albedo effect.



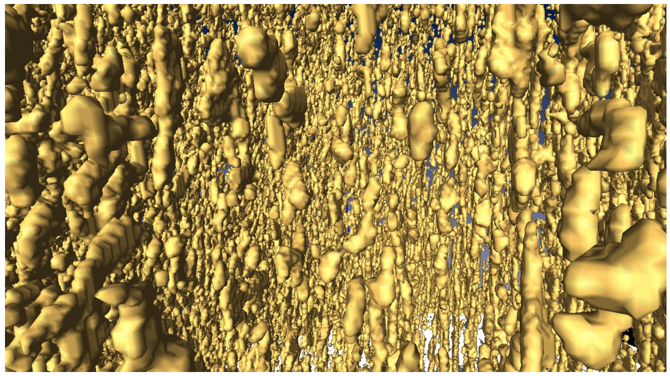
Time series of the percent difference in ice extent in March (the month of ice extent maximum) and September (the month of ice extent minimum) relative to the mean values for the period 1979–2000. Based on a least squares linear regression for the period 1979–2009, the rate of decrease for the March and September ice extents is -2.5% and -8.9% per decade, respectively. Figure from Perovich *et al*.

16) Donald K. Perovich, Jacqueline A. Richter-Menge, Kathleen F. Jones, and Bonnie Light, "Sunlight, water, and ice: Extreme Arctic sea ice melt during the summer of 2007", *Geophysical Research Letters* **35** (2008), L11501. Also available at `http://www.crrel.usace.army.mil/sid/personnel/perovichweb/index1.htm`

There's a lot less sea ice in the Antarctic than in the Arctic. Most of it is the Weddell Sea, and there it seems to be growing, maybe due to increased precipitation.

There's a lot of interesting math involved in understanding the dynamics of sea ice. The ice thickness distribution equation was worked out by Thorndike et al in 1975. The heat equation for ice and snow was worked out by Maykut and Understeiner in 1971. Sea ice dynamics was studied by Kibler.

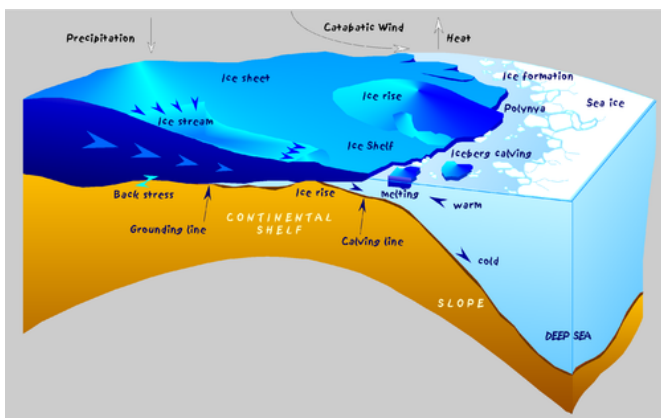
Ice floes have two fractal regimes, one from 1 to 20 meters, another from 100 to 1500 meters. Brine channels have a fractal character well modeled by "diffusion limited aggregation". Brine starts flowing when there's about 5% of brine in the ice — a kind of percolation problem familiar in statistical mechanics. Here's what it looks like when there's 5.7% brine and the temperature is -8 C:

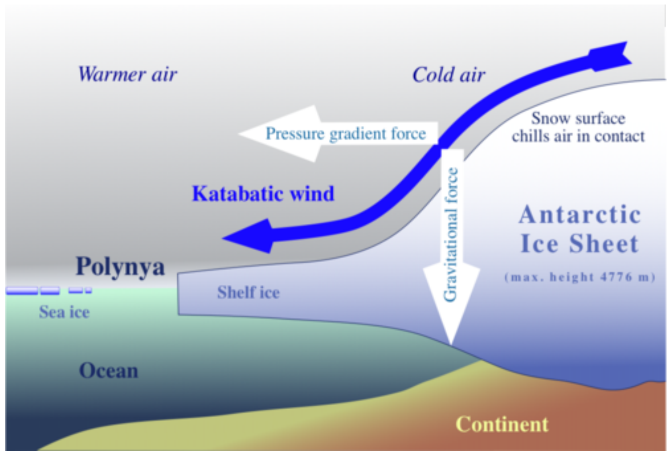


17) Kenneth Golden, "Brine inclusions in a crystal of lab-grown sea ice", `http://www.math.utah.edu/~golden/7.html`

Nobody knows why polycrystalline metals have a log-normal distribution of crystal sizes. Similar behavior, also unexplained, is seen in sea ice.

A "polynya" is an area of open water surrounded by sea ice. Polynyas occupy just 0.001% of the overall area in Antarctic sea ice, but create 1% of the icea. Icy cold katabatic winds blow off the mainland, pushing away ice and creating patches of open water which then refreeze.





There was anomalous export of sea ice through Fran Strait in the 1990s, which may have been one of the preconditions for high ice albedo feedback.

20-40% of sea ice is formed by surface flooding followed by refreezing. This was *not included* in the sea ice models that gave such inaccurate predictions.

The food chain is founded on diatoms. These form "extracellular polymeric substances" — goopy mucus-like stuff made of polysaccharides that protects them and serves as antifreeze. There's a lot of this stuff; the ice gets visibly stained by it.

For more, see:

18) Kenneth M. Golden, "Climate change and the mathematics of transport in sea ice", *AMS Notices*, May 2009. Also available at `http://www.ams.org/notices/200905/`

19) Mathematics Awareness Month, April 2009: "Mathematics and Climate", `http://www.mathaware.org/mam/09/`

Next, Julie Lundquist, who just moved from Lawrence Livermore Labs to the University of Colorado, spoke about wind power:

20) Julie Lunquist, Department of Atmospheric and Oceanic Sciences, University of Colorado, `http://paos.colorado.edu/people/lundquist.php`

With increased reliance on wind, the power grid will need to be redesigned to handle fluctuating power sources. In the US, currently, companies aren't paid for power they generate in excess of the amount they promised to make. So, accurate prediction is a hugely important game. Being off by 1% can cost millions of dollars! Europe has different laws, which encourage firms to maximize the amount of wind power they generate.

If you had your choice about where to build a wind turbine, you'd build it on the ocean or a very flat plain, where the air flows rather smoothly. Hilly terrain leads to annoying turbulence — but sometimes that's your only choice. Then you need to find the best spots, where the turbulence is least bad. Complete simulation of the Navier-Stokes equations is too computationally intensive, so people use fancier tricks. There's a lot of math and physics here.

For weather reports people use "mesoscale simulation" which cleverly treats smaller-scale features in an averaged way — but we need more fine-grained simulations to see how much wind a turbine will get. This is where "large eddy simulation" comes in. Eddy diffusivity is modeled by Monin-Obukhov similarity theory:

21) American Meteorological Society Glossary, "Monin-Obukhov similarity theory", http://amsglossary.allenpress.com/glossary/search?id=monin-obukhov-similarity-theory1

A famous Brookhaven study suggested that the power spectrum of wind has peaks at 4 days, 1/2 day, and 1 minute. This perhaps justifies an approach where different time scales, and thus length scales, are treated separately and the results then combined somehow. The study is actually a bit controversial. But anyway, this is the approach people are taking, and it seems to work.

Night air is stable — but day air is often not, since the ground is hot, and hot air rises. So when a parcel of air moving along hits a hill, it can just shoot upwards, and not come back down! This means lots of turbulence.

The wind turbines at Altamont Pass in California kill more raptors than all other wind farms in the world combined! Old-fashioned wind turbines look like nice places to perch, spelling death to birds. Cracks in concrete attract rodents, which attract raptors, who get killed. The new ones are far better.

For more:

22) National Renewable Energy Laboratory, "Research needs for winds resource characterization", available as http://www.nrel.gov/docs/fy08osti/43521.pdf

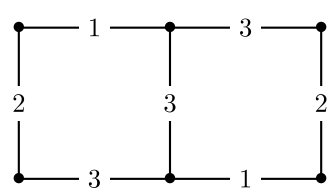
Finally, there was a talk by Ron Lloyd of Fat Spaniel Technologies. This is a company that makes software for solar plants and other sustainable energy companies:

23) Fat Spaniel Technologies, http://www.fatspaniel.com/products/

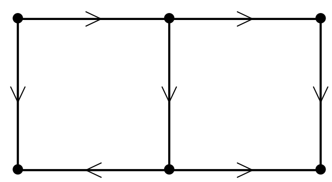
His talk was less technical so I didn't take detailed notes. One big point I took away was this: we need better tools for modelling! This is especially true with the coming of the "smart grid". In its simplest form, this is a power grid that uses lots of data — for example, data about power generation and consumption — to regulate itself and increase efficiency. Surely there will be a lot of math here. Maybe even the topic I've been talking about lately: bond graphs!

But now I want to talk about some very simple aspects of electrical circuits. Last week I listed various kinds of circuits. Now let's go into a bit more detail — starting with the simplest kind: circuits made of just wires and linear resistors, where the currents and voltages are independent of time.

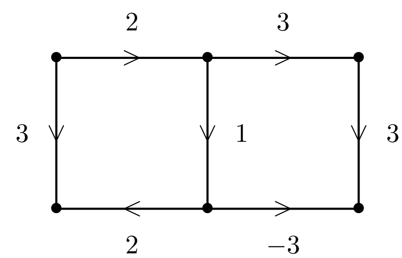
Mathematically, such a circuit is a graph equipped with some extra data. First, each edge has a number associated to it — the "resistance". For example:



Second, we have current flowing through this circuit. To describe this, we first arbitrarily pick an orientation on each edge:

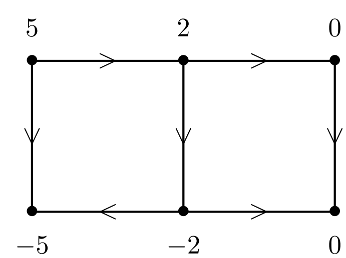


Then we label each edge with a number saying how much "current" is flowing through that edge, in the direction of the arrow:



Electrical engineers call the current I. Mathematically it's good to think of I as a "1-chain": a formal linear combination of oriented edges of our graph, with the coefficients of the linear combination being the numbers shown above.

If we know the current, we can work out a number for each vertex of our graph, saying how much current is flowing out of that vertex, minus how much is flowing in:



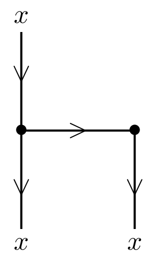
Mathematically we can think of this as a "0-chain": a formal linear combination of the vertices of our graph, with the numbers shown above as coefficients. We call this 0-chain the "boundary" of the 1-chain we started with. Since our current was called I, we call its boundary δI.

Kirchhoff's current law says that

$$\delta I = 0$$

When this holds, let's say our circuit is a "closed". Physically this follows from the law of conservation of electrical charge, together with a reasonable assumption. Current is the flow of charge. If the total current flowing into a vertex wasn't equal to the amount flowing out, charge — positive or negative — would be building up there. But for a closed circuit, we assume it's not.

If a circuit is not closed, let's call it "open". These are interesting too. For example, we might have a circuit like this:

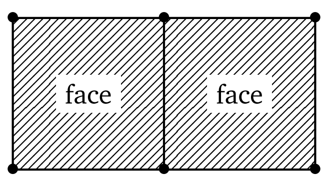


where we have current flowing in the wire on top and flowing out the two wires at bottom. We allow δI to be nonzero at the ends of these wires — the 3 vertices labelled x. This circuit is an "open system" in the sense of "Week 290", because it has these wires dangling out of it. It's not self-contained; we can use it as part of some bigger circuit. We should really formalize this more, but I won't now. Derek Wise did it more generally here:

24) Derek Wise, "Lattice p-form electromagnetism and chain field theory", available as gr-qc/0510033.

The idea here was to get a category where chain complexes are morphisms. In our situation, composing morphisms amounts to gluing the output wires of one circuit into the input wires of another. This is an example of the general philosophy I'm trying to pursue, where open systems are treated as morphisms.

We've talked about 1-chains and 0-chains. . . but we can also back up and talk about 2-chains! Let's suppose our graph is connected — it is in our example — and let's fill it in with enough 2-dimensional "faces" to get something contractible. We can do this in a god-given way if our graph is drawn on the plane: just fill in all the holes!



In electrical engineering these faces are often called "meshes".

This give us a chain complex

$$C_0 \overset{\delta}{\leftarrow} C_1 \overset{\delta}{\leftarrow} C_2$$

Remember, a "chain complex" is just a bunch of vector spaces C_i and linear maps $\delta\colon C_i \to C_{i-1}$, obeying the equation $\delta^2 = 0$. We also get a cochain complex:

$$C^0 \overset{d}{\to} C^1 \overset{d}{\to} C^2$$

meaning a bunch of vector spaces C^i and linear maps $d\colon C^i \to C^{i+1}$, obeying the equation $d^2 = 0$.

As I've already said, it's good to think of the current I as a 1-chain, since then

$$\delta I = 0$$

is Kirchoff's current law. Since our little space is contractible the above equation implies that

$$I = \delta J$$

for some 2-chain J called the "mesh current". This assigns to each face or "mesh" the current flowing around that face.

An electrical circuit also comes with a third piece of data, which I haven't mentioned yet. Each oriented edge should be labelled by a number called the "voltage" across that edge. Electrical engineers call the voltage V. It's good to think of V as a 1-cochain, which assigns to each edge the voltage across that edge.

Why a 1-cochain instead of a 1-chain? Because then

$$dV = 0$$

is the other basic law of electrical circuits — Kirchhoff's voltage law! This law says that the sum of these voltages around a mesh is zero. Since our little space is contractible the above equation implies that

$$V = d\varphi$$

for some 0-cochain φ called the "electrostatic potential". In electrostatics, this potential is a function on space. Here it assigns a number to each vertex of our graph.

Since the space of 1-cochains is the dual of the space of 1-chains, we can take the voltage V and the current I, glom them together, and get a number:

$$V(I)$$

This the "power": that is, the rate at which our network soaks up energy and dissipates it into heat. Note that this is just a fancy version of formula for power that I explained in "Week 290" — power is effort times flow.

I've given you three basic pieces of data labelling our circuit: the resistance R, the current I, and the voltage V. But these aren't independent! Ohm's law says that the voltage across any edge is the current through that times the resistance of that edge. But this remember: current is a 1-chain while voltage is a 1-cochain. So "resistance" can be thought of as a map from 1-chains to 1-cochains:

$$R\colon C_1 \to C^1$$

This lets us write Ohm's law like this:

$$V = RI$$

This, in turn, means the power of our circuit is

$$V(I) = (RI)(I)$$

For physical reasons, this power is always nonnegative. In fact, let's assume it's positive unless $I = 0$. This is just another way of saying that resistance labelling each edge is positive. It can be very interesting to think about circuits with perfectly conducting wires. These would give edges whose resistance is zero. But that's a bit of an idealization, and right now I'd rather allow only *positive* resistances.

Why? Because then we can think of the above formula as the inner product of I with itself! In other words, then there's a unique inner product on 1-chains with

$$(RI)(I) = \langle I, I \rangle$$

In this situation

$$R\colon C_1 \to C^1$$

is the usual isomorphism that we get between a finite-dimensional inner product space and its dual. (For this statement to be true, we'd better assume our graph has finitely many vertices and edges.)

Now, if you've studied de Rham cohomology, all this should start reminding you of Hodge theory. And indeed, it's a baby version of that! So, we're getting a little bit of Hodge theory, but in a setting where our chain complexes are really morphisms in a category. Or more generally, n-morphisms in an n-category!

There's a lot more to say, but that's enough for now. Here are some references on "electrical circuits as chain complexes":

25) Paul Bamberg and Shlomo Sternberg, *A Course of Mathematics for Students of Physics*, Cambridge University, Cambridge, 1982.

Bamberg and Sternberg is a great book overall for folks wanting to get started on mathematical physics. The stuff about circuits starts in chapter 12.

26) P. W. Gross and P. Robert Kotiuga, *Electromagnetic Theory and Computation: A Topological Approach*, Cambridge University Press, 2004.

This book says just a little about electrical circuits of the sort we're discussing, but it says a *lot* about chain complexes and electromagnetism. It's a great place to start if you know some electromagnetism but have never seen a chain complex.

Addenda: I thank Colin Backhurst, G.R.L. Cowan, David Corfield, Mikael Vejdemo Johansson and Tim Silverman for corrections. I thank Garett Leskowitz for pointing out the material in Bamberg and Sternberg's book.

Ed Allen writes:

> *Regarding the silicon technologies for improving efficiency of light capture, the Mazur lab's black silicon projects are something I've been following for a few years:*
>
> *27) Mazur Group, "Optical hyperdoping — black silicon",* `http://mazur-www.harvard.edu/research/detailspage.php?rowid=1`
>
> *28) Wikipedia, "Black silicon",* `http://en.wikipedia.org/wiki/Black_silicon`

29) *Anne-Marie Corley, "Pink silicon is the new black",* Technology Review, *Thursday, July 9, 2009. Also available at* `http://www.technologyreview.com/computing/22975/?a=f`

David Corfield wonders if it's really true that there's a lot less sea ice in the Antarctic than the Arctic:

> *Was that right? Cryosphere has the Arctic sea ice oscillating on average between 5 and 14 million sq km, and the Antarctic between 2 and 15 million sq km.*
>
> *Recently of course that 5 has become 3.*
>
> *Best, David*

For more discussion, visit the n-Category Caf.

> So many young people are forced to specialize in one line or another that a young person can't afford to try and cover this waterfront — only an old fogy who can afford to make a fool of himself. If I don't, who will?
>
> — *John Wheeler*

Week 294

March 21, 2010

Sorry, I've been busy writing papers for the last couple of months. But I'm not done with my story of electrical circuits! It will take a few more episodes for me to get to the really cool part: the symplectic geometry, the complex analysis, and how they fit together using the theory of loop groups. I plan to talk about this in Dennis Sullivan's seminar at the City University of New York later this spring. I haven't written anything about it yet. So, I need to prepare by discussing it here.

You'll understand why I need to prepare if you've heard about Sullivan's seminar. It's a "Russian style" seminar, meaning that it's modeled after Gelfand's famous seminar in Moscow. And what does that mean? Well, Gelfand was famous for asking lots of questions. He wanted to understand all that was said — and he wasn't willing to put up with any nonsense. So, his seminar went on for hours and hours, leaving the speaker exhausted.

Here's a nice description of it:

1) Simon Gindikin, "Essay on the Moscow Gelfand Seminar", in *Advances in Soviet Mathematics* **16**, eds. Sergei Gelfand and Simon Gindikin, 1993. Available at `http://www.math.rutgers.edu/home/gelfand/`

Let me quote a bit:

> *The Gelfand seminar was always an important event in the very vivid mathematical life in Moscow, and, doubtless, one of its leading centers. A considerable number of the best Moscow mathematicians participated in it at one time or another. Mathematicians from other cities used all possible pretexts to visit it. I recall how a group of Leningrad students agreed to take turns to come to Moscow on Mondays (the day of the seminar, to which other events were linked), and then would retell their friends what they had heard there. There were several excellent and very popular seminars in Moscow, but nevertheless the Gelfand seminar was always an event.*
>
> *I would like to point out that, on the other hand, the seminar was very important in Gelfand's own personal mathematical life. Many of us witnessed how strongly his activities were focused on the seminar. When, in the early fifties, at the peak of antisemitism, Gelfand was chased out of Moscow University, he applied all his efforts to seminar. The absence of Gelfand at the seminar, even because of illness, was always something out of the ordinary.*
>
> *One cannot avoid mentioning that the general attitude to the seminar was far from unanimous. Criticism mainly concerned its style, which was rather unusual for a scientific seminar. It was a kind of a theater with a unique stage director playing the leading role in the performance and organizing the supporting cast, most of whom had the highest qualifications. I use this metaphor with the utmost seriousness, without any intention to mean that the seminar was some sort of a spectacle. Gelfand had chosen the hardest and most dangerous genre: to demonstrate in public how he understood mathematics. It was an open lesson*

> *in the grasping of mathematics by one of the most amazing mathematicians of our time. This role could be only be played under the most favorable conditions: the genre dictates the rules of the game, which are not always very convenient for the listeners. This means, for example, that the leader follows only his own intuition in the final choice of the topics of the talks, interrupts them with comments and questions (a privilege not granted to other participants) [. . . .] All this is done with extraordinary generosity, a true passion for mathematics.*
>
> *Let me recall some of the stage director's strategems. An important feature were improvisations of various kinds. The course of the seminar could change dramatically at any moment. Another important* mise en scene *involved the "trial listener" game, in which one of the participants (this could be a student as well as a professor) was instructed to keep informing the seminar of his understanding of the talk, and whenever that information was negative, that part of the report would be repeated. A well-qualified trial listener could usually feel when the head of the seminar wanted an occasion for such a repetition. Also, Gelfand himself had the faculty of being "unable to understand" in situations when everyone around was sure that everything is clear. What extraordinary vistas were opened to the listeners, and sometimes even to the mathematician giving the talk, by this ability not to understand. Gelfand liked that old story of the professor complaining about his students: "Fantastically stupid students — five times I repeat proof, already I understand it myself, and still they don't get it."*
>
> *It has remained beyond my understanding how Gelfand could manage all that physically for so many hours. Formally the seminar was supposed to begin at 6 pm, but usually started with an hour's delays. I am convinced that the free conversations before the actual beginning of the seminar were part of the scenario. The seminar would continue without any break until 10 or 10:30 (I have heard that before my time it was even later). The end of the seminar was in constant conflict with the rules and regulations of Moscow State University. Usually at 10 pm the cleaning woman would make her appearance, wishing to close the proceedings to do her job. After the seminar, people wishing to talk to Gelfand would hang around. The elevator would be turned off, and one would have to find the right staircase, so as not to find oneself stuck in front of a locked door, which meant walking back up to the 14th (where else but in Russia is the locking of doors so popular!). The next riddle was to find the only open exit from the building. Then the last problem (of different levels of difficulty for different participants) — how to get home on public transportation, at that time in the process of closing up. Seeing Gelfand home, the last mathematical conversations would conclude the seminar's ritual. Moscow at night was still safe and life seemed so unbelievably beautiful!*

This is a great example of how taking things really seriously, and pursuing them intelligently, with persistent passion, can infuse them with the kind of intensity that leaves echoes resonating decades later.

Sullivan's seminar is also intense, though it plays to a smaller audience. Like Gelfand's, it's set in a tall building: in fact, a 30-story skyscraper called the Graybar Building, right next to Grand Central Station. The first time I was asked to speak there, my talk was supposed to start at 3 pm. But before that, there was an informal "pre-talk" where people discussed math and sat around eating lunch. Someone went down to get sandwiches, and I was asked what kind I wanted. I said I wasn't hungry, but someone who knew better got me one anyway.

My talk started at 3... and it went on until 9! I loved it: here was someone who really wanted to understand my work. None of the usual routine where everyone starts eyeing the clock impatiently as the allotted hour nears its end. It was clear: this seminar would last as long as it took to get the job done. And when we were done, we all went out to dinner... and talked about math.

So, I should get back to my tale of electrical circuits. I'm really just using these as a nice example of physical systems made of components. Part of my goal is to get you interested in "open systems" — systems that interact with their environment. My physics classes emphasized "closed systems", where we assume that we've modelled all the relevant aspects of what's going on, so the interaction with the outside environment is negligible. Why? It lets us use the marvelous techniques of symplectic mechanics — Hamilton's equations, Noether's theorem giving conserved quantities from symmetries, and all that. These techniques don't work for open systems — at least, not until we generalize them. But almost every device we design is an open system, in a crucial way: we do things to it, and it does things for us. So engineers need to think about open systems.

And mathematical physicists should too — because life gets more interesting when you treat every system as having an "interface" through which it interacts with its environment. For starters, this lets you build bigger systems from components by attaching them along their interfaces. We can also formalize the problem of taking a system and decomposing it into smaller subsystems. In engineering this is called "tearing". For example, we can take this electrical circuit:

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and tear it in two like this:

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Giampiero Campa pointed out an article that's full of wisdom about open systems, the history of control theory, and the cultural differences between mathematics and engineering:

2) Jan C. Willems, "In control, almost from the beginning until the day after tomorrow", *European Journal of Control* **13** (2007), 71–81. Also available as `http://homes.esat.kuleuven.be/~jwillems/Articles/JournalArticles/2007.2.pdf`

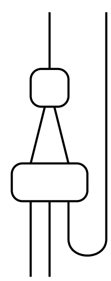
You don't need to know anything about control theory to enjoy this! Well, it helps to know that "control theory" is the art of getting open systems to do what you want. But it's always fun to begin learning a subject by hearing about its history — especially from somebody who was there.

Here's a passage that connects to the point I was just trying to make:

> *One can, one should, ask the question if closed systems, as flows on manifolds and*
>
> $$\frac{dx}{dt} = f(x)$$
>
> *form a good mathematical vantage point from which to embark on the study of dynamics. In my opinion they do not, for a number of reasons. First, in a good theory the state x should be derived from a less structured model. A more serious objection is that closed systems are not good concepts to deal with modeling. A model usually consists of a number of interacting subphenomena that need to be modeled one-by-one. In these sub-models, the influence of the other subsystems needs par force to be viewed as external, and in principle free. Tearing leads to models that are open.*
>
> *If you view a closed system as an interconnection of two systems, these two systems will be open. Systems that take into account unmodeled external influences form therefore a much more logical starting point. Third, many basic laws in physics address open systems. For example, Newton's second law, Maxwell's equations, the gas law, and the first and second laws of thermodynamics. A good setting of dynamics should incorporate these important examples from the beginning. Finally, closed systems put one in the absurd situation that in order to model a system, one ends up having to model also the environment.*

> *These arguments seem obvious and compelling. Twenty five years ago, it was my hope that system theory, with its emphasis on open systems, would by now have been incorporated and accepted as the new starting point for dynamical systems in mathematics. Better, more general, more natural, more apt for modeling, offering interesting new concepts as controllability, observability, dissipativity, model reduction, and with a rich, well developed, domain as linear system theory. It is disappointing that this didn't happen. What seemed like an intellectual imperative did not even begin to happen. Mathematicians and physicists invariably identify dynamical systems with closed systems.*

I think this will change. I think we just need to develop the right framework for open systems. Luckily, a lot of this framework is already available: concepts like operads, n-categories and the like give very general ways of describing how to build big things by gluing together little pieces. For example, a trained mathematician will take one look at this:



and say "that's a morphism in a compact closed category". So, we just need to focus these concepts on the problems of engineering, and explain them in ways that engineers — as opposed to, say, topologists or quantum field theorists — can enjoy.

For a deeper look at Willems' ideas on open systems, try this:

3) Jan C. Willems, "The behavioral approach to open and interconnected systems: modeling by tearing, zooming and linking", *Control Systems Magazine* **27** (2007), 46–99. Also available at `http://homes.esat.kuleuven.be/~jwillems/Articles/JournalArticles/2007.1.pdf`

In particular, people who doubt that engineers could ever enjoy fancy math like operads and n-categories should check out the box near the end, on "polynomial modules and syzygies".

Now, I've been talking recently about "bond graphs". This is a general framework for physical systems which treats variables as coming in groups of four:

- q — displacement
- p — momentum
- $\dot{q}$ — flow
- $\dot{p}$ — effort

If we use the example of a massive object that can move back and forth, q and p stand are its position and momentum, while $\dot{q}$ and $\dot{p}$ are velocity and force. But if we use the example of an electrical circuit, q is charge and p is something fairly obscure called "flux linkage". Then their time derivatives are current, $\dot{q}$, and voltage, $\dot{p}$.

In both these examples the quantity $\dot{q}\dot{p}$ has dimensions of power. Bond graphers consider this very important: the idea is that when we consider mixed systems, like an electrical motor pushing a massive object around, it's *power* that flows from one part to another.

In "Week 289", I listed two examples of systems where $\dot{q}\dot{p}$ does *not* have dimensions of power: thermal systems and economic systems. People do draw bond graphs of these, but they're considered second-class citizens: they're called "pseudo bond graphs".

Jan Willems has some criticisms of the bond graph methodology, including its obession with power — and also its focus on $\dot{q}$ and $\dot{p}$ at the expense of q and p. I've tried to give q and p more importance in my discussion so far, since for people trained in classical mechanics they're of utmost importance. But for people trained in electrical circuits, it's $\dot{q}$ and $\dot{p}$ that seem important: they talk about current and voltage all the time, and a bit less about the other other two.

Here's a summary of Jan Willems' criticisms of bond graphs, taken from a little box in the above paper. I'll paraphrase a bit here and there:

> *The tearing, zooming, and linking methodology for modeling interconnected systems advocated and developed in this article has many things in common with bond graphs. Introduced by Paynter in the 1960s, bond graphs are popular as a methodology for modeling interconnected physical systems, especially in mechanical engineering. For modeling physical systems, bond-graph modeling is a superior alternative to signal-flow diagrams and input/output-based modeling procedures.*
>
> *Bond graphs view each system interconnection in terms of power and energy. The variables associated with terminals are assumed to consist of an effort and a flow, where the (inner) product of effort and flow is power. Connections are formalized by junctions. Using a combination of junctions and component subsystems, complex physical systems can be modeled in a systematic way. The power interpretation automatically takes care of conservation of energy. The philosophy underlying bond graphs is, as stated by P.J. Gawthrop and G.P. Bevan,*
>
> *Power is the universal currency of physical systems.*
>
> *The idea that terminal variables come in pairs, an effort and a flow, with efforts preserved at each interconnection and the sum of flows equated to zero at each interconnection, is appealing and deep. But, in addition to weak mathematical underpinnings and unconventional graph notation with half arrows, bond graphs have some shortcomings as a modeling philosophy, as explained in the section "Bond-Graph Modeling". The main points discussed in that section are the following:*
>
> 1. *The requirement that the product of effort and flow must be power is sometimes not natural, for example, in thermal interconnections.*

2. *In connecting terminals of mechanical systems, bondgraph modeling equates velocities, and sets the sum of the forces equal to zero. In reality one ought to equate positions, not velocities. Equating velocities instead of positions leads to incomplete models.*
3. *Interconnections are made by means of terminals, while energy is transferred through ports. Ports involve many terminals simultaneously. The interconnection of two electrical wires involves equating two terminal potentials and putting the sum of two terminal currents to zero. The product of effort, namely, the electrical potential, and flow, namely, the electrical current, for an electrical connection has the dimension of power, but it is not power. Power involves potential differences, while the interconnection constraints involves the terminal potentials themselves. It is not possible to interpret these interconnection constraints as equating the power on both sides of the interconnection point.*
4. *In many interconnections, it is unnecessary to have to worry about conservation of energy.*

Willems has his own methodology, which he explains. I'll need to learn about it!

I'll get into the deeper aspects of electrical circuits next Week. There are just a few leftovers I want to mention now. I told you about five basic 1-ports in "Week 290": resistors, capacitors, inductors, voltage sources and current sources. Each was defined by a single equation involving q, p, $\dot{q}$ and $\dot{p}$, and perhaps the time variable t. These five are the most important 1-ports. But there are some weirder ones worth thinking about. Here they are:

1. A "short circuit". A linear resistor has $\dot{p} = R\dot{q}$. If the resistance R equals *zero*, you get a "short circuit". Now the relation between voltage and current becomes:

   $$\dot{p} = 0$$

   So, there's always zero voltage across this circuit element — it's a perfect conductor. Or in the language of bond graphs: there's always zero effort across this 1-port.

2. An "open circuit". If you take a linear resistor and say its resistance is *infinite*, you get an "open circuit". Now the relation between voltage and current becomes:

   $$\dot{q} = 0$$

   So, there's always zero current through this circuit element — it's a perfect insulator. Or in the language of bond graphs: there's always zero flow through this 1-port. By the way, the word "open" here has nothing to do with "open system".

The point of these examples is that most linear resistors let us treat current as a function of voltage *or* voltage as a function of current, since R is neither zero nor infinite. But in the these two limiting cases — the short circuit and the open circuit — that's not true. To fit these cases neatly in a unified framework, we shouldn't think of the relation between current and voltage as defining a function. It's just a relation!

In the world of algebraic geometry, a relation defined by polynomial equations is called a "correspondence". One way to get a correspondence is by taking the graph of a function. But it's important to go beyond functions to correspondences. And my claim is that this is important in electrical circuits, too.

But here are some even weirder one ports:

3. A "nullator". Here we bend the rules for 1-ports and impose *two* equations:

$$\dot{p} = 0$$
$$\dot{q} = 0$$

   I don't think you can actually build this thing! The Wikipedia article sounds downright Zen: "In electronics, a nullator is a theoretical linear, time-invariant one-port defined as having zero current and voltage across its terminals. Nullators are strange in the sense that they simultaneously have properties of both a short (zero voltage) and an open circuit (zero current). They are neither current nor voltage sources, yet both at the same time."

4. A "norator". Here we bend the rules in the opposite direction and impose *no* equations: $$$$ Yes, that's a picture of no equations. Truly Zen: what is the sound of no equations clapping? I don't think you can build this thing either! At least, not by itself. . . .

Now, you may wonder why electrical engineers bother talking about things that don't actually exist. That's normally the prerogative of mathematicians. But sometimes if you combine two things that don't exist, you get one that does! This is often how we introduce new kinds of things. For example, $i \times i = -1$ lets us introduce the "imaginary" number i in terms of the "real" number -1.

As far as 1-ports go: if I have one equation too many, and you have one too few, together we're just right. So, there's a 2-port called the "nullor", which is built — theoretically speaking — from a nullator and a norator. Remmber, a 2-port is specified by by two equations involving q_1, $\dot{q}_1$, p_1, $\dot{p}_1$, q_2, $\dot{q}_2$, p_2, $\dot{p}_2$, and perhaps the time variable t. Here are the equations for the nullor:

$$\dot{p}_2 = 0$$
$$\dot{q}_2 = 0$$

So, the first wire acts like a norator while the second acts like a nullator. To see why engineers like this gizmo, try this:

4) Wikipedia, "Nullor", `http://en.wikipedia.org/wiki/Nullor`

For more, try these:

5) Herbert J. Carlin, "Singular network elements", *IEEE Trans. Circuit Theory*, March 1965, vol. **CT-11**, pp. 67–72.

6) P. Kumar and R. Senani, "Bibliography on nullors and their applications in circuit analysis, synthesis and design", *Analog Integrated Circuits and Signal Processing* **33** (2002), 65–76.

Here's the last 1-port I want to mention:

5. The "memristor". This is a 1-port where the momentum p is a function of the displacement q:

$$p = f(q)$$

The function f is usually called the "memristance". It was invented and given this name by Leon Chua in 1971. The idea was that it completes a collection of four closely related 1-ports. In "Week 290" I listed the other three, namely the resistor:

$$\dot{p} = f(\dot{q})$$

the capacitor:

$$q = f(\dot{p})$$

and the inductor:

$$p = f(\dot{q})$$

The memristor came later because it's inherently nonlinear. Why? A *linear* memristor is just a linear resistor, since we can differentiate the linear relationship $p = Mq$ and get $\dot{p} = M\dot{q}$. But if $p = f(q)$ for a nonlinear function f we get something new:

$$\dot{p} = f'(q)\dot{q}$$

So, we see that in general, a memristor acts like a resistor whose resistance is some function of q. But q is the time integral of the current $\dot{q}$. So a nonlinear memristor is like a resistor whose resistance depends on the time integral of the current that has flowed through it! Its resistance depends on its history. So, it has a "memory" — hence the name "memristance".

Memristors have recently been built in a number of ways, which are nicely listed here:

7) Wikipedia, "Memristor", `http://en.wikipedia.org/wiki/Memristor`

Electrical engineering journals are notoriously resistant to the of open access, and I don't think there's a successful equivalent of the "arXiv" in this field. Shame on them! So, you have to nose around to find a freely accessible copy of Chua's original paper on memristors:

8) Leon Chua, "Memristor, the missing circuit element", *IEEE Transactions on Circuit Theory* **18** (1971), 507–519. Also available at `http://www.lane.ufpa.br/rodrigo/chua/Memristor_chua_article.pdf`

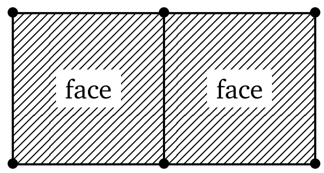
To see what the mechnical or chemical analogue of a memristor is like, try this:

9) G. F. Oster and D. M. Auslander, "The memristor: a new bond graph element", Trans. ASME, *J. Dynamic Systems, Measurement and Control* **94** (1972), 249–252. Also available as `http://nature.berkeley.edu/~goster/pdfs/Memristor.pdf`

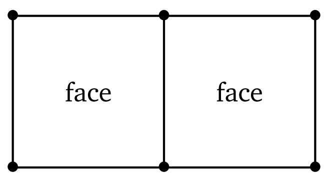
Memristors supposedly have a bunch of interesting applications, but I don't understand them yet. I also don't understand "memcapacitors" and "meminductors". The above PDF file also contains a New Scientist article on the wonders of these.

To wrap up the loose ends, I want to tell you about Tellegen's theorem. Last week I started talking about electrical circuits and chain complexes. I considered circuits built from linear resistors. But now let's talk about completely general electrical circuits.

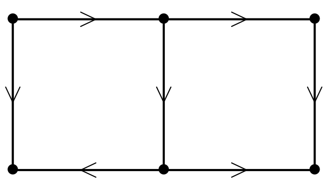
Last time I said an electrical circuit has "vertices", "edges" and "faces":



The faces come in handy: electrical engineers call them "meshes". But they're really just mathematical fictions. When you look at a circuit you don't see faces, just vertices and edges:



So, just for fun, let's leave out the faces today. Let's start with a graph, and orient its edges:



This gives a vector space C_0 consisting of "0-chains": formal linear combinations of vertices. We also get a space C_1 of "1-chains": formal linear combinations of edges, and a linear map

$$C_0 \xleftarrow{\delta} C_1$$

defined as follows: for any edge

$$x \xrightarrow{e} y$$

we have $\delta e = y - x$.

This gadget

$$C_0 \xleftarrow{\delta} C_1$$

is a pathetically puny example of a chain complex: we call it a "2-term chain complex".

If we take the duals of the vector spaces involved, our 2-term chain complex turns around and becomes a 2-term "cochain complex":

$$C^0 \xrightarrow{d} C^1$$

Here d is defined to be the adjoint of δ:

$$(df)(e) = f(\delta e)$$

for any 0-cochain f and any 1-chain e.

What can we do with such pathetically puny mathematical structures?

First, in any electrical circuit, the current I is a 1-chain. Moreover, Kirchoff's current law says:

$$\delta I = 0$$

meaning the total current flowing into any vertex equals the total current flowing out. Last week I stated this law for closed circuits made of resistors, but it's true for any closed circuit as long as the current isn't changing too rapidly with time. Indeed, we can take it as a mathematical definition of what it means for a circuit to be "closed". By "closed" here, I mean that no current is flowing in from outside.

Second, in any electrical circuit, the voltage V is a 1-cochain. Moreover, Kirchoff's voltage law says:

$$V = d\varphi$$

meaning that we can define a "potential" $\varphi(x)$ for each vertex x, with the property that for any edge

$$x \xrightarrow{e} y$$

the voltage $V(e)$ is the difference $\varphi(y) - \varphi(x)$. This law is true for all circuits, as long as the current isn't changing too rapidly with time.

Third, the power dissipated by the circuit equals

$$V(I)$$

Here we are pairing a 1-cochain and a 1-chain to get a number. Again, we talked about this last week, but it's true in general.

But now comes something new!

Let's compute the power $V(I)$ using Kirchoff's voltage law and Kirchoff's current law:

$$V(I) = (d\varphi)(I) = \varphi(\delta I) = 0$$

Hey — it's zero!

At first this might seem strange. The power is always zero???

But maybe it isn't so strange if you think about it: it's a version of conservation of energy. In particular, it fails when we consider circuits with current flowing in from outside: then δI doesn't need to be zero. We don't expect energy conservation in its naive form to hold in that case. Instead, we expect a "power balance equation", as explained in "Week 290".

But maybe it *is* strange. After all, if you have a circuit built from resistors, why should it conserve energy? Didn't I say resistors were dissipative?

I still don't understand this as well as I'd like. The math seems completely trivial to me, but its meaning for circuits still doesn't seem obvious. Can someone explain it in plain English?

Anyway, this result is called "Tellegen's theorem". Clearly you have to be in the right place at the right time to get your name on a theorem! It doesn't have to be hard. It just has to be new and important. If I'd been there when they first discovered numbers, $2 + 2 = 4$ would be called "Baez's theorem".

Still, you might be surprised to discover there's a whole book on Tellegen's theorem:

10) Paul Penfield, Jr., Robert Spence and Simon Duinker, *Tellegen's Theorem and Electrical Networks*, The MIT Press, Cambridge, MA, 1970.

Part of why this result is interesting is that depends on such minimal assumptions. Typically in circuit theory we need to know the voltages V as a function of the currents I, or vice versa, before we can do much. For example, for circuits built from linear resistors, we have a linear map

$$R\colon C_1 \to C^1$$

such that

$$V = RI$$

This is Ohm's law. But Tellegen's theorem doesn't depend on this, or on any relationship between voltages and currents! Indeed, we can take two *different* circuits with the same underlying graph, and let V be the voltage of one circuit at one time, and let I be the current of the other circuit at some other time. We still get

$$V(I) = (d\varphi)(I) = \varphi(\delta I) = 0$$

so long as Kirchoff's voltage and current laws hold for each circuit!

I'm a bit fascinated by this paper, which you can get online:

11) G.F. Oster and C.A. Desoer, "Tellegen's theorem and thermodynamic inequalities", *J. Theor. Biol* **32** (1971), 219–241. Also available at `http://nature.berkeley.edu/~goster/pdfs/Tellagen.pdf`

They give a decent description of Tellegen's theorem, and they use it to derive something they call "Prigogine's theorem", which is supposed to be in here:

12) Ilya Prigogine, *Thermodynamics of Irreversible Processes*, 3rd edition, Wiley, New York, 1968.

I don't understand it well enough to give a beautiful lucid explanation of it. But it's not complicated. It's an inequality that applies to closed circuits built from resistors and capacitors, or analogous systems in chemistry or other subjects.

According to Robert Kotiuga, the chain complex approach to electrical circuits goes back to Weyl:

13) Hermann Weyl, "Reparticion de corriente en una red conductora", *Rev. Mat. Hisp. Amer.* **5** (1923), 153–164.

He also recommend these references:

14) Paul Slepian, *Mathematical Foundations of Network Analysis*, Springer, Berlin, 1968.

15) Harley Flanders, *Differential Forms with Applications to the Physical Sciences*, Dover, New York, 1989, pp. 79–81.

16) Stephen Smale, "On the mathematical foundations of electrical network theory", *J. Diff. Geom.* **7** (1972), 193–210.

17) G. E. Ching, "Topological concepts in networks; an application of homology theory to network analysis", in *Proc. 11th. Midwest Conference on Circuit Theory, University of Notre Dame, 1968*, pp. 165–175.

18) J. P. Roth, "Existence and uniqueness of solutions to electrical network problems via homology sequences", in *Mathematical Aspects of Electrical Network Theory, SIAM-AMS Proceedings III, 1971*, pp. 113–118.

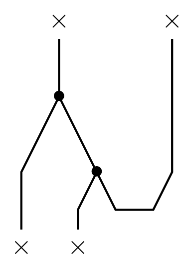
For a quick discussion of Tellegen's theorem, this is also good:

19) Wikipedia, "Tellegen's theorem", `http://en.wikipedia.org/wiki/Tellegen%27s_theorem`

By the way: if you've been paying careful attention and reading between the lines, you'll note that I've been advocating the study of the category where an object is a bunch of points:

$$\times \quad \times \quad \times$$

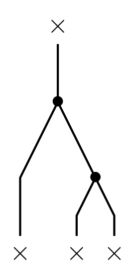
and a morphisms from one bunch of dots to another is graphs with loose ends at the top and bottom:



Here the $\bullet$'s are vertices of the graph, while the the $\times$'s are the "loose ends". We compose these morphisms in the visually evident way, by gluing the loose ends at the top of one to the loose ends at the bottom of the other.

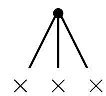
I would like to know all possible slick ways of understanding this category, because it underlies fancier categories where the morphisms are electrical circuits, or Feynman diagrams, or other things.

For one thing, this category is "compact closed". In other words, it's a symmetric monoidal category where every object has a dual. Duality lets us take an input and turn it into an output, like this:

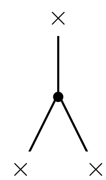


or vice versa.

And in fact, this category is the free compact closed category on one self-dual object, namely x, and one morphism from the unit object to each tensor power of x. The unit object is drawn as the empty set, while the nth tensor power of x is drawn as a list of n $\times$'s. So, for example, when $n = 3$, we have a morphism that looks like a "trivalent vertex":



Using duality we get other trivalent vertices, like this:

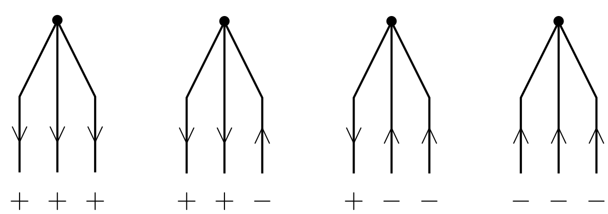


and the upside-down versions of the two I've shown so far.

In this category, a morphism from the unit object to itself is just a finite undirected graph. Or, strictly speaking, it's an isomorphism class of finite undirected graphs!

For electrical circuits it's also nice to equip the edges with orientations, so we can tell whether the current flowing through the edge is positive or negative. At least it *might be* nicer — everyone seems to do it, but maybe it's bit artificial. Anyway, if we want to do this, we should find a category where the morphisms from the unit object to itself are finite *directed* graphs.

I think this is the free compact category on one object $+$, the "positively oriented point" and one morphism from the unit object to any tensor product built by tensoring a bunch of copies of this object $+$ and then a bunch of copies of its dual, $-$. So, among the generating morphisms in this compact closed category, we'll have four trivalent vertices like this:



We then get other trivalent vertices by permuting the outputs or turning outputs into inputs.

I can't help but hope there's a slicker desciption of this category. Anybody know one?

From directed graphs we can get chain complexes, and we've seen how this is important in electrical circuit theory. Can we do something similar to all the morphisms in our category?

Well, we can think of a directed graph as a functor

$$X : G \to \mathsf{Set}$$

where G is category with two objects, "vertex" and "edge", and two morphisms:

$$\mathrm{source}\colon \mathrm{edge} \to \mathrm{vertex}$$
$$\mathrm{target}\colon \mathrm{edge} \to \mathrm{vertex}$$

together with identity morphisms. We can think of G as the "Platonic idea" of a graph, and actual graphs as embodiments of this idea in the world of sets.

Taking this viewpoint, we can compose a directed graph

$$X : G \to \mathsf{Set}$$

with the "free vector space on a set" functor

$$F\colon \mathsf{Set} \to \mathrm{Vect}$$

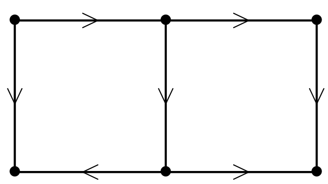
and get a gizmo that's like a graph, but with a vector space of vertices and a vector space of edges. A category theorist might call this a "graph object in Vect".

This may sound scary, but it's not. When we perform this process, we're just letting ourselves take formal linear combinations of vertices, and formal linear combinations of edges. So all we really get is a 2-term chain complex.

This sheds some light on how graphs are related to chain complexes. In fact, we can turn this insight into a little theorem: the category of graph objects in Vect is equivalent to the category of 2-term chain complexes. There's a bit to check here!

In short, waving the magic wand of linearity over the concept of "directed graph", we get the concept of "chain complex". So, there should be some way to take compact closed category I just described and wave the magic wand of linearity over that, too. And the result should be a category important in the theory of electrical circuits.

There's a closely related result that's also interesting. Suppose we have a directed graph:

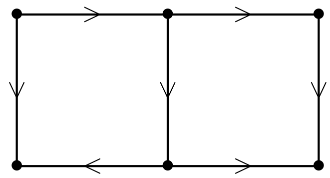


This looks a bit like a category! In fact we can take the free category on a directed graph: this is called a "quiver". And if we wave the magic wand of linearity over a category (in the correct way, since there are different ways), we get a category object in Vect.

But the category of category objects in Vect is *also* equivalent to the category of 2-term chain complexes! Alissa Crans and I called a category object in Vect a "2-vector space", since we can also think of it as a kind of categorified vector space. See Section 3 here:

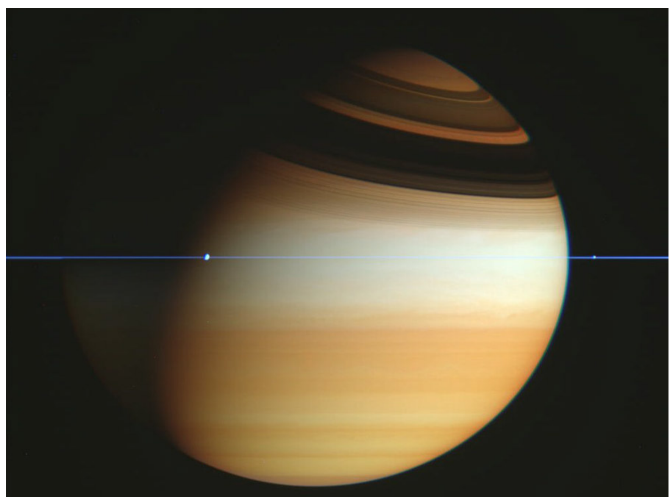
20) John Baez and Alissa Crans, "Higher-dimensional algebra VI: Lie 2-algebras", *Theory and Applications of Categories* **12** (2004), 492–528. Available at `http://www.tac.mta.ca/tac/volumes/12/15/12-15abs.html` and also as `math.QA/0307263`.

This idea was known to Grothendieck quite a while ago — read the paper for the history. But anyway, I think it's neat that we can take the bare bones of an electrical circuit:



and think of it either as a graph, or a category, or a graph or category object in Vect, namely a chain complex — but moreover, we can also think of it as an endomorphism of the unit object in a certain compact closed category!

If you made it this far, you deserve a treat:



21) Astronomy Picture of the Day, "Cassini spacecraft crosses Saturn's ring plane", `http://apod.nasa.gov/apod/ap100215.html`

Saturn's rings edge-on, and a couple of moons, photographed by the Cassini probe! Shadows of the rings are visible on the northern hemisphere.

Addendum: Thomas Riepe points out these remarks by Alain Connes:

> *I soon ran into Dennis Sullivan who used to go up to any newcomers, whatever their field or personality, and ask them questions. He asked questions that you could, superficially, think of as idiotic. But when you started thinking about them, you would soon realize that your answers showed you did not really understand what you were talking about. He has a kind of Socratic power which would push people into a corner, in order to try to understand what they were doing, and so unmask the misunderstandings everyone has. Because everyone*

> *talks about things without necessarily having cleaned out all the hidden corners. He has another remarkable quality; he can explain things you don't know in an incredibly clear and lucid manner. It's by discussing with Dennis that I learnt many of the concepts of differential geometry. He explained them by gestures, without a single formula. I was tremendously lucky to meet him, it forced me to realize that the field I was working in was limited, at least when you see it as tightly closed off. These discussions with Dennis pushed me outside my field, through a visual, oral dialogue.*

This is part of an interview which you can read here:

22) "An interview with Alain Connes, part II", by Catherine Goldstein and George Skandalis, *Newsletter of the European Mathematical Society*, March 2008, pp. 29–34. Also available at `http://www.ems-ph.org/journals/newsletter/pdf/2008-03-67.pdf`

The chemist Jiahao Chen noted some relations between electrical circuits and some aspects of chemistry. I would like to understand these better. He wrote:

> *I am particularly piqued by your recent expositions on bond graphs, and your most recent exposition on bond graphs have finally seem to have touched base with something I have been trying to understand for a very long time. For my PhD work I worked on understanding the flow of electrical charge of atoms when they are bound together in molecules. It turns out that there is a very clean analogue between atomic voltages (electrical potentials) = dE/dq and what we know in chemistry as electronegativity; also, there is an analogue for atomic capacitance = d^2E/dq^2 and what is known as chemical hardness (in the sense of the hard-soft acid-base principle in general chemistry). It has become clear in recent years that the accurate modeling of such charge transfer processes must necessarily take into account not just the charges on atoms, but the flows between them. Then atoms in molecules can be thought of as being voltage-capacitor pairs connected by some kind of network, exactly like an electrical circuit, and the charges can determined by an equation of the form*
>
> *bond capacitance $\times$ charge transfer variables $=$ pairs of voltage differences*
>
> *I have described this construction in the following paper:*
>
> 23) *J. Chen, D. Hundertmark and T. J. Martinez, "A unified theoretical framework for fluctuating-charge models in atom-space and in bond-space",* Journal of Chemical Physics ***129*** *(2008), 214113. DOI: 10.1063/1.3021400. Also available as arXiv:0807.2174.*
>
> *In this paper, I also reported the discovery that despite there being more charge transfer variables (bond variables) than charge variables (vertex variables), it is* always *possible to reformulate equations written in terms of charge transfer variables in terms of equations written into charges, and thus there is a non-obvious 1-1 mapping between these two sets of variables. That this is possible is a non-obvious consequence of Kirchhoff's law, because electrostatic processes*

> *cannot lead to charge flow in a closed loop, and so combinations of bond variables like* $(1 \to 2) + (2 \to 3) + (3 \to 1)$ *must lie in the nullspace of the equation. Thus the working equation*
>
> $$\textit{capacitance} \times \textit{charge} = \textit{transformed voltage}$$
>
> *can be used instead, where the transformation applied to the voltages is a consequence of the topological relationship between the charge transfer variables and charge variables. This transformation turns out to be* exactly *the node branch matrix in the Oster and Desoer paper that was mentioned in your column! (p. 222)*
>
> *I cannot believe that this is merely a coincidence, and certainly your recent exposition on bond graphs seems to be very relevant in a way that could be fruitful to think about. The obvious connection to draw is that the capacitance relation between charges and voltages is exactly one of the four types of* 1*-ports you have described, except that there are as many charges as there are atoms in the molecule. I don't have a good background in algebraic topology, so I don't entirely follow your discussion on chain complexes. Nevertheless I find this interesting that this stuff is somehow related to mundane chemical concepts like electronegativity and charge capacities of atoms, and I hope you would too.*
>
> *Thanks,*
> *Jiahao Chen MIT Chemistry*

In the above comment, E is the energy of an ion and q is its charge, or (up to a factor) the number of electrons attached to it. When Chen says dE/dq is related to "electronegativity", he's referring to how some chemical species — atoms or molecules — attract electrons more than others. This is obviously related to the derivative of energy with respect to the number of electrons. And when he says d^2E/dq^2 is a measure of "hardness", he"s referring to the Pearson acid base concept, or"hard and soft acid and base theory".

In addition to trying to explain the difference between acids and bases, this theory involves a distinction between "hard" and "soft" chemical species. "Hard" ones are small and weakly polarizable, while "soft" ones are big and strongly polarizable. The bigger d^2E/dq^2 is, the harder the species is. Mathematically, a hard species is like a spring that's hard to stretch: remember, a spring that's hard to stretch has a big value of d^2E/dq^2 where E is energy and q is how much the spring is stretched.

I thank Kim Sparre for catching a mistake. He also recommended this reference on electrical circuits and bond graphs:

24) Oyvind Bjrke and Ole Immanuel Franksen, editors, *System Structures in Engineering — Economic Design and Production*, Tapir Publishers, Norway, ca. 1978.

For more discussion, visit the n-Category Caf.

> ... I have almost always felt fortunate to have been able to do research in a mathematics environment. The average competence level is high, there is a

rich history, the subject is stable. All these factors are conducive for science. At the same time, I was never able to feel unequivocally part of the mathematics culture, where, it seems to me, too much value is put on **difficulty** as a virtue in itself. My appreciation for mathematics has more to do with its clarity of thought, its potential of sharply articulating ideas, its virtues as an unambiguous language. I am more inclined to treasure the beauty and importance of Shannon"s ideas on errorless communication, algorithms as the Kalman filter or the FFT, constructs as wavelets and public key cryptography, than the heroics and virtuosity surrounding the four-color problem, Fermat's last theorem, or the Poincar and Riemann conjectures.

— *Jan C. Willems*

Week 295

April 16, 2010

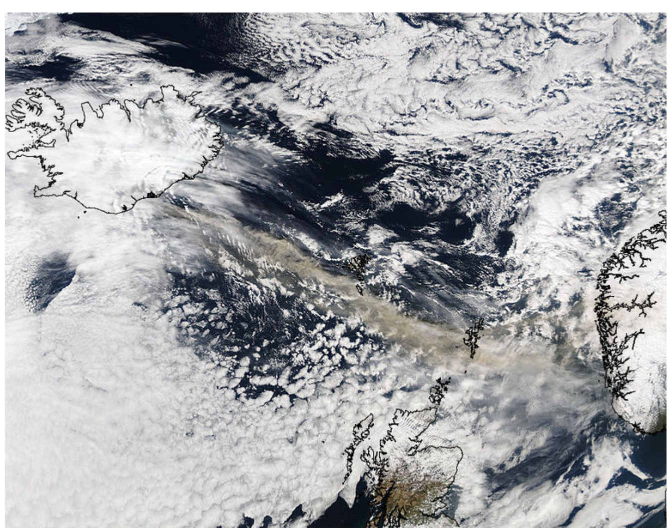
This week I'll talk about the principle of least power, and Poincar duality for electrical circuits, and a generalization of Hamiltonian mechanics that people have introduced for dissipative systems. But first. . . .

photos by Brynjar Gaudi / AP

Now and then the world does something that forcefully reminds us of its power. As you probably know, the Eyjafjallajkull volcano in Iceland is emitting a plume of glass dust which has brought air traffic to a halt over much of Europe. This dust is formed as lava hits cold water and shatters. When sucked into a jet engine, it can heat up to about 1400 degree Celsius and re-melt. And when it cools again, it can stick onto the turbine blades.

This is not good. In 1982, a British Airways Boeing 747 flew through an ash cloud created by a volcano in Indonesia. All four engines cut out. The plane descended from 11,000 meters to 3,700 meters before the engines could be restarted. Whee!

Here's a picture of the Eyjafjallajkull plume, taken yesterday by NASA's "Aqua" satellite:



1) NASA, "Ash plume from Eyjafjallajokull Volcano over the North Atlantic (afternoon overpass)", `http://rapidfire.sci.gsfc.nasa.gov/gallery/?2010105-0415`

Here's what the volcano looked like back in March:

This photo was taken by someone named Bjarni T. He has a great photo gallery here:

2) Bjarni T, "2010 Eruptions of Eyjafjalljkull", `http://www.fotopedia.com/en/2010_eruptions_of_Eyjafjalla` `slideshow/sort/MostVotedFirst/status/default/photos`

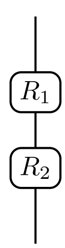
Starting around 1821, the same volcano erupted and put out ash for about 6 months. What will it do this time? Nobody seems to know. If it goes on long enough, will people invent some sort of ash filter for jet engines?

Oh well. Back to electrical circuits. . .

I want to explain the "principle of minimum power" and how we can use it to understand electrical circuits built from linear resistors. In future Weeks this will lead us to some symplectic geometry, complex analysis and loop groups. But I want to start with some very basic stuff! I want to illustrate the principle of minimum power by using it to solve two basic problems: resistors in series and resistors in parallel. But first I should work out the answers to these problems using a more standard textbook approach — just in case you haven't seen this stuff already.

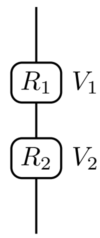
In the textbook approach, we'll use Kirchoff's voltage and current laws over and over again. I explained these laws in "Week 293" and "Week 294" — so if necessary, you can either review what I said there, or just nod and act like you understand what I'm doing.

First, suppose we have two resistors "in series". This means they're stuck together end to end, like this:



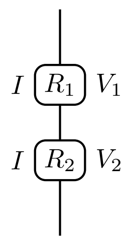
What happens when we put a voltage across this circuit? How much current will flow through?

To answer this, fix the voltage across the whole circuit, say V. By Kirchoff's voltage law, this is the sum of the voltages across the individual resistors, say V_1 and V_2:



$$\boxed{V = V_1 + V_2}$$

Next let's think about the current flowing through each resistor. By Kirchhoff's current law, the current through the first resistor must equal the current through the second one. So, let's call this current I in each case:



$$\boxed{V = V_1 + V_2}$$

Now, Ohm's law says that the voltage across a linear resistor equals the current through it times its resistance. Let's say our resistors are linear. So, we get:

$$IR_1 = V_1$$

and

$$IR_2 = V_2$$

Adding these two equations we get:

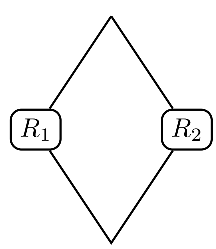
$$I(R_1 + R_2) = V$$

This looks like Ohm's law again, but now for a resistor with resistance

$$R_1 + R_2.$$

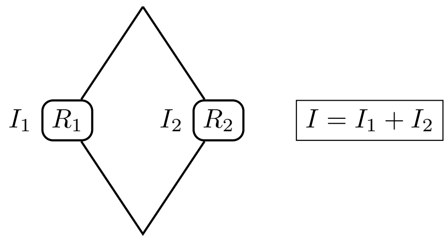
The moral: two resistors in series act like a single resistor whose resistance is the sum of theirs!

Next, suppose we have two resistors "in parallel". This means they're stuck together side by side, like this:

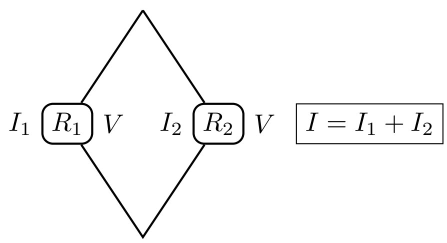


What happens when we make some current flow through this circuit? What will the voltage across it be?

To answer this, fix the current through the whole circuit, say I. By Kirchoff's current law, this is the sum of the currents through the individual resistors, say I_1 and I_2:



Next let's think about the voltage across each resistor. By Kirchhoff's voltage law, the voltage across the first resistor must equal the voltage across the second one. So, let's call this voltage V in each case:



Now, Ohm's law says that the current through a linear resistor equals the voltage across it divided by its resistance. So, if our resistors are linear, we get

$$I_1 = \frac{V}{R_1}$$

and

$$I_2 = \frac{V}{R_2}$$

Adding these two equations we get:

$$I = V\left(\frac{1}{R_1} + \frac{1}{R_2}\right)$$

In our previous problem we were adding up resistances. Now we're adding up reciprocals of resistances. Luckily, there's a name for the reciprocal of a resistance: it's called an "admittance".

The moral: two resistors in parallel act like a single resistor whose admittance is the sum of theirs!

And there's also another moral. If you compare this problem to the previous one, you'll see that everything was almost exactly the same! In fact, I repeated a lot of sentences almost word for word. I just switched certain concepts, which come in pairs:

- current and voltage

- series and parallel
- resistance and admittance

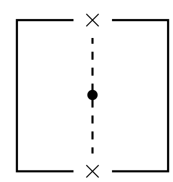
In fact, switching concepts like this is an example of Poincar duality for electrical circuits, as mentioned in "Week 291".

You may know Poincar duality for graphs drawn on a sphere: you get a new graph from an old one by:

- drawing a new vertex in the middle of each old face,
- replacing each edge with a new one that crosses the old one, and
- drawing a new face centered at each old vertex.

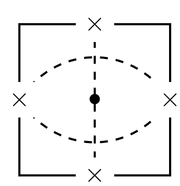
This works fine for "closed" planar circuits — but for circuits with input and output wires, like we've got here, we need Poincar duality for graphs drawn on a closed disk! This is should probably be called "Poincar-Lefschetz duality".

Instead of giving you a long-winded description of how this works, let me just illustrate it. We start with two resistors in series. This is a graph with two edges and three vertices drawn on something that's topologically a closed disk. Let's draw it on a rectangle:

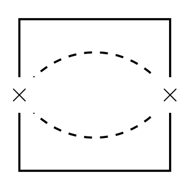


The two dashed edges are the resistors. The two vertices on the boundary of the square, drawn as $\times$'s, are the "input" and "output" vertices. There's also a vertex in the interior of the square, drawn as a little $\bullet$.

Now let's superimpose the Poincar dual graph:



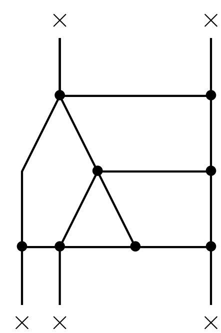
This is a mess, so now let's remove the original graph:



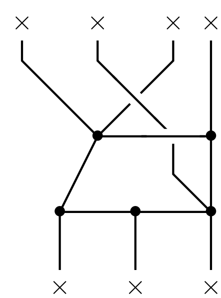
This Poincar dual graph shows two resistors in parallel! There's an "input" at left connected to an "output" at right by two edges, each with a resistor on it. In case you're wondering, the difference between "input" and "output" is purely conventional here.

Poincar duality is cool. But now let's solve the same problems - resistors in series and resistors in parallel — using the "principle of least power". Here's what the principle says. Suppose we have any circuit made of resistors and we fix boundary conditions at the wires leading in and out. Then the circuit will do whatever it takes to minimize the amount of power it uses — that is, turns into heat.

What do I mean by "boundary conditions"? Well, first of all, I'm thinking of an electrical circuit as a graph with resistors on its edges, and with some special vertices that we think of as inputs and outputs:



The inputs and outputs are marked as $\times$'s here. I've drawn a planar graph, but we could also have a nonplanar one, like this:



(Poincar duality works best for planar circuits, but I'm still struggling to find its place in the grand scheme of things — for example, how it permeates the big set of analogies between different physical systems that I explained starting in "Week 288".)

But what do I mean by "boundary conditions"? Well, one sort of boundary condition is to fix the "electrostatic potential" at the input and output vertices of our graph. Remember from last week that the electrostatic potential is a function φ on the vertices of our graph. So, we'll specify the value of this function at the input and output vertices. Then we'll compute its values at all the other vertices using the principle of minimum power.

To do this, we need to remember some stuff from "Week 293" and "Week 294". First, for any edge

$$x \stackrel{e}{\to} y$$

the voltage across that edge, $V(e)$, is given by

$$V(e) = \varphi(y) - - - \varphi(x)$$

Second, since we have a circuit made of linear resistors, the current $I(e)$ through that edge obeys Ohm's law:

$$V(e) = I(e)R(e)$$

where $R(e)$ is the resistance. Third, the power consumed by that edge will be

$$P(e) = V(e)I(e)$$

The principle of minimum power says: fix φ at the input and output vertices. Then, to find φ at the other vertices, just minimize the total power:

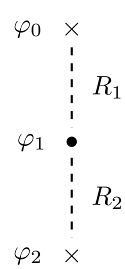
$$P = \sum_e P(e)$$

Using all the equations I've lined up, we see that the total power is indeed a function of φ, since:

$$P(e) = \frac{(\varphi(y) - \varphi(x))^2}{R(e)}$$

The total power is a quadratic function in a bunch of variables, so it's easy to minimize.

Let's actually do this for two resistors in series:



We need to find φ_1 that minimizes the total power

$$P = \frac{(\varphi_1 - \varphi_0)^2}{R_1} + \frac{(\varphi_2 - \varphi_1)^2}{R_2}$$

So, we differentiate P with respect to φ_1 and set the derivative to zero:

$$\frac{2(\varphi_1 - \varphi_0)}{R_1} - \frac{2(\varphi_2 - \varphi_1)}{R_2} = 0$$

This implies that

$$\frac{V_1}{R_1} = \frac{V_2}{R_2}$$

where V_1 and V_2 are the voltages across our two resistors.

By Ohm's law, voltage divided by resistance is current. So, we get

$$I_1 = I_2$$

where I_1 and I_2 are the currents through our two resistors. Hey — the current flowing through the first resistor equals the current flowing through the second one! That's no surprise: it's a special case of Kirchoff's current law! But the cool part is that we *derived* Kirchhoff's current law from the principle of minimum power. This works quite generally, not just in this baby example.

Since the currents I_1 and I_2 are equal, let's call them both I. Then we're back to the textbook approach to this problem. Ohm's law says

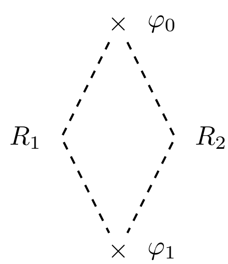
$$IR_1 = V_1$$

and

$$IR_2 = V_2$$

Adding these equations, we see that when you put resistors in series, their resistances add.

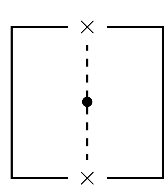
Okay, now let's try two resistors in parallel:



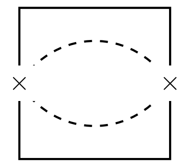
This problem is oddly boring. There are no vertices except the input and the output, so the minimization problem is trivial! If we fix the potential at the input and output, we instantly know the voltages across the two resistors, and then using Ohm's law we get the currents.

Why was this problem more boring than two resistors in series? Shouldn't they be very similar? After all, they're Poincar duals of each other!

Well, yeah. But the problem is, we're not using the Poincar dual boundary conditions. For the resistors in series we had a graph with a vertex in the middle:



For the resistors in parallel we have a graph with a face in the middle:



So, to treat the resistors in parallel in a Poincar dual way, we should use boundary conditions that involve faces rather than vertices. I talked about these faces back in "Week 293": electrical engineers call them "meshes". Each mesh has a current flowing around it. So, our boundary conditions should specify the current flowing around each input or output mesh: that is, each mesh that touches the boundary of our rectangle. We should then find currents flowing around the internal meshes that minimize the total power. And in the process, we should be able to derive Kirchhoff's *voltage* law.

All this could be further illuminated using the chain complex approach I outlined in "Week 293". Let me just sketch how that goes. We can associate a cochain complex to our circuit:

$$C^0 \xrightarrow{d} C^1 \xrightarrow{d} C^2$$

The electrostatic potential φ is a 0-cochain and the voltage

$$V = d\varphi$$

is a 1-cochain. As we've seen, the total power is

$$P = \sum_e \frac{V(e)^2}{R(e)}$$

We can write this in a slicker way using an inner product on the space of 1-cochains:

$$P = \langle V, V \rangle$$

The principle of minimum power says we should find the electrostatic potential φ that minimizes the total power subject to some boundary conditions. So, we're trying to minimize

$$P = \langle d\varphi, d\varphi \rangle$$

while holding φ fixed at some "input and output vertices". If you know some mathematical physics you'll see this is just a discretized version of the minimum principle that gives Laplace's equation!

There's also a dual version of this whole story. Our circuit also gives a chain complex:

$$C_0 \xleftarrow{\delta} C_1 \xleftarrow{\delta} C_2$$

The mesh currents define a 2-chain J and the currents along edges define a 1-chain

$$I = \delta J$$

In these terms, the total power is

$$P = \sum_e R(e) I(e)^2$$

We can write this in a slicker way using an inner product on the space of 1-chains:

$$P = \langle I, I \rangle$$

In fact I already talked about this inner product in "Week 293".

In these terms, the principle of minimum power says we should find the mesh current that minimizes the total power subject to some boundary conditions. So, now we're trying to minimize

$$P = \langle \delta J, \delta J \rangle$$

while holding J fixed along certain "input and output meshes".

In short, everything works the same way in the two dual formulations. In fact, we can reinterpret our chain complex as a cochain complex just by turning it around! This:

$$C_0 \stackrel{\delta}{\leftarrow} C_1 \stackrel{\delta}{\leftarrow} C_2$$

effortlessly becomes this:

$$C_2 \stackrel{\delta}{\rightarrow} C_1 \stackrel{\delta}{\rightarrow} C_0$$

And we didn't even need our graph to be planar! The only point in having the graph be planar is that this gives us a specific choice of meshes. Otherwise, we must choose them ourselves.

Finally, I want to mention an interesting book on nonequilibrium thermodynamics. The "principle of minimum power" is also known as the "principle of least entropy production". I'm very curious about this principle and how it relates to the more familiar "principle of least action" in classical mechanics. This book seems to be pointing towards a unification of the two:

3) Hans Christian ttinger, *Beyond Equilibrium Thermodynamics*, Wiley, 2005.

I thank Arnold Neumaier for pointing it out! It considers a fascinating generalization of Hamiltonian mechanics that applies to systems with dissipation: for example, electrical circuits with resistors, or mechanical systems with friction.

In ordinary Hamiltonian mechanics the space of states is a manifold and time evolution is a flow on this manifold determined by a smooth function called the Hamiltonian, which describes the *energy* of any state. In this generalization the space of states is still a manifold, but now time evolution is determined by two smooth functions: the energy and the *entropy!* In ordinary Hamiltonian mechanics, energy is automatically conserved. In this generalization that's also true, but energy can go into the form of heat. . . and entropy automatically *increases!*

Mathematically, the idea goes like this. We start with a Poisson manifold, but in addition to the skew-symmetric Poisson bracket $\{F, G\}$ of smooth functions on some manifold, we also have a symmetric bilinear bracket $[F, G]$ obeying the Leibniz law

$$[F, GH] = [F, G]H + G[F, H]$$

and this positivity condition:

$$[F,F] \geqslant 0$$

The time evolution of any function is given by a generalization of Hamilton's equations:

$$\frac{dF}{dt} = \{H,F\} + [S,F]$$

where H is a function called the "energy" or "Hamiltonian", and S is a function called the entropy. The first term on the right is the usual one. The new second term describes dissipation: as we shall see, it pushes the state towards increasing entropy.

If we require that

$$[H,F] = \{S,F\} = 0$$

for every function F, then we get conservation of energy, as usual in Hamiltonian mechanics:

$$\frac{dH}{dt} = \{H,H\} + [S,H] = 0$$

But we also get the second law of thermodynamics:

$$\frac{dS}{dt} = \{H,S\} + [S,S] \geqslant 0$$

Entropy always increases!

ttinger calls this framework "GENERIC" — an annoying acronym for "General Equation for the NonEquilibrium Reversible-Irreversible Coupling". There are lots of papers about it. But I'm wondering if any geometers have looked into it!

If we didn't need the equations $[H,F] = \{S,F\} = 0$, we could easily get the necessary brackets starting with a Khler manifold. The imaginary part of the Khler structure is a symplectic structure, say ω, so we can define

$$\{F,G\} = \omega(dF,dG)$$

as usual to get Poisson brackets. The real part of the Khler structure is a Riemannian structure, say g, so we can define

$$[F,G] = g(dF,dG)$$

This satisfies

$$[F,GH] = [F,G]H + F[G,H]$$

and

$$[F,F] \geqslant 0$$

Don't be fooled: this stuff is not rocket science. In particular, the inequality above has a simple meaning: when we move in the direction of the gradient of F, the function F increases. So adding the second term to Hamilton's equations has the effect of pushing the system towards increasing entropy.

Note that I'm being a tad unorthodox by letting ω and g eat cotangent vectors instead of tangent vectors — but that's no big deal. The big deal is this: if we start with a Khler manifold and define brackets this way, we don't get $[H,F] = 0$ or $\{S,F\} = 0$ for all functions F unless H and S are constant! That's no good for applications to physics. To get around this problem, we would need to consider some sort of *degenerate* Khler structure — one where ω and g are degenerate bilinear forms on the cotangent space.

Has anyone thought about such things? They remind me a little of "Dirac structures" and "generalized complex geometry" — but I don't know enough about those subjects to know if they're relevant here.

This GENERIC framework suggests that energy and entropy should be viewed as two parts of a single entity — maybe even its real and imaginary parts! And that in turn reminds me of other strange things, like the idea of using complex-valued Hamiltonians to describe dissipative systems, or the idea of "inverse temperature as imaginary time". I can't tell yet if there's a big idea lurking here, or just a mess. . . .

Addendum: I thank Eric Forgy, Tom Leinster, Gunnar Magnusson and Esa Peuha for catching typos. Also, Esa Peuha noticed that I was cutting corners in my definition of "admittance" as the inverse of "resistance". Admittance is the inverse of resistance in circuits made of linear resistors, which are the circuits I was talking about. But he notes:

> *In Week 295, you claim that admittance is the inverse of resistance, but that's not true; admittance is the inverse of impedance. Of course, resistance and impedance are the same thing for circuits containing only resistors, but not in the presence of capacitors and inductors. Usually it's said that the inverse of resistance is conductance (and the inverse of reactance is susceptance), but that's not quite right: resistance and reactance are the real and imaginary parts of impedance, and conductance and susceptance are the real and imaginary parts of admittance, so resistance, reactance, conductance and susceptance don't usually have physically meaningful inverses.*

Someone pointed out that in the GENERIC formalism, we don't need

$$[H, F] = \{S, F\} = 0$$

for all functions F. To derive the few results I describe, it's enough to have

$$[H, S] = \{S, H\} = 0$$

It seems that ttinger assumes the stronger formulation but only uses the weaker one — see the text before equation (1.22) in his book. This changes the story considerably!

Eugene Lerman pointed out that everything I said about Khler manifolds would work equally well for almost Khler manifolds: nothing I said required that the complex structure relating the symplectic structure and the Riemannian metric be integrable. So, you could say I'm looking to fill in the missing item in this analogy:

> *symplectic : Poisson :: almost Khler : ???*

But, I'd be equally happy to hear from you if you know the missing item in *this* analogy:

> *symplectic : Poisson :: Khler : ???*

For more discussion, visit the n-Category Caf.

> I would rather discover a single fact, even a small one, than debate the great issues at length without discovering anything new at all.
>
> — *Galileo Galilei*

Week 296

April 26, 2010

For many weeks I've been threatening to bring some serious math into my discussion of electrical circuits. Today I'll finally start: I'll try to use a little symplectic geometry to treat electrical circuits made of linear resistors as morphisms in a compact dagger-category.

But first, here's a great book you should all grab:

1) Jerry Shurman, *Geometry of the Quintic*, Wiley, New York, 1997. Also available at `http://people.reed.edu/~jerry/Quintic/quintic.html`

I've recommended this book before. Now Shurman has made it freely available on his website! In 1888, Felix Klein used the icosahedron to solve the general quintic equation:

$$ax^5 + bx^4 + cx^3 + dx^2 + ex + f = 0$$

Shurman's book explains Klein's ideas in a very nice way, using a bit of modern math to make them easier to understand. It's a wonderful story. For a bit about how it connects to other ideas, see "Week 230".

Now, on to electrical circuits. Before I get into the math, I want to remind you why it's worth bothering with. Electrical circuits are interesting and important in themselves, but that's not all! They're also great examples of physical systems built from interacting components. As we've seen starting in "Week 288", there are many *types* of systems like this. But luckily, there's a big mathematically precise analogy relating a bunch of them:

	displacement q	flow $\dot{q}$	momentum p	effort $\dot{p}$
Mechanics (translation)	position	velocity	momentum	force
Mechanics (rotation)	angle	angular velocity	angular momentum	torque
Electronics	charge	current	flux linkage	voltage
Hydraulics	volume	flow	pressure momentum	pressure
Thermodynamics	entropy	entropy flow	temperature momentum	temperature
Chemistry	moles	molar flow	chemical momentum	chemical potential

So, we can go quite far by picking one kind — say, electrical circuits — and focusing on that. The rest are isomorphic.

Even if we focus on systems of one kind, there are lot of choices left:

- We can study fully general nonlinear systems, or restrict our attention to linear ones.
- We can study quantum-mechanical systems, or classical ones.
- We can study dissipative systems (where energy is not conserved), or conservative ones (where it is).
- We we can study dynamical systems (where things change with time) or static ones (where they don't).

- We can study open systems (which interact with their environment) or closed ones (which don't).

In fact all five choices are independent, so we have 32 subjects to study! But in recent weeks I've focused on electrical circuits made of linear resistors where the voltages and currents don't depend on time. This amounts to studying

linear classical dissipative static open systems.

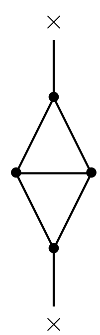
"Linear", "classical" and "static" are all ways to make our system boring — or at least, easy to understand. But "open" brings category theory into the game, since we can combine two open systems by feeding the outputs of one into the inputs of the other — and this can be seen as composing morphisms. Also, we saw last week that linear classical dissipative static open systems can be understood using the principle of least power!

Now I would like to describe a category that has linear classical dissipative static open systems as morphisms. To make things more concrete, let's think of these systems as electrical circuits made of linear resistors.

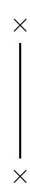
But there is a sixth choice to be made! We can treat these circuits either as "distributed" or "lumped".

What do I mean by this? Well, if we treat a circuit as "distributed", we know about every detail of it: for example, we can say it's made of 27 resistors, with particular resistances, hooked up in a certain way. But if we treat it as "lumped", we treat it as a black box with some wires hanging out. We're not allowed to peek inside the box. All we know is what it does, viewed externally.

For example, we could have a fancy circuit like this:



Each edge has some resistance, as explained in "Week 294", and the x's mark the input and output. Here's another, simpler, circuit of the same sort:



When we treat these circuits as "distributed", they're different. Why? Because they look different. But when we treat them as "lumped", they might be the same! Why? Because no matter what resistances we choose for the edges of the fancy circuit, the current through it is proportional to the voltage across it. . . just like the simple one. So if this constant of proportionality is the same, they count as the same "lumped" circuit.

(In case you're about to object: we're only treating these circuits *statically*. If you feed a rapidly changing voltage across the two circuits, they will behave differently, since it takes time for changes to propagate. But that's irrelevant here.)

More precisely, let us say that two circuits built from linear resistors count as the same "lumped" circuit if:

- they have the same number of inputs, say m,
- they have the same number of outputs, say n,
- the currents on their input and output wires are given by the same function of the electrostatic potentials on those wires, say

$$f\colon \mathbb{R}^{m+n} \to \mathbb{R}^{m+n}$$

Since we're looking at linear circuits, the function f will be linear. However, not every linear function f is allowed! To understand which ones are, it's good to use the principle of least power. Here we describe a lumped circuit using a function

$$Q\colon \mathbb{R}^{m+n} \to \mathbb{R}$$

This gives the power as a function of the potentials at the inputs and outputs. We can recover f by taking the gradient of Q. Since Q is quadratic, its gradient is a linear function of position.

Which functions Q are allowed? Well, this function must be what mathematicians call a "quadratic form": in other words, a homogeneous quadratic polynomial. It must be nonnegative. And, it must not change if we add the same number to each potential.

I suspect that every function Q meeting these three conditions comes from an actual electrical circuit built from resistors. If you know, please tell me!

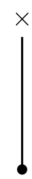
I don't love the third condition, because it depends heavily on the standard basis of the vector space $\mathbb{R}^{m+n}$. I hope we can drop this condition if we allow circuits that include an extra kind of circuit element: besides resistors, also "grounds". A "ground" is a place where a wire is connected to the earth, which — by convention — has potential zero.

For example, suppose we have this circuit, with one input connected to a ground, and no outputs:

The funny little "=" thing is the ground. For this circuit the power is described by a quadratic form Q in one real variable v. If the wire has resistance $R > 0$, we have

$$Q(v) = \frac{v^2}{R}$$

What if we want $Q = 0$? Well, then we should use a circuit like this, instead:



In other words: one input, no outputs, and a wire that just dangles in mid-air instead of being connected to a ground.

Using resistors and grounds, I hope we can build circuits corresponding to arbitrary nonnegative quadratic forms. So, let's try to describe a category where:

- An object is a finite-dimensional real vector space.
- A morphism $Q\colon V \to W$ is a nonnegative quadratic form Q on $V \oplus W$.

How do we compose these morphisms? Using the principle of minimum power! Given morphisms $P\colon U \to V$ and $Q\colon V \to W$, we define their composite $QP\colon U \to W$ by

$$QP(u, w) = \min_{v \in V} \{P(u, v) + Q(v, w)\}$$

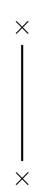
It's easy to check that this is associative: it's analogous to matrix multiplication, but with addition replacing the usual multiplication of numbers, and $\min$ replacing the usual sum. Indeed, this idea has been widely used to reformulate the principle of least action in classical mechanics as a mutant version of the "matrix mechanics" approach to quantum mechanics:

2) G. L. Litvinov, "The Maslov dequantization, idempotent and tropical mathematics: a brief introduction", available as arXiv:math/0507014.

3) John Baez, "Spans in quantum theory", http://math.ucr.edu/home/baez/span/

But the physics is different now: we are minimizing power rather than action.

There's just one slight glitch. Our would-be category doesn't have identity morphisms! This is easy to check mathematically. Physically, the reason is clear. The identity morphism $1\colon \mathbb{R} \to \mathbb{R}$ should correspond to a perfectly conductive wire, like this:



This is also called a "short circuit" — see "Week 294". But what's the corresponding quadratic form? Well, it doesn't exist. But the idea is that the power used by this circuit would be *infinite* if the potentials at the two ends were different. So, heuristically, the quadratic form should be

$$Q(v,w) = +\infty(v-w)^2$$

This doesn't really make sense, except as some sort of mysterious limit of the quadratic form for a resistor with resistance R:

$$Q(v,w) = \frac{(v-w)^2}{R}$$

as R approaches 0 from above. In other words, the perfectly conductive wire is the limiting case of a resistor.

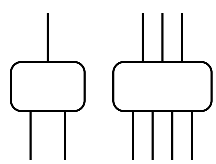
So, what can we do? There are lots of options. One is to note that perfectly conductive wires don't really exist, and be content with what we've got. Namely: a "semicategory", which is like a category, but without identity morphisms.

Most people don't like semicategories. So, if you're like most people, you'll be relieved to hear that any semicategory can be made into a category by formally throwing in identity morphisms. We don't lose any information this way. Even better, a category arises from a semicategory in this way iff it has this special property: whenever the composite of two morphisms is an identity morphism, both must be identity morphisms. So, semicategories aren't really more general than categories. We can think of them as categories with this extra property!

If we extend our semicategory to a category this way, the result has some nice properties. First, it's a "monoidal category", meaning roughly a category with tensor products:

4) nLab, "Monoidal category", `http://ncatlab.org/nlab/show/monoidal+category`

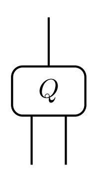
The tensor product corresponds to setting two circuits side by side:



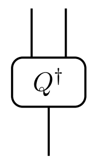
Mathematically, the tensor product of objects V and W is their direct sum $V \oplus W$, while the tensor product of morphisms $Q\colon V \to W$ and $Q'\colon V' \to W'$ is the quadratic form $Q + Q'$ given by:

$$(Q+Q')(v,v',w,w') = Q(v,w) + Q'(v',w')$$

Our category also has "duals for morphisms". Intuitively, this means that we can take any circuit $Q\colon V \to W$ built from resistors:



and reflect it across a horizontal line, switching inputs and outputs like this:



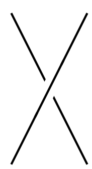
to obtain a new circuit $Q^\dagger\colon W \to V$. Mathematically this operation is defined as follows:

$$Q^\dagger(v, w) = Q(w, v)$$

A category with duals for morphisms is usually called a "dagger-category". It's easy to check that our category is one of those:

5) nLab, "Dagger-category", `http://ncatlab.org/nlab/show/dagger-category`

However, our category has some defects. First of all, there's no morphism corresponding to two perfectly conductive wires that cross like this:



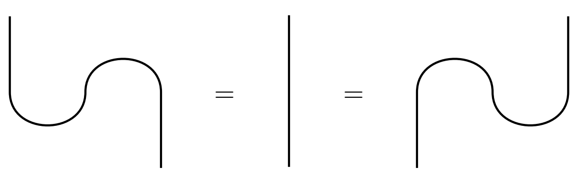
If we had that, we'd get a "symmetric monoidal category":

6) nLab, "Symmetric monoidal category", `http://ncatlab.org/nlab/show/symmetric+monoidal+category`

Our category so far also lacks a morphism corresponding to a perfectly conductive bent wire like this:

or like this:

If we had these morphisms, obeying the obvious "zig-zag identities":



then our monoidal category would have "duals for objects", in the sense explained back in "Week 89".

It seems reasonable to allow all these circuits made of perfectly conductive wires, even though they correspond to idealized limits of circuits we can actually build. They don't cause any mathematical contradictions. And they should give a very nice category: a symmetric monoidal category with duals for objects and morphisms. Categories of this sort are called "dagger-compact":

7) nLab, "Dagger-compact category", `http://ncatlab.org/nlab/show/dagger+compact+category`

Dagger-compact categories are very important in physics. A classic example is the category of finite-dimensional Hilbert spaces, with linear operators as morphisms, and the usual tensor product of vector spaces. Another example is the category of $(n-1)$-dimensional compact oriented manifolds, with n-dimensonal cobordisms as morphisms. The interplay between these examples is important in topological quantum field theory. People like Samson Abramsky, Bob Coecke, Chris Heunen, Dusko Pavlovich, Peter Selinger and Jamie Vicary have done a lot to formulate all of quantum mechanics in terms of dagger-compact categories. Here are the fundamental references:

8) Samson Abramsky and Bob Coecke, "A categorical semantics of quantum protocols", in *Proceedings of the 19th IEEE conference on Logic in Computer Science (LICS04)*, IEEE Computer Science Press (2004). Also available at `quant-ph/0402130`.

9) Peter Selinger, "Dagger compact closed categories and completely positive maps", in *Proceedings of the 3rd International Workshop on Quantum Programming Languages (QPL 2005)*, *ENTCS* **170** (2007), 139–163. Also available at `http://www.mscs.dal.ca/~selinger/papers.html#dagger`

You can use compact dagger-categories to clarify what it means, physically, for a complex Hilbert space to be equipped with an orthonormal basis:

10) Bob Coecke, Dusko Pavlovic, and Jamie Vicary, "A new description of orthogonal bases", available as `arXiv:0810.0812`.

You can use them to explain the traditional approach to quantum logic in terms of orthomodular lattices:

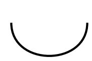
11) Chris Heunen, *Categorical Quantum Models and Logics*, Amsterdam University Press, 2009. Also available at `http://www.comlab.ox.ac.uk/people/chris.heunen/about.html`

You can even use them to motivate the appearance of complex numbers in quantum mechanics:

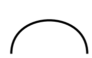
12) Jamie Vicary, "Completeness and the complex numbers", available as `arXiv:0807.2927`.

So, if there is a compact dagger-category of electrical circuits, we should find it and study it. I've decided that category theory should not be saved for fancy stuff like the foundations of quantum theory. It can serve as a general language for studying systems made of parts, and we should take full advantage of it!

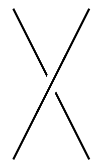
Let's try. Let's take the category described so far and supplement it with a "cup":



a "cap":



and a "symmetry":



We could formally throw in these morphisms, just like we threw in identities. But there is a less artificial solution which solves all these problems in one blow. We can take a lesson from symplectic geometry, and notice that nonnegative quadratic forms are a special case of something called "Lagrangian correspondences". These include identity morphisms as well as the cap, cup, and symmetry.

Let me explain! Suppose Q is a quadratic form on a vector space V. Then its differential dQ is a one-form, so it gives an element of V^* for each point of V. But since Q is quadratic, its differential depends linearly on the point of Q, so we get a linear map

$$dQ\colon V \to V^*$$

This is a highbrow formulation of something I already told you in lowbrow way. But now let's go a bit further. The graph of dQ is a linear subspace of the cartesian product $V \times V^*$. But $V \times V^*$ is better than a mere vector space. We can think of it as the cotangent bundle T^*V. So, it's a "symplectic" vector space:

13) Wikipedia, "Symplectic vector space", `http://en.wikipedia.org/wiki/Symplectic_vector_space`

Namely, it has a "symplectic structure" — that is, a nondegenerate antisymmetric bilinear form ω given by:

$$\omega((v,f),(v',f')) = f(v') - f'(v)$$

And it's a general fact that the graph of any quadratic form on V is a "Lagrangian" subspace of $T^*V = V \times V^*$, meaning a maximal subspace on which ω vanishes.

But, there are Lagrangian subspaces of T^*V that are not the graphs of quadratic forms. There are also "limits" of graphs of quadratic forms — precisely the sort of thing we want now! After all, every circuit made of perfectly conductive wires can be thought of as a limit of circuits made of resistors.

So, we can try a category where:

- An object is a finite-dimensional real vector space.
- A morphism $S\colon V \to W$ is a Lagrangian subspace of $T^*(V \oplus W)$.

Remember that an element of V describes the potentials on the input wires of our circuit, while W does the same job for the output wires. An element of $V \oplus W$ describes the potentials on input and output wires. Currents live in the dual vector space, so an element of $T^*(V \oplus W)$ describes the potentials and currents on input and output wires. The Lagrangian subspace describes the potentials and currents that are allowed by our circuit.

We can also change perspective and say:

- An object is a finite-dimensional real vector space.
- A morphism $S\colon V \to W$ is a Lagrangian subspace of $T^*V \times T^*W$.

Here an element of T^*V describes the potentials and currents on the input wires, while T^*W does the same job for the output wires. As before, the Lagrangian subspace of $T^*V \times T^*W$ describes the potentials and currents that are allowed by our circuit. But now we can think of it as *relation* between T^*V and T^*W. This makes it clear how to compose morphisms: we compose them according to the usual method for composing relations.

This perspective will be familiar to symplectic geometers who know about "Lagrangian correspondences", also known as "canonical relations". We're studying a special case of those, namely the linear case. If you want to learn more, try:

14) Alan Weinstein, "Symplectic categories", available as arXiv:0911.4133.

Weinstein writes:

> *Following in part some (unpublished) ideas of the author, Guillemin and Sternberg observed that the linear canonical relations (i.e., lagrangian subspaces of products of symplectic vector spaces) could be considered as the morphisms of a category, and they constructed a partial quantization of this category (in which lagrangian subspaces are enhanced by half-densities). The automorphism groups in this category are the linear symplectic groups, and the restriction of the Guillemin-Sternberg quantization to each such group is a metaplectic representation. On the other hand, the quantization of certain compositions of canonical relations leads to ill-defined operations at the quantum level, such as the evaluation of a delta "function" at its singular point, or the multiplication of delta functions.*

Here's the reference to Guillemin and Sternberg:

15) Victor Guillemin and Shlomo Sternberg, "Some problems in integral geometry and some related problems in microlocal analysis", *Amer. J. Math.* **101** (1979), 915–955.

I learned symplectic geometry from Guillemin in grad school, so I'm happy to see it being applied to resistors! And the discussion of quantization suggests a way to understand resistors quantum-mechanically. In fact there's a bit of literature on this subject already:

16) Michel H. Devoret, "Quantum fluctuations in electrical circuits", in *Quantum Fluctuations*, eds. S. Reynaud, E. Giacobino and J. Zinn-Justin, Elsevier, 1997. Also available at http://qulab.eng.yale.edu/documents/reprints/Houches_fluctuations.pdf

But for now, the classical theory is interesting enough. I guess I need to start by checking my claim:

Claim: *there is a compact dagger-category where:*

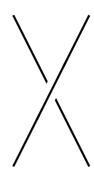
- *An object is a finite-dimensional real vector space.*
- *A morphism $S\colon V \to W$ is a Lagrangian subspace of $T^*V \times T^*W$.*
- *We compose morphisms using composition of relations.*
- *The tensor product is given by direct sum.*
- *The symmetry is the obvious thing.*
- *The dagger of a subspace of $T^*V \times T^*W$ is the corresponding subspace of $T^*W \times T^*V$.*

This category is mathematically elegant, but "too big", because its morphisms include a lot more than Lagrangian subspaces coming from nonnegative quadratic forms, and limits of these. There's a well-known topology on the set of Lagrangian subspaces of a symplectic vector space, so the concept of limit is well-defined here. If we restrict attention to Lagrangian subspaces coming from nonnegative quadratic forms, and limits of these, do we get a subcategory? It might seem obvious — but shockingly, composition is *not continuous* with respect to this well-known topology! Weinstein gives a counterexample. So, there's something nontrivial to check.

If we do get a subcategory, will it still be a compact dagger-category? Yes, I think so, because it contains the cup

and cap:

and symmetry:



So, this would be a very nice thing.

I thank James Dolan, Peter Selinger, Alan Weinstein and Simon Willerton for helping me figure out these ideas.

Addendum: I thank Mikael Vejdemo-Johansson for catching a typo.
For more discussion visit the n-Category Caf.

> Reality has been around since long before you showed up. Don't go calling it nasty names like "bizarre" or "incredible". The universe was propagating complex amplitudes through configuration space for ten billion years before life ever emerged on Earth. Quantum physics is not "weird". You are weird.
>
> — *Eliezer Yudkowsky*

Week 297

May 9, 2010

This week I'll talk about electrical circuits and Dirichlet forms. But first: knot sculptures, special relativity in finance, lazulinos, some peculiar infinite sums, and a marvelous fact about the number 12.

Here are some cool sculptures of knots by Karel Vreeburg:

1) Karel Vreeburg, `http://www.karelvreeburg.nl/site/kunstwerken/357933_Beelden.html`

The polished forms emerge from rough stone much as mathematical abstractions emerge from physical reality. And I'm reminded of what Michelangelo said. "Every block of stone has a statue inside it, and the task of the sculptor is to discover it."

Next — remember that big glitch in the stock market last Thursday, when the Dow Jones dropped 9.2% in less than an hour, and then bounced back? For a while, about a trillion dollars had evaporated!

The worst part is, nobody knows why. But apparently one part of the problem was that some electronic communication systems were lagging behind, seeing a delayed view of what was really going on. But guess how long this lag was. Just 0.1 seconds!

That's only three quarters the time it takes light to circle the Earth. But these days it's considered an unacceptably long time for computer trading. So, we've reached the point where special relativity is important in economics. The Newtonian concept of "the same time at different places" is no longer adequate:

> *A 1-millisecond advantage in trading applications can be worth $100 million a year to a major brokerage firm, by one estimate. The fastest systems, running from traders' desks to exchange data centers, can execute transactions in a few milliseconds — so fast, in fact, that the physical distance between two computers processing a transaction can slow down how fast it happens. This problem is called data latency — delays measured in split seconds. To overcome it, many high-frequency algorithmic traders are moving their systems as close to the Wall Street exchanges as possible.*

This quote is from:

2) Richard Martin, "Wall Street's quest to process data at the speed of light", *Information Week*, April 23, 2007. Also available at `http://www.informationweek.com/news/infrastructure/showArticle.jhtml?articleID=199200297`

See also:

3) Kid Dynamite's World, "Market Speed Bumps", `http://fridayinvegas.blogspot.com/2010/05/market-speed-bumps.html`

where someone comments:

> *What I suspect happened (following on moments after KD's explanation ends) is that some meaningful trigger point on stop loss orders was exceeded. This could have been a small wave of selling from Bloomberg running the video of the crowd getting agitated in Greece (which was at about 2:40PM EST), but whatever the case — a wave of selling started. That in turn brought the price down, which triggered some stop loss orders, which in turn fueled more stop loss orders, along with any humans and machines that just sold on the steep drop.*
>
> *However, given the heavy volume at the time, the HFT systems that would normally jump in (albeit at much lower bids) didn't even get to see accurate representations of the order books, because I was seeing at least a 100ms delay in quotes from ARCA (the only ECN I measured accurately).*
>
> *So, at least with ARCA and probably the other exchanges as well, everyone was running with at least a 100ms delayed snapshot of the world. Given that I stopped calculating this delay when my own software shutdown at 2:41PM (4 minutes before the peak of chaos), this is probably understating matters somewhat.*
>
> *If you can't see that the order book is missing bids because you are operating 100ms behind the actual trades taking place, then there is a meaningful window when the bids in the order book can all be taken out before anyone even knows that they should be placing bids!*
>
> *Further, once you recognize that you are operating with stale information (and 100ms is quite stale if you are seeing the markets plunge the way they were), there is no way you are going to enter orders, since you don't have any clue where to place them, and if you do — you place them with much wider spreads than normal, which in conjunction with market sell orders brings the trading price down along with the bid/ask midpoint.*

I guess it's just a matter of time before *general* relativity becomes important in finance. I thank Mike Stay and Henry Baker for bringing this issue to my attention.

I also enjoyed this blog post by Mike:

4) Mike Stay, "Lazulinos", `http://reperiendi.wordpress.com/2010/04/27/lazulinos/`

It's about a newly discovered quasiparticle with astounding properties. If you want to really understand what's going on, read the paper by Alexander Craigie — there's a link at the end of Mike's post.

Next, an observation from Robert Baillie. Take this series:

$$\frac{\pi}{\sqrt{8}} = 1 + \frac{1}{3} - \frac{1}{5} - \frac{1}{7} + \frac{1}{9} + \frac{1}{11} - \dots$$

Square each term, add them up... and you get the square of the previous sum:

$$\frac{\pi^2}{8} = 1 + \frac{1}{3^2} + \frac{1}{5^2} + \frac{1}{7^2} + \frac{1}{9^2} + \frac{1}{11^2} + \dots$$

Don't tell undergraduates about this — they are already confused enough!

And finally, a comment from Nora Ganter. If you look at the cohomology of the symmetric groups, you find an element of order 12 in $H^3(S_n, \mathbb{Q}/\mathbb{Z})$ for n greater or equal to 4. But the third cohomology of a group classifies ways of extending it to a 2-group. So whenever you realize a finite group as a group of permutations of 4 or more things, you automatically get a way of extending it to a 2-group!

I would like to understand this better. In particular, the number 12 here should be related to the fact that

$$\pi_{k+3}(S^k) = \mathbb{Z}/24$$

for $k \geqslant 5$. After all, stable homotopy groups of spheres are related to the cohomology of symmetric groups, since the group completion of the classifying space of the groupoid of finite sets is $\Omega^\infty S^\infty$ — see "Week 199" if you don't know what I'm talking about here. But I'm confused about the numbers 12 versus 24 here, and also the role of $\mathbb{Q}/\mathbb{Z}$ coefficients.

Does someone know a place where you can look up cohomology groups of the symmetric groups?

Next: electrical circuits!

Last week I discussed electrical circuits made of (linear) resistors and "grounds" — places where wires touch an object whose electrostatic potential is zero. I want to fill in some missing pieces today.

Suppose we have such a circuit with n wires dangling out of it. I've been calling these "inputs" and "outputs" — but today I don't care which ones are inputs and which ones are outputs, so let's call them all "terminals".

We saw last time that our circuit gives a function

$$Q\colon \mathbb{R}^n \to \mathbb{R}$$

This tells you how much power the circuit uses as a function of the electrostatic potential at each terminal.

It's pretty easy to see that Q is a "quadratic form", meaning that

$$Q(\varphi) = \sum_{i,j} Q_{ij}\varphi_i\varphi_j$$

for some matrix Q_{ij}, which we can assume is symmetric. And it's easy to see that Q is "nonnegative", meaning

$$Q(\varphi) \geqslant 0$$

I wildly guessed that every nonnegative quadratic form comes from a circuit made of resistors and grounds. Since then I've learned a few things, thanks to Ben Tilly and Tom Ellis.

For starters, which nonnegative quadratic forms do we get from circuits built only from resistors? We certainly don't get all of them. For example, if $n = 2$, every circuit built from just resistors has

$$Q(\varphi) = c(\varphi_1 - \varphi_2)^2$$

for some nonnegative number c. So, we'll never get this quadratic form:

$$Q(\varphi) = (\varphi_1 + \varphi_2)^2$$

even though it's nonnegative. In general, for any n, we can get a lot of quadratic forms just by connecting each terminal to each other with a resistor. Such circuits give precisely these quadratic forms:

$$Q(\varphi) = \sum_{i,j} c_{ij}(\varphi_i - \varphi_j)^2$$

where the numbers c_{ij} are nonnegative. We can assume without loss of generality that $c_{ii} = 0$. The numbers c_{ij} are *reciprocals* of resistances, so we're allowing resistors with infinite resistance, but not with zero resistance.

It turns out that quadratic forms of the above type are famous: they're called "Dirichlet forms". People have characterized them in lots of ways. Here's one: they're the nonnegative quadratic forms that vanish when φ is constant:

$$\varphi_i = \varphi_j \text{for all } i, j \implies Q(\varphi) = 0$$

and also satisfy the "Markov property":

$$Q(\varphi) \geqslant Q(\psi)$$

when ψ_i is the minimum of φ_i and 1. This characterization is Proposition 1.7 here:

5) Christophe Sabot, "Existence and uniqueness of diffusions on finitely ramified self-similar fractals", Section 1: "Dirichlet forms on finite sets and electrical networks", *Annales Scientifiques de l'cole Normale Suprieure, Sr. 4,* **30** (1997), 605–673. Available at `http://www.numdam.org/numdam-bin/item?id=ASENS_1997_4_30_5_605_0`

Sabot doesn't prove this result, which he considers "well known". Instead, he points us to this book, which is not only fun to read, but also free:

6) P. G. Doyle and J. L. Snell, *Random Walks and Electrical Circuits*, Mathematical Association of America, 1984. Also available at `http://www.math.dartmouth.edu/~doyle/`

You may wonder what random walks and diffusions on fractals have to do with electrical circuits! The idea is that we can take a limit of electrical circuits that get more and more complicated and get a *fractal*. The electrical conductivity of this fractal can be reinterpreted as heat conductivity, using the analogies described back in "Week 289".

And then we can study the heat equation on this fractal. This equation says how heat diffuses with the passage of time.

But there's nothing special about *heat*. We can use the heat equation to describe the diffusion of just about anything. We could even use it to describe the diffusion of tiny drunken men who stumble around aimlessly on our fractal! And that's where "random walks" come in.

It turns out that in situations like this, the heat equation is completely determined by a quadratic form called a "Dirichlet form". But it's not a quadratic form on $\mathbb{R}^n$ anymore: it's a quadratic form on a space of real-valued functions on our fractal.

In fact Dirichlet forms were first studied, not for finite sets or fractals, but for nice regions in Euclidean space — the sort of regions you'd normally consider when studying the heat equation. In this case the Dirichlet form arises from the Laplacian:

$$Q(\varphi) = -\int \varphi \nabla^2 \varphi$$

where φ is a function on our region. The moral is that we should think of any Dirichlet form as a generalized Laplacian!

There's a huge literature on Dirichlet forms. Most of it focuses on analytical subleties that don't matter for our pathetically simple examples. For a little taste, try this review of two books on Dirichlet forms:

7) Review by Daniel Stroock, *Bull. Amer. Math. Soc.* **33** (1996) 87–92. Also available at `http://www.ams.org/journals/bull/1996-33-01/S0273-0979-96-00617-9/`

Among other things, he mentions a simpler characterization of Dirichlet forms. We're only considering quadratic forms

$$Q\colon \mathbb{R}^n \to \mathbb{R}$$

and it turns out such a form is Dirichlet iff

$$Q(\varphi) \geqslant Q(\psi)$$

whenever

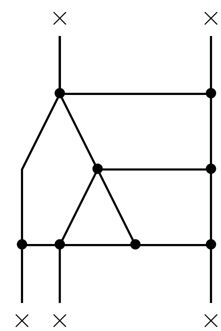
$$|\varphi_i - \varphi_j| \geqslant |\psi_i - \psi_j|$$

for all i, j. It's a fun exercise to see that this is equivalent to our previous characterization. And there's a simple physical idea behind this one: a circuit made of resistors will use more power when the potentials at different terminals differ by bigger amounts!

Okay. . . I'm digressing a bit. Let's get back on track.

We've seen that the quadratic form of a circuit made from resistors is Dirichlet whenever the circuit is of a special form: namely, when it has one resistor connecting each pair of terminals.

But what about other circuits made from resistors, like this?



Here the $\times$'s are the terminals, but there are also other vertices, which I'll call "internal vertices". Also, not every vertex is connected to every other vertex. Do we get a larger class of quadratic forms if we allow more general circuits like this?

No! All we get are Dirichlet forms!

For starters, it doesn't matter that not every vertex is connected to every other vertex. We can connect them with wires that have infinite resistance, and nothing changes. (Remember, we're allowing infinite resistance.)

So, the only interesting thing is the presence of "internal vertices". Why are the quadratic forms of circuits with internal vertices still Dirichlet forms?

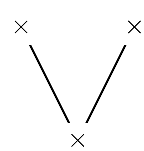
This follows from Sabot's Proposition 1.8. Let me explain the idea. Suppose, for example, that we have a nonnegative quadratic form in 3 variables

$$Q \colon \mathbb{R}^3 \to \mathbb{R}$$

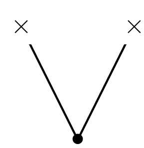
Then we can get a quadratic form in 2 variables by taking the minimum of Q as the third variable ranges freely:

$$P(\varphi_1, \varphi_2) = \min_{\varphi_3}\{Q(\varphi_1, \varphi_2, \varphi_3)\}$$

Physically this corresponds to taking a circuit with 3 terminals, like this:



and treating it as a circuit with 2 terminals by regarding the third terminal as an internal vertex:

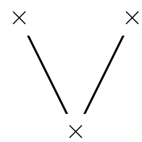


This means we let the potential at this vertex vary freely; by the principle of minimum power, it will do whatever it takes to minimize the power. So, we get a new circuit whose quadratic form is

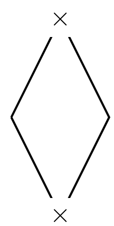
$$P(\varphi_1, \varphi_2) = \min_{\varphi_3}\{Q(\varphi_1, \varphi_2, \varphi_3)\}$$

More generally, we can take a nonnegative quadratic form in n variables, and take any subset of these variables, and get a new quadratic form by this minimization trick. And Sabot claims that if the original form was Dirichlet, so is the new one. He doesn't prove this, but I think it's easy — try it!

Sabot calls this trick for getting new Dirichlet forms from old ones the "trace map". He also describes another trick, the "gluing map". This lets us take the Dirichlet form of a circuit and get a new Dirichlet form by gluing together some terminals. For example, we could start with this circuit:



and glue the top two terminals together, getting this circuit:



Both the trace map and the gluing map have interesting category- theoretic interpretations. For example, the gluing map lets us *compose* electrical circuits — or more precisely, their Dirichlet forms — by gluing the outputs of one onto the inputs of another.

Finally, suppose we allow grounds as well as resistors. Sabot considers circuits of this sort in the following beautiful paper:

8) Christophe Sabot, "Electrical networks, symplectic reductions, and application to the renormalization map of self-similar lattices", *Proc. Sympos. Pure Math.* **72** (2004), 155–205. Also available as arXiv:math-ph/0304015.

He only considers circuits of a special form. They have no internal vertices, just terminals. As before, each pair of terminals is connected with a resistor. But now, each terminal is also connected to the ground via a resistor! Such circuits give exactly these quadratic forms:

$$Q(\varphi) = \sum_{i,j} c_{ij}(\varphi_i - \varphi_j)^2 + \sum_i c_i \varphi_i^2$$

where c_{ij} and c_i are nonnegative numbers.

Let's call these "generalized Dirichlet forms". I believe these generalized Dirichlet forms are characterized by the Markov property:

$$Q(\varphi) \geqslant Q(\psi)$$

when ψ_i is the minimum of φ_i and 1.

These generalized Dirichlet forms don't include *all* the nonnegative quadratic forms. Why? Because, as Ben Tilly pointed out, they don't include quadratic forms where the cross-terms $\varphi_i \varphi_j$ have positive coefficients. So, for example, we don't get this:

$$Q(\varphi_1, \varphi_2) = (\varphi_1 + \varphi_2)^2$$

Sabot claims that generalized Dirichlet forms are closed under the trace map and gluing. Given this, the same argument I already sketched shows that *every* electrical circuit built from resistors and grounds has a quadratic form that's a generalized Dirichlet form!

So, it's all been worked out...

Even better, Sabot explains how quadratic forms on a vector space V give Lagrangian subspaces of T^*V. This is the trick I used last week to introduce wires of zero resistance.

A wire with zero resistance would use an infinite amount of power if you put a different electrostatic potential at each end. KABANG! — the ultimate "short circuit"! So, wires with zero resistance are not physical realistic, but they're useful idealizations: they serve as identity morphisms in the category-theoretic description of electrical circuits. Circuits containing these wires can still be described using Lagrangian subspaces. These subspaces *don't* come from quadratic forms. But they are limits of subspaces that do.

Now we can make this more precise. There's a manifold consisting of all Lagrangian subspaces of T^*V — the "Lagrangian Grassmannian". Sitting in here is the set of generalized Dirichlet forms on V. We can take the closure of that set and get a space $C(V)$. Points in $C(V)$ correspond to circuits built from resistors, grounds, and wires of zero resistance. Sabot says this space is discussed here:

9) Y. Colin de Verdiere, "Reseaux electriques planaires I", *Comment. Math. Helv.* **69** (1994), 351–374. Also available at `http://www-fourier.ujf-grenoble.fr/~ycolver/All-Articles/94a.pdf`.

So, Sabot, Verdiere and the rest of the Dirichlet form crowd have done almost everything I want... *except* phrase their results in the language of category theory! And that, of course, is my real goal: to develop category theory as a language for physics and engineering.

Last week I gave a preliminary try at describing a category whose morphisms are electrical circuits built from resistors and grounds. I said:

Claim: *there is a dagger-compact category where:*

- *An object is a finite-dimensional real vector space.*
- *A morphism $S\colon V \to W$ is a Lagrangian subspace of $T^*V \times T^*W$.*
- *We compose morphisms using composition of relations.*
- *The tensor product is given by direct sum.*
- *The symmetry is the obvious thing.*
- *The dagger of a subspace of $T^*V \times T^*W$ is the corresponding subspace of $T^*W \times T^*V$.*

The problem was that this category has too many morphisms. If we only want physically realistic circuits — or *almost* realistic ones, since we're allowing wires of zero resistance — we should work not with all Lagrangian subspaces of $T^*\mathbb{R}^m \times T^*\mathbb{R}^n$, but only those lying in the subset $C(\mathbb{R}^m \times \mathbb{R}^n)$. So, let's try:

> ***Claim:*** *there is a dagger-compact category where:*
>
> - *An object is a natural number.*
> - *A morphism $S\colon m \to n$ is a point in $C(\mathbb{R}^m \times \mathbb{R}^n)$.*
> - *We compose morphisms using composition of relations.*
> - *The tensor product is given by direct sum.*
> - *The symmetry is the obvious thing.*
> - *The dagger of a point in $C(\mathbb{R}^m \times \mathbb{R}^n)$ is the corresponding point in $C(\mathbb{R}^n \times \mathbb{R}^m)$.*

There are a few things to check here. I haven't checked them all.

By the way: in case you actually want to study this stuff, I should point out that Sabot's second paper uses "Dirichlet form" to mean what I'm calling a generalized Dirichlet form, and uses "conservative Dirichlet form" to mean what I'm calling a Dirichlet form. So, be careful.

Also, here's another worthwhile reference:

10) Jun Kigami, *Analysis on Fractals*, Cambridge U. Press. First 60 pages available at `http://www-an.acs.i.kyoto-u.ac.jp/~kigami/AOF.pdf`

It's full of information on Dirichlet forms and electrical circuits. And it gives yet another characterization of Dirichlet forms! I don't love it — but I might as well tell you about it.

A Dirichlet form on $\mathbb{R}^n$ is a nonnegative quadratic form that vanishes when φ is constant:

$$\varphi_i = \varphi_j \text{for all } i,j \implies Q(\varphi) = 0$$

and satisfies

$$Q(\varphi) \geqslant Q(\psi)$$

whenever

$$\psi_i = \begin{cases} \varphi_i & \text{if } 0 < \varphi_i < 1 \\ 1 & \text{if } \varphi_i > 1 \\ 0 & \text{if } \varphi_i < 1 \end{cases}$$

This is yet another way to say that power decreases when the potentials at the terminals are closer together.

Kigami also explains the relation between Dirichlet forms and Markov processes. His Theorem B.3.4. says that for a measure space X, there is a one-to-one correspondence between Dirichlet forms on $L^2(X)$ and strongly continuous semigroups on $L^2(X)$ that map functions in $L^1(X)$ to functions of the same sort, and map nonnegative functions whose integral is 1 to functions of the same sort. Such semigroups are called "Markov".

The classic example is provided by the heat equation! But in our electrical circuit example, we're considering the pathetically simple case where X is a finite set.

One simple thing that deserves to be emphasized is that a Dirichlet form is not a kind of quadratic form on an abstract vector space. It's a kind of quadratic form on a space of functions! In particular, in my discussion above, $\mathbb{R}^n$ really means the algebra of functions on an n-element set — and in the second dagger-compact category mentioned above, the objects should really be finite sets. I was just working with a skeletal subcategory, to make things less intimidating.

Okay, I'll stop here for now. Later I plan to bring inductors and capacitors into the game. . . and loop groups!

Addendum: My friend Bruce Smith wrote:

> *I can't tell for sure, from what you wrote about grounds in week297 (and the last few Weeks), whether you are aware of this way to think about them: there is a 1-1 correspondence between circuits that can include grounds, and circuits that can't. To implement it, starting with a circuit that can include grounds, just add an extra terminal, call it "G" for "ground", and replace every internal ground with a 0-resistance connection to that terminal G. Also, in your thinking about potentials at terminals, replace "the potential at T_i" with "the potential difference between T_i and G" (or equivalently but differently, require that the potential at G is always 0).*
>
> *(I'm pretty sure you must be aware of this, but somehow it didn't show up as a simplifier in your explanation as much as, or as explicitly as, I thought it ought to.)*
>
> *If 0 resistance bothers you, note that it can be reduced away (by eliminating internal terminals in your resulting circuit) unless you had a ground directly connected to a terminal; if you were allowing that, then in your new circuit you'd better be allowing direct connections between two terminals, but I presume that whatever difficulties this causes in either case are essentially the same.*

For more discussion, visit the n-Category Caf.

> Discussions about theoretical engineering research often feels like visiting a graveyard in the company of Nietzsche. From the beginning of my career until now, I have always been hearing that 'the field is dead', 'circuit theory is dead', 'information theory is dead', 'coding theory is dead', 'control theory is dead', 'system theory is dead', 'linear system theory is dead', 'H_∞ is dead'. Good science, however, is always alive. The community may not appreciate the vibrancy of good ideas, but it is there. The absence of this impatience is one of the things that makes working in a mathematics department simply more pleasant.
>
> — *Jan C. Willems*

Week 298

May 14, 2010

Next week I'm going to New York to talk about the stuff I've been explaining here lately: electrical circuits and category theory. Then — volcanos permitting — I'll fly to Oxford to attend a course on quantum computation:

1) *Foundational Structures in Quantum Computation and Information*, May 24–28, 2010, Oxford University, organized by Bob Coecke and Ross Duncan, `http://web.comlab.ox.ac.uk/people/Bob.Coecke/QICS_School.html`

I look forward to lots of interesting conversations. A bunch of my math pals will be attending — folks like Bruce Bartlett, Eugenia Cheng, Simon Willerton, Jamie Vicary and maybe even my former student Alissa Crans, who lives here in California, but may swing by. I'll talk to Thomas Fischbacher about environmental sustainability and computational field theory, and Dan Ghica about hardware description languages. I also plan to meet Tim Palmer, a physicist at Oxford who works on climate and weather prediction, and one of the people I've been interviewing for the new This Week's Finds. I'm quite excited about that.

(My plan, you see, is to interview people who are applying math and physics to serious practical problems, from global warming and other environmental problems to biotechnology, nanotechnology, artificial intelligence and other potentially revolutionary technologies.)

The weekend after the course, there will be a workshop:

2) *Quantum Physics and Logic*, May 29–30, 2010, Oxford University, organized Bob Coecke, Prakash Panangaden, and Peter Selinger, `http://web.comlab.ox.ac.uk/people/Bob.Coecke/QPL_10.html`

Peter Selinger is the guy who got me interested in categories where the morphisms are electrical circuits! I'll be giving a talk in this workshop — you can see the slides here:

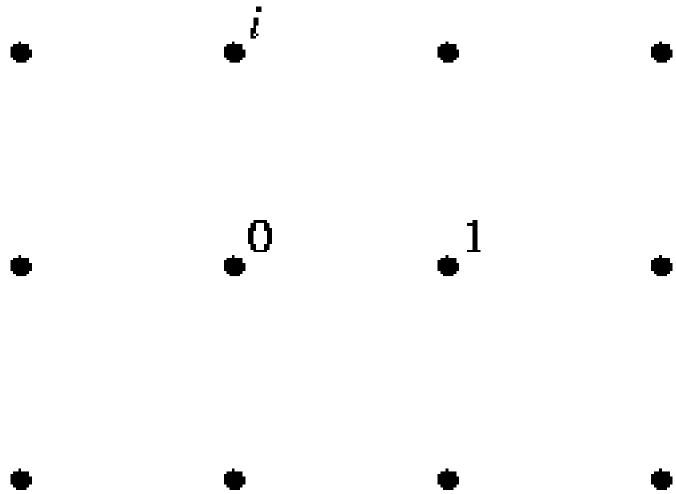
3) John Baez, "Duality in logic and physics", `http://math.ucr.edu/home/baez/dual/`

Alas, I've been so busy getting ready for my talks that I don't feel like writing about electrical circuits today. I was going to, but I need a change of pace. So let me say a bit about octonions, higher gauge theory, string theory and hyperdeterminants.

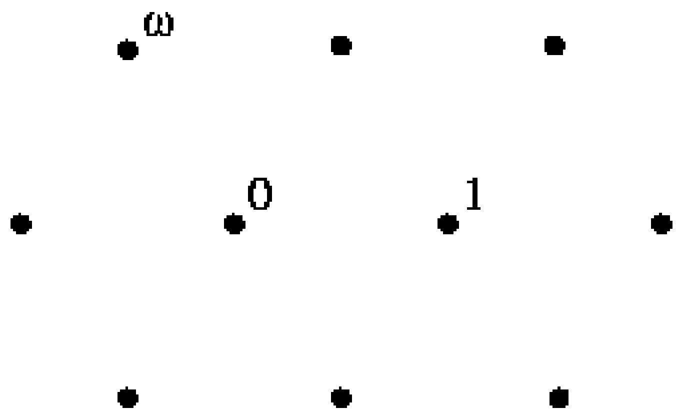
Integers are very special real numbers. But there are also "integers" for the complex numbers, the quaternions and octonions. The most famous are the "Gaussian integers", which are complex numbers like

$$a + bi$$

where a and b are integers. They form a square lattice like this:



The second most famous are the "Eisenstein integers", which form a hexagonal lattice like this:



I explained how these lattices are important in string theory back in "Week 124" — "Week 126".

People have also thought about various kinds of far less well-known are various kinds of quaternionic and octonionic "integers". To learn about these, there's no better place than the magnificent book by Conway and Smith:

4) John H. Conway and Derek A. Smith, *On Quaternions and Octonions: Their Geometry, Arithmetic, and Symmetry*, A. K. Peters, Ltd., Natick, Massachusetts, 2003.

For a little taste, try my summary in "Week 194", and my review here:

5) John Baez, "review of 'On Quaternions and Octonions'", *Bull. Amer. Math. Soc.* **42** (2005), 229–243. Also available at `http://www.ams.org/journals/bull/2005-42-02/S0273-0979-05-01043-8/` and as a webpage at `http://math.ucr.edu/home/baez/octonions/conway_smith/`

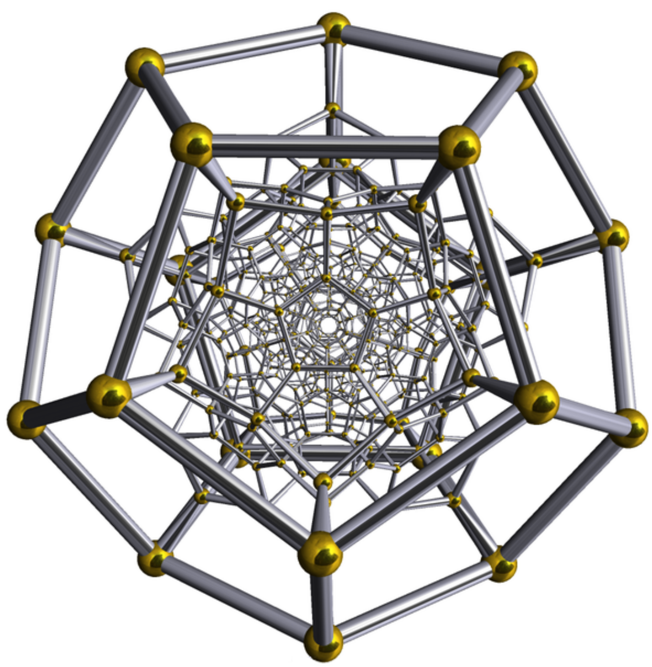
You'll meet the Lipschitz integers and Hurwitz integers sitting inside the quaternions — and the Lipschitzian, Hurwitzian and Gravesian integers sitting inside the octonions. I suspect all these charming names are due to Conway, who has a real gift for terminology. But best of all are the Cayley integers, which form a famous 8-dimensional lattice: the E_8 lattice! This gives the densest lattice packing of spheres in 8 dimensions: each sphere touches 240 others.

But you've heard me rhapsodizing about E_8 for years. What's new this week? Well, recently I was perusing a draft of a paper on various kinds of quaternionic and octonionic integers, which was kindly sent to me by Norman Johnson. I'm afraid that paper is still top secret, but an interesting issue came up.

People know all the finite subgroups of the unit quaternions, otherwise known as $SU(2)$, the double cover of the 3d rotation group. The most famous of these are:

- the 24-element binary tetrahedral group
- the 48-element binary octahedral group
- the 120-element binary icosahedral group

These are the double covers of the rotational symmetry groups of the Platonic solids. Since beautiful structures like this have a way of connecting diverse subjects, you shouldn't be surprised that these groups show up all over in math, from the McKay correspondence and Klein's work on the quintic equation (see "Week 65" and "Week 230") to the theory of modular forms (see "Week 197"). I've talked about the binary icosahedral group many times before: it's also called the 120-cell, and projected down to 3 dimensions it looks like this:



But the best place to learn the classification of finite subgroups of the unit quaternions is the book by Conway and Smith.

For some idiotic reason I'd never pondered the analogous question for the octonions until Norman Johnson brought it up! The unit octonions form a 7-dimensional sphere. They don't form a group, since multiplication of octonions isn't associative. But they form a "loop", which is just like a group but with the associative law dropped. Since they're a smooth manifold, and the group operations are smooth, they form a "Lie loop".

In fact the only spheres that are Lie groups are:

- the unit real numbers: the 0-sphere, also called $\mathbb{Z}/2$ or $\mathrm{O}(1)$
- the unit complex numbers: the 1-sphere, also called $\mathrm{SO}(2)$ or $\mathrm{U}(1)$
- the unit quaternions: the 3-sphere, also called $\mathrm{SU}(2)$ or $\mathrm{Sp}(1)$

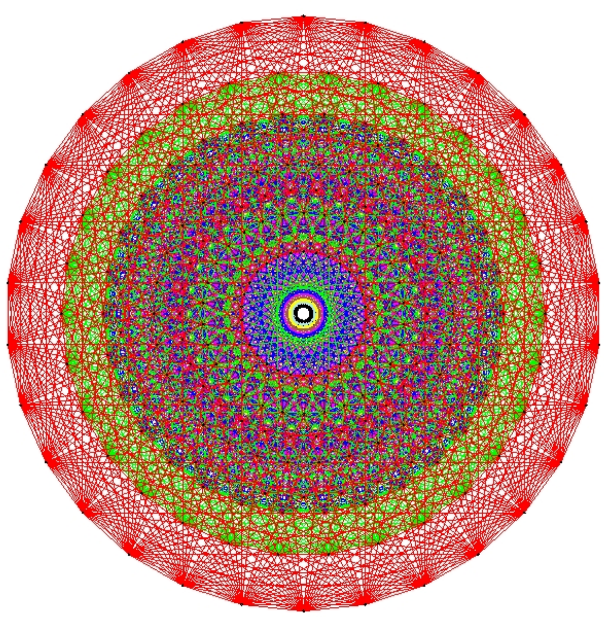
And the only other sphere that's a "Lie loop" is the unit octonions! So we can — and should! — ask: what are the finite subloops of the unit octonions?

Michael Kinyon, an expert on loops, quickly provided two references that settle the question:

6) R. T. Curtis, "Construction of a family of Moufang loops", *Math. Proc. Cambridge Philos. Soc.* **142** (2007), 233–237.

7) P. Boddington and D. Rumynin, "On Curtis' theorem about finite octonionic loops", *Proc. Amer. Math. Soc.* **135** (2007), 1651–1657. Available at `http://www.ams.org/journals/proc/2007-135-06/S0002-9939-07-08707-2/`

There are no huge surprises here. The most exciting finite subloop has 240 elements: it consists of the Cayley integers of length 1, otherwise known as the root vectors of E_8. You've probably seen this thing:



The rest of the finite subloops are either finite subgroups of the unit quaternions or "doubles" of these. No exotic beasts I hadn't dreamt of. But it's very nice to know the full story!

I also have some other news on the octonionic front. John Huerta and I wrote a second paper on division algebras and supersymmetry, where we explain how to construct the "Lie n-superalgebras" that govern classical superstring and super-2-brane theories:

8) John Baez and John Huerta, "Supersymmetry and division algebras II", available as arXiv:1003.3436.

As you may know, Lie groups and their Lie algebras are incredibly important in gauge theory, which describe how particles change when you move them around. Lately people have been developing "higher gauge theory", which does the same job for strings and higher-dimensional membranes. For strings we need Lie 2-groups and their Lie 2-algebras. Lie 2-groups are like Lie groups except that they're *categories* instead of sets... and similarly for Lie 2-algebras. Go up one more dimension and you need math based on 2-*categories*. So, for 2-branes, which look like soap bubbles instead of loops of string, you need Lie 3-groups and their Lie 3-algebras. Etcetera.

So, the race is on to construct, classify and understand Lie n-groups and Lie n-algebras — and redo all of geometry to take advantage of these higher structures! For an easy introduction, try:

9) John Baez and John Huerta, "An invitation to higher gauge theory", available as arXiv:1003.4485.

But for supersymmetric theories, geometry based on manifolds isn't enough: we need supermanifolds. The world is made of bosons and fermions, and supersymmetry is an attempt to unite them. So, a supermanifold has ordinary "even" or "bosonic" coordinate functions that commute with each other, but also "odd" or "fermionic" coordinate functions that anticommute. For the last few decades people have been redoing geometry using supermanifolds. As part of this, they've done a lot of work to construct, classify and understand Lie supergroups and their Lie superalgebras.

Superstring theory combines supersymmetry and higher-dimensional membranes in a beautiful way. It's never made any predictions about the real world, and it may never succeed in doing that. But it's been a real boon for mathematicians. And here's another example: we can now enjoy ourselves developing a theory of Lie n-supergroups and their Lie n-superalgebras!

I might feel guilty indulging in such decadent pleasures, were it not that I plan to start work on more practical projects. But having spent years thinking about division algebras and higher gauge theory, it was irresistible to combine them — especially since my student John Huerta has a knack for this stuff.

And here's what we discovered:

- The real numbers give rise to a Lie 2-superalgebra which describes the symmetries of classical superstrings in 3d spacetime.
- The complex numbers give rise to a Lie 2-superalgebra which describes the symmetries of classical superstrings in 4d spacetime.

- The quaternions give rise to a Lie 2-superalgebra which describes the symmetries of classical superstrings in 6d spacetime.
- The octonions give rise to a Lie 2-superalgebra which describes the symmetries of classical superstrings in 10d spacetime.

3, 4, 6 and 10 — these are two more than the dimensions of the real numbers, complex numbers, quaternions and octonions. I've discussed this pattern many times here. But then we discovered something else:

- The real numbers give rise to a Lie 3-superalgebra which describes the symmetries of classical super-2-branes in 4d spacetime.
- The complex numbers give rise to a Lie 3-superalgebra which describes the symmetries of classical super-2-branes in 5d spacetime.
- The quaternions give rise to a Lie 3-superalgebra which describes the symmetries of classical super-2-branes in 7d spacetime.
- The octonions give rise to a Lie 3-superalgebra which describes the symmetries of classical super-2-branes in 11d spacetime.

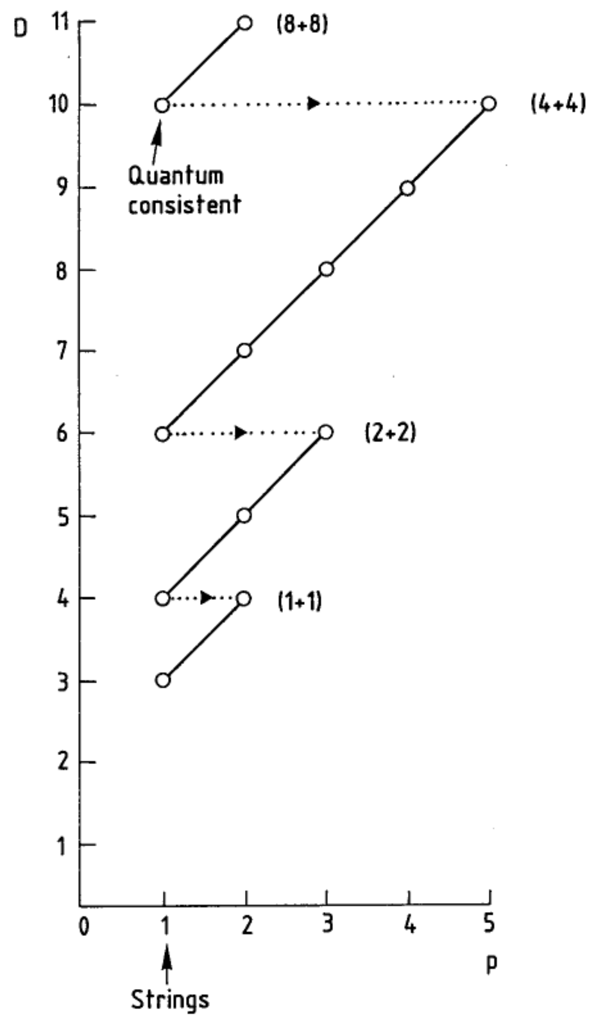
4, 5, 7 and 11 — these are *three* more than the dimensions of the real numbers, complex numbers, quaternions and octonions! And the 11d case is related to "M-theory" — that mysterious dream you've probably heard people muttering about.

You might ask if the pattern keeps going on, like this:

- Do the real numbers give rise to a Lie 4-superalgebra which describes the symmetries of classical super-3-branes in 5d spacetime?
- Do the complex numbers give rise to a Lie 4-superalgebra which describes the symmetries of classical super-3-branes in 6d spacetime?
- Do the quaternions give rise to a Lie 4-superalgebra which describes the symmetries of classical super-3-branes in 8d spacetime?
- Do the octonions give rise to a Lie 4-superalgebra which describes the symmetries of classical super-3-branes in 12d spacetime?

But some calculations by Tevian Dray and John Huerta, together with a lot of physics lore, suggest that the pattern does *not* keep going on — at least not for the most exciting case, the octonionic case. You can see what I mean by looking at the "brane scan" in this classic paper by Duff:

10) Michael J. Duff, "Supermembranes: the first fifteen weeks", *Classical and Quantum Gravity* **5** (1988), 189–205. Also available at `http://ccdb4fs.kek.jp/cgi-bin/img_index?8708425`



The brane-scan

The story seems to fizzle out after 11 dimensions. And that's part of what intrigues me about division algebras and related exceptional structures in math: the funny fragmentary patterns that don't go on forever.

Recently Peter Woit criticized Duff for some remarks in this article:

11) Michael Duff, "Black holes and qubits", *CERN Courier* May 5, 2010. Available at `http://cerncourier.com/cws/article/cern/42328`

12) Peter Woit, "Applying string theory to quantum information theory", `http://www.math.columbia.edu/\~woit/wordpress/?p=2925`

Duff's article describes how he noticed some of the same math showing up in superstring calculations of black hole entropy and patterns of quantum entanglement between qubits. This leads into some nice math involving octonions and related exceptional structures like the Fano plane and Cayley's "hyperdeterminants".

Unfortunately, Duff gets a bit carried away. For example, he says that string theory "predicts" the various ways that three qubits can be entangled. Someone who didn't know physics might jump to the conclusion that this is a prediction whose confirmation lends credence to string theory as a description of the fundamental constituents of nature. It's not!

I also doubt that "superquantum computing" is likely to be practical... though I've read interesting things about supersymmetry in graphene, so I could wind up eating my words.

On the other hand, the math is fascinating. For details, try these papers:

13) Akimasa Miyake and Miki Wadati, "Multipartite entanglement and hyperdeterminants", *Quant. Info. Comp.* **2** (2002), 540–555. Also available as quant-ph/0212146.

14) Michael J. Duff and S. Ferrara, "E_7 and the tripartite entanglement of seven qubits", *Phys. Rev. D* **76** 025018 (2007). Also available as quant-ph/0609227.

15) Michael J. Duff and S. Ferrara, "E_6 and the bipartite entanglement of qutrits", *Phys. Rev. D* **76** 124023 (2007). Also available as arXiv:0704.0507.

or this very nice recent one:

16) Bianca L. Cerchiai and Bert van Geemen, "From qubits to E_7", available as arXiv:1003.4255.

Since I've spent a lot of time talking about the Fano plane, the octonions, and exceptional groups like E_6, E_7 and E_8, let me say just a word or two about hyperdeterminants.

In the 1840's, after his work on determinants, Arthur Cayley invented a theory of "hyperdeterminants" for $2 \times 2 \times 2$ arrays of numbers. They're a bit like determinants of 2×2 matrices, but more complicated. They lay dormant for about a century, but were recently revived by three bigshots: Gel'fand, Kapranov and Zelevinsky. I never understood them, but when Woit called them "extraordinarily obscure", it was like waving a red flag in front of a bull. I charged forward... and now I sort of understand them, at least a little.

Suppose we have a 2×2 matrix of complex numbers. We can think of this as an element of the Hilbert space

$$\mathbb{C}^2 \otimes \mathbb{C}^2$$

so we get a linear functional

$$\mathbb{C}^2 \otimes \mathbb{C}^2 \to \mathbb{C}$$

sending any vector to its inner product with this element. We can then regard this linear functional as a function

$$f : \mathbb{C}^2 \times \mathbb{C}^2 \to \mathbb{C}$$

that's linear in each argument. Is there a nonzero point in $\mathbb{C}^2 \times \mathbb{C}^2$ where the gradient of f vanishes? The answer is yes if and only if the determinant of our 2×2 matrix is zero!

Next, suppose we have a $2 \times 2 \times 2$ array of complex numbers. We can think of this as an element of the Hilbert space

$$\mathbb{C}^2 \otimes \mathbb{C}^2 \otimes \mathbb{C}^2$$

so we get a linear functional

$$\mathbb{C}^2 \otimes \mathbb{C}^2 \otimes \mathbb{C}^2 \to \mathbb{C}$$

sending any vector to its inner product with this element. We can then regard this linear functional as a function

$$f\colon \mathbb{C}^2 \times \mathbb{C}^2 \times \mathbb{C}^2 \to \mathbb{C}$$

that's linear in each argument. Is there a nonzero point in $\mathbb{C}^2 \times \mathbb{C}^2 \times \mathbb{C}^2$ where the gradient of f vanishes? The answer is yes if and only if the hyperdeterminant of our $2 \times 2 \times 2$ array is zero!

Here's another way to think about it. Suppose you have a quantum system made of 3 subsystems, each described by a 2d Hilbert space. The Hilbert space of the whole system is thus

$$\mathbb{C}^2 \otimes \mathbb{C}^2 \otimes \mathbb{C}^2$$

The hyperdeterminant is a function on this space that can be thought of as measuring how correlated — or to use a bit of jargon, "entangled" — the 3 subsystems are. In particular, if we look at unit vectors modulo phase, we get the projective space $\mathbb{CP}^7$. The group

$$\mathrm{GL}(2,\mathbb{C}) \times \mathrm{GL}(2,\mathbb{C}) \times \mathrm{GL}(2,\mathbb{C})$$

acts on this. There's an open dense orbit, coming from the vectors whose hyperdeterminant is nonzero. These states are quite entangled, as you'd expect: entanglement is not an exceptional situation, it's generic. And then there are various smaller orbits, going down to the 1-point orbit coming from vectors like

$$u \otimes v \otimes w$$

These describe states with no entanglement at all!

We can make what I'm saying a bit more precise at the expense of a little more math. The polynomial functions on $\mathbb{C}^2 \otimes \mathbb{C}^2 \otimes \mathbb{C}^2$ that are invariant under the action of $\mathrm{SL}(2,\mathbb{C}) \times \mathrm{SL}(2,\mathbb{C}) \times \mathrm{SL}(2,\mathbb{C})$ form an algebra, and this algebra is generated by the hyperdeterminant, which is a homogeneous cubic polynomial. (Why did I switch from $\mathrm{GL}(2,\mathbb{C})$ to $\mathrm{SL}(2,\mathbb{C})$? Because the hyperdeterminant is not invariant under the bigger group $\mathrm{GL}(2,\mathbb{C}) \times \mathrm{GL}(2,\mathbb{C}) \times \mathrm{GL}(2,\mathbb{C})$. However, it comes close: it gets multiplied by a scalar in a simple way.)

If we try to generalize this idea to tensor products of more spaces, like $\mathbb{C}^2 \otimes \mathbb{C}^2 \otimes \mathbb{C}^2 \otimes \mathbb{C}^2$ we typically get an algebra that's not generated by a single polynomial. The same happens if we jack up the dimension, for example replacing $\mathbb{C}^2$ by $\mathbb{C}^9$. So, the situations where a single polynomial does the job are special.

As an extra lure, let me add that you can write the Lagrangian for a string in $(2+2)$-dimensional spacetime using $2 \times 2 \times 2$ hyperdeterminants! The formula is very pretty and simple:

17) Michael J. Duff, "Hidden symmetries of the Nambu-Goto action", *Phys. Lett.* **B641** (2006), 335–337. Also available as hep-th/0602160.

This being the 21st century, there is even a blog on hyperdeterminants:

18) *Hyperdeterminacy*, `http://hyperdeterminant.wordpress.com/2008/09/25/hello-world/`

The author begins with some introductory posts which are definitely worth reading, so I've linked to the first of those. He expresses the worry that "this may be turn out to be one of the least read blogs in the blogosphere". Go visit, leave a comment, and prove him wrong!

Addenda: On August 8th 2010, I received the following mail from Pter Lvay, who has allowed me to quote it here:

> *Dear John,*
>
> *I used to read your fascinating blog. Recently I have come across your "Week 298" post (14 May) concerning hyperdeterminants and reflections on a paper by Duff which appeared in CERN Courier on the black hole qubit correspondence.*
>
> *Since I know that you are really fascinated by octonions, Freudenthal systems, etc., with this mail I intend to draw your attention to papers of mine connected to the stuff of your "Week 298" post. Moreover, I would also like to add some further hints (which Duff does not mention) why this analogy is worth working out further.*
>
> *Originally I used to work on the field of quantum entanglement, and group theoretical and geometrical methods in quantum physics. My paper:*
>
> *19) Pter Lvay, "The twistor geometry of three-qubit entanglement", available as `quant-ph/0403060`.*
>
> *on the "Cayley" hyperdeterminant, twistors and three qubit entanglement was the one that motivated originally Duff's work (as he mentions in the CERN Courier). Later I have written another paper on the four qubit case (hyperdeterminant):*
>
> *20) Pter Lvay, "On the geometry of four qubit invariants", available as `quant-ph/0605151`.*
>
> *After Duff's and Kallosh and Linde's paper I have shown that the black hole qubit analogy also works for issues concerning DYNAMICS. I have shown that the attractor mechanism (a process used for moduli stabilization on the BH horizon) can be rephrased in this picture as a distillation procedure of GHZ-like states:*
>
> *21) Pter Lvay, "Stringy black holes and the geometry of entanglement", available as `hep-th/0603136`.*
> *Pter Lvay, "A three-qubit interpretation of BPS and non-BPS STU black holes", available as `arXiv:0708.2799`.*
> *Pter Lvay and Szilrd Szalay, "The attractor mechanism as a distillation procedure", available as `arXiv:1004.2346`.*

The physical nature of these moduli, charge and warp factor dependent "states" (or whether is it legitimate to call them states at all) is, however, still unclear.

Independently of Duff and Ferrara during the summer of 2006 I worked out the E_7 *tripartite entanglement of seven qubits picture. My paper came out later because I have bogged down on some maths details (I also wanted to see the* E_7 *generators in the 56 dim rep. acting as qubit gates — without success!) However, the idea that this curious type of entanglement is related to the structure of the Fano plane appears first in this paper:*

22) *Pter Lvay, "Strings, black holes, the tripartite entanglement of seven qubits and the Fano plane", available as* `hep-th/0610314`.

Here I made the conjecture that Freudenthal systems and issues concerning the magic square could be relevant to further developments of the black hole qubit analogy, initiating the nice study of Duff et al*, culminating in their valuable Phys. Reports paper.*

In the meanwhile with my student we have shown that Freudenthal systems are capable of giving hints for solving the classification problem of entanglement classes for systems consisting of special types of indistinguishable constituents (fermions and bosons). The black hole entropy formulas of string theory and supergravity also gave suggestions how to define sensible (tripartite) entanglement measures for these systems:

23) *Pter Lvay and Pter Vrana, "Three fermions with six single particle states can be entangled in two inequivalent ways", available as* `arXiv:0806.4076`.

    Pter Lvay and Pter Vrana, "Special entangled quantum systems and the Freudenthal construction", available as `arXiv:0902.2269`.

Apart from these studies after realizing the relevance of finite geometric ideas with a Slovak co-worker Metod Saniga we have shown that the structure of the E_6 *and* E_7 *invariants giving rise to black hole entropy formulas are related to generalizations of Mermin Square like configurations, and generalized polygons. Our studies motivated partly the nice paper of van Geemen and B. L Cerchiai you mention in your post ("From qubits to* E_7*"). Since the finite geometric structures relevant to the black hole qubit correspondence turned out to be just geometric hyperplanes of incidence geometries for* n*-qubit systems, we conducted a mathematical study on the structure of such hyperplanes giving rise to another incidence geometry: the Veldkamp space. The nice maths results can be found in:*

24) *Pter Vrana and Pter Lvay, "The Veldkamp space of multiple qubits", available as* `arXiv:0906.3655`.

Such notions as symplectic structure, quadratic forms, transvections, and the group $\mathrm{Sp}(2n,2)$ *connected to* n*-qubit systems and black holes appear here. The language of this paper is similar to the van Geemen paper.*

Recently I have shown that certain solutions of the STU model living in 4D and coming from one sector of the $\mathrm{E}_7(7)$ *invariant* $N = 8$ *SUGRA Duff mentions in his paper in connection to three qubits, is really a model living in 3D coming from a coset of* $\mathrm{E}_8(8)$ *related to FOUR QUBITS. In this paper I have given hints that the classification problem for four qubits can be translated to the classification problem of black hole solutions (BPS and non BPS, extremal and even possibly non extremal).*

25) *Pter Lvay, "STU black holes as four qubit systems", available as* `arXiv:1004.3639`.

(The idea that four qubit systems show up in STU truncations first appeared in my Fano-E_7 *paper. This is related to the structure of a coset of* $\mathrm{SO}(4, 4)$*, with triality making its debut via permutation of the qubits.)*

Based on the results of this paper the challenge to relate the BH classification problem based on four qubit systems by fitting together the existing results in the literature was recently taken up by Duff et al.

I hope that these results add some useful hints to update the picture on the black hole qubit correspondence.

I think that the main virtue of this field is that fascinating maths (like octonions, Freudenthal systems, finite geometries etc.) will finally makes its debut to understanding quantum entanglement better. On the string theory side such studies might initiate some new way of looking at existing results in the field of stringy black hole solutions.

Of course finding the underlying physics (if any) is still out there!

With best regards,

Peter Levay
Department of Theoretical Physics
Institute of Physics
Budapest University of Technology
HUNGARY

For more discussion, visit the n-Category Caf.

Quaternions appear to exude an air of nineteenth century decay, as a rather unsuccessful species in the struggle-for-life of mathematical ideas. Mathematicians, admittedly, still keep a warm place in their hearts for the remarkable algebraic properties of quaternions but, alas, such enthusiasm means little to the harder-headed physical scientist.

— Simon L. Altmann

Week 299

June 12, 2010

Two weeks ago I went to Oxford to attend a school on Quantum Information and Computer Science, and then a workshop on Quantum Physics and Logic. So, I'll start by telling you about those.

I'll show you where you can see videos of the talks. You can learn about string diagrams and see how people use these in quantum physics. You can even watch a program called "Quantomatic" automatically carry out some string diagram computations! I'll explain how classical structures give Frobenius algebras, and how complementary classical structures almost give Hopf algebras. And then I'll tell you about Aaron Fenyes' no-cloning theorem for classical mechanics. There was a lot more to the conference, but that's all I have the energy for.

Why? Well, after I came home, my friend the combinatorist Bill Schmitt paid me a visit. He told me a lot of interesting stuff about about "pre-Lie algebras". These are algebraic gadgets with deep connections to trees, operads, and the work of Connes and Kreimer on renormalization in quantum field theory. So, I want to tell you about that stuff, too.

Let's get started. You can see all the talks here:

1) "Oxford Quantum Talks Archive", `http://www.comlab.ox.ac.uk/quantum/content/`

   QICS Spring School 2010, talks available at `http://www.comlab.ox.ac.uk/quantum/content/events.html#QICS%20spring%20school%202010`
   Quantum Physics and Logic 2010, talks available at `http://www.comlab.ox.ac.uk/quantum/content/events.html#Quantum%20Physics%20and%20Logic%202010`

I've been using pictures called "string diagrams" for a long time here on This Week's Finds, and I've tried to explain them, but these talks give a nice systematic treatment:

2) *Introduction to monoidal categories and graphical calculus*.
   Lecture 1 by Chris Heunen, available at `http://www.comlab.ox.ac.uk/quantum/content/1005005/`
   Lecture 2 by Jamie Vicary, available at `http://www.comlab.ox.ac.uk/quantum/content/1005010/`

With the help of these diagrams we can think about many, many things. In particular, we can use them to describe processes in quantum mechanics. Feynman diagrams are an example. But now people in quantum information theory are using them in very different ways. For example, they're using them to study "classical structures" in quantum mechanics.

What's a classical structure? The basic idea is simple. You can't "clone a quantum". In other words, you can't build a "quantum copying machine" where you feed in a quantum system in an arbitrary state and have two identical copies pop out, both in the same state.

Why not? Well, you could try to measure everything about your system and make a copy where all those measurements have the same values. But you can never succeed! Measuring one thing will change the values of other things you already measured, in uncontrollable random ways. So you can never know everything about your system... not all at once! So, it's impossible to make an exact copy.

However, sometimes measuring one thing does *not* mess up the value of something else you measured. Quantities that get along this way are called "commuting" observables. A "classical structure" is a set of commuting observables that's as big as possible. And for each classical structure, we can build a copying machine that works for *these* observables.

For example, there's no way to take an electron, stick it in a copying machine, and duplicate everything about it. You can measure the spin of an electron along any axis. If you measure the spin along the x-axis you get one of two results: "up" or "down". Similarly for the spin along the y-axis. However, measuring the spin along the x-axis messes up its spin along the y-axis.

So, you can build a machine that takes an electron, measures its spin along the x-axis, and spits out two electrons in that same state: either "spin-up" or "spin-down" along the x-axis. But if you put an electron with spin up along the y-axis in this machine, it will not be correctly duplicated.

In this example, there's a classical structure consisting of the spin along the x-axis, and every function of this observable... but this classical structure does *not* include the spin along the y-axis.

Here's a quick sketch of how the math works. If you're a mathematician, this should be far less confusing than the prose you just suffered through. In fact, you may be left wondering why I turned such simple math into such murky prose. But that's typical of quantum mechanics: the math is crystal clear, but when you try to explain how it describes the real world, it starts sounding mysterious.

Suppose H is a Hilbert space. If we were trying to build a quantum copying machine, it would be nice to have a linear operator like this:

$$\begin{gathered} H \to H \otimes H \\ \psi \mapsto \psi \otimes \psi \end{gathered}$$

But this operator is not linear, because doubling ψ would quadruple $\psi \otimes \psi$.

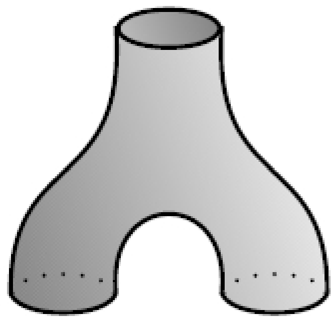
Life gets easier when we have a classical structure. An "observable" in quantum mechanics is a self-adjoint operator. So, a "classical structure" is a maximal set of commuting self-adjoint operators. If H is finite-dimensional, we can get any classical structure from an orthonormal basis. How? Just take all the self-adjoint operators that are diagonal in that basis.

If you pick an orthonormal basis for H, say e_i, there's a unique linear operator that duplicates states in that basis:

$$\begin{gathered} H \to H \otimes H \\ e_i \mapsto e_i \otimes e_i \end{gathered}$$

So, this is how a classical structure gives a "duplication operator".

Here's where the string diagrams come in. We can draw our duplication operator in a funny symbolic way like this:

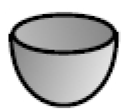


This is a picture of a 2d surface where one circle comes in and two go out — a kind of metaphor for duplication. We'll see why it's a good metaphor in a minute.

There's another thing we can do after we've picked an orthonormal basis for our Hilbert space H: we can define a "deletion operator"

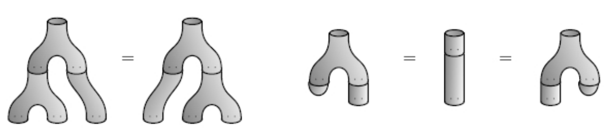
$$\begin{aligned} H &\to \mathbb{C} \\ e_i &\mapsto 1 \end{aligned}$$

which sends each state in the basis to the number 1. Here $\mathbb{C}$ is the complex numbers, a 1-dimensional Hilbert space. We can think of $\mathbb{C}$ as a kind of "garbage bin", and think of our operator as "throwing out" states in our basis. We can draw it as a cup-shaped thing, like this:



This is not really a picture of a garbage bin, though it looks like that too. It's a picture of a 2d surface where one circle comes in and *none* go out!

Now, the cool part is that our duplication and deletion operators satisfy rules that look very intuitive in terms of these pictures. For example, if we duplicate a state and then duplicate one of the copies, it doesn't matter which copy we duplicate. And if we duplicate a state and then delete either copy, it's the same as not doing anything:



These rules are "topologically true": to get one of these equations, we just take one picture and wiggle it and warp it until it looks like another.

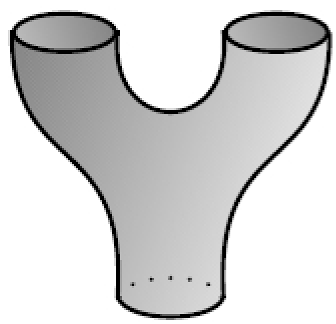
There are a bunch of other rules. Almost all of them are topologically true — exactly what you'd dream up by playing around with pictures of 2d surfaces. But to write down these rules, you need to notice that any operator between Hilbert spaces has an adjoint going the other way. So, besides duplication:

$$H \to H \otimes H$$

we have its adjoint:

$$H \otimes H \to H$$

which we draw just like duplication, except upside-down:



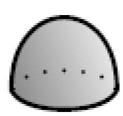
and besides deletion:

$$H \to \mathbb{C}$$

we have its adjoint:

$$\mathbb{C} \to H$$

which we draw just like deletion, except upside-down:



In math we call these four operators the "multiplication":

$$m\colon H \otimes H \to H$$

the "unit":

$$i\colon \mathbb{C} \to H$$

the "comultiplication":

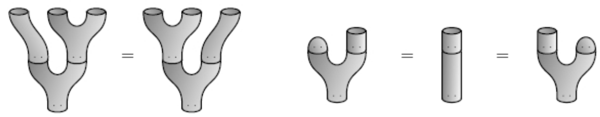
$$\Delta\colon H \to H \otimes H$$

and the "counit":

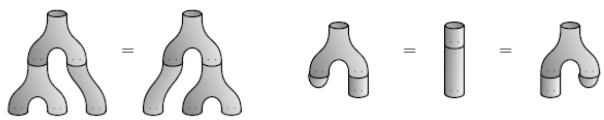
$$e\colon H \to \mathbb{C}$$

And we can summarize all the rules these operators obey by saying that H is a "special commutative dagger-Frobenius algebra". That's a mouthful, but as I said, almost all these rules come from topologically allowed manipulations on 2d surfaces.

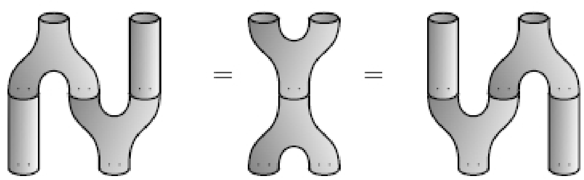
What are these rules? Well, I listed the rules for a Frobenius algebra back in "Week 268". There are the "associative law" and the "left and right unit laws":



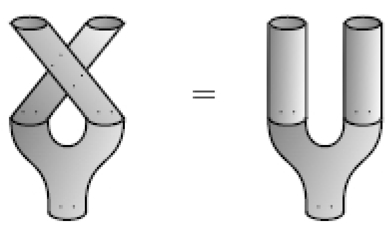
together with the "coassociative law" and "left and right counit laws", which we've already seen:



and, last but not least, the "Frobenius laws":



Next, we say a Frobenius algebra is "commutative" if it obeys this extra law, which is also topologically true:



We say a Frobenius algebra is "special" if it obeys yet another law, which I'll show you later. So the only extra thing you need to know is what's a "dagger-Frobenius" algebra. And that's just a Frobenius algebra that's also a Hilbert space, where the multiplication and unit are adjoint to the comultiplication and counit! For more, try these papers:

3) Bob Coecke and Dusko Pavlovic, "Quantum measurements without sums", in *The Mathematics of Quantum Computation and Technology*, eds. Chen, Kauffman and

Lomonaco, Chapman and Hall/CRC, New York, pp. 559–596. Also available as quant-ph/0608035.

4) Jamie Vicary, "Categorical formulation of quantum algebras", available as arXiv:0805.0432.

5) Bob Coecke, Dusko Pavlovic and Jamie Vicary, "A new description of orthogonal bases", available as arXiv:0810.0812

But what's really going on with all these pictures of 2d surfaces? You see them a lot in topological quantum field theory. Indeed, in "Week 268" I explained that a commutative Frobenius algebra is exactly what we need to get a 2d topological quantum field theory, or TQFT for short. This is a way of making precise the idea that any of these pictures gives a well-defined operator — and warping or wiggling the picture doesn't change the operator.

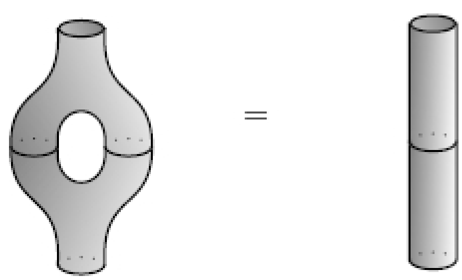
Even better, a commutative *dagger*-Frobenius algebra is exactly what we need to get a "unitary" 2d TQFT. This means that the upside-down version of a given picture gives the adjoint operator.

So, we're seeing a curious fact:

> ***a classical structure on a finite-dimensional Hilbert space gives a unitary 2d TQFT***

This sounds like it should be important, because it links two subjects with very different flavors: the foundations of quantum mechanics, and topological quantum field theory. But I have no idea what it really means.

Maybe you can help me! But before you try your hand at this problem, I should warn you that we don't get *all* unitary 2d TQFTs from classical structures — because we don't get *all* commutative dagger-Frobenius algebras. We only get the "special" ones, which obey this extra rule:



This rule is *not* topologically true. Indeed when it holds, our 2d TQFT is completely insensitive to how many handles our surface has. That makes it sort of boring.

For a closed surface, the number of handles is called the "genus". So, the real puzzle is to understand this more mysterious slogan:

> ***a classical structure on a finite-dimensional Hilbert space is the same as a* genus-independent *unitary 2d TQFT***

I've been mulling this over for about a year now, with no great insights. Mathematically it's almost trivial. But physically, I can't tell if it's the tip of an interesting iceberg, or just a coincidence.

These talks offered some extra clues:

6) Ross Duncan, "Convexity, categorical semantics and the foundations of physics", video available at `http://www.comlab.ox.ac.uk/quantum/content/1005102/`

7) Chris Heunen, "Complementarity in categorical quantum mechanics", video and slides available at `http://www.comlab.ox.ac.uk/quantum/content/1005115/`

8) Simon Perdrix, "Classical-quantum graphical calculus", video available at `http://www.comlab.ox.ac.uk/quantum/content/1005015/`

For more details, try these papers:

9) Bob Coecke, Eric Oliver Paquette and Dusko Pavlovic, "Classical and quantum structuralism", available as `arXiv:0904.1997`.

10) Bob Coecke and Ross Duncan, "Interacting quantum observables: categorical algebra and diagrammatics", available as `arXiv:0906.4725`.

11) Bob Coecke, "Quantum picturalism", available as `arXiv:0908.1787`.

12) Bob Coecke and Simon Perdrix, "Environment and classical channels in categorical quantum mechanics", arXiv/1004.1598.

Among other things, these papers say what you can *do* with a classical structure. You can do a bunch of things — and you can do them all very generally, because you can do them using just pictures. So, you don't need to be working in the category of finite-dimensional Hilbert spaces: any "dagger-compact category" will do. You can define a special commutative dagger-Frobenius algebra in any such category, and this gives a very general concept of classical structure.

This is the sort of thing that makes category theorists drool. But I will restrain myself! I won't work in such generality. I'll just work with finite-dimensional Hilbert spaces, and just sketch a few things we can do with classical structures. I won't even explain how we can do them just using pictures. . . even though that's the really cool part.

For starters, every classical structure determines a "phase group". If the classical structure comes from an orthonormal basis in the way I've described, its phase group consists of all unitary operators that are diagonal in this basis. But the cool part is that we can define this group just using pictures, and prove it's abelian, and so on.

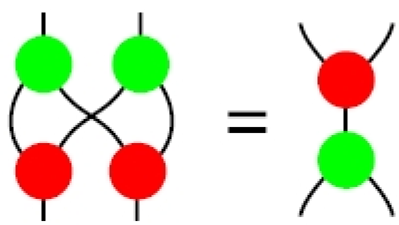
We can also use pictures to define "complementarity". In physics we say position and momentum are complementary because if you know everything about one, you cannot know anything about the other. But we can also say what it means for two classical structures to be complementary. For a finite-dimensional Hilbert space, any classical structure comes from an orthonormal basis — and two of them are "complementary" if they come from "mutually unbiased" bases, meaning bases e_i and f_j such that

$$|\langle e_i, f_j \rangle|$$

is independent of i and j. This means that if you know precisely which state e_i your system is in, it has equal chances of being found in any of the states f_j.

So, for example, the spin-up and spin-down states of an electron as measured along the x-axis form one orthonormal basis. The spin-up and spin-down states as measured along the y-axis form another. And these bases are mutually unbiased. So, knowing everything about the spin along the x-axis tells you nothing about the spin along the y-axis. And vice versa!

When we have two complementary classical structures, we get two ways of making our Hilbert space into a Frobenius algebra. And these are related in a cool way: if we use the multiplication of the first, and the comultiplication of the second, we almost get a "Hopf algebra"! If we had a Hopf algebra, this relation would hold:



where we write the multiplication of our first Frobenius algebra as a red dot, and the comultiplication of the second as a green dot. But in fact this holds only up to a constant factor, so we get a "scaled Hopf algebra". I think this is fascinating because there's a constant interplay between Frobenius algebras and Hopf algebras in mathematics, and here's yet another example.

Now, if you like these pictures, you've got to see "Quantomatic" in action:

13) Lucas Dixon, "Quantomatic demo", video available at `http://www.comlab.ox.ac.uk/quantum/content/1005019/`

14) Lucas Dixon, Ross Duncan, Aleks Kissinger and Alex Merry, "Quantomatic", `http://dream.inf.ed.ac.uk/projects/quantomatic/`

This is a program that automatically carries out calculations involving these pictures that I've been drawing! It's a lot of fun. If you know quantum computation, you'll see that we can describe a lot of quantum logic gates, like controlled not gates and Hadamard gates, using these pictures. And if you know 2-categories, you'll realize that the *processes of rewriting diagrams* are actually 2-morphisms in a 2-category! So higher category is sneaking in to the subject here. I bet we'll see a lot more of it in years to come.

There were many more interesting talks, but I'm running out of energy, so I just want to say one more thing about "cloning":

15) Aaron Fenyes, "There's no cloning in symplectic mechanics", available at `http://math.ucr.edu/home/baez/dual/no-cloning.pdf`

This paper argues that the laws of classical mechanics make it impossible to build a "cloning machine". If we had such a machine, we could put two boxes with balls in them into slots on top of the machine. The machine would copy the position and velocity of the first ball over to the second one. When we were done, the second ball would be a “clone” of the first: a perfect copy.

Let me be a bit more precise. The boxes and the balls inside them are identically made. The first ball, the one we want to clone, has an arbitrary position and velocity. Well, of course its position is somewhere in the box! And if you like, we can say it’s going no faster than 10 miles per hour. The second ball starts out in some fixed state. Let’s say it has zero velocity and it’s sitting right in a little dent in the middle of the box.

We pop the boxes into the machine. The machine can open the tops of the boxes and insert sensors. When we press a big red button, the machine measures the position and velocity of the first ball. It then does whatever it wants, but after a while two boxes come out of the bottom of the machines. . . and then a bell rings.

And when the bell rings, both balls have the same position and velocity that the first ball had when you pressed the button!

A “no-cloning theorem” says you can’t build a machine like this, given some assumptions on the laws of physics. The original no-cloning theorem was due to Wooters and Zurek:

16) W. K. Wootters and H. D. Zurek, “A single quantum cannot be cloned”, *Nature* **299** (1982), 802–803.

You can see a statement and proof here:

17) Wikipedia, “No-cloning theorem”, `http://en.wikipedia.org/wiki/No-cloning_theorem`

This was a *quantum* no-cloning theorem. In fact it was very general: it wasn’t about balls in boxes, it was about *any* quantum system where states are described by unit vectors in a Hilbert space and processes are described by unitary operators.

There have been many no-cloning theorems since then, but what’s new about Aaron Fenyes’ result is that it applies to *classical* mechanics. Again, it’s very general: it’s about any classical system where states are described by points in a symplectic manifold, and processes are described by symplectomorphisms.

If that sounds scary, well, be reassured that most classical mechanics problems fit into this framework. There are some that can’t, but I bet Fenyes’ result can be generalized to cover a lot of those, too.

So, the big question is: how does this result square with the widely shared intuition that we *can* copy classical information: that we *can* exactly measure the position and momentum of a ball, say, and get two balls into that state?

I don’t know the answer, but I’d like to — so let me know if you figure it out. Part of the problem is that when we use phrases like “classical structure” in our study of quantum mechanics, we are *not* talking about full-fledged classical mechanics, in which symplectic structures are important. But even after we work through this semantic issue, there’s a physics issue left to ponder.

After I got back to Riverside, my friend Bill Schmitt visited me. As usual, we spent long evenings listening to music and talking about math. I told you about our last get-together in “Week 265”. Back then, he told me about an amazing 588-page paper on Hopf algebras, combinatorics and category theory by Aguiar and Mahajan. By now that paper has grown into an 836-page book:

18) Marcelo Aguiar and Swapneel Mahajan, "Monoidal functors, species and Hopf algebras", available at `http://www.math.tamu.edu/~maguiar/a.pdf`

In his preface, Andr Joyal calls it "a quantum leap towards the mathematics of the future". Check it out!

Now Bill is working with Aguiar on developing this theory further. There are some marvelous ideas here... but I'd rather tell you about something else: pre-Lie algebras.

The name "pre-Lie algebra" suggests that we're about to do some "centipede mathematics". That's the cruel sport where you take a mathematical concept and see how many legs you can pull off and have it still walk. For example: you take the concept of group, remove the associative law and the identity element, obtaining the concept of "quasigroup"... and then see if there are still any theorems left.

A "pre-Lie algebra" sounds like a Lie algebra with some legs pulled off. But actually it's an *associative* algebra with some legs pulled off! Any associative algebra gives a Lie algebra — but you don't need the full force of the associative law to play this game. It's enough to have pre-Lie algebra.

That doesn't sound too interesting. But Bill convinced me that pre-Lie algebras are important. They were first named by Gerstenhaber:

19) M. Gerstenhaber, "The cohomology structure of an associative ring", *Ann. Math.* **78** (1963), 267–288.

who showed that the Hochschild chain complex of any ring, with grading shifted down by one, is a graded pre-Lie algebra. Later it was noticed that pre-Lie algebras show up in the combinatorics of trees, and are implicit in this old paper by Cayley:

20) Arthur Cayley, "On the theory of the analytical forms called trees", *Phil. Mag.* **13** (1857), 172–176.

The fun really starts when we relate these ideas to quantum field theory and operads...

...but first things first! The definition is simple enough. A pre-Lie algebra is a vector space A equipped with a bilinear product such that

$$[L(a), L(b)] = L([a, b])$$

for every a, b in A.

Huh? Here $L(a)$ stands for left multiplication by a:

$$L(a)b = ab$$

and the brackets denote commutators, so

$$[L(a), L(b)] = L(a)L(b) - L(b)L(a)$$

and

$$[a, b] = ab - ba$$

Putting together these formulas, we see the a pre-Lie algebra is vector space equipped with a bilinear product satisfying this scary equation:

$$a(bc) - b(ac) = (ab)c - (ba)c$$

Now, it's obvious that every associative algebra is a pre-Lie algebra: just take this scary equation and erase the parentheses, and you'll get something true. But not every pre-Lie algebra is associative. I'll give you some examples in a minute.

When we have an associative algebra, we get a Lie algebra with this bracket:

$$[a, b] = ab - ba$$

But we *also* get a Lie algebra this way from any pre-Lie algebra! That's why they're called "pre-Lie". To check this, take the scary equation above and use it to derive the Jacobi identity:

$$[[a, b], c] + [[b, c], a] + [[c, a,], b] = 0$$

Try it! It's fun. Honest!

Okay — so pre-Lie algebras are a cute generalization of associative algebras which are still good enough to give Lie algebras. But now you probably want me to explain how pre-Lie algebras show up in nature. I'll give three examples. The first is from geometry. The second is from quantum field theory. The third involves operads.

First, given a manifold with a flat torsion-free connection D on its tangent bundle, we can make the space of tangent vector fields into a pre-Lie algebra by defining

$$vw = D_v w$$

Second, Connes and Kreimer noticed a certain amazing group that plays an important role in the renormalization of quantum field theories:

21) Alain Connes and Dirk Kreimer, "Hopf algebras, renormalization and noncommutative geometry", *Commun. Math. Phys.* **199** (1998), 203–242. Also available as hep-th/9808042.

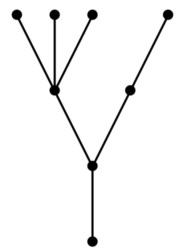
They built this group from Feynman diagrams. How? As you probably know, it's good to build groups starting from Lie algebras. And that's basically what they did. But what Lie algebra did they use?

The answer is easy if you know about pre-Lie algebras. But first I want to sketch the usual story. This is much more lengthy and technical. . . but I want to run you through it, so you can fully appreciate the elegance of the slick approach.

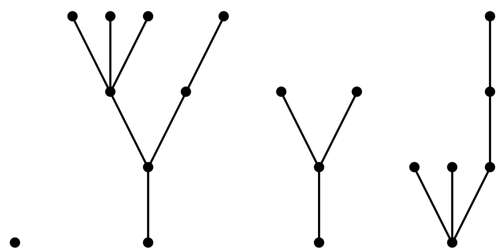
In the usual approach, you need to know that any Lie algebra gives rise to something called a "universal enveloping algebra". This is a cocommutative Hopf algebra, so its dual — defined in a careful way — is a commutative Hopf algebra. Connes and Kreimer started by getting their hands on this commutative Hopf algebra. Then they worked back to the Lie algebra.

To get their commutative Hopf algebra, they began with vector space whose basis consists of Feynman diagrams. But they also found it helpful to consider a simpler problem, where you start with a vector space whose basis consists of rooted forests.

A "rooted tree" looks like this:



The vertex at the bottom is called the "root". A finite collection of rooted trees is called a "rooted forest":



Let me show you how to take the vector space whose basis consists of all rooted forests, and make that into a commutative Hopf algebra. To do this, we need to give our vector space a multiplication and a comultiplication. And it's enough to say how these work on basis vectors, which are rooted forests.

To multiply two rooted forests, we simply set them side by side to get a new rooted forest. This multiplication is obviously associative. It's also commutative, since we don't care about any "ordering" or "planar structure" on our rooted forests.

In short, multiplication is boring. The fun part is comultiplication! To comultiply a rooted forest, we go through all ways of slicing it in a roughly horizontal way. Each slice gives two rooted forests: one below the slice, and one above. Then we form a big sum, where each slice contributes a term

$$(\text{below}) \otimes (\text{above})$$

where the first factor is the forest below the slice, while the second is the forest above the slice. You can see pictures of how this works in Connes and Kreimer's paper. The forest below the slice is sometimes called the "pruned forest", while the forest above is called the "fallen branches". If you've ever trimmed trees, that should give you the idea.

Starting from this Hopf algebra, you can get a Lie algebra. But there's a vastly quicker way to get this Lie algebra... if you know about pre-Lie algebras, that is. It's:

the Lie algebra coming from the free pre-Lie algebra on one generator!

That's what I call slick. Instead of paragraphs of theorems and pictures, a single devastatingly efficient phrase.

But of course we need to see what's lurking in this phrase. Where did the rooted forests go? To answer this, you need to check that the free pre-Lie algebra on one generator has a basis given by rooted trees. Then its universal enveloping algebra will have a basis given by rooted forests!

So, the key question is: why does the free pre-Lie algebra on one generator have a basis given by rooted trees? Let me quickly sketch the answer Bill gave me. This may sound a bit cryptic, but I want to write it down before I forget.

Suppose you have rooted trees a and b and you attach a to b. More precisely: suppose you connect the root of a to some vertex of b using a new edge, forming a new rooted tree. You can do this in lots of ways, so you'll get a linear combination of trees, say ab. And this how multiplication in the free pre-Lie algebra on one generator works!

We can summarize this as follows:

$$ab = \begin{array}{c} a \\ | \\ b \end{array}$$

Here the picture stands for *any* way of attaching a to b. We should really sum over all of them.

When you form a product like $a(bc)$, different things can happen. We can summarize the possibilities like this:

$$a(bc) = \begin{array}{c} a \\ | \\ b \\ | \\ c \end{array} + \begin{array}{ccc} a & & b \\ & \diagdown\,\diagup & \\ & c & \end{array}$$

The point is that we can either attach the root of a to a vertex in b, or a vertex in c. There are fewer possibilities when we form $(ab)c$:

$$(ab)c = \begin{array}{c} a \\ | \\ b \\ | \\ c \end{array}$$

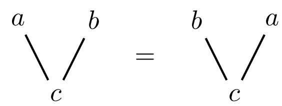
so

$$a(bc) - (ab)c = \begin{array}{ccc} a & & b \\ & \diagdown\,\diagup & \\ & c & \end{array}$$

Now switch a and b in this equation! We get

$$b(ac) - (ba)c = \begin{array}{ccc} b & & a \\ & \diagdown\,\diagup & \\ & c & \end{array}$$

Our rooted trees are not planar, so the answer is really the same:



So, we have

$$a(bc) - (ab)c = b(ac) - (ba)c$$

and this is the definition of a pre-Lie algebra!

This calculation reveals the secret meaning of pre-Lie algebras. The secret is that pre-Lie algebras are all about attaching two things by connecting a special point of the first to an arbitary point of the second! Rooted trees are the universal example, so they give the free pre-Lie algebra on one generator.

This calculation also reveals that a pre-Lie algebra is really a vector space with a bilinear product whose "associator"

$$\{a, b, c\} = (ab)c - a(bc)$$

is symmetric in the last two variables.

Finally, let me tell you the third way to get pre-Lie algebras: from operads. I'll assume you know about linear operads, which I explained in "Week 282".

Suppose $\mathcal{O}$ is any linear operad. Let A be the free $\mathcal{O}$-algebra on one generator. Then A becomes a pre-Lie algebra in a god-given way!

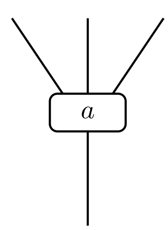
You should have seen this coming, since operads are related to trees. The details are explained here:

22) Frdric Chapoton and Muriel Livernet, "Pre-Lie algebras and the rooted trees operad", *Int. Math. Res. Not.* **2001** (2001), 395–408. Also available as `arXiv:math/0002069`.

The idea is that A is a lot like $\mathcal{O}$, since we get elements of the free $\mathcal{O}$-algebra on one generator by hitting that generator with operations in $\mathcal{O}$. More precisely, we have

$$A = \bigoplus_n \mathcal{O}_n / S_n$$

Here $\mathcal{O}_n$ is the space of n-ary operations in $\mathcal{O}$, which is acted on by the permutation group S_n. So, we can draw an element of A like this:



where a is an n-ary operation in $\mathcal{O}$, but we don't care how the branches of this little tree are permuted.

We can multiply two guys like this by summing over all ways of attaching the output of one to an input of the other and composing them using our operad. And, thanks to the "secret meaning of pre-Lie algebras", this makes A into a pre-Lie algebra!

This is nice. But over dinner, James Dolan, Bill Schmitt and I came up with an even slicker construction which seems to give the same multiplication on A.

A is the free $\mathcal{O}$-algebra on one generator, say x. So, for any element a in A, there's a unique $\mathcal{O}$-algebra endomorphism

$$f(a)\colon A \to A$$

sending x to a. Note that $f(x)$ is the identity. By the general philosophy that "an infinitesimal endomorphism is a derivation", the operator

$$\frac{d}{dt} f(x+ta)\big|_{t=0}$$

is a derivation of A.

(For certain familiar sorts of algebras, you may aready know what a derivation is. These are just special cases of a general concept of derivation for $\mathcal{O}$-algebras. I leave it as an exercise to reinvent this general concept.)

Now, we can define a multiplication on A by

$$ab = \frac{d}{dt} f(x+ta)(b)\big|_{t=0}$$

And this is the same as the multiplication I just described. Can we use this slick description to more efficiently prove that A is a pre-Lie algebra? I don't know.

One last thing:

Any linear operad gives a pre-Lie algebra. But pre-Lie algebras are themselves algebras of a linear operad! This leads to curious self-referential situation, and a nice puzzle.

There's a linear operad whose algebras are pre-Lie algebras. As we have seen, for any linear operad $\mathcal{O}$, the free $\mathcal{O}$-algebra with one generator becomes a pre-Lie algebra. So: the free pre-Lie algebra on one generator becomes a pre-Lie algebra in this way. But of course it already *is* a pre-Lie algebra! Do these pre-Lie structures agree?

If you give up, you can find the answer here:

23) Dominique Manchon, "A short survey on pre-Lie algebras", available at `http://math.univ-bpclermont.fr/~manchon/biblio/ESI-prelie2009.pdf`

But if you want to solve this puzzle on your own, it helps to think about what the operad for pre-Lie algebras looks like. Let's call it PL. It's not hard to guess what it looks like, given everything I've told you so far.

I've told you that the free pre-Lie algebra on one generator has a basis given by rooted trees. And I've told you a general fact: the free $\mathcal{O}$-algebra on one generator is

$$\bigoplus \mathcal{O}_n/S_n$$

So, taking $\mathcal{O} = PL$, it should come as no surprise that PL_n, the space of n-ary operations in PL, has a basis given by rooted trees with n vertices *labelled by numbers* 1 *through* n. Modding out by S_n just gets rid of those labels!

But how do you compose operations in PL?

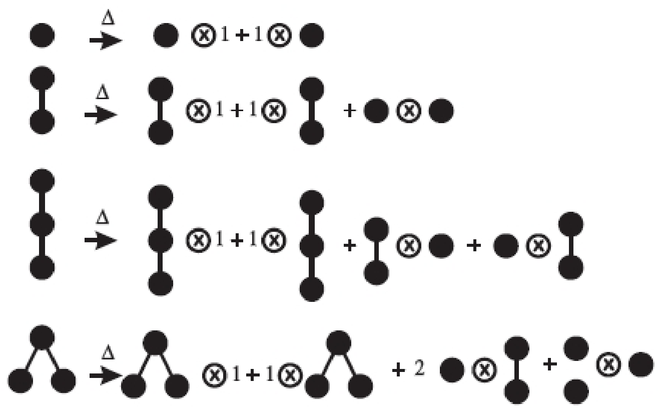
Addendum: I thank Colin Backhurst, Tim van Beek, and James Stasheff for improvements. Eugene Lerman pointed out that this paper provides a nice introduction to the Hopf algebra of rooted trees:

24) Christian Brouder, "Trees, renormalization and differential equations", *Numerical Mathematics* **44** (2004), 425–438. Also available at `http://www-int.impmc.upmc.fr/~brouder/BIT.pdf`

Let me quote:

> ***Abstract.*** *The Butcher group and its underlying Hopf algebra of rooted trees were originally formulated to describe Runge-Kutta methods in numerical analysis. In the past few years, these concepts turned out to have far-reaching applications in several areas of mathematics and physics: they were rediscovered in noncommutative geometry, they describe the combinatorics of renormalization in quantum field theory. The concept of Hopf algebra is introduced using a familiar example and the Hopf algebra of rooted trees is defined. Its role in Runge-Kutta methods, renormalization theory and noncommutative geometry is described.*
>
> ***Introduction***
>
> *This paper tells the story of a mathematical object that was created by John Butcher in 1972 and was rediscovered by Alain Connes, Henri Moscovici and Dirk Kreimer in 1998. Butcher wanted to set up a theory of Runge-Kutta methods in numerical analysis, Connes and Moscovici were working at an index theorem in noncommutative geometry, Kreimer was looking for the mathematical structure behind the renormalization method of quantum field theory, and all these people hit upon the same object: the Hopf algebra of rooted trees. The appearance of an object relevant to so widely different fields is not common. And the fact that a computer scientist discovered it 26 years in advance shows the power of inspiration provided by numerical analysis. Connes and Kreimer themselves noted: "We regard Butcher's work on the classification of numerical integration methods as an impressive example that concrete problem-oriented work can lead to far-reaching conceptual results".*

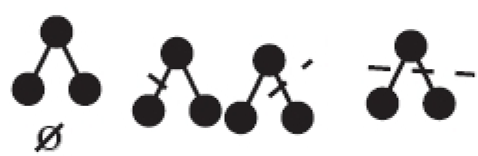
Here are some examples, taken from Connes and Kreimer's paper, that illustrate the comultiplication in the Hopf algebra of rooted forests:



Here Δ stands for the comultiplication. *But beware:* these trees have their root on top — I hear that's how they grow in Europe. So, these pictures are upside-down compared to the description I gave earlier! Now to comultiply a rooted forest we slice it in lots of ways and form a sum of terms

$$\text{(above)} \otimes \text{(below)}$$

For example, the terms in the last line of the picture come from these ways of slicing the given tree:



The first way here is really two ways: we can have the whole tree be above the slice and empty set below, or the empty set above and the whole tree below. So, we get 5 ways to slice the tree, and comultiplying it gives a sum of 5 terms, two of which are equal.

Here's another nice introduction to pre-Lie algebras and the Hopf algebra of rooted forests:

25) Frdric Chapoton, "Operadic point of view on the Hopf algebra of rooted trees", `http://www-math.unice.fr/~patras/CargeseConference/ACQFT09_FredericCHAPOTON.pdf`

But beware: he is using right pre-Lie algebras, where I was using left ones. So, his product obeys the law

$$[R(a), R(b)] = R([a, b])$$

where $R(a)$ stands for right multiplication by a. Right pre-Lie algebras work just as well as left ones.

For more discussion, visit the n-Category Caf.

I can learn only by teaching.
— *John Wheeler*

Week 300

August 11, 2010

This is the last of the old series of This Week's Finds. Soon the new series will start, focused on technology and environmental issues — but still with a hefty helping of math, physics, and other science.

When I decided to do something useful for a change, I realized that the best way to start was by interviewing people who take the future and its challenges seriously, but think about it in very different ways. So far, I've done interviews with:

- Tim Palmer on climate modeling and predictability.
- Thomas Fischbacher on sustainability and permaculture.
- Eliezer Yudkowsky on artificial intelligence and the art of rationality.

I hope to do more. I think it'll be fun having *This Week's Finds* be a dialogue instead of a monologue now and then.

Other things are changing too. I started a new blog! If you're interested in how scientists can help save the planet, I hope you visit:

1) Azimuth, `http://johncarlosbaez.wordpress.com`

This is where you can find This Week's Finds, starting now.

Also, instead of teaching math in hot dry Riverside, I'm now doing research at the Centre for Quantum Technologies in hot and steamy Singapore. This too will be reflected in the new This Week's Finds

But now... **the grand finale of This Week's Finds in Mathematical Physics!**

I'd like to take everything I've been discussing so far and wrap it up in a nice neat package. Unfortunately that's impossible — there are too many loose ends. But I'll do my best: I'll tell you how to categorify the Riemann zeta function. This will give us a chance to visit lots of our old friends one last time: the number 24, string theory, zeta functions, torsors, Joyal's theory of species, groupoidification, and more.

Let me start by telling you how to count.

I'll assume you already know how to count elements of a *set*, and move right along to counting objects in a *groupoid*.

A groupoid is a gadget with a bunch of objects and a bunch of isomorphisms between them. Unlike an element of a set, an object of a groupoid may have symmetries: that is, isomorphisms between it and *itself*. And unlike an element of a set, an object of a groupoid doesn't always count as "1 thing": when it has n symmetries, it counts as "$1/n$th of a thing". That may seem strange, but it's really right. We also need to make sure not to count isomorphic objects as different.

So, to count the objects in our groupoid, we go through it, take one representative of each isomorphism class, and add $1/n$ to our count when this representative has n symmetries.

Let's see how this works. Let's start by counting all the n-element sets.

Now, you may have thought there were infinitely many sets with n elements, and that's true. But remember: we're not counting the *set* of n-element sets — that's way too big. So big, in fact, that people call it a "class" rather than a set! Instead, we're counting the *groupoid* of n-element sets: the groupoid with n-element sets as objects, and one-to-one and onto functions between these as isomorphisms.

All n-element sets are isomorphic, so we only need to look at one. It has $n!$ symmetries: all the permutations of n elements. So, the answer is $1/n!$.

That may seem weird, but remember: in math, you get to make up the rules of the game. The only requirements are that the game be consistent and profoundly fun — so profoundly fun, in fact, that it seems insulting to call it a mere "game".

Now let's be more ambitious: let's count *all* the finite sets. In other words, let's work out the cardinality of the groupoid where the objects are *all* the finite sets, and the isomorphisms are all the one-to-one and onto functions between these.

There's only one 0-element set, and it has $0!$ symmetries, so it counts for $1/0!$. There are tons of 1-element sets, but they're all isomorphic, and they each have $1!$ symmetries, so they count for $1/1!$. Similarly the 2-element sets count for $1/2!$, and so on. So the total count is

$$\frac{1}{0!} + \frac{1}{1!} + \frac{1}{2!} + \ldots = e$$

The base of the natural logarithm is the number of finite sets! You learn something new every day.

Spurred on by our success, you might want to find a groupoid whose cardinality is π. It's not hard to do: you can just find a groupoid whose cardinality is 3, and a groupoid whose cardinality is $.1$, and a groupoid whose cardinality is $.04$, and so on, and lump them all together to get a groupoid whose cardinality is $3.14\ldots$ But this is a silly solution: it doesn't shed any light on the nature of π.

I don't want to go into it in detail now, but the previous problem really *does* shed light on the nature of e: it explains why this number is related to combinatorics, and it gives a purely combinatorial proof that the derivative of e^x is e^x, and lots more. Try these books to see what I mean:

2) Herbert Wilf, *Generatingfunctionology*, Academic Press, Boston, 1994. Available for free at `http://www.cis.upenn.edu/~wilf/`.

3) F. Bergeron, G. Labelle, and P. Leroux, *Combinatorial Species and Tree-Like Structures*, Cambridge, Cambridge U. Press, 1998.

For example: if you take a huge finite set, and randomly pick a permutation of it, the chance every element is mapped to a *different* element is close to $1/e$. It approaches $1/e$ in the limit where the set gets larger and larger. That's well-known — but the neat part is how it's related to the cardinality of the groupoid of finite sets.

Anyway, I have not succeeded in finding a really illuminating groupoid whose cardinality is π, but recently James Dolan found a nice one whose cardinality is $\pi^2/6$, and I want to lead up to that.

Here's a not-so-nice groupoid whose cardinality is $\pi^2/6$. You can build a groupoid as the "disjoint union" of a collection of groups. How? Well, you can think of a group as a groupoid with one object: just one object having that group of symmetries. And you can build more complicated groupoids as disjoint unions of groupoids with one object. So, if you give me a collection of groups, I can take their disjoint union and get a groupoid.

So give me this collection of groups:

$$\mathbb{Z}/1 \times \mathbb{Z}/1, \mathbb{Z}/2 \times \mathbb{Z}/2, \mathbb{Z}/3 \times \mathbb{Z}/3, \ldots$$

where $\mathbb{Z}/n$ is the integers $\mod n$, also called the "cyclic group" with n elements. Then I'll take their disjoint union and get a groupoid, and the cardinality of this groupoid is

$$\frac{1}{1^2} + \frac{1}{2^2} + \frac{1}{3^2} + \ldots = \frac{\pi^2}{6}$$

This is not as silly as the trick I used to get a groupoid whose cardinality is π, but it's still not perfectly satisfying, because I haven't given you a groupoid of "interesting mathematical gadgets and isomorphisms between them", as I did for e. Later we'll see Jim's better answer.

We might also try taking various groupoids of interesting mathematical gadgets and computing their cardinality. For example, how about the groupoid of all finite groups? I think that's infinite — there are just "too many". How about the groupoid of all finite abelian groups? I'm not sure, that could be infinite too.

But suppose we restrict ourselves to abelian groups whose size is some power of a fixed prime p? Then we're in business! The answer isn't a famous number like π, but it was computed by Philip Hall here:

4) Philip Hall, "A partition formula connected with Abelian groups", *Comment. Math. Helv.* **11** (1938), 126–129.

We can write the answer using an infinite product:

$$\frac{1}{(1-p^{-1})(1-p^{-2})(1-p^{-3})\ldots}$$

Or, we can write the answer using an infinite sum:

$$\frac{p(0)}{p^0} + \frac{p(1)}{p^1} + \frac{p(2)}{p^2} + \ldots$$

Here $p(n)$ is the number of "partitions" of n: that is, the number of ways to write it as a sum of positive integers in decreasing order. For example, $p(4) = 5$ since we can write 4 as a sum in 5 ways like this:

1. $4 = 4$
2. $4 = 3 + 1$
3. $4 = 2 + 2$
4. $4 = 2 + 1 + 1$
5. $4 = 1 + 1 + 1 + 1$

If you haven't thought about this before, you can have fun proving that the infinite product equals the infinite sum. It's a cute fact, and quite famous.

But Hall proved something even cuter. This number

$$\frac{p(0)}{p^0} + \frac{p(1)}{p^1} + \frac{p(2)}{p^2} + \dots$$

is also the cardinality of another, really *different* groupoid. Remember how I said you can build a groupoid as the "disjoint union" of a collection of groups? To get this other groupoid, we take the disjoint union of all the abelian groups whose size is a power of p.

Hall didn't know about groupoid cardinality, so here's how he said it:

> *The sum of the reciprocals of the orders of all the Abelian groups of order a power of p is equal to the sum of the reciprocals of the orders of their groups of automorphisms.*

It's pretty easy to see that sum of the reciprocals of the orders of all the Abelian groups of order a power of p is

$$\frac{p(0)}{p^0} + \frac{p(1)}{p^1} + \frac{p(2)}{p^2} + \dots$$

To do this, you just need to show that there are $p(n)$ abelian groups with p^n elements. If I shows you how it works for $n = 4$, you can guess how the proof works in general:

$4 = 4$	$\mathbb{Z}/p^4$
$4 = 3 + 1$	$\mathbb{Z}/p^3 \times \mathbb{Z}/p$
$4 = 2 + 2$	$\mathbb{Z}/p^2 \times \mathbb{Z}/p^2$
$4 = 2 + 1 + 1$	$\mathbb{Z}/p^2 \times \mathbb{Z}/p^2 \times \mathbb{Z}/p$
$4 = 1 + 1 + 1 + 1$	$\mathbb{Z}/p \times \mathbb{Z}/p \times \mathbb{Z}/p \times \mathbb{Z}/p$

So, the hard part is showing that

$$\frac{p(0)}{p^0} + \frac{p(1)}{p^1} + \frac{p(2)}{p^2} + \dots$$

is also the sum of the reciprocals of the sizes of the automorphism groups of all groups whose size is a power of p.

I learned of Hall's result from Aviv Censor, a colleague who is an expert on groupoids. He had instantly realized this result had a nice formulation in terms of groupoid cardinality. We went through several proofs, but we haven't yet been able to extract any deep inner meaning from them:

5) Avinoam Mann, 'Philip Hall's "rather curious" formula for abelian p-groups', *Israel J. Math.* **96** (1996), part B, 445–448.

6) Francis Clarke, "Counting abelian group structures", *Proceedings of the AMS* **134** (2006), 2795–2799.

However, I still have hopes, in part because the math is related to zeta functions... and that's what I want to turn to now.

Let's do another example: what's the cardinality of the groupoid of semisimple commutative rings with n elements?

What's a semisimple commutative ring? Well, since we're only talking about *finite* ones, I can avoid giving the general definition and take advantage of a classification theorem. Finite semisimple commutative rings are the same as finite products of finite fields. There's a finite field with p^n whenever p is prime and n is a positive integer. This field is called $\mathbb{F}_{p^n}$, and it has n symmetries. And that's all the finite fields! In other words, they're all isomorphic to these.

This is enough to work out the cardinality of the groupoid of semisimple commutative rings with n elements. Let's do some examples. Let's try $n = 6$, for example.

This one is pretty easy. The only way to get a finite product of finite fields with 6 elements is to take the product of $\mathbb{F}_2$ and $\mathbb{F}_3$:

$$\mathbb{F}_2 \times \mathbb{F}_3$$

This has just one symmetry — the identity — since that's all the symmetries either factor has, and there's no symmetry that interchanges the two factors. (Hmm... you may need check this, but it's not hard.)

Since we have one object with one symmetry, the groupoid cardinality is

$$\frac{1}{1} = 1$$

Let's try a more interesting one, say $n = 4$. Now there are two options:

1. $\mathbb{F}_4$
2. $\mathbb{F}_2 \times \mathbb{F}_2$

The first option has 2 symmetries: remember, $\mathbb{F}_{p^n}$ has n symmetries. The second option also has 2 symmetries, namely the identity and the symmetry that switches the two factors. So, the groupoid cardinality is

$$\frac{1}{2} + \frac{1}{2} = 1$$

But now let's try something even more interesting, like $n = 16$. Now there are 5 options:

1. $\mathbb{F}_{16}$
2. $\mathbb{F}_8 \times \mathbb{F}_2$
3. $\mathbb{F}_4 \times \mathbb{F}_4$
4. $\mathbb{F}_4 \times \mathbb{F}_2 \times \mathbb{F}_2$
5. $\mathbb{F}_2 \times \mathbb{F}_2 \times \mathbb{F}_2 \times \mathbb{F}_2$

The field $\mathbb{F}_{16}$ has 4 symmetries because $16 = 2^4$, and any field $\mathbb{F}_{p^n}$ has n symmetries. $\mathbb{F}_8 \times \mathbb{F}_2$ has 3 symmetries, coming from the symmetries of the first factor. $\mathbb{F}_4 \times \mathbb{F}_4$ has 2 symmetries in each factor and 2 coming from permutations of the factors, for a total of $2 \times 2 \times 2 = 8$. $\mathbb{F}_4 \times \mathbb{F}_2 \times \mathbb{F}_2$ has 2 symmetries coming from those of the first factor, and 2 symmetries coming from permutations of the last two factors, for a total of $2 \times 2 = 4$ symmetries. And finally, $\mathbb{F}_2 \times \mathbb{F}_2 \times \mathbb{F}_2 \times \mathbb{F}_2$ has 24 symmetries coming from permutations of the factors. So, the cardinality of this groupoid works out to be

$$\frac{1}{4} + \frac{1}{3} + \frac{1}{8} + \frac{1}{4} + \frac{1}{24}$$

Hmm, let's put that on a common denominator:

$$\frac{6}{24} + \frac{8}{24} + \frac{3}{24} + \frac{6}{24} + \frac{1}{24} = \frac{24}{24} = 1$$

So, we're getting the same answer again: 1.

Is this just a weird coincidence? No: this is what we *always* get! For *any* positive integer n, the groupoid of n-element semsimple commutative rings has cardinality 1. For a proof, see:

7) John Baez and James Dolan, "Zeta functions", at `http://ncatlab.org/johnbaez/show/Zeta+functions`

Now, you might think this fact is just a curiosity, but actually it's a step towards categorifying the Riemann zeta function. The Riemann zeta function is

$$\zeta(s) = \sum_{n>0} n^{-s}$$

It's an example of a "Dirichlet series", meaning a series of this form:

$$\sum_{n>0} a_n n^{-s}$$

In fact, any reasonable way of equipping finite sets with extra stuff gives a Dirichlet series — and if this extra stuff is "being a semisimple commutative ring", we get the Riemann zeta function.

To explain this, I need to remind you about "stuff types", and then explain how they give Dirichlet series.

A stuff type is a groupoid Z where the objects are finite sets equipped with "extra stuff" of some type. More precisely, it's a groupoid with a functor to the groupoid of finite sets. For example, Z could be the groupoid of finite semsimple commutative rings — that's the example we care about now. Here the functor forgets that we have a semisimple commutative ring, and only remembers the underlying finite set. In other words, it forgets the "extra stuff".

In this example, the extra stuff is really just extra *structure*, namely the structure of being a semisimple commutative ring. But we could also take Z to be the groupoid of pairs of finite sets. A pair of finite sets is a finite set equipped with honest-to-goodness extra *stuff*, namely another finite set!

Structure is a special case of stuff. If you're not clear on the difference, try this:

8) John Baez and Mike Shulman, "Lectures on n-categories and cohomology", Sec. 2.4: Stuff, structure and properties, in *n-Categories: Foundations and Applications*, eds. John Baez and Peter May, Springer, Berlin, 2009. Also available as arXiv:math/0608420.

Then you can tell your colleagues: "I finally understand *stuff*." And they'll ask: "What stuff?" And you can answer, rolling your eyes condescendingly: "Not any *particular* stuff — just *stuff*, in general!"

But it's not really necessary to understand stuff in general here. Just think of a stuff type as a groupoid where the objects are finite sets equipped with extra bells and whistles of some particular sort.

Now, if we have a stuff type, say Z, we get a list of groupoids $Z(n)$. How? Simple! Objects of Z are finite sets equipped with some particular type of extra stuff. So, we can take the objects of $Z(n)$ to be the *n-element* sets equipped with that type of extra stuff. The groupoid Z will be a disjoint union of these groupoids $Z(n)$.

We can encode the cardinalities of all these groupoids into a Dirichlet series:

$$z(s) = \sum_{n>0} |Z(n)| n^{-s}$$

where $|Z(n)|$ is the cardinality of $Z(n)$. In case you're wondering about the minus sign: it's just a dumb convention, but I'm too overawed by the authority of tradition to dream of questioning it, even though it makes everything to come vastly more ugly.

Anyway: the point is that a Dirichlet series is like the "cardinality" of a stuff type. To show off, we say stuff types *categorify* Dirichlet series: they contain more information, and they're objects in a category (or something even better, like a 2-category) rather than elements of a set.

Let's look at an example. When Z is the groupoid of finite semisimple commutative rings, then

$$|Z(n)| = 1$$

so the corresponding Dirichlet series is the Riemann zeta function:

$$z(s) = \zeta(s)$$

So, we've categorified the Riemann zeta function! Using this, we can construct an interesting groupoid whose cardinality is

$$\zeta(2) = \sum_{n>0} n^{-2} = \frac{\pi^2}{6}$$

How? Well, let's step back and consider a more general problem. Any stuff type Z gives a Dirichlet series

$$z(s) = \sum_{n>0} |Z(n)| n^{-s}$$

How can use this to concoct a groupoid whose cardinality is $z(s)$ for some particular value of s? It's easy when s is a *negative* integer (here that minus sign raises its ugly head). Suppose S is a set with s elements:

$$|S| = s$$

Then we can define a groupoid as follows:

$$Z(-S) = \sum_{n>0} Z(n) \times n^S$$

Here we are playing some notational tricks: n^S means "the set of functions from S to our favorite n-element set", the symbol $\times$ stands for the product of groupoids, and $\sum$ stands for what I've been calling the "disjoint union" of groupoids (known more technically as the "coproduct"). So, $Z(-S)$ is a groupoid. But this formula is supposed to remind us of a simpler one, namely

$$z(-s) = \sum_{n>0} |Z(n)| n^s$$

and indeed it's a *categorified version* of this simpler formula.

In particular, if we take the cardinality of the groupoid $Z(-S)$, we get the number $z(-s)$. To see this, you just need to check each step in this calculation:

$$\begin{aligned} |Z(-S)| &= \left|\sum Z(n) \times n^S\right| \\ &= \sum |Z(n) \times n^S| \\ &= \sum |Z(n)| \times |n^S| \\ &= \sum |Z(n)| \times n^s \\ &= z(-s) \end{aligned}$$

The notation is supposed to make these steps seem plausible.

Even better, the groupoid $Z(-S)$ has a nice description in plain English: it's the groupoid of *finite sets equipped with Z-stuff and a map from the set S*.

Well, okay — I'm afraid that's what passes for plain English among mathematicians! We don't talk to ordinary people very often. But the idea is really simple. Z is some sort of stuff that we can put on a finite set. So, we can do that and also choose a map from S to that set. And there's a groupoid of finite sets equipped with all this extra baggage, and isomorphisms between those.

If this sounds too abstract, let's do an example. Say our favorite example, where Z is the groupoid of finite semisimple commutative rings. Then $Z(-S)$ is the groupoid of finite semisimple commutative rings equipped with a map from the set S.

If this still sounds too abstract, let's do an example. Do I sound repetitious? Well, you see, category theory is the subject where you need examples to explain your examples — and n-category theory is the subject where this process needs to be repeated n times. So, suppose S is a 1-element set — we can just write

$$S = 1$$

Then $Z(-1)$ is a groupoid where the objects are finite semisimple commutative rings with a chosen element. The isomorphisms are ring isomorphisms *that preserve the chosen element*. And the cardinality of this groupoid is

$$|Z(-1)| = \zeta(-1) = 1 + 2 + 3 + \ldots$$

Whoops — it diverges! Luckily, people who study the Riemann zeta function know that

$$1 + 2 + 3 + \dots = -\frac{1}{12}$$

They get this crazy answer by analytically continuing the Riemann zeta function $\zeta(s)$ from values of s with a big positive real part, where it converges, over to values where it doesn't. And it turns out that this trick is very important in physics. In fact, back in "Week 124"–"Week 126", I explained how this formula

$$\zeta(-1) = -\frac{1}{12}$$

is the reason bosonic string theory works best when our string has 24 extra dimensions to wiggle around in besides the 2 dimensions of the string worldsheet itself.

So, if we're willing to allow this analytic continuation trick, we can say that:

> ***the groupoid of finite semisimple commutative rings***
> ***with a chosen element***
> ***has cardinality*** $-1/12$

Someday people will see exactly how this is related to bosonic string theory. Indeed, it should be just a tiny part of a big story connecting number theory to string theory... some of which is explained here:

9) J. M. Luck, P. Moussa, and M. Waldschmidt, eds., *Number Theory and Physics*, Springer Proceedings in Physics, Vol. **47**, Springer-Verlag, Berlin, 1990.

10) C. Itzykson, J. M. Luck, P. Moussa, and M. Waldschmidt, eds, *From Number Theory to Physics*, Springer, Berlin, 1992.

Indeed, as you'll see in these books (or in "Week 126"), the function we saw earlier:

$$\frac{1}{(1-p^{-1})(1-p^{-2})(1-p^{-3})\dots} = \frac{p(0)}{p^0} + \frac{p(1)}{p^1} + \frac{p(2)}{p^2} + \dots$$

is *also* important in string theory: it shows up as a "partition function", in the physical sense, where the number $p(n)$ counts the number of ways a string can have energy n if it has one extra dimension to wiggle around in besides the 2 dimensions of its worldsheet.

But it's the *24th power* of this function that really matters in string theory — because bosonic string theory works best when our string has *24* extra dimensions to wiggle around in. For more details, try:

11) John Baez, "My favorite numbers: 24". Available at `http://math.ucr.edu/home/baez/numbers/24.pdf`

But suppose we don't want to mess with divergent sums: suppose we want a groupoid whose cardinality is, say,

$$\zeta(2) = 1^{-2} + 2^{-2} + 3^{-2} + \dots = \frac{\pi^2}{6}$$

Then we need to categorify the evalution of Dirichlet series at *positive* integers. We can only do this for *certain* stuff types — for example, our favorite one. So, let Z be the groupoid of finite semisimple commutative rings, and let S be a finite set. How can we make sense of

$$Z(S) = \sum_{n>0} Z(n) \times n^{-S}?$$

The hard part is n^{-S}, because this has a minus sign in it. How can we raise an n-element set to the $-S$th power? If we could figure out some sort of groupoid that serves as the *reciprocal* of an n-element set, we'd be done, because then we could take that to the Sth power. Remember, S is a finite set, so to raise something (even a groupoid) to the Sth power, we just multiply a bunch of copies of that something — one copy for each element of S.

So: what's the reciprocal of an n-element set? There's no answer in general — but there's a nice answer when that set is a *group*, because then that group gives a groupoid with one object, and the cardinality of this groupoid is just $1/n$.

Here is where our particular stuff type Z comes to the rescue. Each object of $Z(n)$ is a semisimple commutative ring with n elements, so it has an underlying additive group — which is a group with n elements!

So, we don't interpret $Z(n) \times n^{-S}$ as an ordinary product, but something a bit sneakier, a "twisted product". An object in $Z(n) \times n^{-S}$ is just an object of $Z(n)$, that is an n-element semisimple commutative ring. But we define a symmetry of such an object to be a symmetry of this ring *together with* an S-tuple of elements of its underlying additive group. We compose these symmetries with the help of addition in this group. This ensures that

$$|Z(n) \times n^{-S}| = |Z(n)| \times n^{-s}$$

when $|S| = s$. And this in turn means that

$$\begin{aligned} |Z(S)| &= |\sum Z(n) \times n^{-S}| \\ &= \sum |Z(n) \times n^{-S}| \\ &= \sum |Z(n)| \times n^{-s} \\ &= \zeta(-s) \end{aligned}$$

So, in particular, if S is a 2-element set, we can write

$$S = 2$$

for short and get

$$|Z(2)| = \zeta(2) = \frac{\pi^2}{6}$$

Can we describe the groupoid $Z(2)$ in simple English, suitable for a nice bumper sticker? It's a bit tricky. One reason is that I haven't described the objects of $Z(2)$ as mathematical gadgets of an appealing sort, as I did for $Z(-1)$. Another closely related reason is that I only described the symmetries of any object in $Z(2)$ — or more technically, morphisms from that object to itself. It's much better if we also describe morphisms from one object to another.

For this, it's best to define $Z(n) \times n^{-S}$ with the help of torsors. The idea of a torsor is that you can take the one-object groupoid associated to any group G and find a *different* groupoid, which is nonetheless *equivalent*, and which is a groupoid of appealing mathematical gadgets. These gadgets are called "G-torsors". A "G-torsor" is just a nonempty set on which G acts freely and transitively:

12) John Baez, "Torsors made easy", `http://math.ucr.edu/home/baez/torsors.html`

All G-torsors are isomorphic, and the group of symmetries of any G-torsor is G.

Now, any ring R has an underlying additive group, which I will simply call R. So, the concept of "R-torsor" makes sense. This lets us define an object of $Z(n) \times n^{-S}$ to be an n-element semisimple commutative ring R together with an S-tuple of R-torsors.

But what about the morphisms between these? We define a morphism between these to be a ring isomorphism together with an S-tuple of torsor isomorphisms. There's a trick hiding here: a ring isomorphism $f\colon R \to R'$ lets us take any R-torsor and turn it into an R'-torsor, or vice versa. So, it lets us talk about an isomorphism from an R-torsor to an R'-torsor — a concept that at first might have seemed nonsensical.

Anyway, it's easy to check that this definition is compatible with our earlier one. So, we see:

> ***the groupoid of finite semisimple commutative rings***
> ***equipped with an n-tuple of torsors***
> ***has cardinality*** $\zeta(n)$

I did a silly change of variables here: I thought this bumper sticker would sell better if I said "n-tuple" instead of "S-tuple". Here n is any positive integer.

While we're selling bumper stickers, we might as well include this one:

> ***the groupoid of finite semisimple commutative rings***
> ***equipped with a pair of torsors***
> ***has cardinality*** $\pi^2/6$

Now, you might think this fact is just a curiosity. But I don't think so: it's actually a step towards categorifying the general theory of zeta functions. You see, the Riemann zeta function is just one of *many* zeta functions. As Hasse and Weil discovered, every sufficiently nice commutative ring R has a zeta function. The Riemann zeta function is just the simplest example: the one where R is the ring of integers. And the cool part is that *all* these zeta functions come from stuff types using the recipe I described!

How does this work? Well, from any commutative ring R, we can build a stuff type Z_R as follows: an object of Z_R is a finite semisimple commutative ring together with a homomorphism from R to that ring. Then it turns out the Dirichlet series of this stuff type, say

$$\zeta_R(s) = \sum_{n>0} |Z_R(n)| n^{-s}$$

is the usual Hasse-Weil zeta function of the ring R!

Of course, that fact is vastly more interesting if you already know and love Hasse-Weil zeta functions. You can find a definition of them either in my paper with Jim, or here:

12) Jean-Pierre Serre, "Zeta and L functions", *Arithmetical Algebraic Geometry* (Proc. Conf. Purdue Univ., 1963), Harper and Row, 1965, pp. 82–92.

But the basic idea is simple. You can think of any commutative ring R as the functions on some space — a funny sort of space called an "affine scheme". You're probably used to spaces where all the points look alike — just little black dots. But the points of an affine scheme come in many different colors: for starters, one color for each prime power p^k! The Hasse-Weil zeta function of R is a clever trick for encoding the information about the numbers of points of these different colors in a single function.

Why do we get points of different colors? I explained this back in "Week 205". The idea is that for any commutative ring k, we can look at the homomorphisms

$$f\colon R \to k$$

and these are called the "k-points" of our affine scheme. In particular, we can take k to be a finite field, say $\mathbb{F}_{p^n}$. So, we get a set of points for each prime power p^n. The Hasse-Weil zeta function is a trick for keeping track of many $\mathbb{F}_{p^n}$-points there are for each prime power p^n.

Given all this, you shouldn't be surprised that we can get the Hasse-Weil zeta function of R by taking the Dirichlet series of the stuff type Z_R, where an object is a finite semisimple commutative ring k together with a homomorphism $f\colon R \to k$. Especially if you remember that finite semisimple commutative rings are built from finite fields!

In fact, this whole theory of Hasse-Weil zeta functions works for gadgets much more general than commutative rings, also known as affine schemes. They can be defined for "schemes of finite type over the integers", and that's how Serre and other algebraic geometers usually do it. But Jim and I do it even more generally, in a way that doesn't require any expertise in algebraic geometry. Which is good, because we don't have any.

I won't explain that here — it's in our paper.

I'll wrap up by making one more connection explicit — it's sort of lurking in what I've said, but maybe it's not quite obvious.

First of all, this idea of getting Dirichlet series from stuff types is part of the groupoidification program. Stuff types are a generalization of "structure types", often called "species". Andr Joyal developed the theory of species and showed how any species gives rise to a formal power series called its generating function. I told you about this back in "Week 185" and "Week 190". The recipe gets even simpler when we go up to stuff types: the generating function of a stuff type Z is just

$$\sum_{n \geqslant 0} |Z(n)| z^n$$

Since we can also describe states of the quantum harmonic oscillator as power series, with z^n corresponding to the nth energy level, this lets us view stuff types as states of a categorified quantum harmonic oscillator! This explains the combinatorics of Feynman diagrams:

14) Jeffrey Morton, "Categorified algebra and quantum mechanics", *TAC* **16** (2006), 785–854, available at `http://www.emis.de/journals/TAC/volumes/16/29/16-29abs.html` Also available as `arXiv:math/0601458`.

And, it's a nice test case of the groupoidification program, where we categorify lots of algebra by saying "wherever we see a number, let's try to think of it as the cardinality of a groupoid":

15) John Baez, Alex Hoffnung and Christopher Walker, "Higher-dimensional algebra VII: Groupoidification", available as arXiv:0908.4305.

But now I'm telling you something new! I'm saying that any stuff type also gives a Dirichlet series, namely

$$\sum_{n>0} |Z(n)| n^{-s}$$

This should make you wonder what's going on. My paper with Jim explains it — at least for structure types. The point is that the groupoid of finite sets has two monoidal structures: $+$ and $\times$. This gives the category of structure types two monoidal structures, using a trick called "Day convolution". The first of these categorifies the usual product of formal power series, while the second categorifies the usual product of Dirichlet series. People in combinatorics love the first one, since they love chopping a set into two disjoint pieces and putting a structure on each piece. People in number theory secretly love the second one, without fully realizing it, because they love taking a number and decomposing it into prime factors. But they both fit into a single picture!

There's a lot more to say about this, because actually the category of structure types has *five* monoidal structures, all fitting together in a nice way. You can read a bit about this here:

16) nLab, "Schur functors", `http://ncatlab.org/nlab/show/Schur+functor`

This is something Todd Trimble and I are writing, which will eventually evolve into an actual paper. We consider structure types for which there is a *vector space* of structures for each finite set instead of a *set* of structures. But much of the abstract theory is similar. In particular, there are still five monoidal structures.

Someday soon, I hope to show that two of the monoidal structures on the category of species make it into a "ring category", while the other two — the ones I told you about, in fact! — are better thought of as "comonoidal" structures, making it into a "coring category". Putting these together, the category of species should become a "biring category". Then the fifth monoidal structure, called "plethysm", should make it into a monoid in the monoidal bicategory of biring categories!

This sounds far-out, but it's all been worked out at a decategorified level: rings, corings, birings, and monoids in the category of birings:

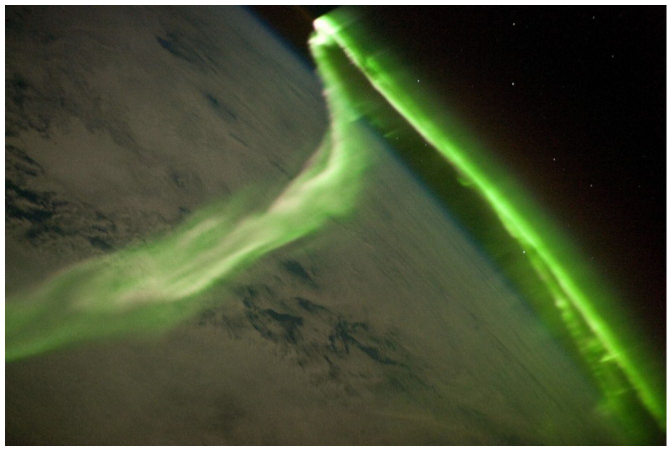
17) D. Tall and Gavin Wraith, "Representable functors and operations on rings", *Proc. London Math. Soc.* **3**, 1970, 619–643.

18) James Borger and B. Wieland, "Plethystic algebra", *Advances in Mathematics* **194** (2005), 246–283. Also available at `http://wwwmaths.anu.edu.au/~borger/papers/03/paper03.html`

19) Andrew Stacey and S. Whitehouse, "The hunting of the Hopf ring", *Homology, Homotopy and Applications* **11** (2009), 75–132, available at `http://intlpress.com/HHA/v11/n2/a6/` Also available as arXiv:0711.3722.

Borger and Wieland call a monoid in the category of birings a "plethory". The star example is the algebra of symmetric functions. But this is basically just a decategorified version of the category of Vect-valued species. So, the whole story should categorify.

In short: starting from very simple ideas, we can very quickly find a treasure trove of rich structures. Indeed, these structures are *already staring us in the face* — we just need to know how to open our eyes. They clarify and unify a lot of seemingly esoteric and disconnected things that mathematicians and physicists love.



I think we are just beginning to glimpse the real beauty of math and physics. I bet it will be both simpler and more startling than most people expect.

I would love to spend the rest of my life chasing glimpses of this beauty. I wish we lived in a world where everyone had enough of the bare necessities of life to do the same if they wanted — or at least a world that was safely heading in that direction, a world where politicians were looking ahead and tackling problems before they became desperately serious, a world where the oceans weren't dying.

But we don't.

Addenda: For more discussion, and to see ***all future issues of This Week's Finds***, visit Azimuth.

In particular, in the comments on "week300" you will see a proof that the cardinality of the groupoid of abelian groups diverges.

> During the journey we commonly forget its goal. Almost every profession is chosen as a means to an end but continued as an end in itself. Forgetting our objectives is the most frequent act of stupidity.
>
> — *Friedrich Nietzsche*