This Week's Finds in Mathematical Physics

Weeks 51 to 100

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Contents

Week 51

April 23, 1995

For people in theoretical physics, Trieste is a kind of mecca. It's an Italian town on the Adriatic quite near the border with Slovenia, and it's quite charming, especially the castle of Maximilian near the coast, built when parts of northern Italy were under Hapsburg rule. Maximilian later took his architect with him to Mexico when he became Emperor there, who built another castle for him in Mexico City. (The Mexicans, apparently unimpressed, overthrew and killed Maximilian.) These days, physicists visit Trieste partially for the charm of the area, but mainly to go to the ICTP and SISSA, two physics institutes, the latter of which has grad students, the former of which is purely for research. There are lots of conferences and workshops at Trieste, and I was lucky enough to be invited to Trieste while one I found interesting was going on.

As I described to some extent in "Week 44" and "Week 45", Seiberg and Witten have recently shaken up the subject of Donaldson theory by using some physical reasoning to radically simplify the computations involved. Donaldson theory has always had a lot to do with physics, since it uses the special features of of gauge theory in 4 dimensions to obtain invariants of 4-dimensional manifolds. So perhaps it is not surprising that physicists have had a lot to say about Donaldson theory all along, even before the recent Seiberg-Witten revolution. And indeed, at Trieste lots of mathematicians and physicists were busy talking to each other about Donaldson theory, trying to catch up with the latest stuff and trying to see what to do next.

Now I don't know much about Donaldson theory, but I have a vague interest in it for various reasons. First, it's *supposed* to be a 4-dimensional topological quantum field theory, or TQFT. Indeed, the very first paper on TQFTs was about Donaldson theory in 4 dimensions:

1) "Topological quantum field theory", by Edward Witten, *Comm. Math. Phys.* **117** (1988) 353.

Only later did Witten turn to the comparatively easier case of Chern-Simons theory, which is a 3-dimensional TQFT:

2) "Quantum field theory and the Jones polynomial", by Edward Witten, *Comm. Math. Phys.* **121** (1989) 351.

However, when *mathematicians* talk about TQFTs they usually mean something satisfying Atiyah's axioms for a TQFT (which are nicely presented in his book — see "Week 39"). Now it turns out that Chern-Simons theory can be rigorously constructed as a TQFT satisfying these axioms most efficiently using braided monoidal categories, which play a big role in 3d topology. So it makes quite a bit of sense in a *general* sort of way that Crane and Frenkel are trying to construct Donaldson theory using braided monoidal 2-categories, which seem to play a comparable role in 4d topology. In the paper which I cite in "Week 50", they begin to construct a braided monoidal 2-category related to the group $\mathrm{SU}(2)$, which they conjecture gives a TQFT related to Donaldson theory. That also makes some *general* sense, because Donaldson theory, at least "old" Donaldson theory, is closely related to gauge theory with gauge group $\mathrm{SU}(2)$. Still, I've always wanted to see a more *specific* reason why Donaldson theory should be related to the Crane-Frenkel ideas, not necessarily a proof, but at least a good heuristic argument.

Luckily George Thompson, who invited me to Trieste, knows a bunch about TQFTs. Unluckily he was sick and I never really got to talk to him very much! But luckily his collaborator Matthias Blau was also there, so I took the opportunity to pester him with questions. I learned a bit, most of which is in their paper:

3) "$N = 2$ topological gauge theory, the Euler characteristic of moduli spaces, and the Casson invariant", by Matthias Blau and George Thompson, *Comm. Math. Phys.* **152** (1993), 41–71.

This paper helped me a lot in understanding Crane and Frenkel's ideas. But so that this "week" doesn't get too long, I'll just focus on one basic aspect of the paper, which is the importance of supersymmetric quantum theory for TQFTs. Then next week I'll say a bit more about the Donaldson theory business.

If you look at Witten's paper on Donaldson theory above, you'll see he writes down the Lagrangian for a "supersymmetric" field theory, which is supposed to be a TQFT, namely, Donaldson theory. Supersymmetric field theories treat bosons and fermions in an even-handed way. But why does supersymmetry show up here? The connection with TQFTs is actually pretty simple and beautiful, at least in essence.

Suppose we are doing quantum field theory, and "space" (as opposed to "spacetime") is some manifold M. Then we have some Hilbert space of states $Z(M)$ and some Hamiltonian H, which is a self-adjoint operator on $Z(M)$. To evolve a state (a vector in $Z(M)$) in time, we hit it with the unitary operator $\exp(-itH)$, where t is the amount of time we want to evolve by, and the minus sign is just a convention designed to confuse you.

We can think of this geometrically as follows. We are taking spacetime to be $[0, t] \times M$. You can visualize spacetime as a kind of pipe, if you want, and then imagine sticking in the state ψ at one end and having $\exp(-itH)\psi$ pop out at the other end.

Now say we bend the pipe around and connect the input end to the output end! Then we get the spacetime $S^1 \times M$, where S^1 is the circle of circumference t, formed by gluing the two ends of the interval $[0, t]$ together. For this kind of "closed" spacetime, or compact manifold, a quantum field theory should give us not an operator like $\exp(-itH)$, but a number, the "partition function", which in this case is just the *trace* $\mathrm{tr}(\exp(-itH))$.

The deep reason for this is that taking the trace of an operator — remember, that means taking the sum of the diagonal entries, when you think of it as a matrix — is really very much like as taking a pipe and bending it around, connecting the input end to the output end, forming a closed loop. This may seem bizarre, but observe that taking the sum of the diagonal entries really is just a quantitative measure of how much the "output constructively interferes with the input". (And a very nice one, since it winds up not depending on the basis in which we write the matrix!) This sort of idea is basic in the Bohm-Aharonov effect, where we take a particle in an electomagnetic field around a loop and see how much it interferes with itself, and it is also the basic idea of a "Wilson loop", where we do the same thing for a particle in a gauge field. In other words, the trace measures the amount of "positive feedback". If this still seems bizarre, or just vague, take a look at:

4) *Knots and Physics*, by Louis Kauffman, World Scientific Press, Singapore, 1991.

You will see that the same idea shows up in knot theory, where taking a trace corresponds to taking something (like a braid or tangle) and folding it over to connect the input and output. In a later "week" I'll talk a bit about a new paper by Joyal, Street and Verity that studies the notion of "trace", "feedback" and "folding over" in a really general context, the context of category theory.

Anyway, the partition function $\mathrm{tr}(\exp(-itH))$ typically depends on t, or in other words, it depends on the circumference of our circle S^1, not just on the topology of the manifold $S^1 \times M$. In a TQFT, the partition function is only supposed to depend on the topology of spacetime! So, how can we get $\mathrm{tr}(\exp(-itH))$ to be independent of t?

There is a banal answer and a clever answer. The banal answer is to take $H = 0$! Then $\mathrm{tr}(\exp(-itH)) = \mathrm{tr}(1)$ is just the *dimension* of the Hilbert space:

$$\mathrm{tr}(\exp(-itH)) = dim(Z(M)).$$

Actually this isn't quite as banal as it may sound; indeed, the basic equation of quantum gravity is the Wheeler-DeWitt equation,

$$H\psi = 0,$$

which must hold for all physical states. In other words, in quantum gravity there is a big space of "kinematical states" on which H is an operator, but the really meaningful "physical states" are just those in the subspace

$$Z(M) = \psi : H\psi = 0.$$

Read "Week 11" for more on this.

But there is a clever answer involving supersymmetry! You might hope that there were some more interesting self-adjoint operators H such that $\mathrm{tr}(\exp(-itH))$ is time-independent, but there aren't. So we seem stuck. This reminds me of a course I took from Raoul Bott. He said "so, we think about the problem... and still we are stuck, so what should we do? SUPERTHINK!"

Recall that the Hamiltonian of a free particle in quantum mechanics is — up to boring constants — just minus the Laplacian on configuration space which is some Riemannian manifold that the particle roams around on. For this Hamiltonian, $\mathrm{tr}(\exp(-itH))$ doesn't quite make sense, since the Hilbert space is infinite-dimensional and the sum of the diagonal matrix entries diverges. But $\mathrm{tr}(\exp(-tH))$ often *does* converge. This is why folks often replace t by $-it$ in formulas, which is called "going to imaginary time" or a "Wick transform"; it really amounts to replacing Schrodinger's equation by the heat equation: i.e., instead of a quantum particle, we have a particle undergoing Brownian motion! In any event, $\mathrm{tr}(\exp(-tH))$ certainly depends on t in these situations, but there is something very similar that does NOT.

Namely, let's replace the Laplacian on *functions* by the Laplacian on *differential forms*. I won't try to remind you what these are; I'll simply note that functions are 0-forms, but there are also 1-forms, 2-forms, and so on — tensor fields of various sorts — and the Laplacian of a j-form is another j-form. So for each j we get a kind of Hamiltonian H_j, which is just minus the Laplacian on j-forms. We can also consider the space of *all* forms, never mind the j, and on this space there is a Hamiltonian H, which is just minus the Laplacian on *all* forms. Now, we could try to take the trace of $\exp(-tH)$, but it's more interesting to take the "supertrace":

$$\mathrm{str}(\exp(-tH)) = \mathrm{tr}(\exp(-tH_0)) - \mathrm{tr}(\exp(-tH_1)) + \mathrm{tr}(\exp(-tH_2)) - \dots$$

in other words, the trace of $\exp(-tH)$ acting on even forms, *minus* the trace on odd forms.

Why?? Well, odd forms are sort of "fermionic" in nature, while even forms are sort of "bosonic". The idea of supersymmetry is to throw in minus signs when you've got "odd things", because they are like fermions, and physicists know that lots formulas for fermions are just like formulas for bosons, which are "even things", except for these signs. That's the rough idea. It's all related to how, when you interchange two identical bosons, their wavefunction remains unchanged, while for fermions it picks up a phase of -1.

Now the amazing cool thing is that $\mathrm{str}(\exp(-tH))$ is independent of t. This follows from some stuff called Hodge theory, or, if you want to really show off, index theory. Basically it works like this. If you have an operator A with eigenvalues λ_i, then

$$\mathrm{tr}(\exp(-tA)) = \sum_i \exp(-t\lambda_i)$$

if the sum makes sense. We can use this formula to write out $\mathrm{str}(\exp(-tH))$ in terms of eigenvalues of the Laplacians H_j, and it turns out that all the terms coming from nonzero eigenvalues exactly cancel! So all that's left is the part coming from the zero eigenvalues, which is independent of t. If you believe this for a second, it means we can compute the supertrace by taking the limit as $t \to \infty$. The eigenvalues are all nonnegative, so all the quantities $\exp(-t\lambda_i)$ go to zero except for the zero eigenvalues, and we're left with $\mathrm{str}(\exp(-tH))$ being equal to the alternating sum of the dimensions of the spaces

$$\{\psi \mid H_j\psi = 0\}$$

Now in fact, Hodge theory tells us that these spaces are really just the "cohomology groups" of our configuration space, so the answer we get for $\mathrm{str}(\exp(-tH))$ is what folks call the "Euler characteristic" of our configuration space... an important topological invariant.

So, generalizing the heck out of this idea, we can hope to get TQFTs from supersymmetric quantum field theories as follows. Start with some recipe for associating to each choice of "space" M a "configuration space" $C(M)$... some space of fields on M, typically. Let $Z(M)$ be the space of all forms on $C(M)$, and let H be the minus the Laplacian, as an operator on $Z(M)$. Then we expect that the partition function $\mathrm{str}(\exp(-tH))$ will be independent of t. This is just what one wants in a TQFT. Moreover, the partition function will be the Euler characteristic of the configuration space $C(M)$.

But what if we want to get a TQFT out of this trick, and avoid reference to the Laplacian? Then we can just do the following equivalent thing (at least it's morally equivalent: there will usually be things to check). Let $Z_+(M)$ be the direct sum of all the even cohomology groups of $C(M)$, and let $Z_-(M)$ be the direct sum of all the odd ones. Then

$$\mathrm{str}(\exp(-tH)) = dim(Z_+(M)) - dim(Z_-(M))$$

so what we expect is, not quite a TQFT in the Atiyah sense, but a "superTQFT" whose space of states has an "even" part equal to $Z_+(M)$ and an "odd" part equal to $Z_-(M)$;

the right hand side is then the "superdimension" of the space of states this "superTQFT" assigns to M.

Now actually in real life things get tricky because the configuration space $C(M)$ might be infinite-dimensional, or a singular variety. If $C(M)$ is too weird, it gets hard to say what its Euler characteristic should be! But as Blau and Thompson's paper and the references in it point out, one can often still make it make sense, with enough work. In particular, when we are dealing with Donaldson theory, $C(M)$ is just the moduli space of flat $\mathrm{SU}(2)$ connections on M. This means that the partition function of $S^1 \times M$ should be the Euler characteristic of moduli space, better known as the Casson invariant. And what is the vector space our superTQFT assigns to M? Well, it's called Floer homology. Now actually there are a lot of subtleties here I'm deliberately sloughing over. Read Blau and Thompson's paper for some of them — and read the references for more!

Week 52

May 9, 1995

So, last "week", I said a bit about how supersymmetry could be handy for constructing topological quantum field theories, and this week I want to say a bit more about what that has to do with getting a purely combinatorial description of Donaldson theory.

But first, I want to lighten things up a bit by mentioning a good science fiction novel!

1) *Permutation City*, by Greg Egan, published in Britain by Millenium (should be available in the U.S. by autumn).

There is a lot of popular interest these days in the anthropic principle. Roughly, this claims to explain certain features of the universe by noting that if the universe didn't have those features, there would be no intelligent life. So, presumably, the very fact that we are here and asking certain questions guarantees that the questions will have certain answers.

Of course, the anthropic principle is controversial. Suppose one could really show that if the universe didn't have property X, there would be no intelligent life. Does this really count as an "explanation" of property X? People like arguing about this. But this question is much too subtle for a simple-minded soul such as myself. I'm still stuck on more basic things!

For example, are there any examples where we *can* really show that if the universe didn't have property X, there would be no intelligent life? It seems that to answer this, we need to have some idea about what we're counting as "all possible universes", and what counts as "intelligent life". So far we only know ONE example of a universe and ONE example of intelligent life, so it is difficult to become an expert on these subjects! It'd be all to easy for us to unthinkingly assume that all intelligent life is carbon-based, metabolizes using oxidation, and eats pizza, just because folks around here do.

Our unthinking parochialism is probably all the worse as far as different universes are concerned! What counts as a possible universe, anyway? Rather depressingly, we must admit we don't even know the laws of *this* universe, so we don't really know what it takes for a universe to be possible, in the strong sense of capable of actually existing as a universe. We are a little bit better off if we consider all *logically possible* universes, but not a whole lot better. Certainly every axiom system counts as a logically possible set of laws of a universe - every set of axioms in every possible formal system. But who is to say that universes must have laws of this form? We don't even know for sure that *ours* does!

So this whole topic will remain a hopeless quagmire until one takes a small, carefully limited piece of it and studies that. People studying artificial life are addressing one of these bite-sized pieces, and getting some interesting results. I hope everyone has heard about Thomas Ray's program Tierra, for example: he created an artificial ecosystem - one could call it a "possible universe" - and found, after seeding it with one self-reproducing program, a rapid evolution of parasites, etc., following many of the patterns of ecology here. But so far, perhaps merely due to time and memory limitations, no intelligence!

One of the cool things about "Permutation City" is an imagined cellular automaton, the "Autoverse", which is complicated enough to allow life. But something much cooler is the main theme of the book. Egan calls it the "Dust Theory". It's an absolutely outrageous theory, but if you think about it carefully, you'll see that it's rather hard to spot a flaw. It depends on the tricky puzzles concealed in the issue of "isomorphism".

Being a mathematician, one thing that always puzzled me about the notions of "intelligent life" and "all possible universes" was the question of isomorphisms between universes. Certainly we all agree that, say, the Heisenberg "matrix mechanics" and Schrodinger "wave mechanics" formulations of quantum mechanics are isomorphic. In both of them, the space of states is a Hilbert space, but in one the states are described as sequences of numbers, while in the other they are described as wavefunctions. At first they look like quite different theories. But in a while people realized that there was a unitary operator from Heisenberg's space of states to Schrodinger's, and that via this correspondence all of matrix mechanics is equivalent to wave mechanics.

So does Heisenberg's universe count as the same one as Schrodinger's, or a different one? It seems clear that they're the same. But say we had two quantum-mechanical systems whose Hamiltonians have the same eigenvalues (or spectrum); does that mean they are the "same" system, really? Is that all there is to a physical system, a list of eigenvalues??? If we are going to go around talking about "all possible universes", it would probably pay to think a little about this sort of thing!

Say we had two candidates for "laws of the universe", written down as axioms in different formal systems. How would we decide if these were describing different universes, or were simply different ways of talking about the same universe? Pretty soon it becomes clear that the issue is not a black-and-white one of "same" versus "different" universes. Instead, laws of physics, or universes satisfying these laws, can turn out to be isomorphic or not depending on how much structure you want the isomorphism to preserve. And even if they are isomorphic, there may not be a "unique" isomorphism or a "canonical" isomorphism. (Very roughly speaking, a canonical isomorphism is a "God-given best one", but one can use some category theory to make this precise.) If you think about this carefully you'll see that our universe could be isomorphic to some very different-seeming ones, or could have some very different-seeming ones 'embedded' in it.

Greg Egan takes this issue and runs with it – in a very interesting direction. Everyone interested in cellular automata, artificial life, virtual reality, or other issues of simulation should read this, as well as anyone who likes philosophy or just a good story.

Okay, back to business here...

2) Alberto Cattaneo, "Teorie topologiche di tipo BF ed invarianti dei nodi", doctoral thesis, department of physics, University of Milan.

   Alberto Cattaneo, Paolo Cotta-Ramusino, Juerg Froehlich, and Maurizio Martellini, "Topological BF theories in 3 and 4 dimensions", preprint available as hep-th/9505027.

So, last week I said a wee bit about Blau and Thompson's paper on supersymmetry and the Casson invariant. All I said was that suitably concocted supersymmetric field theories could be used to compute the Euler characteristics of your favorite spaces, and that Blau and Thompson talked about one which computed the Casson invariant, which is (in a rather subtle sense) the Euler characteristic of the moduli space of flat connections on a trivial $\mathrm{SU}(2)$ bundle over a 3-manifold. Traditionally one requires that the

3-manifold be a homology 3-sphere, but Kevin Walker showed how to do it for rational homology spheres, and Blau and Thompson mention other work in which the Casson invariant is generalized still further.

But I didn't say *which* supersymmetric field theory computes the Casson invariant for you. The answer is, $N = 2$ supersymmetric BF theory with gauge group $\mathrm{SU}(2)$. So now I should say a little about BF theory. Actually I have already mentioned it here and there, especially in "Week 36". But I should say a bit more. This is going to be pretty technical, though, so fasten your seatbelts.

The people I know who are the most excited about BF theory are the folks I was visiting at Milan, namely Cotta-Ramusino, Martellini and his student Cattaneo. They are working on BF theory in 3 and 4 dimensions. Let me talk about BF theory in 3 dimensions, which is what's most directly relevant here. Well, in *any* dimension, say n, the fields in BF theory are a connection A on a trivial bundle (take your favorite gauge group G), whose curvature F we'll think of as a 2-form taking values in the Lie algebra of G, and Lie-algebra-valued $(n-2)$-form B. Then the Lagrangian of the theory is

$$L(B,F) = \mathrm{tr}(B \wedge F)$$

where in the "trace" we're using something like the Killing form to get an honest n-form which we can integrate over spacetime.

But in 3 dimensions, since B is a 1-form, you can add an extra "cosmological constant" term and take as your Lagrangian

$$L(B,F,c) = \mathrm{tr}(B \wedge F + (c^2/3)B \wedge B \wedge B)$$

where I have put in "$c^2/3$" as my "cosmological constant" for insidious reasons to become clear momentarily. Now what the article by Cattaneo, Cotta-Ramusino, Froehlich and Martellini makes really clear is how BF theory is related to Chern-Simons theory. This is implicit in Witten's work on 3d gravity (see "Week 16"), which is just the special case where G is $\mathrm{SO}(2,1)$ or $\mathrm{SO}(3)$, and where the cosmological constant really is the usual cosmological constant. But I'd never noticed it. Recall that the Chern-Simons action is

$$L(A) = \mathrm{tr}(A \wedge dA + (2/3)A \wedge A \wedge A)$$

Thus if we have 1-form B around as well, we can set

$$A' = A + cB,$$
$$A'' = A - cB$$

so we get two different Chern-Simons theories with actions $L(A')$ and $L(A'')$, respectively. OR, we can form a theory whose action is the difference of these two, and, lo and behold:

$$L(A') - L(A'') = 4cL(B,F,c).$$

In other words, BF theory with cosmological constant is just a "difference of two Chern-Simons theories". Fans of topological quantum field theory may perhaps be more familiar with this if I point out that the Turaev-Viro theory is just BF theory with gauge group $\mathrm{SU}(2)$, and the fact that the partition function for this theory is the absolute value squared of that for Chern-Simons theory is a special case of what I'm talking about. The nice thing about all this is that the funny phases coming from framings in Chern-Simons theory precisely cancel out when you form this "difference of two Chern-Simons theories".

Now the Casson invariant is related to BF theory in 3 dimensions *without* cosmological constant, i.e., taking $c = 0$. We might get worried by the equation above, which we can't solve for $L(B, F)$ when $c = 0$, but as Cattaneo and company point out,

$$L(B, F) = \lim_{c \to 0} \frac{L(A') - L(A'')}{4c}$$

so BF theory without cosmological constant is just a limiting case, actually a kind of *derivative* of Chern-Simons theory. They use this to make clearer the relation between the vacuum expectation values of Wilson loops in Chern-Simons theory — which give you the HOMFLY polynomial for $G = \mathrm{SU}(N)$ — and the corresponding vacuum expectation values in BF theory without cosmological constant — which give you the Alexander polynomial! Very pretty stuff.

Now back to the Casson invariant and some flagrant speculation on my part concerning Crane and Frenkel's ideas on Donaldson theory. (I said last week that this is where I was heading, and now I'm almost there!) Okay: we know how to define Chern-Simons theory in a purely combinatorial way using quantum groups. I.e., we can compute the partition function of Chern-Simons theory with gauge group G using the quantum version of the group G... let me just call it "quantum G". If we take c to be imaginary above, one can show that BF theory with cosmological constant can be computed in a very similar way starting with the quantum group corresponding to the *complexification* of G, i.e. "quantum $\mathbb{C}G$". The point is that $A + cB$ can then be thought of as a connection on a bundle with gauge group $\mathbb{C}G$. So far this is not flagrant speculation. Slightly more flagrantly, but not really very much at all, the formulas above hint that BF theory without cosmological constant can be computed in a similar way starting with the quantum group corresponding to the *tangent bundle* of G, or "quantum TG". (The tangent bundle of a Lie group is again a Lie group, and as we let $c \to 0$ what we are really doing is taking a limit in which $\mathbb{C}G$ approaches TG; folks call this a "contraction", and in the $\mathrm{SU}(2)$ case many of the details appear in Witten's paper on 3d quantum gravity; the tangent bundle of $\mathrm{SO}(2, 1)$ being just the Poincare group in 3 dimensions.) If anyone knows whether folks have worked out the quantization of these tangent bundle groups, let me know! I think some examples have been worked out.

Okay, but Blau and Thompson say that to compute the Casson invariant you need to use, not BF theory with gauge group $\mathrm{SU}(2)$, but *supersymmetric* BF theory with gauge group $\mathrm{SU}(2)$. Well, no problemo — just compute it with "quantum super-$T(\mathrm{SU}(2))$"! Here I'm getting a bit flagrant; there *are* theories of quantum supergroups, but I don't know much about them, especially "quantum super-TG" for G compact semisimple. Again, if anybody does, please let me know! (Actually Blau told me to check out a paper by Saleur and somebody on this, but I never did....)

Okay, but now let's get seriously flagrant. Recall that the Casson invariant is really the Euler characteristic of something, just a number, but this number is just the superdimension of a super-vector-space, namely the Floer cohomology. From numbers to vector spaces: this is a typical sort of "categorification" process that one would expect as one goes from 3d to 4d TQFTs. And indeed, folks suspect that the Floer cohomology is the space of states for a 4d TQFT, or something like a 4d TQFT, namely Donaldson theory. ("Something like it" because of many quirky twists that one wouldn't expect of a full-fledged TQFT satisfying the Atiyah axioms.) So, just as the Casson invariant is associated to a certain Hopf algebra, namely "quantum super-$T(\mathrm{SU}(2))$", we'd expect Donaldson theory to be associated to a certain Hopf *category*, the "categorification of quantum super-$T(\mathrm{SU}(2))$". So all we need to do is figure out how to categorify quantum super-$T(\mathrm{SU}(2))$ and we've got a purely combinatorial definition of Donaldson theory!

Well, that's not quite so easy, of course. And I may have made, not only the inevitable errors involved in painting a simplified sketch of what is bound to be a rather big task, but also other worse errors. Still, this business should clarify, if only a wee bit, what Crane and Frenkel are up to when they are categorifying $\mathrm{SU}(2)$. In fact, it's likely that working with $\mathrm{SU}(2)$ rather than $T(\mathrm{SU}(2))$ will remove some of the divergences from the state sum, since, being compact, $\mathrm{SU}(2)$ has a discrete set of representations (and quantum $\mathrm{SU}(2)$ has finitely many interesting ones, at roots of unity). So they may get a theory that's allied to but not exactly the same as Donaldson theory, yet better-behaved as far as the TQFT axioms go.

If anyone actually does anything interesting with these ideas I'd very much appreciate hearing about it.

Week 53

May 18, 1995

Near the end of April I was invited by Ronnie Brown to Bangor, Wales for a very exciting get-together. Readers of "This Week's Finds" will know I'm interested in n-categories and higher-dimensional algebra these days. Brown is the originator of the term "higher-dimensional algebra" and has been sort of a prophet of the subject for many years. Tim Porter at Bangor also works on this subject; I'll try to say a bit more about his stuff next week. And visiting Bangor at the time were John Power and Ross Street, two category theorists who do a lot of work on n-categories. So I had a chance to learn some more higher-dimensional algebra and category theory and see what these folks thought of my crazy ideas.

1) Ronald Brown, "Out of line", *Royal Institution Proceedings* **64**, 207–243.

Brown is very interested in explaining mathematics to the public, and this paper is based on a talk he gave to a general audience. It is a very accessible introduction to what mathematics is really all about, and what higher-dimensional algebra is about in particular. "Out of line" is a pun, of course, because not only does higher-dimensional algebra seek to burst free of certain habits of "linear thinking" that tend to go along with symbol string manipulation, it also has been a bit outside the mainstream of mathematics until recently.

Now, when I speak of "linear thinking" I am not indulging in some vague new-agey complaint against rationality. I mean something very precise: the tendency to focus ones energy on operations that are easily modelled by the juxtaposition of symbols in a line. The primordial example is addition: we have a bunch of sticks in a row:

$$|||||$$

and we say there are "5" sticks and write

$$1 + 1 + 1 + 1 + 1 = 5.$$

Fine. But when we have a 2-dimensional array of sticks:

$$||||$$
$$||||$$

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we are in a hurry to bring the situation to linear form by making up a new operation, "multiplication", and saying we have 2×4 sticks. This isn't so bad for plenty of purposes; it's not as if I'm against times tables! But certain things, particular in topology, can get obscured by neglecting operations that correspond most naturally to higher-dimensional forms of juxtaposition, and Brown's paper explains some of these, and how to deal with these problems. The point is not to avoid linear notation; it's to avoid falling into certain mental traps it can lead you into if you're not careful!

2) A. J. Power, "Why tricategories?", preprint available as `ECS-LFCS-94-289` from Laboratory for Foundations of Computer Science, University of Edinburgh. Also available at `http://www.ima.umn.edu/talks/workshops/SP6.7-18.04/power/power.pdf`

When I mentioned this paper to a friend, she puzzledly asked " 'Why try categories?'?" And indeed, one must have tried categories and enjoyed them before moving on to bicategories, tricategories and that great beckoning terra incognita of mathematics, n-category theory.

In a sense I already know "why tricategories". I think they're great, and in a paper with James Dolan — summarized in "Week 49" — I did my best to get everyone else interested in general n-categories. For me, the great thing about n-category theory is that it strives to formalize the notion of "process" in all its recursive splendor. An n-category is a mathematical structure containing not only objects, which one might think of as "things", and morphisms, which one might think of as "processes leading from one thing to another", but also 2-morphisms, which are "processes leading from one process to another", and then 3-morphisms, etc., on up to n-morphisms.

In physics and topology applications, the k-morphisms can often be thought of as k-dimensional geometrical objects, since (as the Greeks knew) the process of motion of a point traces out a 1-dimensional figure, and similarly the motion of a 1-dimensional figure traces out a 2-dimensional surface... and n-dimensional spacetime is in some rough sense "traced out" by the motion of $(n-1)$-dimensional spacelike slices through time. If you think this is vague, you're right — and that's why one needs n-category theory, to make it precise! When one understands n-categories (which so far we really do only up to $n = 3$) one sees that the possibilities inherent in n-dimensional topology match up very nicely with one might have stumbled on not knowing topology at all, but just playing around with this iterated notion of processes between processes between processes... This "natural correspondence" between purely algebraic concepts and topological ones is what makes topological quantum field theory tick, and I can't help but feel that it will have quite a bit to say about physics eventually.

Power, however, gives a quite different set of reasons for being interested in tricategories. These are drawn from computer science and logic, and they make me realize yet again how poor and outdated my education in logic was, and how much interesting stuff there is going on in the subject!

At a completely naive level, one might already expect that relation between "processes" and "things" is subtle and interesting in computer science. For after all, we can think of a program either as a process that turns one thing into another, or as data, a sort of thing, which other programs can act on. Power does not really emphasize this issue explicitly, but I can't help remarking on it, especially because it reminds me of the curious fact that in mathematical physics, "time is the last dimension".

That is, in topological quantum field theory, the n-morphisms in an n-category, which are the processes having no further processes going between them, represent the passage of time. And indeed, for many practical purposes it appears that $n = 4$ is where things leave off, since spacetime appears 4-dimensional. On the other hand, nobody knows any mathematical reason why one has to stop at any given n. So ultimately we should try to understand "ω-categories", having n-morphisms for all n greater than or equal to zero (0-morphisms being simply objects, and 1-morphisms being morphisms). This corresponds philosophically to the notion that every "process" can also be regarded as a "thing" which other processes can transform. Moreover, we should also try to understand "$\mathbb{Z}$-categories", having n-morphisms for all integers n, even negative ones! In this world, where there is no bottom as well as no top, every "thing" can also be regarded as a "process".

But I digress. Power is actually more interested in a different (though perhaps related) hierarchy. Sometimes people like to say computers just do stuff with bunches of numbers, but that's pretty misleading. Of course computers *can* do things with numbers, but that's one of the simpler mathematical things they can do. A number is an element of a set (the set of real numbers, or some set of more computer-manageable numbers.) And computers have no problem dealing with elements of sets. But computers can also deal with sets themselves — and more fancy mathematical objects.

Many mathematical objects are sets, or bunches of sets, equipped with operations satisfying equational laws. For example, a group is a set equipped with a product and inverse operation satisfying various laws. Sometimes these operations are only defined if certain conditions hold, of course. For example, a category is a set of "objects" and a set of "morphisms", together with various operations like composition of morphisms, but one can only compose two morphisms $f\colon x \to y$ and $g\colon w \to z$ if $y = w$. Other examples might include graphs, partially ordered sets... and all sorts of things computer scientists know and love.

We could call all of these "sets with essentially algebraic structure." Mathematically sophisticated computer scientists want to be able define data types corresponding to arbitrary sorts of sets with essentially algebraic structure, and to play around with them easily. So they need to ponder such things in considerable generality.

Note that in all cases, there is not just a bunch of objects to play with — like "groups" or "partially ordered sets" — but a *category* in which the morphisms are structure-preserving maps between the objects in question. For example, there is a category Grp whose objects are groups and whose morphisms are group homomorphisms.

The categories one gets this way are of a certain sort. Power calls them "categories of models of finite limit theories". To define this requires a bit of know-how, but it's basically simple. For example, suppose I were trying to explain the definition of a category to a computer scientist; I might say, every category has a set ob of objects and a set mor of morphisms; every morphism has an object called its source (or domain), so there is a function

$$\mathrm{source}\colon \mathrm{mor} \to \mathrm{ob}$$

and similarly every morphism has an object called its target (or codomain) so there is a function

$$\mathrm{target}\colon \mathrm{mor} \to \mathrm{ob}.$$

Now, we can compose a morphism f and a morphism g to get fg if $\mathrm{target}(f) = \mathrm{source}(g)$, so we have a composition function

$$\mathrm{composition}\colon C \to \mathrm{mor}$$

defined only on the subset C of $\mathrm{mor} \times \mathrm{mor}$ that is the biggest subset making the following diagram commute:

$$\begin{array}{ccc} C & \xrightarrow{p_1} & \mathrm{mor} \\ {\scriptstyle p_2}\downarrow & & \downarrow{\scriptstyle \mathrm{target}} \\ \mathrm{mor} & \xrightarrow{\mathrm{source}} & \mathrm{ob} \end{array}$$

where $p_1 \colon (f, g) \mapsto f$ and $p_2 \colon (f, g) \mapsto g$.

Now category theorists have a slick way of dealing with these functions defined only a subset satisfying equational conditions; instead of talking about the "biggest subset" C they would say that C is the "limit" of the diagram

$$\begin{array}{ccc} & & \mathrm{mor} \\ & & \downarrow{\scriptstyle \mathrm{target}} \\ \mathrm{mor} & \xrightarrow{\mathrm{source}} & \mathrm{ob} \end{array}$$

If you don't get this, don't worry; in a sense it's just another way of talking about the same thing, with the advantage of being infinitely more general, since one can talk about the limit of any diagram, though here we will only need limits of *finite* diagrams.

Then, after having lined up these ingredients (and I have left some out!), I could go ahead and say what equational laws they need to satisfy, like associativity of composition; and if I wanted I could write all these laws out using commutative diagrams, too! Then I would have laid out the "theory of categories" — a complete description of the operations in a category and the laws they obey.

The theory of categories is a typical example of a "finite limit theory", because what I really did above, in describing the "theory of categories", is describe a CATEGORY, say Th, having objects called ob and mor, and morphisms called source, target, $\mathrm{composition}$, and so on, such that various diagrams commute! Moreover, we should think of Th as a category with all finite limits, that is, one in which all finite diagrams have limits. That allows us to deal with things like the object C above, which are defined as limits of finite diagrams.

So we have this thing Th, the "theory of categories". And then, any *particular* category is a "model" of this theory Th. A "model" assigns to each object in Th a particular set — for example, "mor" above gets assigned the set of morphisms in some particular category $\mathcal{C}$ — and assigns to each morphism in Th a particular function — for example, "composition" above gets assigned the function representing composition in $\mathcal{C}$. Moreover, this assignment satisfies a bunch of utterly obvious consistency conditions which one summarizes by saying that a "model of the theory Th is a functor from Th to Set that preserves finite limits". In logic, you know, a model of a theory is something that assigns to each thingie in the theory an actual thingie, in such a way that all the stuff the theory SAYS is true about these thingies, IS true!

Now if you are with me thus far you either know this stuff better than I do, or else I congratulate you, because the example I picked was deliberately self-referential and confusing — I was using category theory to describe the theory of categories, and also, the theory Th itself was a category! But the world of thought does have a funny way of wrapping back on itself like that... so it's good to get used to it.

In fact there is a big literature on "sets with essentially algebraic structure" and "categories of models of finite limit theories"... this is a branch of logic they never taught me about in school, but it definitely exists, and Power gives some references to it:

3) P. Gabriel and F. Ulmer, *Lokal praesentierbare Kategorien*, in Springer Lecture Notes in Math **221** (1971).

G. Kelly, Structures defined by finite limits in the enriched context I, *Cahiers de Top. et. Geom. Diff.* **23** (1982), 3–41.

Michael Makkai and Robert Pare, "Accessible categories: the foundations of categorical model theory", in *Contemp. Math.* **104** (1989).

But let's dig in a bit further, since really the fun is just starting. Now, I told you what a model of one of these finite limit theories Th was, but not what a morphism between models is! Well, if a model is a sort of functor, a morphism between them should be a sort of natural transformation between functors; that's how it usually goes. So there is really a category $\mathsf{Mod}(\mathsf{Th})$ of models of one of these theories Th. If Th were the theory of categories as above, $\mathsf{Mod}(\mathsf{Th})$ would be the category of (small) categories, which is called Cat. To take a less fiendish example, if Th were the theory of groups, $\mathsf{Mod}(\mathsf{Th})$ would the category Grp.

But now suppose one wanted to build a computer language that could not only deal with all sorts of data types corresponding to different "sets with essentially algebraic structure", but also various "categories with essentially algebraic structure". For if one likes category theory well enough to do a lot of computer science using it, it makes sense to let the computer itself join the fun, by creating a language in which it's easy to talk about categories. After all, our eventual goal with computers is to have them completely replace computer scientists, right?

Well, in a way "categories with essentially algebraic structure" aren't terribly different from sets with essentially algebraic structure. Roughly, the idea is that instead of having a data type that consists of a bunch of sets with functions between them satisfying some equational laws, we have a data type consisting of a bunch of categories, functors between them, and natural transformations between THEM satisfying equational laws. What this means is that if we try to copy the above stuff, instead of a "theory" we will have a "2-theory" Th, which is some sort of 2-category, and then a model of this would be a 2-functor from Th to Cat. We want to wind up getting a 2-category $\mathsf{Mod}(\mathsf{Th})$ of models of Th.

But actually carrying this out is a bit tricky, and much of Power's paper goes into the details of various proposed schemes. Of course there is no reason in principle to stop here, other than our limited understanding of n-categories, sheer bewilderment, or boredom. Reasoning about n-categories always tends to drag in $(n+1)$-categories, because the collection of all n-categories with some particular structure (such as the "essentially algebraic structures" I've focussed on here, but also other sorts) typically forms an $(n+1)$-category. This is how Power motivates tricategories. Right now we are stuck at $n = 3$, but there are good reasons to expect that pretty soon we'll go beyond that. In fact, Power and Street showed me a sketch of their ideas on tetracategories. . . .

Week 54

June 2, 1995

I just got back from a quantum gravity conference in Warsaw, and I'm dying to talk about some of the stuff I heard there, but first I should describe some work on topology and higher-dimensional algebra that I have been meaning to discuss for some time now.

1) Timothy Porter, 'Abstract homotopy theory: the interaction of category theory and homotopy theory, lectures from the school on "Categories and Topology" ', Department of Mathematics, Universita di Genova, report #199, March 1992.

Timothy Porter is another expert on higher-dimensional algebra whom I met in Bangor, Wales, where he teaches. As paper 3) below makes clear, he is very interested in the relationship between traditional themes in topology and the new-fangled topological quantum field theories (TQFTs) people have been coming up with these days. The above paper does not mention TQFTs; instead, it is an overview of various approaches that people have used to study homotopy theory in an algebraic way. But anyone seriously interested in the intersection of physics and topology would do well to get ahold of it, since it's a pleasant way to get acquainted with some of the beautiful techniques algebraic topologists have been developing, which many physicists are just starting to catch up with.

What's homotopy theory? Well, roughly, it's the study of the properties of spaces that are preserved by a wide class of stretchings and squashings, called "homotopies".

For example, a closed disc D and a one-point set $\{p\}$ are quite different as topological spaces, in that there is no continuous map from one to the other having a continuous inverse. (This is obvious because they have a different number of points!) But there is clearly something similar about them, because you can squash a disc down to a point without crushing any holes in the process (since the disc has no holes). To formalize this, note that we can find continuous functions

$$f\colon D \to \{p\}$$

and

$$g\colon \{p\} \to D$$

that are inverses "up to homotopy". For example, let f be the only possible function from D to $\{p\}$, taking every point in D to p, and let g be the map that sends p to the point 0, where we think of D as the unit disc in the plane. Now if we first do g and then do f we are back where we started from, so gf is the identity on $\{p\}$. But if we first do f and then g we are NOT necessarily back where we started from: instead, the function fg takes every point in D to the point 0 in D. So fg is not the identity. But it is "homotopic" to the identity, by which I mean that there is a continuously varying family of continuous functions F_t from D to itself, such that $F_0 = fg$ and F_1 is the identity on D. Simply let F_t be scalar multiplication by t! As t goes from 1 to 0, we see that F_t squashes the disc down to a point.

A bit more precisely, and more generally too, if we have two topological spaces X and Y we say that two continuous functions $f, g\colon X \to Y$ are homotopic if there is a continuous function

$$F\colon [0,1]\times X\to Y$$

such that

$$F(0,x)=f(x)$$

and

$$F(1,x)=g(x).$$

Intuitively, this means that f can be "continuously deformed" into g. Then we say that two spaces X and Y are homotopic if there are continuous functions $f\colon X\to Y$, $g\colon Y\to X$ which are inverse up to homotopy, i.e., such that gf and fg are homotopic to the identity on X and Y, respectively.

The main goal in homotopy theory is to understand when functions are homotopic and when spaces are homotopic. This is incredibly hard in *general*, but in special cases a huge amount is known. To take a random (but important) example, people know that all maps from the sphere to the circle are homotopic. Remember that algebraists call the sphere S^2 since its surface is 2-dimensional, and call the circle S^1; in general the unit sphere in $\mathbb{R}^{n+1}$ is called S^n. So for short, one says that all maps from S^2 to S^1 are homotopic. But: there are infinitely many different nonhomotopic maps from $S^3 to S^2! In fact there is a nice way to label all these "homotopy classes" of maps by integers. And then: there are only two homotopy classes of maps from S^4 to S^3. There are also only two homotopy classes of maps from S^5 to S^4, and from S^6 to S^5, and so on.

Now, the famous topologist J. H. C. Whitehead put forth an important program in 1950, as follows: "The ultimate aim of *algebraic homotopy* is to construct a purely algebraic theory, which is equivalent to homotopy theory in the same way that 'analytic' is equivalent to 'pure' projective geometry." Since then a lot of people have approached this program from various angles, and Porter's paper tours some of the key ideas involved.

Part of the reason for pursuing this program is simply to get good at computing things, in a manner similar to how analytic geometry helps you solve problems in "pure" geometry. This is not my main interest; if I want to know how many homotopy classes of maps there are from S^9 to S^6, or something, I know where to look it up, or whom to ask — which is infinitely more efficient than trying to figure it out myself! And indeed, there is a formidable collection of tools out there for solving various sorts of specific homotopy-theoretic problems, not all of which rely crucially on a *general* purely algebraic theory of homotopy.

I'm more interested in this program for another reason, which is simply to find an algebraic language for talking about things being true "up to homotopy". As I've tried to explain in recent "weeks", there are many situations where equations should be replaced by some weaker form of equivalence. Taking this seriously leads naturally to the study of n-categories, in which equations between j-morphisms can be replaced by specified $(j+1)$-morphisms. But Porter describes a host of different (though related) formalisms set up to handle this sort of issue. A few of the main ones are: simplicial sets, simplicial objects in more general categories, Kan complexes, Quillen's "model categories", Cat^n groups, and homotopy coherent diagrams. Understanding how all these formalisms are related and what they are good for is quite a job, but this paper helps one get started.

So far everything I've actually said is quite elementary — I've made reference to some impressive buzzwords without explaining them, but that doesn't count. So I should put in something for the folks who want more! Let me say a word or two about Cat^n groups. The definition of these is a typical mind-blowing piece of higher-dimensional algebra, so I can't resist explaining it. (After a while these definitions stop seeming so mind-boggling, and then one is presumably beginning understand the point of the subject!) In "Week 53" I gave a definition of a category using category theory. This might seem completely circular and useless, but of course I was illustrating quite generally how one could define a "model" of a "finite limit theory" using category theory. The idea was that a category is a *set* of objects, a *set* of morphisms, together with various *functions* like the source and target functions which assign to any morphism (or "arrow") its source and target (or "tail" and "tip"). These sets and functions needed to satisfy various axioms, of course.

Now *sets* and *functions* are the objects and morphisms in the category of sets, which folks call Set. So in "Week 53" I cooked up a little category Th called "the theory of categories", which has objects called "ob" and "mor", morphisms called "s" and "t", etc.. These were completely abstract gizmos, not actual sets and functions. But we required them to satisfy the exact same laws that the sets of objects and morphisms, and the source and target functions, and so on, satisfy in an actual category. Then a functor from Th to Set which preserves finite limits is called a "model" of the theory of categories, because it assigns to the completely abstract gizmos actual sets and functions satisfying the same laws. In other words, if we have a functor

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

we have an actual set $F(\mathrm{ob})$ of objects, an actual set $F(\mathrm{mor})$ of morphisms, an actual function $F(s)$ from $F(\mathrm{ob})$ to $F(\mathrm{mor})$, and so on. In short, we have an actual category!

Now to get this trick to work we didn't need much to be true about the category Set: all we needed was that it had finite limits. (Ignore this technical stuff about limits if you don't get it; you can still get the basic idea here.) And there are lots of categories with this property, like the category Grp of groups. So we can also talk about a model of the theory of categories in the category of groups! What is this? Well, it's just a functor from Th to Grp that preserves finite limits. More concretely, it's exactly like a category, except everywhere in the definition of category where you see the word "set", scratch that out and write in "group", and everywhere you see the word "function", scratch that out and write in "homomorphism". So you have a *group* of objects, a *group* of morphisms, together with various *homomorphisms* like the source and target, and so on... satisfying laws perfectly analogous to those in the definition of a category!

Folks call this kind of thing a "categorical group", a "category object in Grp" or an "internal category in Grp". From the point of view of sheer audacity alone, it's a wonderful concept: we have taken the definition of a category and transplanted it from the soil in which it was born, namely the category Set, into new soil, namely the category Grp! But more remarkably still, the study of these "categorical groups" is equivalent to the study of "homotopy 2-types" - that is, topological spaces, but where you only care about them up to homotopy, and even more drastically, where nothing above dimension 2 concerns you. This result is due (as far as I can tell) to Ronnie Brown and C. B. Spencer, building on earlier work of Mac Lane and Whitehead.

But why stop here? Consider the category Cat(Grp) of these category objects in Grp. Take my word for it, such a thing exists and it has finite limits. That means we can look for models of the theory of categories in Cat(Grp) — i.e., functors from Th to Cat(Grp), preserving finite limits. In fact, *there* things form a category, say $\mathsf{Cat}^2(\mathsf{Grp})$, and *this* category has finite limits, so we can look for models of the theory of categories in *this* category, and *these* form a category $\mathsf{Cat}^3(\mathsf{Grp})$, which also has finite limits… etc. So we can construct an insanely recursive hierarchy:

- groups
- category objects in the the category of groups
- category objects in the category of (category objects in the category of groups)
- etc. . . .

Now, truly wonderfully, L. Loday showed that the study of $\mathsf{Cat}^n(\mathsf{Grp})$ is equivalent (in a certain precise sense) to the study of homotopy n-types — i.e., homotopy theory where you don't care about phenomena above dimension n:

2) L. Loday, "Spaces with finitely many non-trivial homotopy groups", *Jour. Pure Appl. Algebra* **24** (1982), 179–202.

Subsequently, Ronnie Brown, Loday and others have done some interesting topology using this fact. But the most remarkable thing, in a way, is how taking a perfectly basic concept, the concept of GROUP, and then doing category theory "internally" in the category of groups in an iterated fashion, winds up being very much the same as doing topology - or at least homotopy theory. This suggests that there is something deeply algebraic about homotopy theory in the first place.

3) Timothy Porter, "Interpretations of Yetter's notion of G-coloring: simplicial fibre bundles and non-abelian cohomology", available at http://citeseer.ist.psu.edu/100965.html

Physicists know and love the Dijkgraaf-Witten model, a 2+1-dimensional TQFT that depends on a finite group G. It's easy to compute the Hilbert space of states for any compact oriented 2-manifold in this TQFT. Just triangulate your 2-manifold and let your Hilbert space have as a basis the set of all possible ways of labelling the edges with elements of G such that $g_1 g_2 g_3 = 1$ whenever we have 3 edges going counterclockwise around any triangle. Yetter figured out how to generalize this to get an interesting TQFT from any finite categorical group:

4) David N. Yetter, "Topological quantum field theories associated to finite groups and crossed G-sets", *Journal of Knot Theory and its Ramifications* **1** (1992), 1–20.

   "TQFTs from homotopy 2-types", *Journal of Knot Theory and its Ramifications* **2** (1993), 113–123.

This should be the beginning of some bigger pattern relating homotopy theory and TQFTs. Jim Dolan and I have our own theories as to how this pattern should work (see "Week 49") but they are still a rather long ways from being theorems. Porter, who is an expert in simplicial methods, makes the relationship (or ONE of the relationships) very clear in this special case.

5) Justin Roberts, "Skein theory and Turaev-Viro invariants", preprint.

"Refined state-sum invariants of 3- and 4-manifolds", preprint.

"Skeins and mapping class groups", *Math. Proc. Camb. Phil. Soc.* **115** (1994), 53–77.

G. Masbaum and Justin Roberts, "On central extensions of mapping class groups", *Mathematica Gottingensis, Schriftenreihe des Sonderforschungsbereichs Geometrie und Analysis*, Heft **42** (1993).

The first two papers here might be the most interesting for physicists. They both deal with 3d and 4d TQFTs constructed using quantum $\mathrm{SU}(2)$: in particular, the Turaev-Viro theory in dimension 3, and the Crane-Yetter-Broda theory in dimension 4. The first theory is interesting physically because it corresponds to 3d Euclidean quantum gravity with cosmological constant. The second theory is interesting mainly because it's one of the few 4d TQFTs for which the Atiyah axioms have been shown; sometime I will explain the Lagrangian for this theory, which seems not to be well-known.

In Roberts' first paper, which was already discussed in "Week 14", he gave a simple proof that the partition function for the Turaev-Viro theory was the absolute value squared of that for Chern-Simons theory, perhaps the most famous of TQFTs. He also showed that the partition function for the Crane-Yetter-Broda theory was a function of the signature and Euler characteristic (classical invariants of 4-manifolds). In the second paper, he considers observables for the TV and CYB theories depending on cohomology classes in the manifold. I wish I had energy now to explain a bit more about these observables, since they are very interesting, but I don't!

6) Lawrence Breen, "On the Classification of 2-Gerbes and 2-Stacks", *Asterisque* **225**, 1994.

Suffice it to say that if gerbes and stacks — which are, very roughly, sort of like sheaves of categories — are too simple to interest you, it's time to read about 2-gerbes and 2-stacks — which are roughly like sheaves of 2-categories.

Week 55

June 4, 1995

I recently went to a workshop on canonical quantum gravity in Warsaw, organized by Jerzy Kijowski and Jerzy Lewandowski, and I learned some interesting things. I'll talk about some of them in this issue, and some in the next.

Conferences are a funny thing. On science newsgroups on the net, there is very little talk about conferences. This is probably because the people who really understand conferences are too busy flying from one conference to the next to post to newsgroups very often. Academic success is in part measured by the number of conference invitations one receives, the prestige of the conferences, and the type of invitation. For example, a big plenary lecture on an impressive stage, preceded by a little warmup where someone explains how great you are, counts for infinitely many talks in those parallel sessions where dozens of people get 10 minutes each to explain their work before the moderator begins to make little coughs indicating that it's time for the next one, while all the while people drift in and out in a feeble attempt to find the really interesting talks. Still, giving any sort of talk is regarded as better than giving none, so academics spend a lot of time doing this sort of thing.

One of the great dangers of being a successful academic, in fact, is that one may get invited to so many conferences that one never has time to think. Winning the Nobel prize is purported to be the kiss of death in this respect. Of course, it's a universal platitude that the real thinking at conferences gets done not during the talks, but informally in small groups. But the funny thing is that at most conferences people are so worn out after going to a day's worth of talks that they have limited energy for serious conversation afterwards: they usually seem more interested in finding the good local restaurants and scenic attractions. If people could have conferences with no lectures whatsoever, or maybe one a day, it would probably be more productive. But the idea that a bunch of people could figure something out just by sitting around and chatting informally is absolutely foreign to our conception of "work". People expect to receive money from bureaucrats to go to conferences, but to convince a bureaucrat that you are deserve the money, you need to give a lecture, so of course all conferences have too many lectures.

Turning back towards Warsaw, a city with a marvelous mathematical history, I am reminded of Gian-Carlo Rota's biographical sketch of Stanislaw Ulam, in which (as a master of irony) he talks about how lazy Ulam was: all he wanted to do was sit around in cafes and come up with interesting conjectures and research programs, and leave it to others to work them out. And this in turn reminds me of the Scottish Cafe, where Polish mathematicians used to hang out and write on the tablecloths, until the owner provided them with a notebook, in which many famous conjectures were formulated, and I believe prizes like bottles of wine were offered for their solutions. Was the Scottish Cafe in Warsaw? [No, Lwow.] Does it still exist? I completely forgot to check while I was there. The Banach Center, in which the conference participants stayed, comes from a later stratum of Polish mathematical history; it was built after the war, and one room still contains a portrait of Lenin. I know that because a film crew used it to shoot a scene for a historical movie!

Anyway, I enjoyed this conference in Warsaw quite a bit, because a lot of people working on the loop representation of quantum gravity were there, and I managed to have a fair number of serious conversations. Before going into what I learned there, I should say that I just found a fun thing for people to read who are interested in quantum gravity, but are not necessarily specialists:

1) Gary Au, "The quest for quantum gravity", available as `gr/qc-9506001`.

This consists mainly of interviews with Chris Isham, Abhay Ashtekar and Edward Witten. What's nice is that the interviews are conducted by someone who knows physics. The questions and answers are technical enough to convey some of the real substance of the subject, while still (I hope) non-technical enough so that you don't have to be an expert to get a lot out of them. Isham talks mainly about the "problem of time" in quantum gravity, Ashtekar talks mainly about the loop representation of quantum gravity, and Witten talks about string theory.

Anyway, Ashtekar and a bunch of other good people were at this Warsaw conference, which is why I went. The main topics of conversation were spin networks and their use in studying the area and volume operators in quantum gravity. As I explained earlier in "Week 43", one may very roughly think of a spin network as a graph whose edges are labelled with "spins" $0,1/2,1,3/2$, and so on, and who vertices are labelled with certain gadgets called "intertwining operators" (which in the simplest case are just the Clebsch-Gordon coefficients you learn about when studying angular momentum in quantum mechanics). Penrose introduced these as abstract graphs (see "Week 22" and "Week 41"), as a kind of substitute for thinking of space as a manifold, but more recently Rovelli and Smolin started thinking of them as graphs embedded into 3d space, and saw that these were a really natural way to describe states of quantum gravity: even better than loops, because they form an orthonormal basis! Actually, it was mainly me who proved in a really rigorous way that they form an orthonormal basis, but Rovelli and Smolin had already been doing calculations using this idea for a while. One thing they computed was the eigenvalues of the observables in quantum gravity corresponding to the area of a surface in space, or the volume of a region.

Now there are all sorts of technical caveats and subtleties that I don't want to get into here, but in a really rough sort of sense, what their answers suggest is that IF the loop representation of quantum gravity is right, and we are on the right track about how it works, then the area of surfaces comes in certain (not integer, but discrete) multiples of the Planck length squared, and the volume of regions comes in multiples of the Planck length cubed. Note: that was a big "IF". This is especially interesting because it doesn't arise by assuming from the start that spacetime has a discrete structure. In fact, their computations assume spacetime is a continuous manifold. Nonetheless this discreteness pops out. It's not completely surprising: after all, Schrodinger's equation for the hydrogen atom is a perfectly "continuous" sort of thing, a partial differential equation, but the energy of the bound states winds up being a discrete sort of thing. Still, it's sort of exciting and new.

An interesting thing happened at the conference. Renate Loll, who works on the loop representation of gauge theories and also lattice gauge theory, has recently developed a lattice formulation of quantum gravity closely modelled after the loop representation:

2) Renate Loll, "Nonperturbative solutions for lattice quantum gravity", preprint available as `gr-qc/9502006`.

This has the wonderful feature that it's perfectly rigorous and also one can start using computers to start calculating things with it. For example, the most subtle aspect of the loop representation of quantum gravity is the Wheeler-DeWitt equation

$$H\psi = 0$$

where H is an operator called the "Hamiltonian constraint". More on this later; my point here is just that physical states of quantum gravity need to satisfy this equation. Getting H to be well-defined is tricky when space is a continuum, but in Loll's lattice version of theory (which is an approximation to the full continuum theory) she has already done this, so one can now start trying numerically to find solutions and see what they look like. She has also found some explicit solutions.

Also, she did some work on the volume operator in her lattice approach, and came up with a result in contradiction to Rovelli and Smolin's paper on the subject (cited in "Week 43"). They had said that states corresponding to trivalent spin networks — spin networks with only 3 edges at each vertex — could have nonzero volume. But using her version of the theory she computed that trivalent states — states with only 3 nonzero spins at the edges of the lattice incident to any vertex — all had zero volume, and that she needed at least 4 nonzero spins to get volume! The volume operator, in case you're wondering, acts as a certain sum over vertices: each one winds up contributing a certain finite amount of volume, which the theory allows you to compute.

This led to a whole lot of discussion and scribbling on the blackboards of the Banach center. I found it truly delightful to see all these physicists drawing pictures of spin networks and doing graphical computations just the way a knot theorist like Kauffman does all the time. It was as if the universe had this spin network aspect to it, and everyone was finally starting to catch on. Either that or mass delusion! I hadn't quite gotten the hang of how to compute these volume operators before, so it was a great chance to learn: one person would do a computation, then someone else would do it a different way and get a different answer, then someone else would do it yet another way and get yet another answer, and so on, so you could ask lots of questions without seeming too dumb. Even I did a computation after a while, and got zero volume for at least a certain class of trivalent vertices. The votes in favor of trivalent vertices having zero volume kept piling up. Finally Smolin noticed that he and Rovelli had made a sign mistake. This is incredibly easy to do, since there are lots of tricky sign conventions in spin network theory. Fundamentally these are due to the fact that spin-$1/2$ particles are fermions... but I don't think people fully understand the physical implications of this. (There is also a marvelous category-theoretic explanation of it, but I fear that if I go into that all the physicists will stop reading. Maybe some other time.) Rovelli and Smolin got pretty depressed about this for a while, but I tried to reassure them that only people who write really interesting papers ever get anybody to find the mistakes.

So perhaps we know a little more about the meaning of volume in a quantum theory of spacetime.

Spin networks are very beautiful and simple things. To learn about them, in addition to the various papers listed in the "weeks" above, one can now turn to Rovelli and Smolin's paper:

3) C. Rovelli and L. Smolin, "Spin networks in quantum gravity", preprint available in LaTeX form as `gr/qc-9505006`.

If you are more of a mathematician, or less of an expert on quantum gravity, you might also try a review article I wrote about them, which starts with a quick summary of what the heck canonical quantum gravity is about, why it's hard to do, and why the loop representation seems to help:

4) J. Baez, "Spin networks in nonperturbative canonical quantum gravity", preprint available in LaTeX form as `gr-qg/9504036`, or via ftp from `math.ucr.edu`, as the file `net.tex` in the directory `baez`.

Now so far I have been trying to make things sound simple, but here I should point out that when one talks about "states of quantum gravity" there are at least three quite different things one might mean. This is because the loop representation follows Dirac's general philosophy of quantizing systems with constraints, with some extra twists here and there. As I've repeatedly explained (e.g. "Week 43"), Einstein's equation for general relativity has 10 components, and if you split spacetime up into space and time (more or less arbitrarily — there's no "best" way) 4 of these can be seen as constraints that the metric on space and its first time derivative must satisfy (at any given time), while the remaining 6 describe how the metric on space evolves in time (which makes sense, because the metric has 6 components). When you follow Dirac's procedure for quantizing the equations what you do is this. First you forget about the constraint and get a big space of states, the "kinematical state space". There are lots of mathematical choices involved here, but Ashtekar and Lewandowski came up with a particular nice way of doing this rigorously, and one calls this space of states "L^2 of the space of $\mathrm{SU}(2)$ connections modulo gauge transformations with respect to the Ashtekar-Lewandowski generalized measure". Spin networks form an orthonormal basis of this Hilbert space. All the stuff about area and volume operators above refers to operators on this space.

Then, however, you need to deal with the constraints. Now 3 of the 4 constraints simply amount to requiring that your states be invariant under all diffeomorphisms of space, so these are usually dealt with first, and called the "diffeomorphism constraint". Imposing these constraints are a bit tricky; naively one would first guess that this "diffeomorphism- invariant state space" is just a subspace of the original kinematical state space, but actually it's not quite so simple. In any event, there are also spin network states at the diffeomorphism-invariant level, corresponding not to *particular* embeddings of graphs in space, but to diffeomorphism equivalence classes thereof. This again has been used by Rovelli, Smolin and others for a while now, but it was first rigorously shown in the following paper:

5) Abhay Ashtekar, Jerzy Lewandowski, Don Marolf, Jose Mourao, and Thomas Thiemann, "Quantization of diffeomorphism invariant theories of connections with local degrees of freedom", to appear in the November 1995 *Jour. Math. Phys.* special issue on diffeomorphism-invariant field theory, preprint available as `gr-qc/9504018`.

This paper is nice in part because it doesn't assume you already have read every previous paper about this stuff; instead, it describes the general plan of the loop representation before constructing the diffeomorphism- invariant spin network states. Also, buried in an appendix somewhere, it gives nice conceptual formulas for the area and volume operators, which serve as a complement to Rovelli and Smolin's explicit computations of their matrix elements in terms of the spin network basis.

Anyway, after taking care of the diffeomorphism constraint, one finally needs to take care of the Hamiltonian constraint, meaning one needs to find states satisfying the Wheeler-DeWitt equation. This is the hardest thing to make rigorous, and the most exciting aspect of the whole subject, because it expresses the fact that "physical states" of quantum gravity are invariant under diffeomorphisms of space-TIME, not just space. There is much more to say about this, but I won't go into it here.

Now besides Loll and Rovelli and Smolin, all the authors of the above paper except Mourao were at the conference in Warsaw, so there was a large contingent of spin network fans around, not even counting some other folks whose work I will get to in a while. This is why I was so eager to go there, especially because my own talk was on a rather esoteric subject which only these experts could be expected to give a darn about. Namely. . . .

The breakthrough of Ashtekar and Lewandowski, when it came to making the loop representation rigorous, involved working with piecewise real-analytic loops rather than piecewise smooth loops. (Actually Penrose suggested this idea.) This is because piecewise smooth loops can intersect in crazy ways, like in a Cantor set, which nobody could figure out how to handle. But the price of this breakthrough was that one had to assume the 3-manifold representing space was real-analytic, and things then only work nicely for real-analytic diffeomorphisms, as opposed to smooth ones. This always bugged me, so I have been working away for about a year trying to deal with smooth loops, and finally I got smart and teamed up with Steve Sawin, and we recently figured out how to get things to work with smooth loops (at least a bunch of things, like the Ashtekar-Lewandowski generalized measure). Our paper will be out pretty soon, but for now anyone who wants a taste of the mathematical technology involved should look at:

6) Steve Sawin, "Path integration in two-dimensional topological quantum field theory", to appear in the October 1995 *Jour. Math. Phys.* issue on diffeomorphism-invariant field theory, preprint available as `gr/qc-9505040`.

Loop representation ideas are applicable not only to canonical quantum gravity but also to path integrals in gauge theory, because in both cases one is doing integrals over a space of connections mod gauge transformations. Here Sawin uses them to give a rigorous formulation of 2d TQFTs in terms of path integrals. There aren't many unitary 2d TQFTs, and all of them are isomorphic to 2-dimensional quantum gravity with the usual Einstein-Hilbert action, with different values of the coupling constant, or else direct sums of such theories.

Next "week" I'll talk about cool new idea Smolin has about TQFTs, quantum gravity, and Bekenstein's bound on the entropy of a physical system in terms of its surface area.

Week 56

June 16, 1995

I got a copy of the following paper when I showed up in Warsaw:

1) Lee Smolin, "Linking topological quantum field theory and nonperturbative quantum gravity", available as gr-qc/9505028.

and then I spent a fair amount of time reading it and thinking about it throughout the conference. If the big hypothesis formulated in this paper is correct, I think we are on the verge of having a really beautiful theory of 4-dimensional quantum gravity, at least given certain boundary conditions. Mind you, I just mean a really beautiful theory, not necessarily a physically correct theory. But beautiful theories have a certain tendency to be right, or at least close, so let me explain this hypothesis.

First of all, we have to remember that Ashtekar reformulated Einstein's equation so that the configuration space for general relativity on the spacetime $\mathbb{R}\times S$, instead of being the space of *metrics* on a 3-manifold S, is a space of *connections* on S. A connection is just what a physicist often calls a vector potential, but for any old gauge theory, not just electromagnetism. Different gauge theories have different gauge groups, so I had better tell you the gauge group of Ashtekar's version of general relativity: it's $\mathrm{SL}(2,\mathbb{C})$, the group of 2×2 complex matrices with determinant equal to 1. And I should probably tell you which bundle over S we have an $\mathrm{SL}(2,\mathbb{C})$ connection on... but luckily, all $\mathrm{SL}(2,\mathbb{C})$ bundles over 3-manifolds are trivial, so I can cut corners by saying it's the trivial bundle. We can think of a connection A on the trivial $\mathrm{SL}(2,\mathbb{C})$ bundle over S as 1-forms taking values in the Lie algebra $\mathfrak{sl}(2,\mathbb{C})$, consisting of 2×2 complex matrices with trace zero.

Okay, so naively you might think a state in the *quantum* version of general relativity a la Ashtekar is just a wavefunction $\psi(A)$. That's not too far wrong and I won't bother about certain nitpicky technicalities here (again, for the full story try net.tex). But there's one very important catch I can't ignore: general relativity has *constraint* equations, meaning that ψ has to satisfy some equations. The first constraint, the Gauss law, just says that we must have

$$\psi(A) = \psi(A')$$

whenever A' is the result of doing a gauge transformation to A. Or at the very least, this should hold up to a phase; the point is that ψ is only supposed to record physically significant information about the state of the universe, and two connections are physically equivalent if they differ by a gauge transformation. The second constraint, the diffeomorphism constraint, says we need to have

$$\psi(A) = \psi(A')$$

when A' is the result of applying a diffeomorphism of space, S, to A. Again, the point is that two solutions of general relativity are physically the same if they differ only by a coordinate transformation, or — *roughly* the same thing — a diffeomorphism. The third constraint is the real killer. It's meaning is that $\psi(A)$ doesn't change when we do a diffeomorphism of spaceTIME to the connection A, but it's usually formulated 'infinitesimally' as the Wheeler-DeWitt equation

$$H\psi = 0$$

meaning roughly that the time derivative of ψ is zero. Think of it as a screwy quantum gravity version of Schrodinger's equation, where $d\psi/dt$ had better be zero!

It's hard to find explicit solutions of these equations. Indeed, it's hard to know what the heck these equations *mean* in a sufficiently precise way to recognize a solution if we found one! However, things were even worse back in the old days. Back in the old days when we thought of states as wavefunctions on the space of metrics, we didn't know ANY solutions of these equations. But nowadays we are very happy, because we know infinitely many times as many solutions! To be precise, we now know ONE solution. This is called the Chern-Simons state, and it was discovered by Kodama:

2) H. Kodama, "Holomorphic wavefunction of the universe", *Phys. Rev.* **D42** (1990), 2548–2565.

Now actually people have proposed other explicit solutions, but personally I have certain worries about all those other solutions, and I am not alone in this, whereas everyone seems to agree that, no matter how you slice it, the Chern-Simons state is a solution.

Now there is a slight catch: the Chern-Simons state is a solution of quantum gravity *with cosmological constant*. This is an extra term that Einstein threw into his equations so that they wouldn't make the obviously ridiculous prediction that the universe is expanding. When Hubble took a look and saw galactic redshifts all over, Einstein called this extra term the biggest blunder in his life. That kind of remark, coming from that kind of person, might make you a little bit reluctant to get too excited about having found a state of quantum gravity with this extra term thrown in! Luckily it turns out that you can take the limit as the cosmological constant goes to zero, and get a state of the theory where the cosmological constant is zero. I like to call this the 'flat state', because it's zero except where the connection A is flat.

(In fact, if the space S is not simply connected, there are lots of different flat states, because there is what experts call a moduli space of flat connections, i.e., lots of different flat connections modulo gauge transformations. Not many people talk too much about these flat states, but I do so in my paper `net.tex` and also the harder one `knot.tex`.)

Now what is this Chern-Simons state? Well, there is a wonderful thing you can compute from a connection A on a (compact oriented) 3-manifold S, called the Chern-Simons action:

$$CS(A) = \int_S \mathrm{tr}(A \wedge dA + (2/3)A \wedge A \wedge A)$$

which looks weird when you first see it, but gradually starts seeming very sensible and nice. The reason why folks like it is that it doesn't change when you do a small gauge transformation — i.e., one you can get to following a continuous path from the identity — and it changes only by an integral multiple of $8\pi^2$ if you do a large gauge transformation. Plus, it's diffeomorphism-invariant. It's incredibly hard to write down many functions of A with these properties, so they are precious. There are deeper reasons why it's so nice, but let's leave it at that for now.

So, the Chern-Simons state is

$$\psi(A) = \exp(-6CS(A)/\Lambda)$$

where Λ is the cosmological constant. Don't worry about the factor of 6 too much; depending on how you set up various things you might get different numbers, and I can never keep certain factors of 2 straight in this particular calculation. Note however that it looks as if things go completely haywire as Λ approaches zero, which is why my earlier remark about the 'flat state' is a bit nontrivial.

Why does this satisfy the constraints? Well, I just said above that the Chern-Simons action was hand-tailored to have the gauge-invariance and diffeomorphism-invariance we want, so the only big surprise is that we *also* have a solution of the Wheeler-DeWitt equation. Well, we do: a two-line computation shows it.

But clearly nature, or at least the goddess of mathematics, is trying to tell us something if this Chern-Simons state, which has all sorts of wonderful properties relating to *3-dimensional* geometry, is also a solution of the Wheeler-DeWitt equation, which is all about *4-dimensional* geometry, since it expresses the invariance of ψ under evolution in TIME. I have been thinking about this for several years now and I think I finally really understand it. There are probably people out there to whom it's perfectly obvious, because it's not really all that complicated, but unfortunately none of these people has ever explained it. Let me briefly say, for those who know about such things, that it all comes down to the fact that the Chern-Simons action was *born* as a 3-dimensional spinoff of a 4-dimensional thing called the 2nd Chern class. (If you want more details, I explained this as well as I could at the time in `knot.tex`.)

What is the physical meaning of the Chern-Simons state? As far as I know Kodama's paper hasn't been vastly surpassed in explaining this. He shows that in the classical limit this state becomes something called the anti-deSitter universe, a solution of Einstein's equation describing a (roughly) exponentially expanding universe. If you are wondering what this has to do with Einstein's introduction of the constant to KEEP the universe from expanding, let me just say this. In our current big bang theory the universe is expanding, but the presence of matter, or any sort of positive energy density, tends to pull it back in, and if there is enough matter it'll collapse again. Einstein's stuck in a cosmological constant term to give the vacuum some negative energy density, exactly enough to counteract the positive energy density of matter, so things would neither collapse nor expand, but instead remain in an (unstable, alas) equilibrium. In the deSitter universe there's no matter, just a cosmological constant of the opposite sign, so that the vacuum has positive energy density. In the anti-deSitter universe (invented by deSitter's nemesis anti-deSitter) there's no matter either, but the cosmological constant has the sign giving the vacuum negative energy density, which pushes the universe to keep expanding faster and faster.

Now in addition to this physical interpretation, the Chern-Simons state also has some remarkable relationships to knot theory, which were discovered by Witten and, since then, studied intensively by lots of people. I have written a lot in This Week's Finds about this! But briefly, there should be an invariant of knots and links associated to any state of quantum gravity, and the one associated to the Chern-Simons state is the Kauffman bracket, a close relative of the Jones polynomial, which is distinguished by having a very simple, beautiful definition, and also lots of wonderful relationships to an algebraic structure, the quantum group $SU_q(2)$. I should add that in addition to an invariant of knots and links, a state of quantum gravity should also give an invariant of *spin networks*, and indeed the Kauffman bracket extends to a wonderful invariant of spin networks. One can read about this in many places, but perhaps the most detailed account is Kauffman and Lins' book referred to in "Week 30".

So the question arises: are all these wonderful features of the Chern-Simons state of quantum gravity very special things that tell us very little about quantum gravity in general, or are they important clues that, if we understood them, would reveal a lot about the nature of *all* states of quantum gravity?

Of course, everyone who has fallen in love with the beauty of Chern-Simons theory would *like* the answer to be the latter. Louis Crane, for example, is deeply convinced that Chern-Simons theory is indeed the key to the whole business. He has written many papers on the subject, most of which I've gone over in earlier Finds, and also one brand new one, which is actually a very readable introduction to the grand scheme he has in mind:

3) Louis Crane: "Clock and category: is quantum gravity algebraic?", to appear in the November 1995 special issue of *Jour. Math. Phys.* on diffeomorphism-invariant physics, preprint available as gr-qc/9504038.

This grand scheme involves a thorough refashioning of quantum gravity in terms of category theory, and uses some of the very beautiful mathematics of n-categories, but neglecting all these subtleties, let us simply say that he argues that if the universe is IN the Chern-Simons state, there is no need to understand any other states! He doesn't really think all there is in the universe is gravity, of course, so he envisages a souped-up theory containing other forces and particles, but he argues that a generalization of quantum gravity to include all these other forces and particles will still have a special state, and that that's the state of the universe.

Being a conservative fellow myself, I prefer to remain agnostic on this issue, but I did write a paper showing how, if you assumed that space, the manifold above I called S, is a 3-dimensional sphere — a so-called S^3 — then if quantum gravity was in the Chern-Simons state, there would be very nice Hilbert spaces of "relative states" on each "half" of space. The relative state notion is often associated with Everett, who made a big deal out of the fact that, even if a two-part system was in a single state (a "pure state" in the language of quantum theory), each part could be regarded as being in a probabilistic mixture of lots of states (a "mixed state"). Whether or not you like the "many-worlds interpretation" of quantum theory which Everett's thesis spawned, it is true that a single pure state on a two-part system specifies a whole *space* of states on each half. So my idea was to take S^3, arbitrarily split it into two balls (D^3's in math jargon), and work out the space of states on each half. If you want to wax rhapsodic of this you can call one half the "observer" and the other the "observed", though it's crucial that there is a symmetry interchanging the two — there's not any way to tell them apart.

On the more technical side, there is a lot of nice (though well- understood) knot theory involved. For example, a special property of the quantum group $SU_q(2)$ — called the "positivity of the Markov trace", and discovered by Jones of Jones polynomial fame - equips each space of states with an inner product, even in this situation where we have no idea of an inner product to begin with. This paper is:

4) John Baez, "Quantum gravity and the algebra of tangles", *Jour. Class. Quant. Grav.* **10** (1993), 673–694, also available (without the all-important pictures!) as tang.tex.

So what has Lee Smolin done? Actually I have spent so much time leading up to it that now I'm too tired to do it justice! So I'll explain it next time. But let me just say, in order to tantalize you into tuning in to the next episode, that he carefully analyzes quantum gravity on a ball, imposing boundary conditions that let you see why relative states of Chern-Simons theory give states of quantum gravity. And then he makes the hypothesis that I mentioned at the beginning of this article. This is that *all* states of quantum gravity with these boundary conditions come from relative states of Chern-Simons theory. He even gives some good evidence for this hypothesis, coming originally from Hawking's work on the thermal radiation produced by black holes! (To be continued.)

Week 57

July 3, 1995

This week I'll start by finishing up my introduction to the following paper:

1) Lee Smolin, "Linking topological quantum field theory and nonperturbative quantum gravity", available as gr-qc/9505028.

So: recall where we were. Let me not repeat the details, but simply note that we were playing around with quantum gravity on a 4-dimensional spacetime, using the Ashtekar 'new variables' formalism, and we'd noticed that in the theory with nonzero cosmological constant Λ, there is an explicit solution of the theory, the 'Chern-Simons' state. Actually I hadn't really shown that this state satisfies the key equation, the Wheeler-DeWitt equation, but if you know the formulas it's easy to check.

Now one might think that one solution isn't all that much, apart from it being a whole lot better than none, which was the situation before these discoveries. However, as I began to explain last time, one can get a whole slew of states if one takes as ones space S, not a closed 3-dimensional manifold (as we were doing at first) but a 3-manifold with boundary. This is where Lee Smolin starts. He considers quantum gravity with certain 'self-dual boundary conditions' that are specially compatible with Chern-Simons theory.

There is presumably an enormous space of states of quantum gravity satisfying these boundary conditions, although we don't really know what they look like. Say we want to understand these states as much as possible. What do they look like? Well, first of all, the loop representation gives us a nice picture of the 'kinematical states' — i.e., states not necessarily satisfying the diffeomorphism constraint or the Wheeler-DeWitt equation. (This picture may be wrong, of course, but let me throw caution to the winds and just explain the picture.) Every kinematical state is a linear combination of 'spin network states'. For more on spin networks, check out "Week 55" and the references in there, but let me remind you what spin networks look like in this case.

For simplicity and ease of visualization, you can pretend S is a ball, so its boundary is a sphere. Think of a spin network state as a graph embedded in this ball, possibly with some edges ending on the the boundary, with all the edges labelled by spins $j = 0, 1/2, 1, 3/2, \ldots$, and with the vertices labelled by some extra numbers that we won't worry about here. Let's call the points where edges end on the boundary 'punctures', because the idea is that they really poke through the boundary and keep on going. Physically, these edges graph represent 'flux tubes of area': if we measure the area of some surface in this state (or at least a surface that doesn't intersect the vertices), the area is just the quantity

$$L^2\sqrt{j(j+1)}$$

summed over all edges that poke through the surface, where L is the Planck length and j is the spin labelling that edge. Gauge theories often have "flux tube" solutions when you quantize them: for example, type II superconductors admit flux tubes of the magnetic field, while superfluids admit flux tubes of angular momentum (vortices). The idea behind spin networks in quantum gravity, physically speaking, is that gravity is a gauge field which at the Planck scale is organized into branching flux tubes of area.

But we want to understand, not the kinematical states in general, but the actual physical states, which satisfy the diffeomorphism constraint and the Wheeler-DeWitt equation. We can start by measuring everything we measure by doing experiments right at the boundary of S. More precisely, we can try to find a maximal set of commuting observables that 'live on the boundary' in this sense. For example, the area of any patch of S counts as one of these observables, and all these 'surface patch area' observables commute. If we measure all of them, we know everything there is to know about the area of all regions on the boundary of S. Thanks to spin network technology, as described above, specifying all their eigenvalues amounts to specifying the location of a bunch of punctures on the boundary of S, together with the spins labelling the edges ending there.

Now Chern-Simons theory gives an obvious candidate for the space of physical states of quantum gravity for which these 'surface patch area' observables have specified eigenvalues. In fact, if you hand Chern-Simons theory a surface like the boundary of S, together with a bunch of punctures labelled by spins, it gives you a FINITE-DIMENSIONAL state space. Let's not explain just now how it gives you this state space; let's simply mumble that it gives you this space by virtue of being an 'extended topological quantum field theory.' If you want, you can think of these states as being the 'relative states' I discussed in last week's Finds, but not all of them: only those for which the 'surface patch area' observables have specified eigenvalues. There is a wonderfully simple combinatorial recipe for describing all these states in terms of spin networks living in S, having edges that end at the punctures, with the right spins at these ends.

Smolin's hypothesis is that this finite-dimensional space of states coming from Chern-Simons theory *is* the space of all physical states of quantum gravity on S that

1) satisfy the self-dual boundary conditions, and

2) have the specified values of the surface patch area observables.

Now if this hypothesis is true, it means we have a wonderfully simple description of all the physical states on S satisfying the self-dual boundary conditions!

So why should such a wonderful thing be true? I wish I knew! In fact, I'm busily trying to figure it out. Smolin doesn't give any direct evidence that it *is* true, so it might not be. But he does give some very interesting indirect evidence, coming from thermodynamics.

Thanks to work by Hawking, Bekenstein and others, there is a lot of evidence that if one takes quantum gravity into account, the maximal entropy of any system contained in a region with surface area A should be proportional to A. The basic idea is this. For various reasons, one expects that the entropy of a black hole is proportional to the area of its event horizon. For example, when you smash some black holes together it turns out that the total area of the event horizons goes up — this is called the 'second law of black hole thermodynamics'. This and many more fancy thought experiments suggest that when you have some black holes around the right notion of entropy should include a term proportional to the total area of their event horizons. Now suppose you had some other system which had even MORE entropy than this, but the same surface area. Then you could dump in extra matter until it became a black hole, which would therefore have less entropy, violating the second law.

This is a hand-waving argument, all right! It's not the sort of thing that would convince a mathematician. But it does suggest an intriguing connection between the vast literature on black hole thermodynamics and the more mathematical problem of relating quantum gravity and Chern-Simons theory.

Now the maximum entropy of a system is proportional to the logarithm of the total number of states it can assume. So if the 'Bekenstein bound' holds, the dimension of the space of states of a system contained in a region with surface area A is proportional to $\exp(A/c)$ for some constant c (which should be about the Planck length squared). Now the remarkable thing about Smolin's hypothesis is that if it's true, this is what one gets, because the dimension of the space given by Chern-Simons theory does grow like this.

There is another approach leading to this conclusion that the space of states of a bounded region should have dimensional proportional to $\exp(A/c)$, called the 't Hooft-Susskind holographic hypothesis. I was going to bone up on this for This Week's Finds, but I have been too busy! It's getting late and I'm getting bleary-eyed, so I'll stop here. I will simply give the references to this 'holographic hypothesis'; if anyone wants to explain it, please post to `sci.physics.research` — I'd be immensely grateful.

2) G 't Hooft, "Dimensional reduction in quantum gravity", preprint available as `gr-qc/9310006`.

3) L. Susskind, "The world as a hologram", to appear in the November 1995 special issue of *Jour. Math. Phys.* on diffeomorphism-invariant physics, preprint available as `hep-th/9409089`.

L. Susskind, "Strings, black holes and Lorentz contractions", preprint available as `hep-th/9308139`.

Note: in earlier Finds I referred to the October 1995 special issue of *Jour. Math. Phys.*, but now I've heard it's coming out in November.

Week 58

July 12, 1995

A few weeks ago I went to the IVth Porto Meeting on Knot Theory and Physics, to which I had been kindly invited by Jose Mourao. Quite a few of the (rather few) believers in the relevance of n-categories to physics were there. I spoke on higher-dimensional algebra and topological quantum field theory, and also a bit on spin networks. Louis Crane spoke on his ideas, especially the idea of getting 4-dimensional TQFTs out of state-sum models. And John Barrett spoke on

1) John Barrett, "Quantum gravity as topological quantum field theory", to appear in the November 1995 special issue of *Jour. Math. Physics*, also available as gr-qc/9506070.

This is a nice introduction to the concepts of topological quantum field theory (TQFT) that doesn't get bogged down in the (still substantial) technicalities. In particular, it pays more emphasis than usual to the physical interpretation of TQFTs, and how this meshes with more traditional issues in the interpretation of quantum mechanics. One of the main things I got out of the conference, in fact, was a sense that there is a budding field along these lines, just crying out to be developed. As Barrett notes, Atiyah's axioms for a TQFT can really be seen as coming from combining

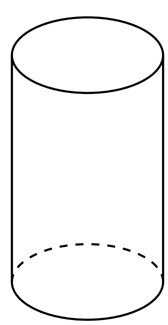
a) The rules of quantum mechanics for composing amplitudes

and

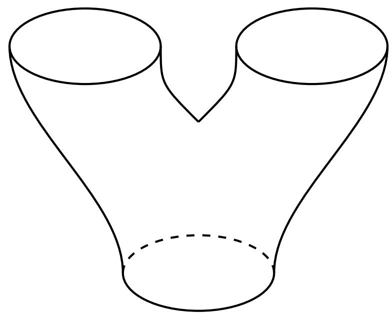
b) Functoriality, or the correct behavior under diffeomorphisms of manifolds.

Indeed, he convincingly recovers the TQFT axioms from these two principles. And of course these two principles could be roughly called "basic quantum mechanics" and "general covariance"... lending credence to the idea that whatever the theory of quantum gravity turns out to be, it should be something closely related to a TQFT. (I should emphasize, though, that this question is one of the big puzzles in the subject.)

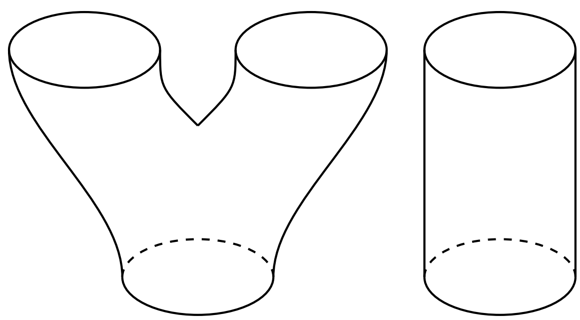
The richness inherent in b) makes the business of erecting a formalism to interpret topological quantum field theory much more interesting than the (by now) rather stale discussions that only treat a), or "basic quantum mechanics". In particular, in a TQFT, every way of combining manifolds — spaces or spacetimes — yields a corresponding rule for composing amplitudes. For example, if we have two spacetimes that look like



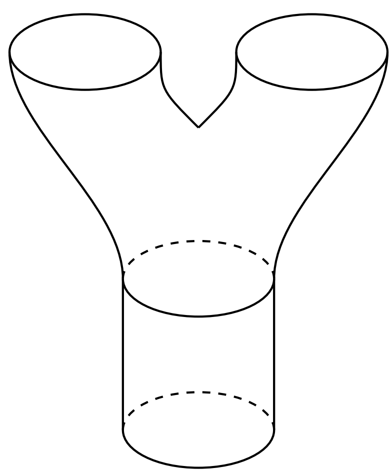
(that's supposed to look like a pipe!) and



— that is, a cylinder and a "trinion" (or upside-down pair of pants) — we can combine them either "horizontally" like this:



or "vertically" like this:



Corresponding to each spacetime we have a "time evolution operator" — a linear operator that describes how states going in one end pop out the other, "evolved in time". And corresponding to horizontal and vertical composition of spacetimes we have two ways to compose operators: horizontal composition usually being called "tensor product", and vertical composition being called simply "composition". These two ways satisfy some compatibility conditions, as well.

Now if one has read a bit about n-categories and/or "extended" topological quantum field theories, one already knows that this is just the tip of the iceberg. If we allow ourselves to cut spacetimes into smaller bits — e.g., pieces with "corners", such as tetrahedra or their higher-dimensional kin — one gets more possible ways of composing operators, and more compatibility conditions. These become algebraically rather sophisticated, but luckily, there is a huge amount of evidence that existing TQFTs extend to more sophisticated structures of this sort, through a miraculous harmony between algebra and topology.

This leads to some interesting new concepts when it comes to the physical interpretation of extended TQFTs. As Crane described in his talk (see also his papers listed in "Week 2", Week 23 and Week 56), in a 4-dimensional extended TQFT one expects the following sort of thing. If we think of an "observer" as a 3-manifold with boundary — imagine a person being the 3-manifold and his skin being the boundary, if one likes — the extended TQFT should assign to his boundary a "Hilbert category" or "2-Hilbert space". This is the categorical analog of a Hilbert space. In other words, just as a Hilbert space is a *set* in which you can *sum* things and *multiply* them by *complex numbers*, and get *complex numbers* by taking *inner products* of things, a 2-Hilbert space is an analogous structure in which every term surrounded by asterisks is replaced by its analog one step up the categorical ladder. This means:

$$\begin{aligned}
\text{set} &\to \text{category}\\
\text{sum} &\to \text{direct sum}\\
\text{multiply} &\to \text{tensor}\\
\text{complex numbers} &\to \text{vector spaces}\\
\text{inner products} &\to \text{homs}
\end{aligned}$$

There's a good chance that you know the analogy between numbers and vector spaces: just as you can add numbers and multiply them, you can take direct sums and tensor products of vector spaces, and many of the same rules still apply (in a somewhat more sophisticated form, because laws that were equations are now isomorphisms). A little less familiar is the analogy between inner products and "homs". Given two vectors v and w in a Hilbert space you can take the inner product $\langle v, w\rangle$ and get a number; similarly, given two (finite-dimensional) Hilbert spaces V and W you can form $\hom(V, W)$ — that is, the set of all linear maps from V to W — and get a Hilbert space. The same thing works in any "2-Hilbert space".

The most basic example of a 2-Hilbert space would be Hilb, the category of finite-dimensional Hilbert spaces, but also $\mathsf{Reps}(G)$, the category of finite-dimensional unitary representations of a finite group. (Similar remarks hold for quantum groups at root of unity.) Just as the inner product is linear in one argument and conjugate-linear in the other, "$\hom$" behaves nicely under direct sums in each argument, but each argument behaves a bit differently under tensor product, so one can say it's "linear" in one and "conjugate-linear" in the other.

So anyway, just as in a 4d TQFT a 3-manifold M determines a Hilbert space $Z(M)$, and a 4-manifold N with boundary equal to M determines a vector $Z(N)$ in $Z(M)$, something similar happens in an extended TQFT. (For experts, here I'm really talking about "unitary" TQFTs and extended TQFTs — these are the physically sensible ones.) Namely, a "skin of observation" or 2-manifold S determines a 2-Hilbert space $Z(S)$, and an "observer" or 3-manifold M with boundary equal to S determines an object in $Z(S)$. Now, given two observers M and M' with the same "skin" — for example, the observer "you" and the observer "everything in the world except you" — one gets two objects $Z(M)$ and $Z(M')$ in $Z(S)$, so one can form the "inner product" $\hom(Z(M), Z(M'))$, which is a Hilbert space. This is *your* Hilbert space for describing states of *everything in the world except you*. Note that we are using the term "observer" here in a somewhat whimsical sense; in particular, every region of space counts as an observer in this game, so we can flip things around and form the inner product $\hom(Z(M'), Z(M))$, which is the Hilbert space that *everything in the world except you* can use to describe states of *you*. These two Hilbert spaces, with roles reversed, are conjugate to each other (using an obvious but perhaps slightly unfamiliar definition of "conjugate" Hilbert space), so they're pretty much the same.

Now this may at first seem weird, but if you think about it, it becomes a bit less so. Of course, all of this stuff simply follows from the notion of a unitary extended TQFT, and whether the actual laws of physics are given by such a structure is a separate issue. But there is clearly a lot of relevance to the "holographic hypothesis" and Lee Smolin's more mathematical version of that hypothesis, as sketched in "Week 57". The basic idea, there as here, is that we are concentrating our attention on the things about a system that can be measured at its boundary, and what we measure there can be either thought of describing the state of the "inside" or dually the "outside".

I think if I go out on a limb here, and rhapsodize a bit, the point might be clearer: but don't take this too seriously. Namely: all of the stuff you see, hear, and otherwise observe about the world — which seems to be "information about the outside" — is also stuff going on in your brain, hence "information about the inside". What this stuff really is, of course, is *correlations* between the inside and the outside. This is the reason for the duality between observer and observed mentioned above. Note: we need not worry here whether or not there's "really" a lot more going on outside than what you observe. The point is simply that everything *you* observe about what's going on in the world outside is correlated to stuff that the world could observe about what is going on in you. (Maybe.)

I should perhaps also add that the mathematicians are getting a bit behind on the job of developing the "higher linear algebra" needed to support this sort of physics. So it's a bit hard to point to a good reference for all this 2-Hilbert space stuff. I'm slowly writing a paper on it, but for now the best sources seem to be Kapranov and Voevodsky's work on 2-vector spaces:

2) M. Kapranov and V. Voevodsky, "2-Categories and Zamolodchikov tetrahedra equations", in *Proc. Symp. Pure Math.* **56**, Part 2 (1994), AMS, Providence, pp. 177–260.

(see also "Week 4") Dan Freed's work on higher algebraic structures in gauge theory ("Week 12", "Week 48"), and David Yetter's new paper:

3) David Yetter, "Categorical linear algebra: a setting for questions from physics and low-dimensional topology", Kansas U. preprint, available as `http://math.ucr.edu/home/baez/yetter.pdf` and `http://math.ucr.edu/home/baez/yetter.ps`

This treats 2-vector spaces in a very beautiful way, but not 2-Hilbert spaces. Definitely worth reading for anyone interested in this sort of thing!

While visiting Porto, I managed somehow to miss talking to Eugenia Cesar de Sa, which was really a pity because she was the one who developed the way of describing 4-manifolds that Broda (see "Week 9", "Week 10") used to construct a 4-dimensional TQFT. This TQFT was later shown by Roberts (see "Week 14") to be isomorphic to that described by Crane and Yetter using a state sum model — i.e., by a discrete analog of a path integral in which one chops spacetime up into 4-dimensional "hypertetrahedra" (better known as 4-simplices!), labels their 2d and 3d faces by spins, and sums over labellings. Her work is cited in the Broda reference in "Week 17", but I managed luckily to get a copy of her thesis:

4) Eugenia Cesar de Sa, *Automorphisms of 3-manifolds and representations of 4-manifolds*, Ph.D. thesis, University of Warwick, 1977.

This should let me learn more about 4-dimensional topology, a fascinating subject on which I'm woefully ignorant.

One reason why Broda's work, and thus de Sa's, is interesting to me, is that people have suspected for a while that the Crane-Yetter-Broda theory, which is constructed purely combinatorially, is isomorphic to BF theory with cosmological term. BF theory (see "Week 36") is a 4-dimensional field theory that can be described starting from a Lagrangian in the traditional manner of physics. The theory "with cosmological term" can be regarded as a baby version of quantum gravity with nonzero cosmological constant, a baby version having only one state, the "Chern-Simons state". As I discussed in "Week 56", it's this Chern-Simons state that plays a key role in Smolin's attempt to "exactly solve" quantum gravity. Thus I suspect that BF theory is a good thing to understand really well. Recently I showed in

5) John Baez, "4-dimensional BF theory with cosmological term as a topological quantum field theory", available as q-alg/9507006.

that the Crane-Yetter-Broda theory is indeed isomorphic as a TQFT to a certain BF theory. With a bit more work, this should give us a state sum model for the BF theory that's a baby version of quantum gravity in 4 dimensions. This should come in handy for studying Smolin's hypothesis and its ramifications.

6) Timothy Porter, "TQFTs from homotopy n-types", University of Wales, Bangor preprint, available at `http://www.bangor.ac.uk/~mas013/preprint.html`

The Dijkgraaf-Witten model is an n-dimensional TQFT one gets from a finite group G. It's given by a really simple state sum model. Chop your manifold into simplices; then the allowed "states" are just labellings of the edges with elements of G subject to the constraint that the product around any triangle is 1. You can think of a labelling as a kind of "connection" that tells you how to parallel transport along the edges, and the constraint says the connection is flat. Expectation values of physical observables are then computed as sums over these states. In fact, this TQFT is a baby version of BF theory *without* cosmological constant. A toy model of a toy model of quantum gravity, in other words: the classical solutions of BF theory without cosmological constant are just flat connections on some G-bundle where G is a Lie group, while the Dijkgraaf-Witten model does something similar for a finite group.

In a previous paper (see "Week 54") Porter studied the Dijkgraaf-Witten model and a generalization of it due to Yetter that allows one to label faces with things too... one can think of this generalization as allowing "curvature", because the product of elements of G around a triangle need no longer be 1; instead, it's something determined by the labelling of the face.

7) David Yetter, "TQFTs from homotopy 2-types", *Journal of Knot Theory and its Ramifications* **2** (1993), 113–123.

In his new paper Porter takes this idea to its logical conclusion and constructs analogous theories that allow labellings of simplices in any dimension. Technically, the input data is no longer just a finite group, but a finite simplicial group G.

What's a simplicial group? It's a wonderful thing; using the "internalization" trick I've referred to in some previous Finds, all I need to say is that it's a simplicial object in the category of groups. A simplicial set is a bunch of sets, one for each natural number, together with a bunch of "face" and "degeneracy" maps satisfying the same laws that the face and degeneracy maps do for a simplex. (Students of singular or simplicial homology will know what I'm talking about.) Similarly, a simplicial group is a bunch of *groups*, together with a bunch of of "face" and "degeneracy" *homomorphisms* satisfying the same laws.

A triangulated manifold gives a simplicial set in an obvious way, and from any simplicial set one can obtain a simplicial groupoid (like a simplicial group, but with groupoids instead!) called its "loop groupoid". The sort of labellings Porter considers are homomorphisms from this simplicial groupoid to the given simplicial group G.

I will refrain from trying to say what all this has to do with homotopy n-types. Nonetheless, from a pure mathematics point of view, that's the most exciting aspect of the whole business! Part of the puzzle about TQFTs is their relation to traditional algebraic topology (and not-so-traditional algebraic topology like nonabelian cohomology, n-stacks, etc.), and this work serves as a big clue about that relationship.

Week 59

August 3, 1995

> As you crack your eyes one morning your reason is assaulted by a strange sight. Over your head, humming quietly, there floats a monitor, an ethereal otherworldly screen on which is written a curious message. "I am the Screen of ultimate Truth. I am bulging with information and ask nothing better than to be allowed to impart it."

It would be nice if more math books started with something attention-grabbing like this. In fact, it appears near the beginning of

1) Geoffrey M. Dixon, *Division Algebras: Octonions, Quaternions, Complex Numbers and the Algebraic Design of Physics*, Springer Verlag, 1994.

Dixon is convinced that the details of the Standard Model of particle interactions can be understood better by taking certain mathematical structures very seriously. There are very few algebras over the reals where we can divide by nonzero elements: if we demand associativity and commutativity, just the reals themselves and the complex numbers. If we drop the demand for commutativity, we also get a 4-dimensional algebra called the quaternions, invented by Hamilton. If in addition we drop the demand for associativity, and ask only that our algebra be "alternative", we also get an 8-dimensional algebra called the octonions, or Cayley numbers. (I'll say what "alternative" means in "Week 61") Clearly these are very special structures, and also clearly they play an important role in physics... or do they?

Well, few people doubt that the real numbers are fundamental to physics (though some advocates of the discrete might prefer the integers), and with emergence of quantum theory, if not sooner, the basic role of the complex numbers also became clear. Hamilton discovered the quaternions in the 1800s, and used them to formulate a beautiful theory of rotations in 3-dimensional space. They fell out of favor somewhat when the vectors of Gibbs proved simpler for many purposes, but their deeper importance became clear when people started studying spin: indeed, the Pauli matrices so important in physics are closely related to the quaternions, and it is the group of unit quaternions, $\mathrm{SU}(2)$, rather than the group of rotations in 3d space, $\mathrm{SO}(3)$, which turns out to be the symmetry group whose different representations correspond to particles of different spin. But what about the octonions?

Well, there are not too many places in physics yet where the octonions reach out and grab one with the force the reals, complexes, and quaternions do. But they are certainly out there, they have a certain beauty to them, and they are the natural stopping-point of a certain finite sequence of structures, so it is natural for people of a certain temperament to believe that they are there for a reason. Dixon makes a passionate case for this in the beginning of his book.

Suppose you were confronted with the Screen of Truth. What would you ask it? Dixon, being a physicist, naturally fantasizes asking it why the universe is the way it is! What kind of answer could this possibly have? Perhaps there is only one consistent way for things to be, and mathematics, with its unique and beautiful structures that are pure expressions of logical necessity, is trying to tell us something about this?

On the one hand this seems outrageous… especially to the hard-nosed pragmatist or empiricist in us. It seems old-fashioned, naive, and too good to be true. On the other hand, the universe *is* outrageous! It's outrageous that it exists in the first place, and doubly outrageous that it has the particular physical laws it does and no others. It has only been through the old-fashioned, naive belief that we can understand it using mathematics that we discovered what we have of its physical laws. So maybe eventually we *will* see that the basic structures of mathematics determine, in some mysterious sense, all the basic laws of physics. Or maybe we won't. In either case, there is a long way yet to go. As Dixon's Screen of Truth comments, before it departs:

> *"Do you believe that were I to explain as much of what I know as you" "could comprehend that you would recognize it, that you would say, oh" "yes, this is but an extension of what we have already done, and though" "the mathematics is broader, the principles deeper, I am not surprised?" "Do you think you have asked even a fraction of the questions you need" "to ask?"*

Anyway, it is at least worth considering all the beautiful mathematical structures one runs into for their potential importance. For example, the octonions.

In order to write this week's Finds, I needed to learn a little about the octonions. I wanted some good descriptions of the octonions, that hopefully would "explain" them or at least make them easy to remember. So I asked for help on sci.physics.research, and I got some help from Greg Kuperberg, Ezra Getzler, Matthew Wiener, and Alexander Vlasov. After a while Geoffrey Dixon got wind of this and referred me to his work! I'll probably talk to him later this summer when I go back to Cambridge Massachusetts, and hopefully I'll learn more about octonions and the like.

But for now let me just give a quick beginner's introduction to the octonions. A lot of this appears in

2) William Fulton and Joe Harris, *Representation Theory — a First Course*, Springer Verlag, Berlin, 1991.

I should add that this book is a very good place to learn about Lie groups, Lie algebras, and their representations… I wish I had taken a course based on this book when I was in grad school!

Let's start with the real numbers. Then the complex number

$$a + bi$$

can be thought of as a pair

$$(a, b)$$

of real numbers. Addition is done component-wise, and multiplication goes like this:

$$(a, b)(c, d) = (ac - db, da + bc)$$

We can also define the conjugate of a complex number by

$$(a, b)^* = (a, -b).$$

Now that we have the complex numbers, we can define the quaternions in a similar way. A quaternion can be thought of as a pair

$$(a, b)$$

of complex numbers. Addition is component-wise and multiplication goes like this

$$(a, b)(c, d) = (ac - d^*b, da + bc^*)$$

This is just like how we defined multiplication of complex numbers, but with a couple of conjugates (*'s) thrown in. To emphasize how similar the two multiplications are, we could have included the conjugates in the first formula, since the conjugate of a real number is just itself.

We can also define the conjugate of a quaternion by

$$(a, b)^* = (a^*, -b).$$

The game continues! Now we can define an octonion to be a pair of quaternions; as before, we add these component-wise and multiply them as follows:

$$(a, b)(c, d) = (ac - d^*b, da + bc^*).$$

One can also define the conjugate of an octonion by

$$(a, b)^* = (a^*, -b).$$

Why do the real numbers, complex numbers, quaternions and octonions have multiplicative inverses? I take it as obvious for the real numbers. For the complex numbers, you can check that

$$(a, b)^*(a, b) = (a, b)(a, b)^* = K(1, 0)$$

where K is a real number called the "norm squared" of (a, b). The multiplicative identity for the complex numbers is $(1, 0)$. This means that the multiplicative inverse of (a, b) is $(a, b)^*/K$. You can check that the same holds for the quaternions and octonions!

This game of getting new algebras from old is called the "Cayley-Dickson" construction. Of course, the fun we've had so far should make you want to keep playing this game and develop a 16-dimensional algebra, the "hexadecanions," consisting of pairs of octonions equipped with the same sort of multiplication law. What do you get? Why aren't there multiplicative inverses anymore? I haven't checked, because this is my summer vacation! I am learning about octonions just for fun, since I just finished writing some rather technical papers, and my idea of fun does not presently include multiplying two hexadecanions together to see why the norm-squared law $(a, b)(a, b)^* = (a, b)^*(a, b) = K(1, 0)$ breaks down. But I'm sure someone out there will enjoy doing this. . . and I'm sure someone else out there has already done it! So they should let me know what happens. There is something out there called "Pfister forms", which I think might be related.

[Toby Bartels did some nice work on hexadecanions in response to the above challenge, which appears at the end of this article.]

Now if we unravel the above definition of quaternions, by writing the quaternion $(a + bi, c + di)$ as $a + bi + cj + dk$, we see that the multiplication law is

$$i^2 = j^2 = k^2 = -1,$$

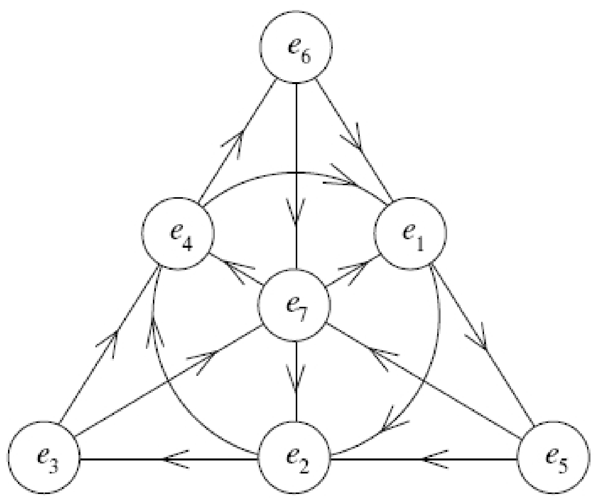
and

$$ij = -ji = k, \quad jk = -kj = i, \quad ki = -ik = j.$$

For more about the inner meaning of these rules, see "Week 5". Similarly, we can unravel the above definition of octonions by writing the octonion $(a + bi + cj + dk, e + fi + gj + hk)$ as

$$a + be_1 + ce_2 + de_3 + ee_4 + fe_5 + ge_6 + he_7.$$

Note: since mathematicians are very impersonal, they usually call these seven dwarves $e_1, \ldots, e_7$ instead of Sleepy, Grumpy, etc. as in the Disney movie. Any one of these 7 guys times himself is -1. Also, any two distinct ones anticommute; for example, $e_3e_7 = -e_7e_3$. There is a nice way to remember how to multiply them using the "Fano plane". This is a projective plane with 7 points, where by a "projective plane" I mean that any two points determine an abstract sort of "line", which in this case consists of just 3 points, and any two lines intersect in a point. It looks like this:



The "lines" are the 3 edges of the big triangle, the 3 lines going through a vertex, the center and the midpoint of the opposite edge, and the circle including e_1, e_2, and e_3. All the "lines" are cyclically ordered, and that tells you how to multiply the seven dwarves. For example, the line that's actually a circle goes clockwise, so $e_1e_2 = e_4$, $e_2e_4 = e_1$, and $e_4e_1 = e_2$. The lines that are edges of the big triangle also point clockwise, so for example $e_5e_2 = e_3$, and cyclic permutations thereof, and $e_6e_3 = e_4$. The lines that go through the center point from the vertex to the midpoint of the opposite edge, so for example $e_3e_7 = e_1$. I hope that made sense; you can work it out yourself, of course.

My convention for numbering the seven dwarves in the picture above is *completely arbitrary*, so don't bother remembering it — make up your own if you prefer! The convention I chose looks sort of weird at first, but it has a couple of endearing features:

- Index cycling: if $e_ie_j = e_k$, then $e_{i+1}e_{j+1} = e_{k+1}$.
- Index doubling: if $e_ie_j = e_k$, then $e_{2i}e_{2j} = e_{2k}$.

Here we add and multiply $\mod 7$. Index doubling corresponds to rotating the Fano plane.

So those are the octonions in a nutshell. I should say a bit about how they relate to triality for $\mathrm{SO}(8)$, the exceptional Lie group G_2, the group $\mathrm{SU}(3)$ which is so important in the study of the strong force, and to lattices like E_8, $\Lambda16$ and the Leech lattice. But I will postpone that; for now you can consult Fulton and Harris, and also various papers by Dixon:

3) Geoffrey Dixon, "Octonion X-product orbits", preprint available as hep-th/9410202.

"Octonion X-product and E_8 lattices", preprint available as hep-th/9411063.

"Octonions: E_8 lattice to $\Lambda16$", preprint available as hep-th/9501007.

"Octonions: invariant representation of the Leech lattice", preprint available as hep-th/9504040.

"Octonions: invariant Leech lattice exposed", preprint available as hep-th/9506080.

I am not presently in a position to assess these papers or Dixon's work relating division algebras and the Standard Model, but hopefully sometime I will be able to say a bit more.

Let me wrap up by saying a bit about the Leech lattice. As described in my review of Conway and Sloane's book ("Week 20", there is a wonderful branch of mathematics that studies the densest ways of packing spheres in n dimensions. Most of the results so far concern lattice packings, packings in which the centers of the spheres form a subset of $\mathbb{R}^n$ closed under addition and scalar multiplication by integers. When $n = 8$, the densest known packing is given by the so-called E_8 lattice. In "Week 20" I described how to get this lattice using the quaternions and the icosahedron. Briefly, it goes as follows. The group of rotational symmetries of the icosahedron (not counting reflections) is a subgroup of the rotation group $\mathrm{SO}(3)$ containing 60 elements. As mentioned above, $\mathrm{SO}(3)$ has as a double cover the group $\mathrm{SU}(2)$ of unit quaternions. So there is a 120-element subgroup of $\mathrm{SU}(2)$ consisting of elements that map to elements of $\mathrm{SO}(3)$ that are symmetries of the icosahedron. Now form all integer linear combinations of these 120 special elements of $\mathrm{SU}(2)$. We get a subring of the quaternions known as the "icosians''.

We can think of icosians as special quaternions, but we can also think of them as special vectors in $\mathbb{R}^8$, as follows. Every icosian is of the form

$$(a+\sqrt{5}b)+(c+\sqrt{5}d)i+(e+\sqrt{5}f)j+(g+\sqrt{5}h)k$$

with a, b, c, d, e, f, g, h rational — but not all rational values of $a, \ldots, h$ give icosians. The set of all vectors $x = (a, b, c, d, e, f, g, h)$ in $\mathbb{R}^8$ that correspond to icosians in this way is the E_8 lattice!

The Leech lattice is the densest known packing in 24 dimensions. It has all sorts of remarkable properties. Here is an easy way to get ones hands on it. First consider triples of icosians (x, y, z). Let L be the set of such triples with

$$x = y = z \mod h$$

and

$$x + y + z = 0 \mod h^*$$

where h is the quaternion $(-\sqrt{5}+i+j+k)/2$. Since we can think of an icosian as a vector in $\mathbb{R}^8$, we can think of L as a subset of $\mathbb{R}^{24}$. It is a lattice, and in fact, it's the Leech lattice! I have a bit more to say about the Leech lattice in "Week 20", but the real place to go for information on this beast is Conway and Sloane's book. It turns out to be related to all sorts of other "exceptional'' algebraic structures. People have found uses for many of these in string theory, so if string theory is right, maybe they are important in physics. Personally, I want to understand them more deeply as pure mathematics before worrying too much about their applications to physics.

Here is what Toby Bartels wrote:

> *From: Toby Bartels Subject: Re: why hexadecanions have no inverses To: John Baez Date: Sun, 20 Aug 1995*
>
> *I spent a couple days thinking about why hexadecanions have no inverses, and the first thing I want to say about it is that they do. However, these inverses are of limited applicability, because the hexadecanions are not a division algebra. A division algebra allows you to conclude, given $xy=0$, that x or y is 0. If your algebra has inverses, you might try to multiply this equation by the inverse of x or y (whichever one isn't 0) to prove the other is 0, but this only works if the algebra is associative. Since the octonions and hexadecanions aren't associative, there's no reason (yet) to think either of these is a division algebra. It turns out that the octonions are a division algebra, despite not being associative, but the hexadecanions aren't.*
>
> *Why aren't the hexadecanions a division algebra? Because the real numbers aren't of characteristic 2. Allow me to explain.*
>
> *I will prove below that the 2^n onions are a division algebra only if the 2^{n-1} onions are associative. So, the question becomes: why aren't the octonions associative? Well, I've found a proof that 2^n onions are associative only if 2^{n-1} onions are commutative. So, why aren't the quaternions commutative? Again, I have a proof that 2^n onions are commutative only if 2^{n-1} onions equal their own conjugates. So, why don't the complex numbers equal their own conjugates? I have a proof that 2^n onions do equal their own conjugates, but it works only if the 2^{n-1} onions are of characteristic 2. The real numbers are not of characteristic 2, so the complex numbers don't equal their own conjugates, so the quaternions aren't commutative, so the octonions aren't associative, so the hexadecanions aren't a division algebra.*
>
> *I require a few identities about conjugates that hold for all 2^n onions: $(x^*)^* = x$, $(x+y)^* = x^* + y^*$, and $(xy)^* = y^* x^*$. (If these identities are reminiscent of identities for transposes of matrices, it is no coincidence.) I will prove these by induction. That is, if an identity holds for 2^{n-1} onions, I show it holds for 2^n onions. Since they hold trivially for the reals ($n=0$), they hold for all.*
>
> $$((a,b)^*)^* = (a^*, -b)^* = ((a^*)^*, -(-b)).$$

By the induction hypothesis and the nature of addition (an Abelian group),

$$((a^*)^*, -(-b)) = (a, b).$$

$$((a, b) + (c, d))^* = (a + c, b + d)^* = ((a + c)^*, -(b + d)).$$

By the induction hypothesis and addition again,

$$((a+c)^*, -(b+d)) = (a^* + c^*, -b + -d) = (a^*, -b) + (c^*, -d) = (a, b)^* + (c, d)^*.$$

The next proof needs the distribution of multiplication over addition.

$$(a, b)((c, d) + (e, f)) = (a, b)(c+e, d+f) = (a(c+e) - (d+f)^* b, (d+f)a + b(c+e)^*).$$

By the induction hypothesis and the identity immediately above,

$$\begin{aligned} &(a(c + e) - (d + f)^* b, (d + f)a + b(c + e)^*) \\ &= (ac + ae - d^* b - f^* b, da + fa + bc^* + be^*) \\ &= (ac - d^* b, da + bc^*) + (ae - f^* b, fa + be^*) \\ &= (a, b)(c, d) + (a, b)(e, f). \end{aligned}$$

Also,

$$\begin{aligned} &((a, b) + (c, d))(e, f) \\ &= (a + c, b + d)(e, f) \\ &= ((a + c)e - f^*(b + d), f(a + c) + (b + d)e^*). \end{aligned}$$

By the induction hypothesis again,

$$\begin{aligned} &((a + c)e - f^*(b + d), f(a + c) + (b + d)e^*) \\ &= (ae + ce - f^* b - f^* d, fa + fc + be^* + de^*) \\ &= (ae - f^* b, fa + be^*) + (ce - f^* d, fc + de^*) \\ &= (a, b)(e, f) + (c, d)(e, f). \end{aligned}$$

$$((a, b)(c, d))^* = (ac - d^* b, da + bc^*)^* = ((ac - d^* b)^*, -(da + bc^*)).$$

Using the induction hypothesis and each of the above identities,

$$\begin{aligned} &((ac - d^* b)^*, -(da + bc^*)) \\ &= (c^* a^* - (-b)^*(-d), -da + (-b)c^*) \\ &= (c^*, -d)(a^*, -b) \\ &= (c, d)^*(a, b)^*. \end{aligned}$$

In light of the above identities, if I ever come across, say, $(xy^* + z)^*$*, I'll simply write* $yx^* + z^*$ *without a moment's hesitation.*

Since inductive proofs have been so useful, I'll use one to prove that 2^n *onions always have inverses. First, I'll extend the method in John's article, beginning with an inductive proof that* $xx^* = x^* x$ *is real.*

$$(a, b)(a, b)^* = (a, b)(a^*, -b) = (aa^* + b^* b, 0),$$

and

$$(a,b)^*(a,b) = (a^*,-b)(a,b) = (a^*a + b^*b, 0).$$

The inductive hypothesis states that both $a^*a = aa^*$ and b^*b are real, so $(a,b)(a,b)^* = (a,b)^*(a,b)$ is real. Since the sum of a positive real and a nonnegative real is positive, I can take this as a proof by induction that $xx^* = x^*x$ is not only real, but is also positive unless $x = 0$ (which will be important). All you have to do now is check that these things are true of the 2^0 onions, and they are, quite trivially (since the 2^0 onions are the reals).

Since the 2^n onions are always a vector space over the reals (as mentioned in John's article),

$$x(x^*/(xx^*)) = (xx^*)/(xx^*) = 1.$$

Since one can always divide by the real xx^*, the inverse of x is $x^*/(xx^*)$ in any 2^n onion algebra.

To continue with the streak of inductive proofs, I will now try to prove that the 2^n onions are always a division algebra. (I will fail.) Assume

$$0 = (0,0) = (a,b)(c,d) = (ac - d^*b, da + bc^*).$$

This gives the system of equations

$$ac - d^*b = 0 = da + bc^*.$$

Multiplying,

$$(ac)c^* - (d^*b)c^* = 0c^* = 0 = d^*0 = d^*(da) + d^*(bc^*).$$

If 2^{n-1} onions are associative, I can add the equations to get

$$a(cc^*) + (d^*d)a = 0.$$

Since cc^* and d^*d are real, they commute with a, and the division algebra nature of 2^{n-1} onions allows me to conclude that $cc^* + d^*d = 0$ (which implies $c = d = 0$ in light of positive definiteness) or that $a = 0$ (from which the original equation gives $b = 0$). Thus, the octonions are a division algebra (since the quaternions are associative), but the hexadecanions aren't (since the octonions aren't associative).

(If you're reading carefully, you realize that I haven't really proved that the hexadecanions aren't a division algebra. I've failed to prove that they are, but that's not the same thing. When I first wrote this, I wasn't reading carefully; I will return to plug this hole later.)

Thus, the 2^n onions are a division algebra iff the 2^{n-1} onions are a division algebra and are associative. So, let's try to prove associativity of 2^n onions by induction.

$$\begin{aligned} &((a,b)(c,d))(e,f) \\ &= (ac - d^*b, da + bc^*)(e,f) \\ &= ((ac - d^*b)e - f^*(da + bc^*), f(ac - d^*b) + (da + bc^*)e^*) \\ &= ((ac)e - (d^*b)e - f^*(da) - f^*(bc^*), f(ac) - f(d^*b) + (da)e^* + (bc^*)e^*). \end{aligned}$$

On the other hand,

$$\begin{aligned}&(a,b)((c,d)(e,f))\\&=(a,b)(ce-f^*d,fc+de^*)\\&=(a(ce-f^*d)-(fc+de^*)^*b,(fc+de^*)a+b(ce-f^*d)^*)\\&=(a(ce)-a(f^*d)-(c^*f^*)b-(ed^*)b,(fc)a+(de^*)a+b(e^*c^*)-b(d^*f)).\end{aligned}$$

Assuming the induction hypothesis that 2^{n-1} onions are associative, these are equal in general iff 2^{n-1} onions also are commutative.

Thus, 2^n onions are associative iff 2^{n-1} onions are associative and are commutative. So, let's try to prove commutativity of 2^n onions by induction.

$$(a,b)(c,d)=(ac-d^*b,da+bc^*).$$

On the other hand,

$$(c,d)(a,b)=(ca-b^*d,bc+da^*).$$

Assuming the induction hypothesis that 2^{n-1} onions are commutative, these are equal in general iff 2^{n-1} onions also equal their own conjugates.

Thus, 2^n onions are commutative iff 2^{n-1} onions are commutative and equal their own conjugates. So, let's try to prove conjugate equality of 2^n onions by induction.

$$(a,b)=(a,b).$$

On the other hand,

$$(a,b)^*=(a^*,-b).$$

Assuming the induction hypothesis that 2^{n-1} onions equal their own conjugates, these are equal in general iff 2^{n-1} onions also have characteristic 2. ($b=-b$ means $0=b+b=1b+1b=(1+1)b=2b$; this is true in general iff $0=2$, which is what characteristic 2 means.)

Thus, 2^n onions equal their own conjugates iff 2^{n-1} onions equal their own conjugates and have characteristic 2. Since the reals don't have characteristic 2, there's no point in trying to prove anything about that by induction. However, it's a general result that any algebra has characteristic 2 if it has a superalgebra of characteristic 2. Since the 2^n onions are all superalgebras of the reals (which means the reals are always isomorphic to a subset of the 2^n onions), none of the 2^n onions can have characteristic 2 if the reals don't.

In summary, the definition of the reals as the complete ordered field, along with an initial definition that $x^*=x$ in the reals, allows trivial proofs that: they form a division algebra, they are associative, they are commutative, and they equal their own conjugates, but they don't have characteristic 2. (All of these, in fact, are true of any ordered field with this definition of conjugate, complete or not.) From this and the above considerations, the complex numbers form a division algebra, are associative, and are commutative, but they neither equal their own conjugates nor have characteristic 2. From this, the quaternions form *a division algebra and are associative, but they neither are commutative, equal their own conjugates, nor have characteristic 2. From this, the octonions form a division algebra but they neither associative, are commutative, equal their own conjugates, nor have characteristic 2. Finally, the hexadecanions neither form a division algebra, are associative, are commutative, equal their own conjugates, nor have characteristic 2.*

At this point, I must return to the logical hole I mentioned earlier. But I want to work with a different algebraic concept than a division algebra; instead I will use (inspired by Doug Merrit's post to `sci.physics.research`*) what I guess is called 'alternativity', which says* $x(xy) = (xx)y$*. I don't like putting alternativity into the pattern, since associativity implies alternativity. All the other properties (commutativity, conjugate equality, characteristic) are logically independent in general. I'd like to prove that every associative* 2^n *onion algebra is alternative, just as I proved every commutative one was associative, without its having been obvious to begin with. Well, I will be disappointed even more badly later on.*

Taking the conjugate of $x(xy) = (xx)y$*,*

$$(y^*x^*)x^* = y^*(x^*x^*),$$

so left alternativity implies right alternativity, for 2^n *onions.*

I require an additional general identity of 2^n *onions. Earlier, I proved by induction that* xx^* *was real, but now I need the reality of* $x + x^*$*. Like everything else, this is proved by induction.*

$$(a, b) + (a, b)^* = (a, b) + (a^*, -b) = (a + a^*, 0).$$

Thus, if $a + a^*$ *is real,* $(a, b) + (a, b)^*$ *is real. Since* $x + x^*$ *is real when* x *is real,* $x + x^*$ *is real when* x *is any* 2^n *onion.*

Now suppose we're working in an alternative 2^n *onion algebra.*

$$x(xy) + x^*(xy) = (x + x^*)(xy).$$

Since $x + x^*$ *is real, it associates, so*

$$x(xy) + x^*(xy) = ((x + x^*)x)y = (xx)y + (x^*x)y.$$

Since $x(xy) = (xx)y$*,*

$$x^*(xy) = (x^*x)y,$$

which will be needed.

Let's attempt to prove by induction that 2^n *onions are always alternative.*

$$\begin{aligned} &(a, b)((a, b)(c, d)) \\ &= (a, b)(ac - d^*b, da + bc^*) \\ &= (a(ac - d^*b) - (da + bc^*)^*b, (da + bc^*)a + b(ac - d^*b)^*) \\ &= (a(ac) - a(d^*b) - (a^*d^*)b - (cb^*)b, (da)a + (bc^*)a + b(c^*a^*) - b(b^*d)). \end{aligned}$$

Meanwhile,

$$\begin{aligned}&((a,b)(a,b))(c,d)\\&=(aa-b^*b,ba+ba^*)(c,d)\\&=((aa)c-(b^*b)c-d^*(ba)-d^*(ba^*),d(aa)-d(b^*b)+(ba)c^*+(ba^*)c^*).\end{aligned}$$

These are indeed equal in general iff 2^{n-1} *onions are associative.*

The last sentence may not be immediately obvious. The induction hypothesis and its corollaries leave us with $x(yz)+(x^*y)z=y(zx)+y(zx^*)$ *as a necessary and sufficient condition. It may not be clear that associativity implies this, much less vice versa. However, the reality of* $x+x^*$ *once more enters the picture.*

$$y(zx)+y(zx^*)=y(z(x+x^*))=(x+x^*)(yz)=x(yz)+x^*(yz).$$

Thus, the condition becomes

$$x(yz)+(x^*y)z=x(yz)+x^*(yz),$$

which is equivalent, in the general case, to associativity.

To sum up the findings so far: For any n, the 2^n *onions form a vector space over the reals.* $x+x^*$ *and* xx^* *are real if* x *is any* 2^n *onion; additionally,* $xx^*=x^*x$. *Every* 2^n *onion has an inverse, which is a real multiple of its conjugate. Conjugation is analogous to matrix transposition in that*

$$(x^*)^*=x,(x+y)^*=x^*+y^*,and(xy)^*=y^*x^*.$$

Multiplication distributes over addition every time. For no n do all 2^n *onions equal their own negatives.* 2^{n+1} *onions equal their own conjugates iff* 2^n *onions equal their own conjugates and their own negatives. all* 2^{n+1} *onions commute iff all* 2^n *onions commute and equal their own conjugates.* 2^{n+1} *onions are associative iff* 2^n *onions are associative and commutative.* 2^{n+1} *onions are alternative iff* 2^n *onions are alternative and associative. The* 2^n *onions form a division algebra if they are alternative.*

I will be satisfied if I can prove the converse of the last statement. In light of the results about alternativity, my original attempt to prove that division of 2^n *onions requires associativity of* 2^{n-1} *onions looks even more convincing, (since alternativity of* 2^{n-1} *onions can be included in the induction hypothesis), but it's still not valid. I still haven't shown that, if* 2^{n-1} *onions aren't alternative, there must be non0* 2^n *onions* x *and* y *such that* $xy=0$. *There doesn't seem to be any reason why there shouldn't be, but there just might happen not to be any. So, despite the inelegance of it all, in order to prove that the hexadecanions aren't a division algebra, I'm forced to exhibit non-*0 x *and* y *such that* $xy=0$.

Just playing around, I found

$$\begin{aligned}&(e_1,e_4)(-1,e_5)\\&=(e_1(-1)-(e_5)*e_4,e_5e_1+e_4(-1)*)\\&=(-e_1+e_5e_4,e_5e_1-e_4).\end{aligned}$$

> *Since* $e_5e_4 = (0,i)(0,1) = (i,0) = e_1$ *and* $e_5e_1 = (0,i)(i,0) = (0,i*i) = (0,1) = e_4$*,*
> $$(e_1,e_4)(-1,e_5) = (0,0) = 0.$$
> *The* 2^n *onions can't be a division algebra if the* 2^{n-1} *onions aren't. If* $xy = 0$ *in the* 2^{n-1} *onions,* $(x,0)(y,0) = (xy,0) = (0,0) = 0$*. Thus, the octonions and below are the only* 2^n *onions to be division algebras. Still, I wish I had a proof of this that didn't require the ugly brute force use of a specific counterexample. (This is the interested reader's cue . . .)*
>
> *– Toby*

By the way, in a post to `sci.physics.research` on November 2, 1999, Ralph Hartley pointed out that even if we start with a field of characteristic 2, repeatedly applying the Cayley-Dickson construction will *not* lead to an infinite sequence of division algebras, because it's not true in this case that if x is nonzero, xx^* is nonzero. The problem is that a field of characteristic 2 can't be an ordered field.

Week 60

August 8, 1995

The end of a sabbatical is a somewhat sad affair... so many plans one had, and so few accomplished! As I pack my bags to return from Cambridge England to Cambridge Massachusetts, and then wing my way back to Riverside, I find I have quite a stack of preprints that I meant to include in This Week's Finds, but haven't gotten around to yet.

1) N. P. Landsman, "Rieffel induction as generalized quantum Marsden-Weinstein reduction", *Journal of Geometry and Physics* **15** (1995), 285–319.

Marsden-Weinstein reduction, also called symplectic reduction, is the modern way to deal with constraints in classical mechanics problems. It's a two-step procedure where first one looks at the subspace of your phase space on which the constraints vanish, and then a quotient of this by a certain equivalence relation. For example, if you have a particle in a plane, its phase space is $\mathbb{R}^4$, with coordinates (x, y, p_x, p_y) representing the x and y components of the position and the x and y components of the momentum. If we have a constraint $x = 0$, Marsden-Weinstein reduction tells us first to form the subspace of our phase space on which $x = 0$, and then quotient by the equivalence relation where two points are equivalent if they have the same value of p_x. We get down to $\mathbb{R}^2$, with coordinates (y, p_y). But Marsden- Weinstein reduction works in great generality and has become a basic part of the toolkit of mathematical physics.

What's the quantum analog of Marsden-Weinstein reduction? That's what this paper is about. Quantum mechanics in the presence of constraints is a tricky and important business, and there are lots of theories about how to do it. Gauge theories all have constraints, and so does general relativity... and in quantizing general relativity, the presence of constraints is what gives rise to the "problem of time". (See "Week 43" for more on this.) What attracted my attention to this paper is a two-stage procedure for dealing with contraints, quite analogous to Marsden-Weinstein reduction. This should shed some interesting light on the problem of time.

2) T. Ohtsuki, "Finite type invariants of integral homology 3-spheres", preprint, 1994.

   L. Rozansky, "The trivial connection contribution to Witten's invariant and finite type invariants of rational homology spheres", preprint available as q-alg/9505015.

   Stavros Garoufalidis, "On finite type 3-manifold invariants I", MIT preprint, 1995.

   Stavros Garoufalidis and Jerome Levine, "On finite type 3-manifold invariants II", MIT preprint, June 1995. (Garoufalidis is at stavros@math.mit.edu, and Levine is at levine@max.math.brandeis.edu.)

   Ruth J. Lawrence, "Asymptotic expansions of Witten-Reshetikhin-Turaev invariants for some simple 3-manifolds", to appear in *Jour. Math. Physics*.

Chern-Simons theory gives invariant of links in $\mathbb{R}^3$, which are functions of Planck's constant $\hbar$, and if one expands them as power series in h, the coefficients are link invariants with special properties, which one summarizes by calling them "Vassiliev invariants" or "invariants of finite type". (See "Week 3" for more.) But the partition function of Chern-Simons theory on a compact oriented 3-manifold is also interesting; it's an invariant of the 3-manifold defined for certain values of $\hbar$. (Often instead one thinks of it instead as a function of a quantity q, the limit $q \to 1$ corresponding to the limit $\hbar \to 0$.)

Recently people have studied the partition function of special 3-manifolds called homology spheres, which have the same homology as S^3. (People have looked at both integral and rational homology spheres.) After a bit of subtle fiddling, one can extract from the partition function of a homology sphere a power series in

$$\hbar' = q - 1,$$

and the coefficients of the powers of $\hbar'$ have been conjectured by Rozansky to have nice properties which one may summarize by calling them "finite type" invariants, in analogy to the link invariant case. (Namely, that they transform in nice ways under Dehn surgery.) For example, the coefficient of $\hbar'$ itself is 6 times the Casson invariant of the (integral) homology 3-sphere. So there appears to be a budding branch of "perturbative 3-manifold invariant theory". I just wish I understood better what's really going on behind all this!

3) Thomas Friedrich, "Neue Invarianten der 4-dimensionalen Mannigfaltigkeiten", Berlin preprint.

This is a nice introduction to the new Seiberg-Witten approach to Donaldson theory, which does not assume you already know the old stuff by heart. Very pretty mathematics!

4) Andre Joyal, Ross Street, and Dominic Verity, "Traced monoidal categories", to appear in *Math. Proc. Camb. Phil. Soc.*.

This is an abstract characterization of monoidal categories (categories with tensor products) which have a good notion of the "trace" of a morphism. Many abstract treatments of traces assume that your category is "rigid symmetric" or "balanced", meaning that your objects have duals and you can switch around objects in order to define the trace of a morphism $f\colon V \to V$ in a manner analogous to how one usually does it in linear algebra, as a certain composite:

$$I \to V \otimes V^* \xrightarrow{f \otimes 1} V \otimes V^* \to I$$

where I is the "unit object" for the tensor product (e.g. the complex numbers, when we're working in the category of vector spaces.) But one does not really need all this extra structure if all one wants is a good notion of "trace". This paper isolates the bare minimum. As one might expect if one knows the relation between knot theory and category theory, there are lots of nice pictures of tangles in this paper!

5) Michael Reisenberger, "Worldsheet formulations of gauge theories and gravity", University of Utrecht preprint, 1994, available as gr-qc/9412035.

The loop representation of a gauge theory describes states as linear combinations of loops in space, or more generally, "spin networks". What's the spacetime picture of which this is a spacelike slice? The obvious thing that comes to mind is a two-dimensional surface of some sort. I've advocated this point of view myself in an attempt to relate the loop representation of gravity to string theory (see "Week 18"). Here Reisenberger makes some progress in making this precise for some simpler theories analogous to gravity — for example, BF theory.

And now for some things I *did* manage to finish up on my sabbatical:

6) John Baez and Stephen Sawin, "Functional integration on spaces of connections", available as q-alg/9507023.

As I described in "Week 55", it's now possible to set up a rigorous version of the loop representation without assuming (as had earlier been required) that ones manifold is real-analytic and the loops are all analytic. This means that one can do things in a manner invariant under all diffeomorphisms, not just analytic ones. To achieve this, one needs to ponder rather carefully the complicated ways smooth paths, even embedded ones, can intersect (for example, they can intersect in a Cantor set).

7) John Baez, Javier P. Muniain and Dardo Piriz, "Quantum gravity hamiltonian for manifolds with boundary", available as gr-qc/9501016.

When space is a compact manifold with boundary, there is no Hamiltonian in quantum gravity, just a Hamiltonian constraint (see "Week 43"). This makes it tricky to understand time evolution in the theory — the "problem of time". But with a boundary, there is a Hamiltonian, given by a surface integral over the boundary. (The reason is that, at least when the equations of motion hold, the Hamiltonian is a total divergence, so you can use Gauss' theorem to express it as an integral over the boundary, which of course is zero when there is no boundary.)

Rovelli and Smolin (see "Week 42") worked out a loop representation of quantum gravity — in a heuristic sort of way which various slower sorts like myself have been struggling to make rigorous in the subsequent years — and a key step in this was expressing the Hamiltonian constraint in terms of loops. In this paper we do the same sort of thing for the Hamiltonian, when there is a boundary. This requires considering not only loops but also paths that start and end at the boundary.

Remarkably, the Hamiltonian acts on paths that start and end at the boundary in a manner very similar to the Hamiltonian constraint for quantum gravity coupled to massless chiral spinors (e.g. neutrinos, if neutrinos are really massless and have a "handedness" as they appear to). This suggests that on a manifold with boundary, the degrees of freedom "living on the boundary" are described by a chiral spinor field. Steve Carlip has already shown something very similar for quantum gravity in 2+1 dimensional spacetime, a more tractable simplified model — see "Week 41". Moreover, he used this to explain why the entropy of a black hole is proportional to its area (or length in 2+1 dimensions). The idea is that the entropy is really accounted for by the degrees of freedom of the event horizon itself. It would be nice to do something similar in 3+1-dimensional spacetime.

Week 61

August 24, 1995

I'd like to return to the theme of octonions, which I began to explore in "Week 59". The recipe I described there, which starts with the real numbers, and then builds up the complex numbers, quaternions, octonions, hexadecanions etc. by a recursive process, is called the "Cayley-Dickson process". Now let me describe a way to obtain the octonions using a special property of rotations in 8-dimensional space, called "triality". I'll start with a gentle introduction to the theory of rotation groups; for this, a nice reference is the book by Fulton and Harris that I mentioned in "Week 59". Then I will turn up the heat a bit and describe triality and how to use it to get the octonions. I learned some of this stuff from:

1) Alex J. Feingold, Igor B. Frenkel, and John F. X. Rees, *Spinor construction of vertex operator algebras, triality, and* $\mathrm{E}_8^{(1)}$, Contemp. Math. **121**, AMS, Providence Rhode Island.

I should emphasize, however, that what I will talk about is older, while the above book starts with triality and then does far more sophisticated things. An older reference for what I'll talk about is

2) Claude Chevalley, *The algebraic theory of spinors*, Columbia U. Press, New York, 1954.

I think the concept of triality goes back to Cartan, but I don't really know the history. By the way, I'd really appreciate any corrections to what I say below.

Okay, so, how should we start? Well, probably we should start with the group of rotations in n-dimensional Euclidean space. This group is called $\mathrm{SO}(n)$. It is not simply connected if $n > 1$, meaning that there are loops in it which cannot be continuously shrunk to a point. This is easy to see for $\mathrm{SO}(2)$, which is just the circle — or, if you prefer, the unit complex numbers. It's a bit trickier to see for $\mathrm{SO}(3)$, but it is easy enough to demonstrate — either mathematically or via the famous "belt trick" — that the loop consisting of a 360 degree rotation around an axis cannot be continuously shrunk to a point, while the loop consisting of a 720 degree rotation around an axis can.

This "doubly connected" property of $\mathrm{SO}(3)$ implies that it has an interesting "double cover", a group G in which all loops *can* be contracted to a point, together with a two-to-one function $F\colon G \to \mathrm{SO}(3)$ with $F(gh) = F(g)F(h)$. (This sort of function, the nice kind of function between groups, is called a "homomorphism".) And this double cover G is just $\mathrm{SU}(2)$, the group of 2×2 complex matrices which are unitary and have determinant 1. Better yet — if we are warming up for the octonions — we can think of $\mathrm{SU}(2)$ as the unit quaternions!

Now elements of $\mathrm{SO}(n)$ are just $n \times n$ real matrices which are orthogonal and have determinant 1, so given an element g of $\mathrm{SO}(n)$ and a vector v in $\mathbb{R}^n$, we can do matrix multiplication to get a new vector gv in $\mathbb{R}^n$, which of course is just the result of rotating v by the rotation g. This makes $\mathbb{R}^n$ into a "representation" of $\mathrm{SO}(n)$, meaning simply that

$$(gh)v = g(hv)$$

and

$$1v = v.$$

We call $\mathbb{R}^n$ the "vector" representation of the rotation group $\mathrm{SO}(n)$, for obvious reasons.

Now $\mathrm{SO}(n)$ has lots of other representations, too. If we consider $\mathrm{SO}(3)$, for example, there is in addition to the vector representation (which is 3-dimensional) also the trivial 1-dimensional representation (where the group element g acts on a complex number v by leaving it alone!) and also interesting representations of dimensions 5, 7, 9, etc.. The interesting representation of dimension $2j+1$ is called the "spin-j" representation by physicists. All representations of $\mathrm{SO}(3)$ can be built up from these representations, and none of these representations can be broken down into smaller ones — one says they are irreducible.

But the double cover of $\mathrm{SO}(3)$, namely $\mathrm{SU}(2)$, has more representations! Using the two-to-one homomorphism $F\colon \mathrm{SU}(2) \to \mathrm{SO}(3)$ we can convert any representation of $\mathrm{SO}(3)$ into one of $\mathrm{SU}(2)$, but not vice versa. For example, since $\mathrm{SU}(2)$ consists of 2×2 complex matrices, it has a representation on $\mathbb{C}^2$, given by the obvious matrix multiplication. This is called the "spinor" or "spin-$1/2$" representation of $\mathrm{SU}(2)$. It doesn't come from a representation of $\mathrm{SO}(3)$.

To digress a bit, the reason physicists got so interested in $\mathrm{SU}(2)$ is that to describe what happens when you rotate a particle (in the framework of quantum theory) it turns out you need, not just the representations of $\mathrm{SO}(3)$, but of its double cover, $\mathrm{SU}(2)$. E.g., an electron, proton or neutron is described by the spin-$1/2$ representation. This implies that when you turn an electron around 360 degrees about an axis, its wavefunction changes sign, but when you rotate it another 360 degrees, its wavefunction is back to where it started. You can't describe this behavior using representations of $\mathrm{SO}(3)$, but you can using $\mathrm{SU}(2)$. In general, for any $j = 0, 1/2, 1, 3/2, 2, \ldots$, there is an irreducible representation of $\mathrm{SU}(2)$, called the "spin-j" representation, which is $(2j+1)$-dimensional. Only when the spin is an integer does the representation come from one of $\mathrm{SO}(3)$.

Things get more complicated when we consider rotations in higher dimensional space. For any n greater than or equal to 3, the group $\mathrm{SO}(n)$ is doubly connected, and has a simply connected double cover, which in general is called $\mathrm{Spin}(n)$. Folks have figured out all the representations of $\mathrm{Spin}(n)$ and which of these come from representations of $\mathrm{SO}(n)$. It is more complicated for $n > 3$ than for $n = 3$ (in particular, they aren't just classified by "spin"), but it is still quite comprehensible and charming. Just to head off any confusions that might occur, let me emphasize that it's sort of a lucky coincidence that $\mathrm{Spin}(3) = \mathrm{SU}(2)$. In general, the spin groups don't have too much to do with the groups $\mathrm{SU}(n)$ of $n \times n$ unitary complex matrices with determinant 1.

There is, however, a doubly lucky coincidence in dimension 4; namely, $\mathrm{Spin}(4) = \mathrm{SU}(2) \times \mathrm{SU}(2)$. In other words, an element of $\mathrm{Spin}(4)$ can be thought of as a pair of $\mathrm{SU}(2)$ matrices, and we multiply these pairs like $(g, g')(h, h') = (gh, g'h')$. This implies that the irreducible representations of $\mathrm{Spin}(4)$ are given by a "tensor product" of two irreducible representations of $\mathrm{SU}(2)$, so we can classify them by pairs of spins, say (j, j'). The dimension of the (j, j') representation is $(2j+1)(2j'+1)$, since the dimension of a tensor product is the product of the dimensions. In particular, we call the $(1/2, 0)$ representation the "left-handed" spinor representation and the $(0, 1/2)$ representation the "right-handed" spinor representation. The reason is that a reflection transforms one into the other. Since spacetime is 4-dimensional, representations of $\mathrm{Spin}(4)$ are important in physics, although really one should keep track of the fact that time works a bit differently than space, which $\mathrm{Spin}(4)$ fails to do. In any event, ignoring the subtleties about how time works differently than space, we can roughly say that the existence of left-handed and right-handed spinor representations of $\mathrm{Spin}(4)$ is the reason why massless spin-$1/2$ particles such as neutrinos can have a "handedness" or "chirality".

More generally, it turns out that the representation theory of $\mathrm{Spin}(n)$ depends strongly on whether n is even or odd. When n is even (and bigger than 2), it turns out that $\mathrm{Spin}(n)$ has left-handed and right-handed spinor representations, each of dimension $2^{n/2-1}$. When n is odd there is just one spinor representation. Of course, there is always the representation of $\mathrm{Spin}(n)$ coming from the vector representation of $\mathrm{SO}(n)$, which is n-dimensional.

This leads to something very curious. If you are an ordinary 4-dimensional physicist you undoubtedly tend to think of spinors as "smaller" than vectors, since the spinor representations are 2-dimensional, while the vector representation is 3-dimensional. However, in general, when the dimension n of space (or spacetime) is even, the dimension of the spinor representations is $2^{n/2-1}$, while that of the vector representation is n, so after a while the spinor representation catches up with the vector representation and becomes bigger!

This is a little bit curious, or at least it may seem so at first, but what's *really* curious is what happens exactly when the spinor representation catches up with the vector representation. That's when $2^{n/2-1} = n$, or $n = 8$. The group $\mathrm{Spin}(8)$ has three 8-dimensional irreducible representations: the vector, left-handed spinor, and right-handed spinor representation. While they are not equivalent to each other, they are darn close; they are related by a symmetry of $\mathrm{Spin}(8)$ called "triality". And, to top it off, the octonions can be seen as a kind of spin-off of this triality symmetry... as one might have guessed, from all this 8-dimensional stuff. One can, in fact, describe the product of octonions in these terms.

So now let's dig in a bit deeper and describe how this triality business works. For this, unfortunately, I will need to assume some vague familiarity with exterior algebras, Clifford algebras, and their relation to the spin group. But we will have a fair amount of fun getting our hands on a 24-dimensional beast called the Chevalley algebra, which contains the vector and spinor representations of $\mathrm{Spin}(8)$!

Start with an 8-dimensional *complex* vector space V with a nondegenerate symmetric bilinear form on it. We can think of V as the representation of $\mathrm{SO}(8)$, hence $\mathrm{Spin}(8)$, where now I've switched notation and write $\mathrm{SO}(8)$ to mean $\mathrm{SO}(8,\mathbb{C})$, and $\mathrm{Spin}(8)$ to mean $\mathrm{Spin}(8,\mathbb{C})$. We can split V into two 4-dimensional subspaces V_+ and V_- such that $\langle v, w\rangle = 0$ whenever v and w are either both in V_+, or both in V_-. Let Cliff be the Clifford algebra over V. Note that as a vector space, there is a natural identification of Cliff with

$$\bigwedge V_+ \otimes \bigwedge V_-$$

where $\bigwedge$ means "exterior algebra" and $\otimes$ means "tensor product". (If you are physicist you may enjoy following Dirac and thinking of $\bigwedge V_+$ as a Fock space for "holes", and $\bigwedge V_-$ as a Fock space for "particles". If you don't enjoy this, ignore it! We will now to proceed to fill all the holes.) Pick a nonzero vector v in $\bigwedge^4 V_-$, the top exterior power of V_-. Let S denote the subspace of Cliff consisting of all elements of the form uv with u in Cliff. Note that Cliff and S are representations of Cliff by left multiplication, and therefore are representations of $\mathrm{Spin}(8)$ — because $\mathrm{Spin}(8)$ sits inside Cliff. (This is a standard way to get one's hands on the spin groups.)

Note that $\bigwedge V_+$ and $\bigwedge V_-$ both have dimension $2^4 = 16$. We can think of both of these as subspace of Cliff; for example, we can think of the vector u in $\bigwedge V_+$ as the vector $1 \otimes u$ in Cliff. Note that $uv = 0$ when u is in $\bigwedge V_+$. (For physicists: since the sea of holes is filled, you can't create another.) Thus S consists of vectors of the form uv where u lies in $\bigwedge V_-$, and if you think a bit, it follows that S is 16-dimensional.

So we have our hands on a 16-dimensional representation of $\mathrm{Spin}(8)$, namely S. However, we can split it into two 8-dimensional representations, the left- and right-handed spinor representations, as follows. Let

$$\bigwedge^{\text{even}} V_-$$

denote the part of the exterior algebra consisting of stuff with even degree, and

$$\bigwedge^{\text{odd}} V_-$$

the part with odd degree. Then we can write $S = S_+ \oplus S_-$, where $\oplus$ means "direct sum" and

$$S_+ = (\bigwedge^{even} V_-)v, \quad S_- = (\bigwedge^{odd} V_-)v.$$

Now, since any element of Cliff that's in $\mathrm{Spin}(8)$ has even degree in Cliff, and since even times even is even, while even times odd is odd, it follows that as a representation of $\mathrm{Spin}(8)$, S splits into S_+ and S_-, which we call the left-handed and right-handed spinors, respectively. (Actually I don't know which one is called which, but being left-handed myself I think the positive one should obviously be called the left-handed one.)

Note, by the way, that everything so far works quite generally for $\mathrm{Spin}(n)$ when n is even, and it's only in the last paragraph that I used the fact that n was even. I certainly haven't done anything weird using the fact that n is 8. So as a bonus we're learning some quite general stuff about spinors.

Now let's do something weird using the fact that n is 8. We've got these three 8-dimensional representations of $\mathrm{Spin}(8)$ on our hands, namely V, S_+, and S_-. How do they relate? Recall that $S_+ + S_- = S$ is a representation of Cliff, and since V sits inside Cliff as the elements of degree 1, we have for any a in V,

$$ab \text{ is in } S_- \text{ if } b \text{ is in } S_+$$

and

$$ab \text{ is in } S_+ \text{ if } a \text{ is in } S_-$$

If we are in the mood, this might tempt us to lump V, S_+, and S_- together to form a 24-dimensional algebra! Let's call this the Chevalley algebra and write

$$\mathrm{Chev} = V + S_+ + S_-$$

Of course, we need to figure out how to multiply any two guys in Chev. We define the product of any two guys in V to be zero, and ditto for S_+ or S_-. But we can find an interesting way to multiply a guy in S_+ by a guy in S_- to get a guy in V. I think the vector representation always sits inside the tensor product of the left- and right-handed spinor representations when space is even-dimensional, and that this is what we're looking for. But explicitly, here's what we do in this case. There is a kind of * operation on Cliff given by

$$(abc\ldots def)^* = fed\ldots cba$$

where $a, b, c, \ldots, d, e, f$ lie in V. This lets us define a symmetric bilinear form on S by

$$\langle x, y\rangle v = x^* y$$

Together with the symmetric bilinear form we started with on V, this gives us a symmetric bilinear form on all of $\mathrm{Chev}, defining\langle a, b\rangle$ to be 0 if a is in V and b is in S_+ or S_-. This bilinear form on Chev turns out to be nondegenerate, and $\langle a, b\rangle = 0$ whenever a and b lie in different ones of three summands of Chev.

So now Chev has a nondegenerate symmetric bilinear form it. This lets us define a cubic form on Chev! Say we have (a, b, c) in $V \oplus S_+ \oplus S_- = \mathrm{Chev}$. Then we define our cubic form F by

$$F(a, b, c) = \langle ab, c\rangle$$

using the fact that we already know how to multiply a guy in V with a guy in S_+, and get a guy in S_-.

You probably know — if you've survived this far! — that from a quadratic form you can get a symmetric bilinear form by "polarization". Well, similarly, we can get a symmetric trilinear form f on Chev by polarizing F. Explicitly, for any $u1, u2, u3$ in Chev, we have

$$f(u1, u2, u3) = F(u1+u2+u3) - F(u1+u2) - F(u2+u3) - F(u1+u3) + F(u1) + F(u2) + F(u3).$$

Then, since we have a nondegenerate symmetric bilinear form on Chev, we can turn f into a product on Chev, by setting

$$\langle u1u2, u3\rangle = f(u1, u2, u3).$$

The assiduous reader can check that this product on Chev agrees with the product we had partially defined so far; the only new thing it does is define the product of a guy in S_+ and a guy in S_-, obtaining something in V. This product turns out to be commutative, but not associative.

Now, if I were really gung-ho about describing triality, I would describe how the group of permutations of 3 letters, S_3, acts as automorphisms of Chev in a way that lets one scramble the summands V, S_+, and S_- at will. In fact, S_3 acts as automorphisms of $\mathrm{Spin}(8)$ in a way that gives rise to this action on Chev. But right now I'm running out of steam, so I think I'll just say how to get the octonions out of the Chevalley algebra!

It's simple: pick a vector v in V with $\langle v, v\rangle = 1$, and a vector s_+ in S_+ with $\langle s_+, s_+\rangle = 1$. Then $s_- = vs_+$ is a vector in S_- with $\langle s_-, s_-\rangle = 1$. We now turn V into the octonions as follows. Given v and w in V, define their octonion product v^*w to be

$$v^*w = (vs_-)(ws_+)$$

where the product on the right hand side is the product in the Chevalley algebra. In other words: take v and turn it into something in S_+ by forming vs_-. Take w and turn it into something in S_- by forming ws_+. The product of these is then something in V. In short, we form the octonions from the three 8-dimensional representations of $\mathrm{Spin}(8)$ by a kind of ring-around-the-rosie process using triality!

Note: what we just obtained was a *complex* 8-dimensional algebra, which is the complexification of the octonions. Using the fact that the vector representation of $\mathrm{SO}(8,\mathbb{C})$ on $\mathbb{C}^8$ contains the vector representation of $\mathrm{SO}(8,\mathbb{R})$ on $\mathbb{R}^8$ as a "real part", we should be able to get the octonions themselves.

One can work out the details following the book of Fulton and Harris, and the references therein. I should add that they do a lot more fun stuff involving the exceptional Lie groups and the 27-dimensional exceptional Jordan algebra. . . all of this "exceptional" stuff seems to form a unified whole! There is a lot more fun stuff along these lines in

3) Ian R. Porteous, *Topological Geometry*, Cambridge U. Press, Cambridge, 1981.

In particular, to correct a widespread misimpression, it's worth noting that there are a lot of nonassociative division algebras over the reals besides the octonions; Porteous describes one of dimension 4 in his book. However, all division algebras over $\mathbb{R}$ are of dimension 1,2,4, or 8. Also, all normed division algebras over $\mathbb{R}$ are the reals, complexes, quaternions, or octonions, and these are also all the alternative division algebras over $\mathbb{R}$, as well. . . where an "alternative" algebra is one for which any two elements generate an associative algebra. Nota bene: here a division algebra is one such that for all nonzero x, the map $y \mapsto xy$ is invertible. In the finite-dimensional case, this implies that every element has a left and right inverse. If assume associativity, the converse is true, but in the nonassociative case it ain't. Whew! Nonassociative algebras are tricky, if you're used to associative ones, so you're interested, you might try:

4) R. D. Schafer, *An Introduction to Non-Associative Algebras*, Dover, New York, 1995.

In addition to the people listed in "Week 59", I should thank Dan Asimov, Michael Kinyon, Frank Smith, and Dave Rusin for help with this post. I also thank Doug Merritt for reminding me about the following nice book on quaternions, octonions, and all sorts of similar algebras:

5) I. L. Kantor and A. S. *Solodovnikov, Hypercomplex Numbers — an Elementary Introduction to Algebras*, Springer-Verlag, Berlin, 1989; translation of "Giperkompleksnye chisla", Moscow, 1973.

Back in the old days when there weren't too many algebras around besides the reals, complexes and quaternions, people called algebras "hypercomplex numbers".

Week 62

August 28, 1995

Now I'd like to talk about a fascinating subject of importance in both mathematics and physics, the subject of "ADE classifications". Here A, D, and E aren't abbreviations for anything; they are just names for certain diagrams. But these diagrams show up all over the place when you start trying to classify beautiful and symmetrical things.

Let's start with something nice and simple: the Platonic solids. It's not terribly hard to classify all the regular polyhedra in 3-dimensional Euclidean space. Roughly, it goes like this. The faces could all be equilateral triangles. Obviously there need to be at least 3 faces meeting at each vertex to get a polyhedron. If there are exactly 3, you have a tetrahedron. If there are 4, you have an octahedron. If there are 5, you have an icosahedron. There can't be 6 or more, since when you have 6 they lie flat in the plane, and more is even worse. The faces could also be squares. If there are 3 squares meeting at each vertex you have a cube. There can't be 4 or more, since when you have 4 they lie flat in the plane. The faces could also be regular pentagons. If there are 3 pentagons meeting at each vertex you have a dodecahedron. There can't be 4 or more, since when you have 4 you already have more than 360 degree's worth of angles.

So, there we are: the 5 regular polyhedra are the tetrahedron, octahedron, icosahedron, cube, and dodecahedron! Of course, we haven't shown these solids actually exist. Sometimes people forget that you really need to check that all these possibilities are realized! But the Greeks did that a while back. This is perhaps the first example of an ADE classification.

This had such beauty that in his "Timaeus" dialog, Plato suggested that the 4 elements were made of these solids, not counting for the dodecahedron. Interestingly, Plato considered decomposing the faces of these solids into "elementary triangles", in order to explain how one element could turn into another. This is presumably why he left out the dodecahedron: one can't chop up a regular pentagon into 30-60-90 triangles. In a passage that's notoriously hard to translate, he suggested that the dodecahedron corresponding to some sort of "quintessence", or perhaps the zodiac. It's worth pointing out, also, that Plato explicitly says it's okay if someone comes up with a better scheme. He makes it clear that he is just trying to lay out an *example* of a mathematical scheme for explaining the elements, to get people interested.

Later, of course, Kepler suggested that the 5 Platonic solids corresponded to the orbits of the 5 planets:

As it turns out, Plato and Kepler were in the right ball-park, but not really right. Both the solar system and atoms are described pretty well by similar laws - the inverse-square force laws for gravity and electrostatics. And solving this problem (in either the classical or quantum case) does indeed require a deep understanding of rotations in 3-dimensional space. It's sort of amusing, however, that the Platonic solids have as their symmetries finite subgroups of the rotation group in 3 dimensions, while the study of quantum-mechanical atoms instead involves the theory of "representations" of this group, which are in some sense dual. The rotation group in n dimensions, by the way, is called $\mathrm{SO}(n)$. See "Week 61" for a bit more about it. For a grand tour of the inverse square law, both classical and quantum, read:

1) Victor Guillemin and Shlomo Sternberg, *Variations on a Theme by Kepler*, American Mathematical Society, Providence, Rhode Island, 1990.

You will see, among other things, that the real reason the inverse square force law problem is exactly solvable is that it has a hidden symmetry under $\mathrm{SO}(4)$, not just $\mathrm{SO}(3)$.

But I digress! Recall how I said that "obviously" a regular polyhedron has to have 3 faces meeting at each vertex? What would happen if you relaxed the definition a little bit, and let there be just 2 faces meeting at a vertex? Well, then any regular polygon could qualify as a regular polyhedron, I guess. Then we would have an infinite series of regular polyhedron with only two faces, together with 5 exceptions, the Platonic solids. That's actually typical of ADE-type classifications: often, when you are classifying really symmetrical things, you find some infinite series of "obvious" or "classical" cases, together with finitely many weird "exceptional" cases.

Before I get further into ADE classifications, let me note that the *problem* of why there are so many ADE classifications, and how they are all related, was explicitly raised by the famous mathematical physicist V. I. Arnol'd, in

2) "Problems of Present Day Mathematics" in *Mathematical Developments Arising from Hilbert's Problems*, ed. F. E. Browder, Proc. Symp. Pure Math. **28**, American Mathematical Society, Providence, Rhode Island, 1976.

This lists a lot of important math problems, following up on Hilbert's famous turn-of-the-century listing of problems. Problem VIII in this book is the "ubiquity of ADE classifications". Arnol'd lists the following examples:

- Platonic solids
- Finite groups generated by reflections
- Weyl groups with roots of equal length
- Representations of quivers
- Singularities of algebraic hypersurfaces with definite intersection form
- Critical points of functions having no moduli

Don't worry if you don't know what those are except for the first one! I'll try to explain some of them. Later I'll also explain two new ones that came out of string theory:

- Minimal models
- Certain "quantum categories"

Perhaps the best single place to start learning about ADE classifications is:

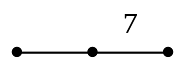
3) M. Hazewinkel, W. Hesselink, D. Siermsa, and F. D. Veldkamp, "The ubiquity of Coxeter-Dynkin diagrams (an introduction to the ADE problem)", *Niew. Arch. Wisk.*, **25** (1977), 257-307. Also available at http://repos.project.cwi.nl:8888/cwi_repository/docs/I/10/10039A.pdf or http://math.ucr.edu/home/baez/hazewinkel_et_al.pdf

Okay, so what the heck is an ADE classification, after all? It's probably good to start by looking at "finite reflection groups." Say we are in n-dimensional Euclidean space. Then given any unit vector v, there is a reflection that takes v to $-v$, and doesn't do anything to the vectors orthogonal to v. Let's call this a "reflection through v". A finite reflection group is a finite group of transformations of Euclidean space such that every element is a product of reflections. For example, the group of symmetries of an equilateral n-gon is a finite reflection group. (This is a useful exercise if you don't see it right off the bat.)

Note that if we do two reflections, we get a rotation. In particular, suppose we have vectors v and w at an angle A from each other, and let r and s be the reflections through v and w, respectively. Then rs is a rotation by the angle $2A$. Draw a picture and check it! This means that if $A = \pi/n$, then $(rs)^n$ is a rotation by the angle 2π, which is the same as no rotation at all, so $(rs)^n = 1$. On the other hand, if A is not a rational number times π, we never have $(rs)^n = 1$, so r and s can not both be in some *finite* reflection group.

With a little more work, we can convince ourselves that any finite reflection group is captured by a "Coxeter diagram". The idea is that the group is generated by reflections through unit vectors that are all at angles of π/n from each other. To keep track of things, we draw a dot for each one of these vectors. Suppose two of the vectors are at an angle π/n from each other. If $n = 2$, we don't bother drawing a line between the two dots. Otherwise, we draw a line between them, and label it with the number n. Typically, if $n = 3$ people don't bother writing the number; they just draw that line. That's what I'll do. (People also sometimes draw $n - 2$ lines instead of writing the number n, but I can't do that here.)

Algebraically speaking, if someone hands us a Coxeter diagram like

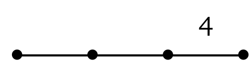


we get a group having one generator for each dot, and with one relation $r^2 = 1$ for each generator r (since that's what reflections do), and one relation of the form $(rs)^n = 1$ for each line connecting dots, or $(rs)^2 = 1$ if there is no line connecting two dots. It turns out that if a Coxeter diagram yields a *finite* group this way, it's a finite reflection group.

However, not every diagram we draw yields a finite group! Here are all the possible Coxeter diagrams giving finite groups. They have names. First there is A_n, which has n dots like this:

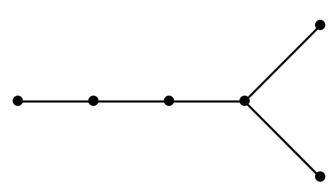
For example, the group of symmetries of the equilateral triangle is A_2. The two dots can correspond to the reflections r and s through two of the altitudes of the triangle, which are at an angle of $\pi/3$ from each other. Thus they satisfy $(rs)^3 = 1$. More generally, A_n corresponds to the group of symmetries of an n-dimensional simplex — which is just the group of permutations of the $n + 1$ vertices.

Then there is B_n, which has n dots, where $n > 1$:

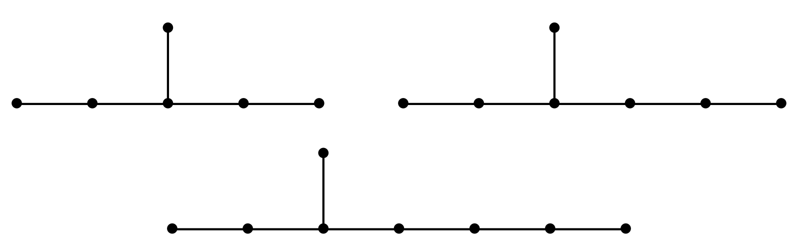


It has just one edge labelled with a 4. B_n turns out to be the group of symmetries of a hypercube or hyperoctahedron in n dimensions.

Then there is D_n, where $n > 3$:

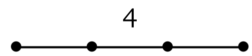


Then there are E_6, E_7, and E_8:

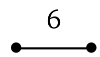


Interestingly, this series does *not* go on. That's what I meant about "classical" versus "exceptional" structures.

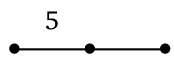
Then there is F_4:

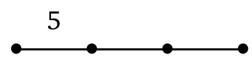


Then there's G_2:



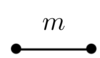
and H_3 and H_4:





H_3 is the group of symmetries of the dodecahedron or icosahedron. H_4 is the group of symmetries of a regular solid in 4 dimensions which I talked about in "Week 20". This regular solid is also called the "unit icosians" — it has 120 vertices, and is a close relative of the icosahedron and dodecahedron. One amazing thing is that it itself *is* a group in a very natural way. There are no "hypericosahedra" or "hyperdodecahedra" in dimensions greater than 4, which is related to the fact that the H series quits at this point.

Finally, there is another infinite series, I_m:



This corresponds to the symmetry group of the $2m$-gon in the plane, and people usually require $m = 5$ or $m > 6$, so as to not count twice some Coxeter diagrams that we've already run into.

THAT'S ALL.

So, we have an "ABDEFGHI classification" of finite reflection groups. (In some future week I had better say what happened to "C".) Note that the symmetry groups of the Platonic solids and some of their higher-dimensional relatives fit in nicely into this classification, so that's one sense in which the Greeks' discovery of these solids counts as the first "ADE classification". But there is at least one another, deeper, way to fit the Platonic solids themselves into an ADE classification. I'll try to say more about this in future weeks.

You may still be wondering what's so special about A, D, and E. I'll have to get to that, too.

Week 63

September 14, 1995

Let me continue the tale of "ADE classifications". Last week I described an "ABDEFGHI classification" of all finite reflection groups - that is, finite symmetry groups of Euclidean space, every element of which is a product of reflections. Now we'll build on that to get other related classifications.

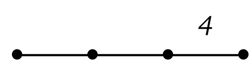
So, recall:

Every element of a finite reflection group is a product of reflections through certain special vectors, which people call "roots". These roots are all at angles π/n from each other, where $n > 1$ is an integer. To describe the group, we draw a diagram with one dot for each root. If two roots are perpendicular we don't draw a line between them, but otherwise, if they are at an angle π/n from each other, we draw a line and label it with the integer n. Actually, the integer $n = 3$ comes up so often that we don't bother labelling the line in this case.

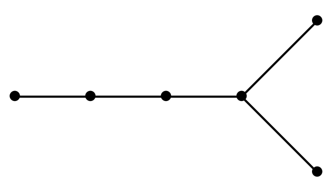
Now, not all of these diagrams correspond to finite reflection groups. The following ones, together with disjoint unions of them, are all the possibilities.

A_n, *which has* n *dots like this:*

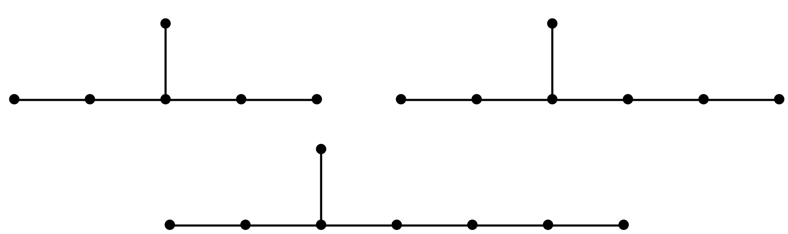
B_n, *which has* n *dots, where* $n > 1$*:*



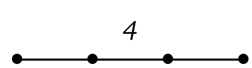
D_n, *which has* n *dots, where* $n > 3$*:*



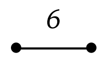
E_6, E_7, *and* E_8*:*



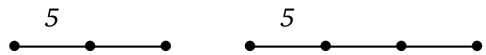
F_4*:*



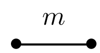
G_2*:*



H_3 *and* H_4*:*



I_m*, where* $m = 5$ *or* $m > 6$*:*



Recall that I_m is the symmetry group of the of regular m-gon, while others of these are the symmetry groups of Platonic solids, and still others are symmetry groups of regular polytopes in n-dimensional space. For example, the symmetry group of the dodecahedron is H_3, while that of its 4-dimensional relative is H_4.

Now you may know that there are no perfect crystals in the shape of a regular dodecahedron. However, iron pyrite comes close. In his wonderful book:

1) Hermann Weyl, *Symmetry*, Princeton University Press, Princeton, New Jersey, 1989.

Weyl suggests that this is how people discovered the regular dodecahedron:

> . . . *the discovery of the last two [Platonic solids] is certainly one of the most beautiful and singular discoveries made in the whole history of mathematics. With a fair amount of certainty, it can be traced to the colonial Greeks in southern Italy. The suggestion has been made that they abstracted the regular dodecahedron from the crystals of pyrite, a sulfurous mineral abundant in Sicily.*

Thus while iron pyrite is nothing but "fool's gold" to the miner, it may have done a good deed by fooling the Greeks into discovering the regular dodecahedron. Could this be why the ratio of the diagonal to the side of a regular pentagon, $(\sqrt{5}+1)/2$, is called the golden ratio? Or am I just getting carried away? One is tempted to call the shape of pyrite crystals the "fool's dodecahedron," but in fact it's called a "pyritohedron". (All this information on pyrite, as well as the puns, I owe to Michael Weiss.)

More recently, I think people have discovered "quasicrystals" (related to Penrose tiles) having true dodecahedral symmetry. But no perfectly repetitive crystals form dodecahedra! And the reason is that there is no lattice having H_3 as its symmetries.

Recall that we get a "lattice" by taking n linearly independent vectors in n-dimensional Euclidean space and forming all linear combinations with integer coefficients. If someone hands us a finite reflection group, we can look for a lattice having it as symmetries. If one exists, we say the group satisfies the "crystallographic condition". The only ones that do are

$$A_n, B_n, D_n, E_6, E_7, E_8, F_4, \text{ and } G_2$$

(and those corresponding to disjoint unions of these diagrams). In other words, the symmetry groups of the pentagon (I_5), the heptagon and so on (I_m with $m > 6$), and the dodecahedron and its 4-dimensional relative (H_3 and H_4) are ruled out.

Now let us turn to the theory of Lie groups. Lie groups are the most important "continuous" (as opposed to discrete) symmetry groups. Examples include the real line (with addition as the group operation), the circle (with addition $\bmod 2\pi$), and the groups $\mathrm{SO}(n)$ and $\mathrm{SU}(n)$ discussed in "Week 61". These groups are incredibly important in both physics and mathematics. Thus it is wonderful, and charmingly ironic, that the same diagrams that classify the oh-so-discrete finite reflection groups also classify some of the most beautiful of Lie groups: the "simple" Lie groups. It turns out that the simple Lie groups correspond to the diagrams of forms A,B,D,E,F, and G. Oh yes, and C. I have to tell you what happened to C.

There is a vast amount known about semisimple Lie groups, and everyone really serious about mathematics winds up needing to learn some of this stuff. I took courses on Lie groups and their Lie algebras in grad school, but it was only later that I really came to appreciate the beauty of the simple Lie groups. Back then I found it mystifying because the work involved in the classification was so algebraic, and I preferred the more geometrical aspects of Lie groups. Part of the reason is that the treatment I learned emphasized the Lie algebras and downplayed the groups. A nice treatment that emphasizes the groups is:

2) John Frank Adams, *Lectures on Lie groups*, Benjamin, New York, 1969.

So what's the basic idea? Let me summarize two semesters of grad school, and tell you the basic stuff about Lie groups and the classification of simple Lie groups. Forgive me if it's a bit rushed, sketchy, and even mildly inaccurate: hopefully the main ideas will shine through the murk this way.

A Lie group is a group that's also a manifold, for which the group operations (multiplication and taking inverses) are smooth functions. This lets you form the tangent space to any point in the group, and the tangent space at the identity plays a special role. It's called the Lie algebra of the group. If we have any element x in the Lie algebra, we can exponentiate it to get an element $\exp(x)$ in the group, and we can keep track of the noncommutativity of the group by forming the "bracket" of elements x and y in the Lie algebra:

$$[x, y] = \frac{d}{dt}\frac{d}{ds}\exp(sx)\exp(ty)\exp(-sx)\exp(-ty)$$

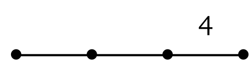
where s and t are real numbers, and we evaluate the derivative at $s, t = 0$. Note that $[x, y] = 0$ if the group is commutative. This bracket operation satisfies some axioms, and algebraists call anything a Lie algebra that satisfies those axioms. For example, you could take $n \times n$ matrices and let $[x, y] = xy - yx$.

Now a Lie algebra is called "semisimple" if for any z, there are x and y with $z = [x, y]$. This is sort of the opposite of an abelian, or commutative, Lie algebra, where $[x, y] = 0$ for all x and y. It turns out that we can take direct sums of Lie algebras by defining operations componentwise, and it turns out that if you have a *compact* Lie group, its Lie algebra is always the direct sum of a semsimple Lie algebra and an abelian one. The abelian ones are pretty trivial, so all the hard works lies in understanding the semisimple ones. Any semisimple one is the direct sum of a bunch of semisimple ones that aren't sums of anything else, and these basic building blocks are called the "simple" ones. They are like the prime numbers of Lie algebra theory. Unlike the prime numbers, though, we can completely classify all of them!

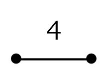
Now how does one classify the simple Lie algebras? Basically, it goes like this. We'll assume our simple Lie algebra is the Lie algebra of a compact Lie group G — it turns out that they all are. Now, sitting inside G there is a maximal commutative subgroup T that's a torus: a product of a bunch of circles. Let $\mathrm{Lie}(T)$ stand for the Lie algebra of this torus T. Now, sitting inside $\mathrm{Lie}(T)$ there is a lattice, consisting of all elements x with $\exp(x) = 1$. This is how lattices sneak into the picture!

Moreover, for some elements g in G, if we "conjugate" T by g, that is, form the set of all elements gtg^{-1} where t is in T, we get T back. In other words, these elements of g act as symmetries of the torus T. Now, if something acts as symmetries of something else, it also acts as symmetries of everything naturally cooked up from that something else. (Roughly speaking, "naturally" means "without dependence on arbitrary choices.) For this reason, these special elements of G also act as symmetries of $\mathrm{Lie}(T)$ and of the lattice sitting inside $\mathrm{Lie}(T)$. So we have a lattice together with a group of symmetries, which by the way is called the Weyl group of G. Now the cool part is that the Weyl group is actually a finite reflection group, so it must correspond to one of the diagrams in the list above! Even better, it turns out that the Lie algebra of G is determined by the lattice together with its Weyl group.

The upshot is that to classify semisimple Lie algebras, all we need is the classification of finite reflection groups satisfying the crystallographic condition — which we've done already using diagrams — together with a classification of lattices having such finite reflection groups as symmetries. It turns out that the operation of taking direct sums of semisimple Lie algebras corresponds to taking disjoint unions of diagrams, so to get the "building blocks" — the *simple* Lie algebras — we only need to worry about the diagrams we've drawn above, not disjoint unions of them. Now it turns out that for every type except B, there is (up to isomorphism) only *one* lattice having that group of symmetries, but for B there are two. Recall the diagram B_n looks like:



with n dots. And recall that the dots correspond to "roots", which in the present context are vectors in $\mathrm{Lie}(T)$. Now it turns out that whenever we have a finite reflection group satisfying the crystallographic condition, we can get a lattice having it as symmetries by taking integer linear combinations of the roots, but *not* necessarily roots that are unit vectors; the lengths of the roots matter. In all cases except B, there is basically just one way to get the lengths right, but for B there are two. We can keep track of the root lengths with some extra markings on our diagrams, and then we get two diagrams, which we call B_n and C_n. One of them has the root at the right of the diagram being longer, and one has the root right next to it being longer. This makes no difference when $n = 2$, since then we just have



which is perfectly symmetrical. So folks usually consider C_n only for $n > 2$, to avoid double counting.

In other words, all the simple Lie algebras are of the form:

- A_n, $n > 0$

- B_n, $n > 1$
- C_n, $n > 2$
- D_n, $n > 3$
- E_6, E_7, E_8
- F_4
- G_2

Okay, so what *are* these things, really? What do they *mean*, and what are the implications of the fact that the symmetries of the forces of nature are given by the some of the corresponding Lie groups? Why are 4 infinite series of them and 5 "exceptional" Lie algebras? What's so special about A, D, and E, that makes people keep talking about "ADE classifications"? What do the exceptional Lie algebras (and their corresponding Lie groups) have to do with octonions? Why do some string theorists think the symmetry group of nature is E_8, the largest exceptional Lie group???

Well, I'm afraid that I'm going camping in a couple of hours, so I'll have to leave you hanging, even though I do know the answers to *some* of these questions. I'll try to finish talking about ADE classifications in the next couple of issues.

... *without fantasy one would never become a mathematician, and what gave me a place among the mathematicians of our day, despite my lack of knowledge and form, was the audacity of my thinking.* - Sophus Lie

Week 64

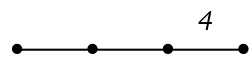
September 23, 1995

I have been talking about different "ADE classifications". This time I'll start by continuing the theme of last Week, namely simple Lie algebras, and then move on to discuss affine Lie algebras and quantum groups. These are algebraic structures that describe the symmetries appearing in quantum field theory in 2 and 3 dimensions. They are very important in string theory and topological quantum field theory, and it's largely physics that has gotten people interested in them.

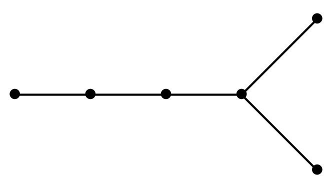
Remember, we began by classifying finite reflection groups. A finite reflection group is simply a finite group of linear transformations of $\mathbb{R}^n$, every element of which is a product of reflections. Finite reflection groups are in 1-1 correspondence with the following "Coxeter diagrams", together with disjoint unions of such diagrams:

A_n*, which has* n *dots like this:*

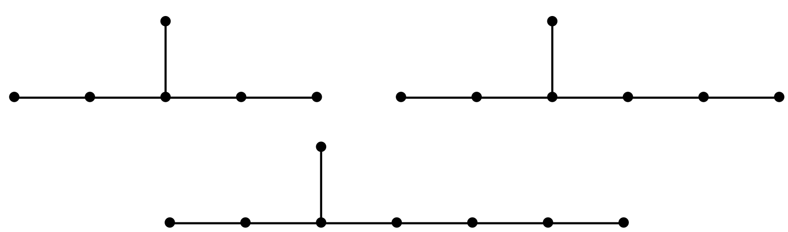
B_n*, which has* n *dots, where* $n > 1$*:*



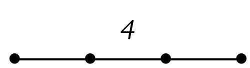
D_n*, which has* n *dots, where* $n > 3$*:*



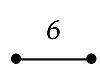
E_6, E_7*, and* E_8*:*



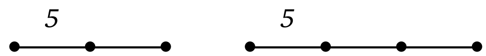
F_4*:*



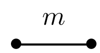
G_2*:*



H_3 and H_4:



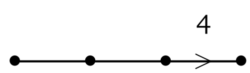
I_m, where $m = 5$ or $m > 6$:



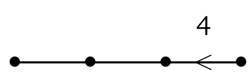
Not all of these finite reflection groups satisfy the "crystallographic condition", namely that they act as symmetries of some lattice. The ones that do are of types A,B,D,E,F, and G, and disjoint unions thereof — but I'm going to stop reminding you about disjoint unions all the time!

Now, if we have a finite reflection group that's the symmetries of some lattice, we can take the dimension of the lattice to be the number of dots in the Coxeter diagram. In fact, the dots correspond to a basis of the lattice, and the lines between them (and their numberings) keep track of the angles between the basis vectors. These basis vectors are called "roots". To describe the lattice completely, in principle we need to know the lengths of the roots as well as the angles between them. But it turns out that except for type B, there is just one choice of lengths that works (up to overall scale). For type B there are two choices, which people call B_n and C_n, respectively. People keep track of the lengths with a "Dynkin diagram" like this:

- B_n has n dots, where $n > 1$:



- C_n has n dots, where $n > 2$:



The arrow points to the shorter root; for B_n all the roots except the last one are $\sqrt{2}$ times as long as the last one, while for C_n all the roots except the last one are $1/\sqrt{2}$ as long. (In fact, the lattices corresponding to B_n and C_n are "dual", in the hopefully obvious sense.) The only reason why we require $n > 2$ for C_n is that B_2 is basically the same as C_2!

Now last Week, I also sketched how the Lie algebras of the compact simple Lie groups were *also* classified by the same Dynkin diagrams of types A, B, C, D, E, F, and G. These were real Lie algebras; we can also switch viewpoint and work with complex Lie algebras if we like, in which case we simply say we're studying the complex simple Lie algebras, as opposed to their "compact real forms".

Unfortunately, I didn't say much about what these Lie algebras actually are! Basically, they go like this:

A_n — The Lie algebra A_n is just $\mathfrak{sl}_{n+1}(\mathbb{C})$, the $(n+1) \times (n+1)$ complex matrices with vanishing trace, which form a Lie algebra with the usual bracket $[x, y] = xy - yx$. The compact real form of $\mathfrak{sl}_n(\mathbb{C})$ is $\mathfrak{su}_n$, and the corresponding compact Lie group is $\mathrm{SU}(n)$, the $n \times n$ unitary matrices with determinant 1. The symmetry group of the electroweak force is $\mathrm{U}(1) \times \mathrm{SU}(2)$, where $\mathrm{U}(1)$ is the 1×1 unitary matrices. The symmetry group of the strong force is $\mathrm{SU}(3)$. The study of A_n is thus a big deal in particle physics. People have also considered "grand unified theories" with symmetry groups like $\mathrm{SU}(5)$.

B_n — The Lie algebra B_n is $\mathfrak{so}_{2n+1}(\mathbb{C})$, the $(2n+1) \times (2n+1)$ skew-symmetric complex matrices with vanishing trace. The compact real form of $\mathfrak{so}_n(\mathbb{C})$ is $\mathfrak{so}_n$, and the corresponding compact Lie group is $\mathrm{SO}(n)$, the $n \times n$ real orthogonal matrices with determinant 1, that is, the rotation group in Euclidean n-space. For some basic cool facts about $\mathrm{SO}(n)$, check out "Week 61".

C_n — The Lie algebra C_n is $\mathfrak{sp}_n(\mathbb{C})$, the $2n \times 2n$ complex matrices of the form

$$\begin{pmatrix} A & B \\ C & D \end{pmatrix}$$

where B and C are symmetric, and D is minus the transpose of A. The compact real form of $\mathfrak{sp}_n(\mathbb{C})$ is $\mathfrak{sp}_n$, and the corresponding compact Lie group is called $\mathrm{Sp}(n)$. This is the group of $n \times n$ quaternionic matrices which preserve the usual inner product on the space $\mathbb{H}^n$ of n-tuples of quaternions.

D_n — The Lie algebra D_n is $\mathfrak{so}_{2n}(\mathbb{C})$, the $2n \times 2n$ skew-symmetric complex matrices with vanishing trace. See B_n above for more about this. It may seem weird that $\mathrm{SO}(n)$ acts so differently depending on whether n is even or odd, but it's true: for example, there are "left-handed" and "right-handed" spinors in even dimensions, but not in odd dimensions. Some clues as to why are given in "Week 61".

Those are the "classical" Lie algebras, and they are things that are pretty easy to reinvent for yourself, and to get interested in for all sorts of reasons. As you can see, they are all about "rotations" in real, complex, and quaternionic vector spaces.

The remaining ones are called "exceptional", and they are much more mysterious. They were only discovered when people figured out the classification of simple Lie algebras. As it turns out, they tend to be related to the octonions! Some other week I will say more about them, but for now, let me just say:

F_4 — This is a 52-dimensional Lie algebra. Its smallest representation is 26-dimensional, consisting of the traceless 3×3 hermitian matrices over the octonions, on which it preserves a trilinear form.

G_2 — This is a 14-dimensional Lie algebra, and the compact Lie group corresponding to its compact real form is also often called G_2. This group is just the group of symmetries (automorphisms) of the octonions! In fact, the smallest representation of this Lie algebra is 7-dimensional, corresponding to the purely imaginary octonions.

E_6 — This is a 78-dimensional Lie algebra. Its smallest representation is 27-dimensional, consisting of all the 3×3 hermitian matrices over the octonions this time, on which it preserves the anticommutator.

E_7 — This is a 133-dimensional Lie algebra. Its smallest representation is 56-dimensional, on which it preserves a tetralinear form.

E_8 — This is a 248-dimensional Lie algebra, the biggest of the exceptional Lie algebras. Its smallest representation is 248-dimensional, the so-called "adjoint" representation, in which it acts on itself. Thus in some vague sense, the simplest way to understand the Lie group corresponding to E_8 is as the symmetries of itself! (Thanks go to Dick Gross for this charming information; I think he said all the other exceptional Lie algebras have representations smaller than themselves, but I forget the sizes.) In "Week 20" I described a way to get its root lattice (the 8-dimensional lattice spanned by the roots) by playing around with the icosahedron and the quaternions.

People have studied simple Lie algebras a lot this century, basically studied the hell out of them, and in fact some people were getting a teeny bit sick of it recently, when along came some new physics that put a lot of new life into the subject. A lot of this new physics is related to string theory and quantum gravity. Some of this physics is "conformal field theory", the study of quantum fields in 2 dimensional spacetime that are invariant under all conformal (angle-preserving) transformations. This is important in string theory because the string worldsheet is 2-dimensional. Some other hunks of this physics go by the name of "topological quantum field theory", which is the study of quantum fields, usually in 3 dimensions so far, that are invariant under *all* transformations (or more precisely, all diffeomorphisms).

Every simple Lie algebra gives rise to an "affine Lie algebra" and a "quantum group". The symmetries of conformal field theories are closely related to affine Lie algebras, and the symmetries of topological quantum field theories are quantum groups. I won't tell you what affine Lie algebras and quantum groups ARE, since it would take quite a while. Instead I'll refer you to a good good introduction to this stuff:

1) Juergen Fuchs, *Affine Lie Algebras and Quantum Groups*, Cambridge Monographs on Mathematical Physics, Cambridge U. Press, Cambridge 1992.

Let me whiz through his table of contents and very roughly sketch what it's all about.

1. **Semisimple Lie algebras**

   This is a nice summary of the theory of semisimple Lie algebras (remember, those are just direct sums of simple Lie algebras) and their representations. Especially if you are a physicist, a slick summary like this might be a better way to start learning the subject than a big fat textbook on the subject. He covers the Dynkin diagram stuff and much, much more.

2. **Affine Lie algebras**

   This starts by describing Kac-Moody algebras, which are certain *infinite-dimensional* analogs of the simple Lie algebras. Fuchs concentrates on a special class of these, the affine Lie algebras, and describes the classification of these using Dynkin diagrams. The main nice thing about the affine Lie algebras is that their corresponding infinite-dimensional Lie groups are very nice: they are almost "loop groups". If we have a Lie group G, the loop group LG is just the set of all smooth functions from the circle to G, which we make into a group by pointwise multiplication. If you're a physicist, this is obviously relevant to string theory, because at each time, a string is just a circle (or bunch of circles), and if you are doing gauge theory on the string, with symmetry group G, the gauge group is then just the loop group LG. So you'd expect the representation theory of loop groups and their Lie algebras to be really important.

   You'd *almost* be right, but there is a slight catch. In quantum theory, what counts are the "projective" representations of a group, that is, representations that satisfy the rule $g(h(v)) = (gh)(v)$ *up to a phase*. (This is because "phases are unobservable in quantum theory" — one of those mottoes that needs to be carefully interpreted to be correct.) The projective representations of the loop group LG correspond to the honest-to-goodness representations of a "central extensions" of LG, a slightly fancier group than LG itself. And the Lie algebra of *this* group is called an affine Lie algebra.

So, people who like gauge theory and string theory need to know a lot about affine Lie algebras and their representations, and that's what this chapter covers. A real heavy-duty string theorist will need to know more about Kac-Moody algebras, so if you're planning on becoming one of those, you'd better also try:

2) Victor Kac, *Infinite Dimensional Lie Algebras*, 3rd ed., Cambridge University Press, Cambridge, 1990.

You'll also need to know more about loop groups, so try:

3) *Loop groups*, by Andrew Pressley and Graeme Segal, Oxford University Press, Oxford, 1986.

3. **WZW theories**

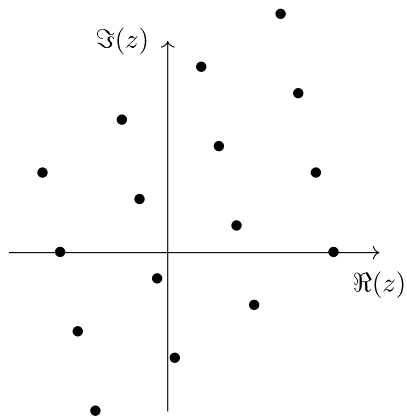
   Well, I just said that physicists liked affine Lie algebras because they were the symmetries of conformal field theories that were also gauge theories. Guess what: a Wess-Zumino-Witten, or WZW, theory, is a conformal field theory that's also a gauge theory! You can think of it as the natural generalization of the wave equation in 2 dimension (which is conformally invariant, btw) from the case of real-valued fields, to general G-valued fields, where G is our favorite Lie group.

4. **Quantum groups**

   When you quantize a WZW theory whose symmetry group G is some simple Lie group, something funny happens. In a sense, the group itself also gets quantized! In other words, the algebraic structure of the group, or its Lie algebra, gets "deformed" in a way that depends on the parameter $\hbar$ (Planck's constant). I have muttered much about quantum groups on This Week's Finds, especially concerning their relevance to topological quantum field theory, and I will not try to explain them any better here! Eventually I will discuss a bunch of books that have come out on quantum groups, and I hope to give a mini-introduction to the subject in the process.

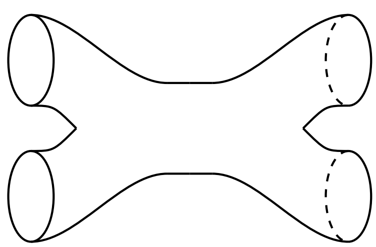
5. **Duality, fusion rules, and modular invariance**

   The previous chapter described quantum groups as abstract algebraic structures, showing how you can get one from any simple Lie algebra. Here Fuchs really shows how you get them from quantizing a WZW theory. WZW theories are invariant under conformal transformations, and quantum groups inherit lots of cool properties from this fact. For example, suppose you form a torus by taking the complex plane and identifying two points if they differ by any number of the form $nz_1 + mz_2$, where z_1 and z_2 are fixed complex numbers and n, m are arbitrary integers. For example, we might identify all these points:

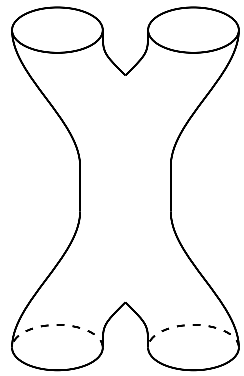


The resulting torus is a "Riemann surface" and it has lots of transformations, called "modular transformations". The group of modular transformations is the discrete group $\mathrm{SL}(2,\mathbb{Z})$ of 2×2 integer matrices with determinant 1; I leave it as an easy exercise to guess how these give transformations of the torus. (This is an example of a "mapping class group" as discussed in "Week 28".) In any event, the way the the WZW theory transforms under modular transformations translates into some cool properties of the corresponding quantum group, which Fuchs discusses. That's roughly what "modular invariance" means.

Similarly, "fusion rules" have to do with the thrice-punctured sphere, or "trinion", which is another Riemann surface. And "duality" has to do with the sphere with four punctures, which can be viewed as built up from trinions in either of two "dual" ways:



or



This is one of the reasons string theory was first discovered — we can think of the above pictures as two Feynman diagrams for interacting strings, and the fact that they are really just distorted versions of each other gives rise to identities among Feynman diagrams. Similarly, this fact gives rise to identities satisfied by the fusion rules of quantum groups.

So — Fuchs' book should make clear how, starting from the austere beauty of the Dynkin diagrams, we get not only simple Lie groups, but also a wealth of more complicated structures that people find important in modern theoretical physics.

Mathematics, rightly viewed, possesses not only truth, but supreme beauty - a beauty cold and austere, like that of sculpture, without appeal to any part of our weaker nature, without the gorgeous trappings of painting or music, yet sublimely pure, and capable of a stern perfection such as only the greatest art can show. - Bertrand Russell.

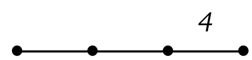
Week 65

October 3, 1995

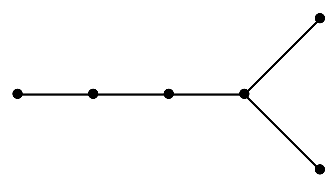
This time I'll finish up talking about "ADE classifications" for a while, although there is certainly more to say. Recall where we were: the following diagrams correspond to the simple Lie algebras, and they also define certain lattices, the "root lattices" of those Lie algebras:

A_n, *which has n dots like this:*

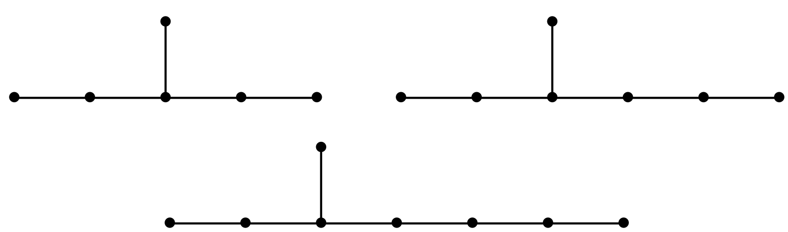
B_n, *which has n dots, where $n > 1$:*



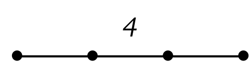
D_n, *which has n dots, where $n > 3$:*



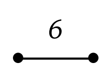
E_6, E_7, *and* E_8*:*



F_4*:*



G_2*:*



The dots in one of these "Dynkin diagrams" correspond to certain set of basis vectors, or "roots", of the lattice. The lines, with their decorative numbers and arrows, give enough information to recover the lattice from the diagram. In particular, two dots that are not connected by a line correspond to roots that are at a 90 degree angle from each other, while two dots connected by an unnumbered line correspond to roots that are at a 60 degree angle from each other. Numbered lines mean the angle between roots is something else, and the arrows point from the longer to the shorter root in this case, as partially explained in "Week 63".

However, we will now concentrate on the cases A, D, and E, where all the roots are 90 or 60 degrees from each other, and they are all the same length — usually taken to be length 2. These are the "simply laced" Dynkin diagrams. I want to explain what's so special about them! But first, I should describe the corresponding lattices more explicitly, to make it clear how simple they really are.

The following information, and more, can be found in Chapter 4 of:

1) *Sphere Packings, Lattices and Groups*, J. H. Conway and N. J. A. Sloane, second edition, Grundlehren der mathematischen Wissenschaften **290**, Springer, Berlin, 1993.

which I described in more detail in "Week 20".

So, what are A, D, and E like?

A. We can describe the lattice A_n as the set of all $(n{+}1)$-tuples of integers $(x_1, ..., x_{n+1})$ such that

$$x_1 + \ldots + x_{n+1} = 0.$$

It's a fun exercise to show that A_2 is a 2-dimensional hexagonal lattice, the sort of lattice you use to pack pennies as densely as possible. Similarly, A_3 gives a standard way of packing grapefruit, which is in fact the densest lattice packing of spheres in 3 dimensions. (Hsiang has claimed to have shown it's the densest packing, lattice or not, but this remains controversial.)

D. We can describe D_n as the set of all n-tuples of integers $(x_1, ..., x_n)$ such that

$$x_1 + \ldots + x_n \quad \text{is even.}$$

Or, if you like, you can imagine taking an n-dimensional checkerboard, coloring the cubes alternately red and black, and taking the center of each red cube. In four dimensions, D_4 gives a denser packing of spheres than A_4; in fact, it gives the densest lattice packing possible. Moreover, D_5 gives the densest lattice packing of in dimension 5. However, in dimensions 6, 7, and 8, the E_n lattices are the best!

E. We can describe E_8 as the set of 8-tuples $(x_1, ..., x_8)$ such that the x_i are either all integers or all half-integers — a half-integer being an integer plus $1/2$ — and

$$x_1 + \ldots + x_8 \quad \text{is even.}$$

Each point has 240 closest neighbors. Alternatively, as described in "Week 20", we can describe E_8 in a slick way in terms of the quaternions. Another neat way to think of E_8 is in terms of the octonions! Not too surprising, perhaps, since the octonions and E_8 are both 8-dimensional, but it's still sorta neat. For the details, check out

2) Geoffrey Dixon, "Octonion X-product and E_8 lattices", preprint available as `hep-th/9411063`.

Briefly, this goes as follows. In "Week 59" we described a multiplication table for the "seven dwarves" — a basis of the imaginary octonions — but there are lots of other multiplication tables that would also give an algebra isomorphic to the octonions. Given any unit octonion a, we can define an "octonion $\times$-product" as follows:

$$b \times c = (ba)(a^* c)$$

where a^* is the conjugate of a (as defined in "Week 59") and the product on the right-hand side is the usual octonion product, parenthesized because it ain't associative. For exactly 480 choices of the unit octonion a, the $\times$-product gives us a new multiplication table for the seven dwarves, such that we get an algebra isomorphic to the octonions again! 240 of these choices have all rational coordinates (in terms of the seven dwarves), and these are precisely the 240 closest neighbors of the origin in a copy of the E_8 lattice! The other 240 have all irrational coordinates, and these are the closest neighbors to the origin of a *different* copy of the E_8 lattice. (Here we've rescaled the E_8 lattice so the nearest neighbors have distance 1 from the origin, instead of $\sqrt{2}$ as above.)

Once we have E_8 in hand, we can get its little pals E_7 and E_6 as follows. To get E_7, just take all the vectors in E_8 that are perpendicular to some closest neighbor of the origin. To get E_6, find a copy of the lattice A_2 in E_8 (there are lots) and then take all the vectors in E_8 perpendicular to everything in that copy of A_2.

So, now that we have a nodding acquaintance with A, D, and E, let me describe some of the many places they show up. First, what's so great about these lattices, apart from the fact that they're the root lattices of simple Lie algebras, with a special "simply-laced" property? I don't think I really understand the answer to this in a deep way, but I know various things to say!

First, Witt's theorem says that the A, D, and E lattices and their direct sums are the only integral lattices having a basis consisting of vectors v with $\|v\|^2 = 2$. Here a lattice is "integral" if the dot product of any two vectors in it is an integer. In fact, any integral lattice having a basis consisting of vectors with $\|v\|^2$ equal to 1 or 2 is a direct sum of copies of A, D, and E lattices and the integers, thought of as a 1-dimensional lattice.

This makes ADE classifications pop up in various places in math and physics. For example, there is a cool relationship between the ADE diagrams and the symmetry groups of the Platonic solids, called the McKay correspondence. Briefly, this is what you do to get it. First, learn about $\mathrm{SO}(3)$ and $\mathrm{SU}(2)$ from "Week 61" or somewhere. Then, take the symmetry group of a Platonic solid, or more generally any finite subgroup G of $\mathrm{SO}(3)$. Since $\mathrm{SO}(3)$ has $\mathrm{SU}(2)$ as a double cover, you can get a double cover of G, say $\widetilde{G}$, sitting inside $\mathrm{SU}(2)$. For example, if G was the symmetry group of the icosahedron, $\widetilde{G}$ would be the icosians (see "Week 24").

Since $\widetilde{G}$ is finite, it has finitely many irreducible representations. Draw a dot for each of the irreducible representations. One of these will be 2-dimensional, coming from the spin-$1/2$ representation of $\mathrm{SU}(2)$. Now, when you tensor this 2d rep with any other irreducible rep R, you get a direct sum of irreducible reps; draw one line from the dot for R to each other dot for each time that other irreducible rep appears in this direct sum. What do you get? Well, you get an "affine Dynkin diagram" of type A, D, or E, which is like the usual Dynkin diagram but with an extra dot thrown in (corresponding to the trivial rep of $\widetilde{G}$). And, you get all of them this way!

In fact, playing around with this stuff some more, you can get the affine Dynkin diagrams of the other simple Lie algebras. There is a lot more to this... you should probably look at:

3) John McKay, "Graphs, singularities and finite groups", in *Proc. Symp. Pure Math.* vol **37**, Amer. Math. Soc. (1980), pages 183– and 265–.

   John McKay, "Representations and Coxeter Graphs", in *The Geometric Vein* Coxeter Festschrift (1982), Springer-Verlag, Berlin, pages 549–.

   John McKay, A rapid introduction to ADE theory, `http://math.ucr.edu/home/baez/ADE.html`

4) Pavel Etinghof and Mikhail Khovanov, Representations of tensor categories and Dynkin diagrams, preprint available as `hep-th/9408078`.

McKay does lots of other mindblowing group theory. He's clearly in tune with the symmetries of the universe... and occaisionally he deigns to post to the net! A beautiful way of thinking about the McKay correspondence in terms of category theory appears in the paper by Etinghof and Khovanov; what we are really doing, it turns out, is classifying the representations of the tensor category of unitary representations of $\mathrm{SU}(2)$. This tensor category is generated by one object, the spin-$1/2$ representation, meaning that every other representation sits inside some tensor power of that one. This way of thinking of it is important in

5) Jurg Froehlich and Thomas Kerler, *Quantum Groups, Quantum Categories, and Quantum Field Theory*, Springer Lecture Notes in Mathematics **1542**, Springer-Verlag, Berlin, 1991.

Here Froehlich and Kerler give a classification of certain "quantum categories" that show up in conformal field theory and topological quantum field theory. These are certain braided tensor categories with properties like those of the categories of representations of quantum groups at roots of unity. In such categories, every object has a "quantum dimension", which need not be integral, and Froehlich and Kerler concentrate on those categories which are generated by a single object of quantum dimension less than 2, getting an ADE-type classification of them. The category of representations of $\mathrm{SU}(2)$, on the other hand, is generated by a single object of dimension equal to 2 — the spin-$1/2$ representation — so Froehlich and Kerler are basically generalizing the McKay stuff to certain quantum groups related to $\mathrm{SU}(2)$.

Where else do ADE diagrams show up? Well, here I won't try to say anything about their role in the representation theory of "quivers", or in singularity theory; these are covered pretty well in

6) M. Hazewinkel, W. Hesselink, D. Siermsa, and F. D. Veldkamp, "The ubiquity of Coxeter-Dynkin diagrams (an introduction to the ADE problem)", *Niew. Arch. Wisk.*, **25** (1977), 257–307.

Instead, I'll mention something more recent. In string theory, there is a Lie algebra called the Virasoro algebra that plays a crucial role; its almost just the Lie algebra of the group of diffeomorphisms of the circle, but it's actually just one dimension bigger, being a "central extension" thereof; projective representations of the Lie algebra of the group of diffeomorphisms of the circle correspond to honest representations of the Virasoro algebra. An important task in string theory was to classify the unitary representations of the Virasoro algebra having a given "central charge" c (this describes the action of that one extra dimension) and "conformal weight" h (this describes the action of dilations). It turns out that to get unitary reps one needs c and h to be nonnegative. The representations with c between 0 and 1 are especially nice, for reasons I don't really understand, and they are called "minimal models". An ADE classification of these was conjectured by Capelli and Zuber, and proved by

7) Capelli and Zuber, *Comm. Math. Phys.* **113** (1987) 1.

8) Kato, *Mod. Phys. Lett.* **A2** (1987) 585.

Friedan, Qiu, and Shenker also played a big role in this, in part by figuring out the allowed values of c. For a good introduction to this stuff and what it has to do with honest *physics* (which I admit I've been slacking off on here), try:

9) Claude Itzykson and Jean-Michel Drouffe, *Statistical Field Theory, 1: From Brownian Motion to Renormalization and Lattice Gauge Theory*, and *2: Strong Coupling, Monte Carlo Methods, Conformal Field Theory and Random Systems.* Cambridge U. Press, 1989.

I will probably come back to this ADE stuff as time goes by, since I'm sort of fascinated by it, and hopefully folks can refer back to the last few weeks when I do, so they'll remember what I'm talking about. But in the next few Weeks I want to catch up with some new developments in math and physics that have happened in the last few months. . .

A mathematician, like a painter or poet, is a maker of patterns. If his patterns are more permanent than theirs, it is because they are made with ideas - Godfrey Hardy

Week 66

October 10, 1995

Well, I want to get back to talking about some honest physics, but I think this week I won't get around to it, since I can't resist mentioning two tidbits of a more mathematical sort. The first one is about π, and the second one is about the Monster. The second one *does* have a lot to do with string theory, if only indirectly.

First, thanks to my friend Steven Finch, I just found out that Simon Plouffe, Peter Borwein and David Bailey have computed the ten billionth digit in the hexadecimal (i.e., base 16) expansion of π. They use a wonderful formula which lets one compute a given digit of π in base 16 without needing to compute all the preceding digits! Namely, π is the sum from $n = 0$ to ∞ of

$$\left[\frac{4}{8n+1} - \frac{2}{8n+4} - \frac{1}{8n+5} - \frac{1}{8n+6}\right]\frac{1}{16^n}$$

Since the quantity in square brackets is not an integer, it requires cleverness to use this formula to get a given digit of π, but they figured out a way. Moreover, their method generalizes to a variety of other constants. If you can use the World-Wide Web, try the following sites:

1) "The ten billionth hexadecimal digit of π is 9", by Simon Plouffe, `http://groups.google.com/groups?selm=451p8p%24qcr%40morgoth.sfu.ca&output=gplain`

2) David Bailey, Peter Borwein and Simon Plouffe, "On the rapid computation of various polylogarithmic constants", available in postscript form from `http://www.cecm.sfu.ca/personal/pborwein/PISTUFF/Apistuff.html`

3) "The miraculous Bailey-Borwein-Plouffe π algorithm", by Steven Finch, `http://www.lacim.uqam.ca/~plouffe/Simon/Miraculous.pdf`

The first one is an announcement that appeared on `sci.math`, and lists the billionth digits of π^2, $\ln(2)$, and some other constants. The second one has the details. The third one gives a good overview of what's up.

Can we hope for a similar formula in base 10? More importantly, could these ideas let us prove that π is "normal", that is, that every possible string of digits appears in it with the frequency one would expect of a "random" number? It seems that this would be a natural avenue of attack.

Next, a tidbit of a more erudite sort concerning the elusive Monster manifold. Recall from "Week 63" and "Week 64" that the compact simple Lie groups can classified into 4 infinite families and 5 exceptions. I have always been puzzled by these "exceptional Lie groups", so I tried to explain a bit about how they are related to some other "exceptional structures" in mathematics, such as the icosahedron and the octonions. In physics, Witten has suggested that the correct theory of our universe might also be an exceptional structure of some sort. This idea has found some support in string theory, which uses the exceptional Lie group E_8 and other structures I'll mention a bit later. In a more hand-waving way, one may argue that the theory of our universe must be incredibly special, since out of all the theories we can write down, just this *one* describes the universe that actually *exists*. All sorts of simpler universes apparently don't exist. So maybe the theory of the universe needs to use special, "exceptional" mathematics for some reason, even though it's complicated.

Anyway, as a hard-nosed mathematician, vague musings along these lines get tiresome to me rather quickly. Instead, what interests me most about this stuff is the whole idea of "exceptional structures" — symmetrical structures that don't fit into the neat regular families in classification theorems. The remarkable fact is that many of them are deeply related. As Geoffrey Dixon put it to me, they seem to have a "holographic" quality, meaning that each one contains in encoded form some of the information needed to construct all the rest! It thus seems pointless to hope that one is "the explanation" for the rest, but I would still like some conceptual "explanation" for the whole collection of them — though I have no idea what it should be.

Surely a clue must lie in the theory of finite simple groups. Just as the simple Lie groups are the building blocks of the theory of continuous symmetries, these are the building blocks of the theory of discrete — indeed finite — symmetries. More precisely "finite simple" group is a group G with finitely many elements and no normal subgroups, that is, no nontrivial subgroups H such that h in H implies ghg^{-1} in H for all g in G. This condition means that you cannot form the "quotient group" G/H, which one can think of as a "more simplified" version of G.

The classification of the finite simple groups is one of remarkable achievements of 20th-century mathematics. The entire proof of the classification theorem is estimated to take 10,000 pages, done by many mathematicians. To start learning about it, try:

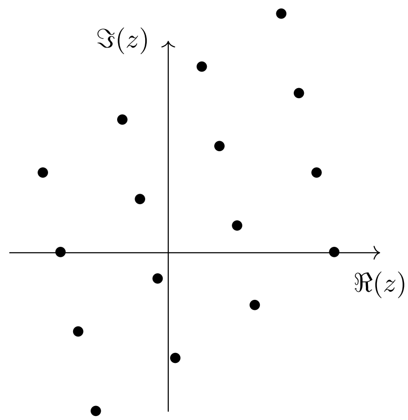
4) Ron Solomon, "On finite simple groups and their classification", *AMS Notices Vol.* **45**, February 1995, 231–239.

and the references therein. Again, there are some infinite families and 26 exceptions called the "sporadic" groups. The biggest of these is the Monster, with

$$
\begin{aligned}
&246 \cdot 320 \cdot 59 \cdot 76 \cdot 112 \cdot 133 \cdot 17 \cdot 19 \cdot 23 \cdot 29 \cdot 31 \cdot 41 \cdot 47 \cdot 59 \cdot 71 \\
&= 808017424794512875886459904961710757005754368000000000
\end{aligned}
$$

elements. It is a kind of Mt. Everest of the sporadic groups, and all the routes to it I know involve a tough climb through all sorts of exceptional structures: E_8 (see "Week 65"), the Leech lattice (see "Week 20"), the Golay code, the Parker loop, the Griess algebra, and more. I certainly don't understand this stuff. . . .

Even before the Monster was proved to exist, it started casting its enormous shadow over mathematics. For example, consider the theory of modular functions. What are those? Well, consider a lattice in the complex plane, like



These are important in complex analysis, as described in "Week 13". To describe one of these you can specify two "periods" ω_1 and ω_2, complex numbers such that every point in the lattice of the form

$$n\omega 1 + m\omega 2.$$

But this description is redundant, because if we choose instead to use

$$\omega_1' = a\omega_1 + b\omega_2$$
$$\omega_2' = c\omega_1 + b\omega_2$$

we'll get the same lattice as long as the matrix of integers

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix}$$

is invertible and its inverse also consists of integers. These transformations preserve the "handedness" of the basis ω_1, ω_2 if they have determinant 1, and that's generally a good thing to require. The group of 2×2 invertible matrices over the integers with determinant 1 is called $\mathrm{SL}(2,\mathbb{Z})$, or the "modular group" in this context. I said a bit about it and its role in string theory in "Week 64".

Now, if we are only interested in parametrizing the different *shapes* of lattices, where two rotated or dilated versions of the same lattice count as having the same shape, it suffices to use one complex number, the ratio

$$\tau = \frac{\omega_1}{\omega_2}.$$

We might as well assume τ is in the upper halfplane, H, while we're at it. But for the reason given above, this description is redundant; if we have a lattice described by τ, and a matrix in $\mathrm{SL}(2,\mathbb{Z})$, we get a new guy τ' which really describes the same shaped lattice. If you work it out,

$$\tau' = \frac{a\tau + b}{c\tau + d}.$$

So the space of different possible shapes of lattices in the complex plane is really the quotient

$$H/\mathrm{SL}(2,\mathbb{Z}).$$

Now, a function on this space is just a function of τ that doesn't change when you replace τ by τ' as above. In other words, it's basically just a function depending only on the shape of a 2d lattice. Now it turns out that there is essentially just one "nice" function of this sort, called j; all other "nice" functions of this sort are functions of j. (For experts, what I mean is that the meromorphic $\mathrm{SL}(2,\mathbb{Z})$-invariant functions on H union the point at infinity are all rational functions of this function j.)

It looks like this:

$$j(\tau) = q^{-1} + 744 + 196884q + 21493706q^2 + \ldots$$

where $q = \exp(2\pi i\tau)$. In fact, starting from a simple situation, we have quickly gotten into quite deep waters. The simplest explicit formula I know for j involves lattices in 24-dimensional space! This could easily be due to my limited knowledge of this stuff, but it suits my present purpose: first, we get a vague glimpse of where E_8 and the Leech lattice come in, and second, we get a vague glimpse of the mysterious significance of the numbers 24 and 26 in string theory.

So what is this j function, anyway? Well, it turns out we can define it as follows. First form the Dedekind eta function

$$\eta(q) = q^{\frac{1}{24}} \prod_{n=1}^{\infty} (1 - q^n).$$

This is not invariant under the modular group, but it transforms in a pretty simple way. Then take the E_8 lattice — remember, that's a very nice lattice in 8 dimensions, in fact the only "even unimodular" lattice in 8 dimensions, meaning that the inner product of any two vectors in the lattice is even, and the volume of each fundamental domain in it equals 1. Now take the direct sum of 3 copies of E_8 to get an even unimodular lattice L in 24 dimensions. Then form the theta function

$$\theta(q) = \sum_{x \in L} q^{\langle x,x\rangle/2}.$$

In other words, we take all lattice points x and sum q to the power of their norm squared over 2. Now we have

$$j(\tau) = \frac{\theta(q)}{\eta(q)^2 4}$$

Quite a witches' brew of a formula, no? If someone could explain to me the deep inner reason for *why* this works, I'd be delighted, but right now I am clueless. I will say this, though: we could replace L with any other even unimodular lattice in 24 dimensions and get a function differing from j only by a constant. Guess how many even unimodular lattices there are in 24 dimensions? Why, 24, of course! These "Niemeier lattices" were classified by Niemeier in 1968. All but one of them have vectors with length squared equal to 2, but there is one whose shortest vector has length squared equal to 4, and that's the Leech lattice. This one has a very charming relation to 26-dimensional spacetime, described in "Week 20".

Since the constant term in j can be changed by picking different lattices in 24 dimensions, and constant functions aren't very interesting anyway, we can say that the first interesting coefficient in the above power series for j is 196884. Then, right around when the Monster was being dreamt up, McKay noticed that the dimension of its smallest nontrivial representation, namely 196883, was suspiciously similar. Coincidence? No. It turns out that all the coefficients of j can be computed from the dimensions of the irreducible representations of the Monster! Similarly, Ogg noticed in the study of the modular group, the primes 2, 3, 5, 7, 11, 13, 17, 19, 23, 29, 31, 41, 47, 59 and 71 play a special role. He went to a talk on the Monster and noticed the "coincidence". Then he wrote a paper offering a bottle of Jack Daniels to anyone who could explain it. This was the beginning of a subject called "Monstrous Moonshine"... the mysterious relation between the Monster and the modular group.

Well, as it eventually turned out, one way to get ahold of the Monster is as a group of symmetries of a certain algebra of observables for a string theory, or more precisely, a "vertex operator algebra":

5) Igor Frenkel, James Lepowsky, and Arne Meurman, *Vertex Operator Algebras and the Monster*, Academic Press, Boston, 1988.

The relation of string theory to modular invariance and 26 dimensional spacetime then "explains" some of the mysterious stuff mentioned above. (By the way, the authors of the above book say the fact that there are 26 sporadic finite simple groups is just a coincidence. I'm not so sure myself... not that I understand any of this stuff, but it's just too spooky how the number 26 keeps coming up all over!)

Anway, now let me fast-forward to some recent news. I vaguely gather that people would like to explain the relation between the Monster and string theory more deeply, by finding a 24-dimensional manifold having the Monster as symmetries, and cooking up a field theory of maps from the string worldsheet to this "Monster manifold", so that the associated vertex operator algebra would have a good reason for having the Monster as symmetries. Apparently Hirzebruch has offered a prize for anyone who could do this in a nice way, by finding a "24-manifold with $p_1 = 0$ whose Witten genus is $(j - 744)\Delta$" on which the Monster acts. Recently, Mike Hopkins at MIT and Mark Mahowald at Northwestern have succeeded in doing the first part, the part in quotes above. They haven't gotten a Monster action yet. Their construction uses a lot of homotopy theory.

I don't have much of a clue about any of this stuff, but Allen Knutson suggests that I read

6) Friedrich Hirzebruch, Thomas Berger, and Rainer Jung, *Manifolds and modular forms*, translated by Peter S. Landweber, pub. Braunschweig, Vieweg, 1992.

for more about this "Witten genus" stuff. He also has referred me to the following articles by Borcherds:

7) Richard E. Borcherds, "The Monster Lie-algebra", *Adv. Math.* **83** (1990), 30–47.

Richard E. Borcherds, "Monstrous Moonshine and monstrous Lie-superalgebras", *Invent. Math.* **109** (1992), 405–444.

For your entertainment and edification I include the abstract of the second one below:

We prove Conway and Norton's moonshine conjectures for the infinite dimensional representation of the monster simple group constructed by Frenkel, Lepowsky and Meurman. To do this we use the no-ghost theorem from string theory to construct a family of generalized Kac-Moody superalgebras of rank 2, which are closely related to the monster and several of the other sporadic simple groups. The denominator formulas of these superalgebras imply relations between the Thompson functions of elements of the monster (i.e. the traces of elements of the monster on Frenkel, Lepowsky, and Meurman's representation), which are the replication formulas conjectured by Conway and Norton. These replication formulas are strong enough to verify that the Thompson functions have most of the "moonshine" properties conjectured by Conway and Norton, and in particular they are modular functions of genus 0. We also construct a second family of Kac-Moody superalgebras related to elements of Conway's sporadic simple group Co1. These superalgebras have even rank between 2 and 26; for example two of the Lie algebras we get have ranks 26 and 18, and one of the superalgebras has rank 10. The denominator formulas of these algebras give some new infinite product identities, in the same way that the denominator formulas of the affine Kac-Moody algebras give the Macdonald identities.

Week 67

October 23, 1995

I'm pretty darn busy now, so the forthcoming Weeks will probably be pretty hastily written. This time I'll mainly write about quantum gravity.

1) Margaret Wertheim, *Pythagoras' Trousers: God, Physics, and the Gender Wars*, Times Books/Random House, New York, 1995.

I enjoyed this book, despite or perhaps because of the fact that it may annoy lots of physicists. It notes that, starting with Pythagoras, theoretical physics has always had a crypto-religious aspect. With Pythagoras it was obvious; he seems to have been the leader of a special sort of religious cult. With people like Kepler, Newton and Einstein it is only slightly less obvious. The operative mythology in every case is that of the mage. Think of Einstein, stereotypically with long white hair (though most of best work was actually done before his hair got white), a powerful but benign figure devoted to finding out "the thoughts of God". The mage, of course, is typically male, and Wertheim argues that this makes it harder for women to become physicists. (A lot of the same comments would apply to mathematics.) It is not a very scholarly book, but I wouldn't dismiss it.

2) Stephen W. Hawking, Virtual black holes, available as hep-th/9510029.

Hawking likes the "Euclidean path-integral approach" to quantum gravity. The word "Euclidean" is a horrible misnomer here, but it seems to have stuck. It should really read "Riemannian", the idea being to replace the Lorentzian metric on spacetime by one in which time is on the same footing as space. One thus attempts to compute answers to quantum gravity problems by integrating over all Riemannian metrics on some 4-manifold, possibly with some boundary conditions. Of course, this is tough — impossible so far — to make rigorous. But Hawking isn't scared; he also wants to sum over all 4-manifolds (possibly having a fixed boundary). Of course, to do this one needs to have some idea of what "all 4-manifolds" are. Lots of people like to consider wormholes, which means considering 4-manifolds that aren't simply connected. Here, however, Hawking argues against wormholes, and concentrates on simply-connected 4-manifolds. He writes: "Barring some pure mathematical details, it seems that the topology of simply connected four-manifolds can be essentially represented by gluing together three elementary units, which I call bubbles. The three elementary units are $S^2 \times S^2$, $\mathbb{CP}^2$, and $K3$. The latter two have orientation reversed versions, $-\mathbb{CP}^2$ and $-K3$. $S^2 \times S^2$ is just the product of the 2-dimensional sphere with itself, and he argues that this sort of bubble corresponds to a virtual black hole pair. He considers the effect on the Euclidean path integral when you have lots of these around (i.e., when you take the connected sum of S^4 with lots of these). He argues that particles scattering off these lose quantum coherence, i.e., pure states turn to mixed states. And he argues that this effect is very small at low energies *except* for scalar fields, leading him to predict that we may never observe the Higgs particle! Yes, a real honest particle physics prediction from quantum gravity! As he notes,"unless quantum gravity can make contact with observation, it will become as academic as arguments about how many angels can dance on the head of a pin". I suspect he also realizes that he'll never get a Nobel prize unless he goes out on a limb like this. He also gives an argument for why the"θ angle" measuring CP violation by the strong force may be zero. This parameter sits in front of a term in the Standard Model Lagrangian; there seems to be no good reason for it to be zero, but measurements of the neutron electric dipole moment show that it has to be less than 10^{-9}, according to the following book...

3) Kerson Huang, *Quarks, Leptons, and Gauge Fields*, World Scientific Publishing Co., Singapore, 1982.

Perhaps there are better bounds now, but this book is certainly one of the nicest introductions to the Standard Model, and if you want to learn about this "θ angle" stuff, it's a good place to start.

Anyway, rather than going further into the physics, let me say a bit about the "pure mathematical details". Here I got some help from Greg Kuperberg, Misha Verbitsky, and Zhenghan Wang (via Xiao-Song Lin, a topologist who is now here at Riverside). Needless to say, the mistakes are mine alone, and corrections and comments are welcome!

First of all, Hawking must be talking about homeomorphism classes of compact oriented simply-connected smooth 4-manifolds, rather than diffeomorphism classes, because if we take the connected sum of 9 copies of $\mathbb{CP}^2$ and one of $-\mathbb{CP}^2$, that has infinitely many different smooth structures. Why the physics depends only on the homeomorphism class is beyond me... maybe he is being rather optimistic. But let's follow suit and talk about homeomorphism classes. Folks consider two cases, depending on whether the intersection form on the second cohomology is even or odd. If our 4-manifold has an odd intersection form, Donaldson showed that it is an connected sum of copies of $\mathbb{CP}^2$ and $-\mathbb{CP}^2$. If its intersection form is even, we don't know yet, but if the "11/8 conjecture" is true, it must be a connected sum of $K3$'s and $S^2 \times S^2$'s. Here I cannot resist adding that $K3$ is a 4-manifold whose intersection is $\mathrm{E}_8 \oplus \mathrm{E}_8 \oplus H \oplus H \oplus H$, where H is the 2×2 matrix

$$\begin{pmatrix} 0 & 1 \\ 0 & 1 \end{pmatrix}$$

and E_8 is the nondegenerate symmetric 8×8 matrix describing the inner products of vectors in the wonderful lattice, also called E_8, which I discussed in "Week 65"! So E_8 raises its ugly head yet again.... By the way, H is just the intersection form of $S^2 \times S^2$, while the intersection form of $\mathbb{CP}^2$ is just the 1×1 matrix (1).

Even if the 11/8 conjecture is not true, we could if necessary resort to Wall's theorem, which implies that any 4-manifold becomes homeomorphic — even diffeomorphic — to a connected sum of the 5 basic types of "bubbles" after one takes its connected sum with sufficiently many copies of $S^2 \times S^2$. This suggests that if Euclidean path integral is dominated by the case where there are lots of virtual black holes around, Hawking's arguments could be correct at the level of diffeomorphism, rather than merely homeomorphism. Indeed, he says that "in the wormhole picture, one considered metrics that were multiply connected by wormholes. Thus one concentrated on metrics [I'd say topologies!] with large values of the first Betti number[....] However, in the quantum bubbles picture, one concentrates on spaces with large values of the second Betti number."

4) Ted Jacobson, "Thermodynamics of spacetime: the Einstein equation of state", available as `gr-qc/9504004`.

Well, here's another paper on quantum gravity, also very good, which seems at first to directly contradict Hawking's paper. Actually, however, I think it's another piece in the puzzle. The idea here is to derive Einstein's equation from thermodynamics! More precisely, "The key idea is to demand that this relation hold for all the local Rindler causal horizons through each spacetime point, with [the change in heat] and [the temperature] interpreted as the energy flux and Unruh temperature seen by an accelerated observer just inside the horizon. This requires that gravitational lensing by matter energy distorts the causal structure of spacetime in just such a way that the Einstein equation holds". It's a very clever mix of classical and quantum (or semiclassical) arguments. It suggests that all sorts of quantum theories on the microscale could wind up yielding Einstein's equation on the macroscale.

5) Lee Smolin, "The Bekenstein bound, topological quantum field theory and pluralistic quantum field theory", available as gr-qc/9508064.

This is a continued exploration of the themes of Smolin's earlier paper, reviewed in "Week 56" and "Week 57". Particularly interesting is the general notion of "pluralistic quantum field theory", in which different observers have different Hilbert spaces. This falls out naturally in the n-categorical approach pursued by Crane (see "Week 56"), which I am also busily studying.

6) Rodolfo Gambini, Octavio Obregon and Jorge Pullin, "Towards a loop representation for quantum canonical supergravity", available as hep-th/9508036.

Some knot theorists and quantum group theorists had better take a look at this! This paper considers the analog of $\mathrm{SU}(2)$ Chern-Simons theory where you use the supergroup $G\mathrm{SU}(2)$, and perturbatively work out the skein relations of the associated link invariant (up to a certain low order in $\hbar$). If someone understood the quantum supergroup "quantum $G\mathrm{SU}(2)$", they could do this stuff nonperturbatively, and maybe get an interesting invariant of links and 3-manifolds, and make some physicists happy in the process.

7) Roh Suan Tung and Ted Jacobson, "Spinor one-forms as gravitational potentials", available as gr-qc/9502037.

This paper writes out a new Lagrangian for general relativity, closely related to the action that gives general relativity in the Ashtekar variables. It's incredibly simple and beautiful! I am hoping that if I work on it someday, it will explain to me the mysterious relation between quantum gravity and spinor fields (see the end of "Week 60").

8) Joseph Polchinski and Edward Witten, "Evidence for heterotic — type I string duality", available as hep-th/9510169.

I'm no string theorist, so I've been lagging vastly behind the new work on "dualities" that has revived interest in the subject. Roughly speaking, though, it seems folks have discovered a host of secret symmetries relating superficially different string theories... making them, in some deeper sense, the same. The heterotic and type I strings are two of the most famous string theories, so if they were really equivalent as this paper suggests, it would be very interesting.

Week 68

October 29, 1995

Okay, now the time has come to speak of many things: of topoi, glueballs, communication between branches in the many-worlds interpretation of quantum theory, knots, and quantum gravity.

1) Robert Goldblatt, *Topoi: the Categorial Analysis of Logic*, Studies in logic and the foundations of mathematics vol. **98**, North-Holland, New York, 1984.

If you've ever been interested in logic, you've got to read this book. Unless you learn a bit about topoi, you are really missing lots of the fun. The basic idea is simple and profound: abstract the basic concepts of set theory, so as to define the notion of a "topos", a kind of universe like the world of classical logic and set theory, but far more general!

For example, there are "intuitionistic" topoi in which Brouwer reigns supreme — that is, you can't do proof by contradiction, you can't use the axiom of choice, etc.. There is also the "effective topos" of Hyland in which Turing reigns supreme — for example, the only functions are the effectively computable ones. There is also a "finitary" topos in which all sets are finite. So there are topoi to satisfy various sorts of ascetic mathematicians who want a stripped-down, minimal form of mathematics.

However, there are also topoi for the folks who want a mathematical universe with lots of horsepower and all the options! There are topoi in which everything is a function of time: the membership of sets, the truth-values of propositions, and so on all depend on time. There are topoi in which everything has a particular group of symmetries. Then there are *really* high-powered things like topoi of sheaves on a category equipped with a Grothendieck topology. . . .

And so on: not an attempt to pick out "the" universe of logic and mathematics, but instead, an effort to systematically examine a bunch of them and how they relate to each other. The details can be intimidating, but Goldblatt explains them very nicely. A glance at the subject headings reveal some of the delights in store: "elementary truth", "local truth", "geometric logic", etc..

What is a topos, precisely? Well, most people would need to limber up a little bit before getting the precise definition. . . so let me just start you off with some mental stretching exercises. In classical logic we are used to working with two truth values, True and False. Let's call the set of truth values Ω, just to make it sound impressive — and because it's traditional in topos theory. So, we are used to doing all our logic with

$$\Omega = \{\mathrm{True}, \mathrm{False}\}.$$

In set theory, one of the things we do with Ω is describe subsets of a given set X. In other words, to describe a subset Y of X, we can say for each member of X, whether it is True or False that it is a member of Y. Thus we can describe the subset Y by giving a function

$$f\colon X \to \Omega.$$

We say x is in Y if $f(x) = \mathrm{True}$, but x is not in Y if $f(x) = \mathrm{False}$.

Now say we wanted to describe a topos of "time-dependent sets". But instead of "time-dependent sets", let's act like topos theorists and call them simply "objects", and instead of talking about one being a "subset" of another, let's say one is a "subobject" of another. To keep life simple, let's consider only two times: today and tomorrow. So we can think of an object X in this topos as a pair (X_1, X_2) of sets: one set X_1 today, and another set X_2 tomorrow. We say that an object Y is a "subobject" of X if Y_1 is a subset of X_1 and Y_2 is a subset of X_2. The idea is that we want Y to be contained in X both today and tomorrow.

Now, to describe a subobject Y of X, we can what's in Y today, and also what's in Y tomorrow. We would like to do so using some kind of function, or what topos theorists call a "morphism",

$$f : X \to \Omega.$$

Clearly we can't do this with our old truth values set $\{\mathrm{True}, \mathrm{False}\}$. Instead, we should use a truth values *object* Ω that keeps track of what's true or false today and what's true or false tomorrow. In other words, Ω should now be the pair of sets

$$(\{\mathrm{True}, \mathrm{False}\}, \{\mathrm{True}, \mathrm{False}\})$$

Now what is that "morphism" f exactly? Well, it's like one function today and another function tomorrow, or in other words, a pair of functions! In general, a morphism $f\colon S \to T$ between objects in this topos is a pair of functions (f_1, f_2), with $f_1\colon S_1 \to T_1$ and $f_2\colon S_2 \to T_2$. Then in our particular case, the morphism $f\colon X \to \Omega$ will say which elements of X_1 are in Y_1, and which elements of X_2 are in Y_2.

This is a pretty simple example of what the objects and morphisms in a topos can be like. They can be a lot weirder. The key thing is that you can do a lot of the same things with them that you can do with sets and functions. Also, you can do a lot of the same things with Ω that you can with $\{\mathrm{True}, \mathrm{False}\}$. Note that in our example, like in the classical example where $\Omega = \{\mathrm{True}, \mathrm{False}\}$, the subobject classifier has a bunch of logical operations on it: morphisms like

$$\mathrm{Not}\colon \Omega \to \Omega \mathrm{And} \quad : \Omega \times \Omega \to \Omega \mathrm{Or}\colon \Omega \times \Omega \to \Omega$$

and so on. In our example, and in the classical example, these make Ω into what folks call a boolean algebra. Basically, all the usual rule of logic apply. In general, though, Ω only needs to be a Heyting algebra. This is more general than a boolean algebra, and it can be sort of intuitionistic in flavor; for example, NotNot doesn't need to equal the identity morphism $1\colon \Omega \to \Omega$, so proof by contradiction doesn't necessarily work. A typical example of a Heyting algebra Ω is the set of open sets in a topological space, with And and Or being intersection and union, and with Not being the *interior* of the complement. This gives a little hint as to what topoi have to do with topology.

After studying this sort of thing a while, it's rather hard to go on pretending that the Zermelo-Fraenkel axioms of set theory, which were cooked up in the early 20th century to escape the logical paradoxes of Russell and others, are the last word on "foundations". One can develop topos theory within set theory if one wishes, but one can also set up topos theory from scratch, as a kind of pluralistic foundation of mathematics.

For a deeper but still friendly and expository introduction to topoi, try

2) Saunders Mac Lane and Ieke Moerdijk, *Sheaves in Geometry and Logic: A First Introduction to Topos Theory*, Springer-Verlag, New York, 1992.

Here you can learn about "Brouwer's theorem: all functions are continuous" (in a suitably intuitionistic topos, of course). You can also learn topos-theoretic versions of Cohen's proofs of the independence of the continuum hypothesis and the axiom of choice.

Goldblatt's book teaches you all the category theory you need to learn about topoi. . . but for people who already know some category theory, let me give the precise definition of a topos (or more precisely, an elementary topos, to distinguish it from a "Grothendieck topos"): it's a category with finite limits and power objects. This automatically implies a lot of things, such as the existence of the subobject classifier Ω that I was talking about.

To get deeper into topos theory, try:

3) Michael Barr and Charles Wells, *Toposes, Triples and Theories*, Springer-Verlag, New York, 1983. Available for free electronically at `http://www.cwru.edu/artsci/math/wells/pub/ttt.html`

Now let me catch up on some things more directly related to physics:

4) Frank Close, "Are glueballs and hybrids found?", available as `hep-ph/9509245`. To appear in Proceedings of Hadron95.

J. Sexton, A. Vaccarino, D. Weingarten, "Numerical evidence for the observation of a scalar glueball", available as `hep-lat/9510022`.

Thanks go to Greg Kilcup for bringing these to my attention. Have they found a glueball??? That would be really exciting. What's a glueball, you ask? Well, quantum chromodynamics, our best theory of the strong force, says that that the strong force is carried by particles called "gluons". Like electromagnetism, the strong force is a gauge field, but it's a nonabelian gauge field, so the gluons themselves have charge, or "color". Thus they interact in a nonlinear way. This is what lets them bind together quarks in such a tight way. But perhaps, in addition to pairs of quarks and antiquarks held together by gluons — i.e., mesons — and triples of quarks held together by gluons — i.e., baryons — there could be short-lived assemblages consisting entirely of gluons, held together by their self-interactions. These are called glueballs, but we don't know if these exist.

However, to my surprise, it turns out that there are now some candidates out there! The first paper suggests that the $f_0(1500)$, a neutral spin-zero particle with mass around 1500 MeV, is a glueball. The second paper argues instead that this is basically a quark-antiquark pair (made of a strange quark and a strange antiquark. . . where "strange" is the technical name for one of the 6 quarks!). It presents evidence from a numerical simulation and argues that the "θ" or $f_J(1710)$, a neutral particle with even spin and mass 1710 MeV, is a glueball. Part of the subtlety here is that, thanks to the superposition principle, there is not a perfectly sharp distinction between a glueball with some virtual quark-antiquark pairs in it, and a quark-antiquark pair with a bunch of virtual gluons in it. There can be "hybrids" that are a bit of both a linear combination of a meson and a glueball! (This phenomenon of "hybridization" is also familiar in chemistry.)

It's tough to do nonperturbative computations in nonlinear gauge field theories — basically one needs to approximately compute a path integral, using Monte Carlo technique, approximating spacetime by a lattice (in this case, a $16 \times 16 \times 16 \times 24$ lattice). Computing the properties of a glueball and matching it with an experimentally observed particle would be a marvelous confirmation of quantum chromodynamics. In addition, I find there to be something charming about the idea that in a nonabelian gauge theory we could have a particle made simply of the gauge field itself.

5) R. Plaga, "Proposal for an experimental test of the many-worlds interpretation of quantum mechanics", preprint available as quant-ph/9510007.

John Gribbin brought this one to my attention and asked me what I thought about it. Basically, the idea here is to isolate an ion from its environment in an "ion trap", and then perform a measurement on with two possible outcomes on another quantum system, and to excite the ion only if the first outcome occurs, before the ion has had time to "decohere" or get "entangled" with the environment. Then one checks to see if the ion is excited. The idea is that even if we didn't see the outcome that made us excite the ion, we might see the ion excited, because it was excited in the other "world" or "branch" — the one in which we *did* see the outcome that made us excite the ion. The author gets fairly excited himself, suggesting that "outside physics, interworld communication would lead to truly mind-boggling possibilities".

Does this idea really make sense? First of all, I don't think of this sort of thing as a test of the many-worlds interpretation; I think that all sufficiently sensible interpretations of quantum mechanics (not necessarily *very* sensible, either!) give the same concrete predictions for all experiments, when it comes to what we actually observe. They may make us tell very different stories about what is happening behind the scenes, but not of any testable sort. As soon as one comes up with something that makes different predictions, I think it is (more or less by definition) not a new "interpretation" of quantum theory but an actual new theory. And I don't think the many-worlds interpretation is that.

So the question as I see it is simply, will this experiment work? Will we sometimes see the ion excited even when we didn't excite it? It seems hard; usually the decoherence between the two "branches" prevents this kind of "inter-world communication" (not that I'm particularly fond of this way of talking about it). What exactly is supposed to make this case different? The problem is that the paper is quite sketchy at the crucial point. . . just when the rabbit being pulled from the hat, as it were. I haven't put much time into analyzing it, but some people interested in this sort of thing might enjoy having a go at it.

6) Nicholas Landsman, "Against the Wheeler-DeWitt equation", preprint available as gr-qc/9510033.

I haven't read this one yet, but I had some nice talks with Landsman about his ideas on quantization of constrained systems (see "Week 60") back when I was in Cambridge, England. Quantizing constrained systems is the main problem with the so-called "canonical" approach to quantum gravity (see "Week 43"), so I was eager to see it applied to gravity, and I guess that's what he's done. The title of the paper is deliberately provocative. . . hmmm, I guess I'd better read it soon! Here's the abstract:

The ADM approach to canonical general relativity combined with Dirac's method of quantizing constrained systems leads to the Wheeler-DeWitt equation. A number of mathematical as well as physical difficulties that arise in connection with this equation may be circumvented if one employs a covariant Hamiltonian method in conjunction with a recently developed, mathematically rigorous technique to quantize constrained systems using Rieffel induction. The classical *constraints are cleanly separated into four components of a covariant momentum map coming from the diffeomorphism group of spacetime, each of which is linear in the canonical momenta, plus a single finite-dimensional quadratic constraint that arises in any theory, parametrized or not. The new quantization method is carried through in a minisuperspace example, and is found to produce a "wavefunction of the universe". This differs from the proposals of both Vilenkin and Hartle-Hawking for a closed FRW universe, but happens to coincide with the latter in the open case.*

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7) Pavel Etingof and David Kazhdan, "Quantization of Lie bialgebras, I", preprint available in AMSTeX form as q-alg/9506005.

   "Quantization of Poisson algebraic groups and Poisson homogeneous spaces", preprint available in AMSTeX form as q-alg/9510020.

It sounds like Etinghof and Kazhdan are making serious progress on some questions of Drinfeld about when you can quantize Lie bialgebras and their kin. More stuff I need to read! I need to invent a time machine and keep running it backwards to make my weekends longer and read this stuff!

8) Steve Carlip, "Statistical mechanics and black hole entropy", preprint available as gr-qc/9509024.

   Claudio Teitelboim, "Statistical thermodynamics of a black hole in terms of surface fields", preprint available as hep-th/9510180.

Steve Carlip's paper is a nice introduction to recent ideas, many of them his, on deriving black hole area/entropy relations by thinking of the entropy as associated to degrees of freedom of a field living on the event horizon. I haven't read Teitelboim's paper, but it sounds related.

9) Jorge Griego, "Is the third coefficient of the Jones knot polynomial a quantum state of gravity?", preprint available as gr-qc/9510051.

   "The Kauffman bracket and the Jones polynomial in quantum gravity", preprint available as gr-qc/9510050.

In the loop representation of quantum gravity, states of quantum gravity give rise to link invariants. Which link invariants, though? The Kauffman bracket comes from a state of quantum gravity with cosmological constant... that is something I understand pretty well by now. But Gambini and Pullin also have an argument suggesting that the second coefficient of the Jones polynomial (also known as the Arf invariant) is a state of quantum gravity without cosmological constant. I've tried to make this argument more rigorous and never succeeded. They also floated a conjecture that *all* the coefficients of the Jones polynomial are states of quantum gravity. This confuses me a lot, because the Jones polynomial depends on the orientations of the components of a link, while states of quantum gravity should give link invariants that are independent of orientations. I guess all the odd coefficients of the Jones polynomial are orientation dependent. Thus I'm not shocked that Griego has done calculations indicating that the third coefficient does *not* come from a state of quantum gravity.

Week 69

November 11, 1995

One of the great things about starting to work on quantum gravity was getting to know some of the people in the field. Ever since the development of string theory and the loop representation of quantum gravity, there has been a fair amount of interest in understanding how quantum theory and gravity fit together. Indeed, now that the Standard Model seems to be giving a spectacularly accurate description of all the forces *except* gravity, quantum gravity is one of the few really big mysteries left when it comes to working out the basic laws of physics — or at least, one of the few *obvious* big mysteries. (As soon as one mystery starts becoming less mysterious, new mysteries tend to become more visible.) But back when particle physics was big business, only a few rather special sorts of people were seriously devoted to quantum gravity. These people seem to be often more than averagely interested in philosophy, often more interested in mathematics (which is one of the few solid handholds in this slippery subject), and always more resigned to the fact that Nature does not reveal all her secrets very readily.

One of these folks is Chris Isham, whom I first saw at a conference in Seattle in 1991. The conference was on classical field theory but somehow he, Abhay Ashtekar, and Renate Loll sneaked in and gave some talks on the loop representation of quantum gravity. This is when I first became really interested in this subject, which I was later to work on quite a bit. I remember Isham saying how he had been working on quantum gravity for many years, and that he'd gotten used to the fact that nothing ever worked, but that *this* approach *seemed* to be working so far. He went on to talk about work he'd done with Ashtekar on making the loop representation rigorous, which was based on Gelfand-Naimark spectral theory. He said that as a student, when he'd learned about this theory, he was really excited, because it completely depends on the fact that if we have a space X, we can think of any point x in X as a functional on the space of functions on X, basically defining by defining $x(f)$ to be $f(x)$. He said this with a laugh, but I knew what he meant, because I too had found this idea tremendously exciting when I first learned the Gelfand-Naimark theory. I guess it's something about how what seems at first like some sort of bizarre joke can turn out to be very useful. . . .

Anyway, later, when I decided to work on this sort of thing and was trying to learn more about quantum gravity, I found his review article on the problem of time (see "Week 9") tremendously helpful, and I constantly recommend it to everyone who is trying to get their teeth into this somewhat elusive issue. So it's not surprising that Isham figures prominently in the following nice popular article on the problem of time:

1) Marcia Bartusiak, "When the universe began, what time was it?", *Technology Review* (edited at the Massachusetts Institute of Technology), November/December 1995, pp. 54-63.

If you can find this, read it: it also features Karel Kuchar and Carlo Rovelli.

This spring, I visited Isham at Imperial College in London and found him to be just as interesting in person as in print, and not at all scary. . . a bit of an cynic about all existing approaches to quantum gravity (probably because he sees so clearly how flawed they all are), but thoroughly good-humored about it and perfectly open to all sorts of ideas, even my own nutty ideas about n-categories and physics.

Anyway, Isham has recently written a review article on quantum gravity that gives a nice overview of the basic issues of the field:

2) C. J. Isham, "Structural issues in quantum gravity", plenary session lecture given at the GR14 conference, Florence, August 1995, preprint available as gr-qc/9510063.

One interesting thing about it is the emphasis on the question of whether spacetime is really a manifold the way we all usually think, or perhaps something that just looks like a manifold at sufficiently large distance scales. This is one of those fundamental issues that is rather hard to make direct progress on; one has to sort of sneak up on it, but it's nice to see someone boldly holding the problem up for examination. Often the most important issues are the ones everyone is scared to talk about, because they are so intractable.

Much of Abhay Ashtekar's early work dealt with asymptotically flat solutions of Einstein's equation, but in about 1986 he somehow invented a new formulation of general relativity, which everyone now calls the "new variables" or "Ashtekar variables". In terms of these new variables general relativity looks a whole lot more like Yang-Mills theory (the theory of all the forces *except* gravity), and this let Rovelli and Smolin formulate a radical new approach to quantum gravity, the "loop representation". (For a fun, non-technical introduction to this, try the article by Bartusiak reviewed in "Week 10".)

Nowadays, Ashtekar is the main person behind the drive to make the loop representation of quantum gravity into a mathematically rigorous theory. Thus it's natural that after that first time in Seattle I would wind up seeing him pretty often. . . first at Syracuse University and then at the Center for Gravitational Physics and Geometry which he started at Penn State. It's really impressive how he has organized people into an effective team there. . . and how he is systematically converting people's hopes and dreams concerning the loop representation into a beautiful set of rigorous *theorems*. For a good mathematical introduction to his program, see his paper reviewed in "Week 7". A less mathematical introduction is:

3) Abhay Ashtekar, "Polymer geometry at Planck scale and quantum Einstein equations".

This will probably appear on gr-qc in a while.

I have also seen Renate Loll fairly often in the years since that Seattle conference. She is younger than Ashtekar and Isham (in fact, she was a postdoc with Isham at one point), hence less intimidating to me, which meant that I really enjoyed pestering her with stupid questions when I was just starting to learn about this loop representation stuff. One of her specialities is lattice gauge theory, and recently she has developed a lattice version of quantum gravity that is eminently suitable for computer calculations. The last time I saw her was at a conference in Warsaw this spring (as reported in "Week 55" and "Week 56"). In the process of working on her lattice approach, she gave Rovelli and Smolin a big shock by turning up an error in their computation of the volume operator in quantum gravity. A state of quantum gravity can be visualized roughly as a graph embedded in space, with edges labelled by spins. Rovelli and Smolin had thought there were states of nonzero volume corresponding to graphs with only trivalent vertices (3 edges meeting a vertex, that is). As it turns out, they'd made a sign error, and these states have zero volume; you need a quadrivalent vertex to get some volume. She has just written a paper on this topic:

4) Renate Loll, "Spectrum of the volume operator in quantum gravity", 14 pages in plain tex, with 4 figures (postscript, compressed and uu-encoded), available as gr-qc/9511030.

The abstract reads as follows:

> *The volume operator is an important kinematical quantity in the non-perturbative approach to four-dimensional quantum gravity in the connection formulation. We give a general algorithm for computing its spectrum when acting on four-valent spin network states, evaluate some of the eigenvalue formulae explicitly, and discuss the role played by the Mandelstam constraints.*

Quote of the week:

> "Nothing is too wonderful to be true, if it be consistent with the laws of nature, and in such things as these, experiment is the best test of such consistency."
>
> *Faraday, laboratory diaries, entry 10,040, March 19, 1849.*

Week 70

November 26, 1995

Probably many of the mathematicians reading this know about the Newton Institute in Cambridge, a mathematics institute run by Sir Michael Atiyah. It's a cozy little building, in a quiet neighborhood a certain distance from the center of town, which one can reach by taking a nice walk or bike ride over the bridge near Trinity College, across Grange Road, and down Clarkson Road. Inside it's one big space, with stairways slightly reminiscent of a certain picture by Escher, with a nice little library on the first floor, tea and coffee on the 3rd floor, blackboards in the bathrooms... everything a mathematician could want. This is where Wiles first announced his proof of Fermat's last theorem, and they sell T-shirts there commemorating that fact, which are unfortunately too small to contain the proof itself... as they do not refrain from pointing out.

I just got back from a conference there on New Connections between Mathematics and Computer Science. It was organized by Jeremy Gunawardena, who was eager to expose computer scientists and mathematicians to a wide gamut of new interactions between the two subjects. I spoke about n-categories in logic, topology and physics. Since I don't know anything about computer science, when I first got the invitation I thought it was a mistake: a wrong email address or something! But Gunawardena assured me otherwise. I assumed the idea was that n-categories, being so abstract, must have *some* application to just about *everything*, even computer science. Luckily, some other speakers at the conference gave some very nice applications of n-category theory to computer science, so now I know they really exist.

Unfortunately I had to miss the beginning of the conference, and therefore missed some interesting talks of a geometrical nature by Smale, Gromov, Shub and others. Let me say a bit about some of the talks I did catch. You can find a list of all the speakers and abstracts of their talks at

1) *Basic Research Institute in the Mathematical Sciences*, New Connections web page, 'http://www-uk.hpl.hp.com/brims/"

Richard Jozsa gave an interesting talk on quantum computers, in part outlining Peter Shor's work (see "Week 34") on efficient factoring via quantum computation, but also presenting some new results on "counterfactual quantum computation". It turns out that — in principle — in some cases you can get a quantum computer to help you answer a question, even without running it, just as long as you COULD HAVE run it! (I should add that in practice a lot of things make this quite impractical.) This is a new twist on the Elitzur-Vaidman bomb-testing paradox about how if you have a bunch of bombs and half of them are duds, and the only way you can test a bomb is by lighting the fuse and seeing if it goes off, you can still get a bomb you're sure will work, if you use quantum mechanics. The trick involves getting a fuse that's so sensitive that even one photon will make the bomb go off, and then setting up a beam-splitter, and using the bomb to measure which path the photon followed, before recombining the beams. Check out:

2) A. C. Elitzur and L. Vaidman, "Quantum mechanical interaction-free measurements", *Foundations of Phys.* **23** (1993), 987–997.

Graeme Mitchison and Richard Jozsa, Counterfactual quantum computation, *Proc. Roy. Soc. Lond.* **A457** (2001) 1175–1194. Also available as quant-ph/9907007.

Jean-Yves Girard gave an overview of linear logic. Linear logic is a new version of logic that he invented, which has some new operations besides the good old ones like "and", "or", and "not". For example, there are things like "par" (written as an upside-down ampersand), "!" (usually pronounced "bang") and "?". Ever since I started going to conferences on category theory and computer science I have been hearing a lot about it, and I keep trying to get people to explain these weird new logical operations to me. Unfortunately, I keep getting very different answers, so it has remained rather mysterious to me, even though it seems like a lot of fun (see "Week 40"). Thus I was eager to hear it from the horse's mouth.

Indeed, Girard gave a fascinating talk on it which almost made me feel I understood it. I think the big thing I'd been missing was a good appreciation of topics in proof theory like "cut elimination". He noted that this subject usually appears to be all about the precise manipulation of formulas according to purely syntactic rules: "Very bureaucratic" he joked, "one parenthesis missing and you've had it!" (For full effect, one must imagine this being said in a French accent by someone stylishly dressed entirely in black.) He wanted to get a more *geometrical* way to think about proofs, but to do this it turned out to be important to refine ordinary logic in certain ways.... leading to linear logic. However, I still don't feel up to explaining it, so let me turn you to:

3) Jean-Yves Girard, "Linear logic", *Theoretical Computer Science* **50**, 1–102, 1987.

Jean-Yves Girard, Y. Lafont and P. Taylor, *Proofs and Types*, Cambridge Tracts in Theoretical Computer Science **7**, Cambridge U. Press, 1989. Also available at http://www.cs.man.ac.uk/~pt/stable/Proofs+Types.html

Eric Goubault and Vaughan Pratt talked, in somewhat different ways, about a formalism for treating concurrency using "higher-dimensional automata". The basic idea is simple: say we have two jobs to do, one of which gets us from some starting-point A to some result B, and the other of which gets us from A' to B'. We can represent each task by an arrow, as follows:

$$A \longrightarrow B$$
$$A' \longrightarrow B'$$

We can think of this arrow as a "morphism", that is, a completely abstract sort of operation taking A to B. Or, we can think of it more concretely as an interval of time, where our computer is doing something at each moment. Alternatively, we can think of it more discretely as a sequence of steps, starting with A and winding up with B.

If we now consider doing both these tasks concurrently, we can represent the situation by a square:

$$\begin{array}{ccc} AA' & \longrightarrow & BA' \\ \downarrow & & \downarrow \\ AB' & \longrightarrow & BB' \end{array}$$

Going first across and then down corresponds to completing one task before starting the other, while going first down and then across corresponds to doing the other one first. However, we can also imagine various roughly diagonal paths through the square, corresponding to doing both tasks at the same time. We might go horizontally for a while, then vertically, then diagonally, and so on. Of course, if the two tasks were not completely independent — for example, if some steps of one could only occur after some steps of the other were finished — we would have some constraints on what paths from AA' to BB' were allowed. The idea is then to model these constaints as "holes" in the square, forbidden regions where the path cannot go. There may then be several "essentially distinct" ways of getting from AA' to BB', that is, classes of paths that cannot be deformed into each other.

To anyone who knows homotopy theory, this will seem very familiar, homotopy theory being all about spaces with holes in them, and how those holes prevent you from continuously deforming one path into another. Goubault's title, "Scheduling problems and homotopy theory", emphasized the relationships. But there are also some big differences. Unlike homotopy theory, here the paths are typically required to be "monotonic": they can't double back and go backwards in time. And, as I mentioned, the tasks can be thought of more abstractly than as paths in some space. So we are really talking about 2-categories here: they give a general framework for studying situations with "dots" or "objects", "arrows between dots" or "morphisms", and "arrows between arrows between dots" or "2-morphisms". Similarly, when we study concurrency with more than 2 tasks at a time we can think of it in terms of n-categories.

By the way, since I don't know much about parallel processing, I'm not sure how much the above formalism actually helps the "working man". Probably not much, yet. I get the impression, however, that parallel processing is a complicated problem, and that people are busily dreaming up new formalisms for talking about it, hoping they will eventually be useful for inventing and analyzing parallel programming languages.

Some references for this are:

4) Eric Goubault, Schedulers as abstract interpretations of higher-dimensional automata, in *Proc. PEPM '95 (La Jolla)*, ACM Press, 1995. Also available at `http://www.di.ens.fr/%7Egoubault/GOUBAULTpapers.html`

Eric Goubault and Thomas Jensen, "Homology of higher-dimensional automata", in *Proc. CONCUR '92 (New York)*, Lecture Notes in Computer Science **630**, Springer, 1992. Also available at `http://www.di.ens.fr/%7Egoubault/GOUBAULTpapers.html`

5) Vaughan Pratt, "Time and information in sequential and concurrent computation", in *Proc. Theory and Practice of Parallel Programming*, Sendai, Japan, 1994.

Yves Lafont also gave a talk with strong connections to n-category theory. Recall that a monoid is a set with an associative product having a unit element. One way to describe a monoid is by giving a presentation with "generators", say

$$a, b, c, d,$$

and "relations", say

$$ab = a, \quad da = ac.$$

We get a monoid out of this in an obvious sort of way, namely by taking all strings built from the generators a,b,c, and d, and then identifying two strings if you can get from one to the other by repeated use of the relations. In math jargon, we form the free monoid on the generators and then mod out by the relations.

Suppose our monoid is finitely presented, that is, there are finitely many generators and finitely many relations. How can we tell whether two elements of it are equal? For example, does

$$dacb = acc$$

in the above monoid? Well, if the two are equal, we will always eventually find that out by an exhaustive search, applying the relations mechanicallly in all possible ways. But if they are not, we may never find out! (For the above example, the answer appears at the end of this article in case anyone wants to puzzle over it. Personally, I can't stand this sort of puzzle.) In fact, there is no general algorithm for solving this "word problem for monoids", and in fact one can even write down a *specific* finitely presented monoid for which no algorithm works.

However, sometimes things are nice. Suppose you write the relations as "rewrite rules", that go only one way:

$$ab \to a$$

$$da \to ac$$

Then if you have an equation you are trying to check, you can try to repeatedly apply the rewrite rules to each side, reducing it to "normal form", and see if the normal forms are equal. This will only work, however, if some good things happen! First of all, your rewrite rules had better terminate: it had better be that you can only apply them finitely many times to a given string. This happens to be true for the above pair of rewrite rules, because both rules decrease the number of b's and c's. Second of all, your rewrite rules had better be confluent: it had better be that if I use the rules one way until I can't go any further, and you use them some other way, that we both wind up with the same thing! If both these hold, then we can reduce any string to a unique normal form by applying the rules until we can't do it any more.

Unfortunately, the rules above aren't confluent; if we start with the word dab, you can apply the rules like this

$$dab \to acb$$

while I apply them like this

$$dab \to da \to ac$$

and we both terminate, but at different answers. We could try to cure this by adding a new rule to our list,

$$acb \to ac.$$

This is certainly a valid rule, which cures the problem at hand... but if we foolishly keep adding new rules to our list this way we may only succeed in getting confluence and termination when we have an *infinite* list of rules:

$$\begin{aligned} ab &\to a \\ da &\to ac \\ acb &\to ac \\ accb &\to acc \\ acccb &\to accc \\ accccb &\to acccc \\ &\vdots \end{aligned}$$

and so on. I leave you to check that this is really terminating and confluent. Because it is, and because it's a very predictable list of rules, we can use it to write a computer program in this case to solve the word problem for the monoid at hand. But in fact, if we had been cleverer, we could have invented a *finite* list of rules that was terminating and confluent:

$$\begin{aligned} ab &\to a \\ ac &\to da \end{aligned}$$

Lafont's went on to describe some work by Squier:

6) Craig C. Squier, "Word problems and a homological finiteness condition for monoids", *Jour. Pure Appl. Algebra* **49** (1987), 201–217.

   Craig C. Squier, "A finiteness condition for rewriting systems", revision by F. Otto and Y. Kobayashi, to appear in *Theoretical Computer Science*.

   Craig C. Squier and F. Otto, "The word problem for finitely presented monoids and finite canonical rewriting systems", in *Rewriting Techniques and Applications*, ed. J. P. Jouannuad, Lecture Notes in Computer Science **256**, Springer, Berlin, 1987, 74-82.

which gave general conditions which must hold for there to be a finite terminating and confluent set of rewrite rules for a monoid. The nice thing is that this relies heavily on ideas from n-category theory. Note: we started with a monoid in which the relations are *equations*, but we then started thinking of the relations as rewrite rules or *morphisms*, so what we really have is a monoidal category. We then started worrying about "confluences", or equations between these morphisms. This is typical of "categorification", in which equations are replaced by morphisms, which we then want to satisfy new equations (see "Week 38").

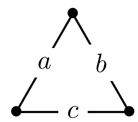
For the experts, let me say exactly how it all goes. Given any monoid M, we can cook up a topological space called its "classifying space" KM, as follows. We can think of KM as a simplicial complex. We start by sticking in one 0-simplex, which we can visualize as a dot like this:

$$\bullet$$

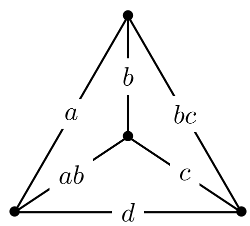
Then we stick in one 1-simplex for each element of the monoid, which we can visualize as an arrow going from the dot to itself. Unrolled a bit, it looks like this:

$$\bullet\!-\ a\ -$$

Really we should draw an arrow going from left to right, but soon things will get too messy if I do that, so I won't. Then, whenever we have $ab = c$ in the monoid, we stick in a 2-simplex, which we can visualize as a triangle like this:



Then, whenever we have $abc = d$ in the monoid, we stick in a 3-simplex, which we can visualize as a tetrahedron like this



And so on... This is a wonderful space whose homology groups depend only on the monoid, so we can call them $H_k(M)$. If we have a presentation of M with only finitely many generators, we can build KM using 1-simplices only for those generators, and it follows that $H_1(M)$ is finitely generated. (More precisely, we can build a space with the same homotopy type as KM using only the generators in our presentation.) Similarly, if we have a presentation with only finitely many relations, we can build KM using only finitely many 2-simplices, so $H_2(M)$ is finitely generated. What Squier showed is that if we can find a finite list of rewrite rules for M which is terminating and confluent, then we can build KM using only finitely many 3-simplices, so $H_3(M)$ is finitely generated! What's nice about this is that homological algebra gives an easy way to compute $H_k(M)$ given a presentation of M, so in some cases we can *prove* that a monoid has no finite list of rewrite rules for M which is terminating and confluent, just by showing that $H_3(M)$ is too big. Examples of this, and many further details, appear in Lafont's work:

7) Yves Lafont and Alain Proute, "Church-Rosser property and homology of monoids", *Mathematical Structures in Computer Science* **1** (1991), 297–326. Also available at `http://iml.univ-mrs.fr/~lafont/publications.html`

Yves Lafont, "A new finiteness condition for monoids presented by complete rewriting systems (after Craig C. Squier)", *Journal of Pure and Applied Algebra* **98** (1995), 229–244. Also available at `http://iml.univ-mrs.fr/~lafont/publications.html`

There were many other interesting talks, but I think I will quit here. Next time I want to talk a bit about topological quantum field theory. (Of course, folks who read "Week 38" will know that Lafont's work is deeply related to topological quantum field theory... but I won't go into that now.)

(Answer: $dacb = ddab = dda = dac = acc$.)

Week 71

December 3, 1995

This week I will get back to mathematical physics... but before I do, I can't resist adding that in my talk in that conference I announced that James Dolan and I had come up with a definition of weak n-categories. (For what these are supposed to be, and what they have to do with physics, see "Week 38", "Week 49", and "Week 53".) John Power was at the talk, and before long his collaborator Ross Street sent me some email from Australia asking about the definition. Gordon, Power, and Street have done a lot of work on n-categories — see "Week 29". Now, Dolan and I have been struggling for several months to put this definition onto paper in a reasonably elegant and comprehensible form, so Street's request was a good excuse to get something done quickly... sacrificing comprehensibility for terseness. The result can be found in

1) John Baez and James Dolan, "n-Categories, sketch of a definition", `http://math.ucr.edu/home/baez/ncat.def.html`

A more readable version will appear as a paper fairly soon. I should add that this will eventually be part of Dolan's thesis, and he has done most of the hard work on it; my role has largely been to push him along and get him to explain everything to me.

On to some physics...

First, it's amusing to note that Maxwell's equations are back in fashion! In the following papers you will see how the duality symmetry of Maxwell's equations (the symmetry between electric and magnetic fields) plays a new role in modern work on 4-dimensional gauge theory. Also, there is some good stuff in Thompson's review article about the Seiberg-Witten theory, which is basically just a $\mathrm{U}(1)$ gauge theory like Maxwell's equations... but with some important extra twists!

2) Erik Verlinde, "Global aspects of electric-magnetic duality", *Nuc. Phys.* **B455** (1995), 211–225, available as `arXiv:hep-th/9506011`.

   George Thompson, "New results in topological field theory and abelian gauge theory", 64 pages, available as `arXiv:hep-th/9511038`.

Next, it's nice to see that work on the loop representation of quantum gravity continues apace:

3) Thomas Thiemann, "An account of transforms on $\overline{(}\mathcal{A}/\mathcal{G})$", available as `arXiv:gr-qc/9511049`.

   Thomas Thiemann, "Reality conditions inducing transforms for quantum gauge field theory and quantum gravity", available as `arXiv:gr-qc/9511057`.

   Abhay Ashtekar, "A generalized Wick transform for gravity", available as `arXiv:gr-qc/9511083`.

   Renate Loll, "Making quantum gravity calculable", preprint available in LaTeX form as `arXiv:gr-qc/9511080`.

   Rodolfo Gambini and Jorge Pullin, "A rigorous solution of the quantum Einstein equations", available as `arXiv:gr-qc/9511042`.

The first three papers here discuss some new work tackling the "reality conditions" and "Hamiltonian constraint", two of the toughest issues in the loop representation of quantum gravity. First, the Hamiltonian constraint is another name for the Wheeler-DeWitt equation

$$H\psi = 0$$

that every physical state of quantum gravity must satisfy (see "Week 11" for why). The "reality conditions" have to do with the fact that this constraint looks different depending on whether we are working with Riemannian or Lorentzian quantum gravity. The constraint is simpler when we work with Riemannian quantum gravity. Classically, in *Riemannian* gravity the metric on spacetime looks like

$$dt^2 + dx^2 + dy^2 + dz^2$$

at any point, if we use suitable local coordinates. In this Riemannian world, time is no different from space! In the real world, the world of *Lorentzian* gravity, the metric looks like

$$-dt^2 + dx^2 + dy^2 + dz^2$$

at any point, in suitable coordinates. Folks often call the Riemannian world the world of "imaginary time", since in some vague sense we can get from the Lorentzian world to the Riemannian world by making the transformation

$$t \mapsto it,$$

called a "Wick transform". It looks simple the way I have just written it, in local coordinates. But a priori it's far from clear that this Wick transform makes any sense globally. Apparently, however, there is something we can do along these lines, which transforms the Hamiltonian for Lorentzian quantum gravity to the better-understood one of Riemannian quantum gravity! Alas, I have been too distracted by n-categories to keep up with the latest work on this, but I'll catch up in a bit. Maybe over Christmas I can relax a bit, lounge in front of a nice warm fire, and read these papers.

The goal of all these machinations, of course, is to find some equations that make mathematical sense, have a good right to be called a "quantized version of Einstein's equation", and let one compute answers to some physics problems. We don't expect that quantum gravity is enough to describe what's really going on in interesting problems... there are lots of other forces and particles out there. Indeed, string theory is founded on the premise that quantum gravity is completely inseparable from the quantum theories of everything else. But here we are taking a different tack, treating quantum gravity by itself in as simple a way as possible, expecting that the predictions of theory will be *qualitatively* of great interest even if they are quantitatively inaccurate.

As described in earlier Finds ("Week 55", "Week 68"), Loll has been working to make quantum gravity "calculable", by working on a discretized version of the theory called lattice quantum gravity. If one does it carefully, it's not too bad to treat space as a lattice in the loop representation of quantum gravity, because even in the continuum the theory is discrete in a certain sense, since the states are described by "spin networks", certain graphs embedded in space. (See "Week 43" for more on these.) Her latest paper is an introduction to some of these issues.

In a somewhat different vein, Gambini and Pullin have been working on relating the loop representation to knot theory. One of their most intriguing results is that the second coefficient of the Alexander-Conway knot polynomial is a solution of the Hamiltonian constraint. Here they argue for this result using a lattice regularization of the theory.

Now let me turn to a variety of other matters...

4) Matt Greenwood and Xiao-Song Lin, "On Vassiliev knot invariants induced from finite type", available as arXiv:q-alg/9506001.

   Lev Rozansky, "On finite type invariants of links and rational homology spheres derived from the Jones polynomial and Witten–Reshetikhin–Turaev invariant", available as arXiv:q-alg/9511025.

   Scott Axelrod, "Overview and warmup example for perturbation theory with instantons", available as arXiv:hep-th/9511196.

These papers all deal with perturbative Chern-Simons theory and its spinoffs. The first two consider homology 3-spheres. In Chern-Simons theory, this makes the moduli space of flat $\mathrm{SU}(2)$ connections trivial, thus eliminating some subtleties in the perturbation theory. A homology 3-sphere is a 3-manifold whose homology is the same as that of the 3-sphere... the first one was discovered by Poincare when he was studying his conjecture that every 3-manifold with the homology of a 3-sphere is a 3-sphere. It turns out that you can get a counterexample if you just take an ordinary 3-sphere, cut out a solid torus embedded in the shape of a trefoil knot, and stick it back in with the meridian and longitude (the short way around, and the long way around) switched — making sure they wind up pointing in the correct directions. This is called "doing Dehn surgery on the trefoil". It gives something with the homology of a 3-sphere that's not a 3-sphere. So Poincare had to revise his conjecture to say that every 3-manifold *homotopic* to a 3-sphere is (homeomorphic to) a 3-sphere. This improved "Poincare conjecture" remains unsolved... its analog is known to be true in every dimension other than 3! Since every possible counterexample to the Poincare conjecture is a homology 3-sphere, it makes some sense to ponder these manifolds carefully.

Now, just as perturbative Chern-Simons theory gives certain special invariants of links, said to be of "finite type", the same is true for homology 3-spheres. When we say a link invariants is of finite type, all we mean is that it satisfies a simple property described in "Week 3". There is a similar but subtler definition for an invariant of homology 3-spheres to be of finite type; they need to transform in a nice way under Dehn surgery. (See "Week 60" for more references.)

The second paper concentrates precisely on those subtleties due to the moduli space of flat connections, developing perturbation theory in the presence of "instantons" (here, nontrivial flat connections).

5) Alan Carey, Jouko Mickelsson, and Michael Murray, "Index theory, gerbes, and Hamiltonian quantization", available as arXiv:hep-th/9511151.

   Alan Carey, M. K. Murray and B. L. Wang, "Higher bundle gerbes and cohomology classes in gauge theories", available as arXiv:hep-th/9511169

Higher-dimensional algebra is sneaking into physics in yet another way: gerbs! What's a gerb? Roughly speaking, it's a sheaf of groupoids, but there are some other ways of thinking of them that come in handy in physics. See "Week 25" for a review of Brylinski's excellent book on gerbs, and also:

6) Jean-Luc Brylinski, "Holomorphic gerbes and the Beilinson regulator", in *Proc. Int. Conf. on K-Theory (Strasbourg, 1992)*, to appear in Asterisque.

   Jean-Luc Brylinski, "The geometry of degree-four characteristic classes and of line bundles on loop spaces I", Duke Math. Jour. 75 (1994), 603–638.

   Jean-Luc Brylinski, "Cech cocyles for characteristic classes", J.-L. Brylinski and D. A. McLaughlin.

7) Joe Polchinski, "Recent results in string duality", available as arXiv:hep-th/9511157.

This should help folks keep up with the ongoing burst of work on dualities relating superficially different string theories.

8) Leonard Susskind and John Uglum, "String physics and black holes", available as arXiv:hep-th/9511227.

Among other things, this review discusses the "holographic hypothesis" mentioned in "Week 57":

9) Boguslaw Broda, "A gauge-field approach to 3- and 4-manifold invariants", available in TeX form as arXiv:q-alg/9511010.

This summarizes the Reshetikhin-Turaev construction of 3d topological quantum field theories from quantum groups, and Broda's own closely related approach to 4d topological quantum field theories.

10) John Baez and Martin Neuchl, "Higher-dimensional algebra I: braided monoidal 2-categories", available as arXiv:q-alg/9511013.

In this paper, we begin with a brief sketch of what is known and conjectured concerning braided monoidal 2-categories and their applications to 4d topological quantum field theories and 2-tangles (surfaces embedded in 4-dimensional space). Then we give concise definitions of semistrict monoidal 2-categories and braided monoidal 2-categories, and show how these may be unpacked to give long explicit definitions similar to, but not quite the same as, those given by Kapranov and Voevodsky. Finally, we describe how to construct a semistrict braided monoidal 2-category $Z(\mathcal{C})$ as the 'center' of a semistrict monoidal category $\mathcal{C}$. This is analogous to the construction of a braided monoidal category as the center, or 'quantum double', of a monoidal category. The idea is to develop algebra that will do for 4-dimensional topology what quantum groups and braided monoidal categories did for 3d topology. As a corollary of the center construction, we prove a strictification theorem for braided monoidal 2-categories.

Week 72

February 1, 1996

It's been a while since I've written an issue of This Week's Finds... due to holiday distractions and a bunch of papers that need writing up. But tonight I just can't seem to get any work done, so let me do a bit of catching up.

I'm no string theorist, but I still can't help hearing all the rumbling noises over in that direction: first about all the dualities relating seemingly different string theories, and then about the mysterious "M-theory" in 11 dimensions which seems to underlie all these developments. Let me try to explain a bit of this stuff... in the hopes that I prompt some string theorists to correct me and explain it better! I will simplify everything a lot to keep people from getting scared of the math involved. But I may also make some mistakes, so the experts should be kind to me and try to distinguish between the simplifications and the mistakes.

Recall that it's hard to get a consistent string theory — one that's not plagued by infinite answers to interesting questions. But this difficulty is generally regarded as a good thing, because it drastically limits the number of different versions of string theory one needs to think about. It's often said that there are only 5 consistent string theories: the type I theory, the type IIA and IIB theory, and the two kinds of heterotic string theory. I'm not sure exactly what this statement means, but certainly it's only meant to cover supersymmetric string theories, which can handle fermions (like the electron and neutrino) in addition to bosons (like the photon).

Type I strings are "open strings" — not closed loops — and they live in 10 dimensional spacetime, meaning that you need the dimension to be 10 to make certain nasty infinities cancel out. Type II strings also live in 10 dimensions, but they are "closed strings". That means that they look like a circle, so there are vibrational modes that march around clockwise and other modes that march around counterclockwise, and these are supposed to correspond to different particles that we see. We can think of these vibrational modes as moving around the circle at the speed of light; they are called "left-movers" and "right-movers". Now fermions which move at the speed of light are able to be rather asymmetric and only spin one way (when viewed head-on). We say they have a "chirality" or handedness. Ordinary neutrinos, for example, are left-handed. This asymmetry of nature shocked everyone when first discovered, but it appears to be a fact of life, and it's certainly a fact of mathematics. In the type IIA string theory, the left-moving and right-moving fermionic vibrational modes have opposite chiralities, while in the IIB theory, they have the same chirality. When I last checked, the type IIA theory seemed to fit our universe a bit better than the IIB theory.

But lots of people say the heterotic theory matches our universe even better. The name "heterotic" refers to the fact that this theory is supposed to have "hybrid vigor". It's quite bizarre: the left-movers are purely bosonic — no fermions — and live in 26-dimensional spacetime, the way non-supersymmetric string theories do. The right-movers, on the other hand, are supersymmetric and live in 10- dimensional spacetime. It sounds not merely heterotic, but downright schizophrenic! But in fact, the 26-dimensional spacetime can also thought of as being 10-dimensional, with 16 extra "curled-up dimensions" in the shape of a torus. This torus has two possible shapes: $\mathbb{R}^16$

modulo the $E_8 \times E_8$ lattice or the D_{16}^* lattice. (For some of the wonders of E_8 and other lattices, check out "Week 64" and "Week 65". The D_{16}^* lattice is related to the D_{16} lattice described in those Weeks, but not quite the same.)

Now there is still lots of room for toying with these theories depending on how you "compactify": how you think of 10-dimensional spacetime as 4-dimensional spacetime plus 6 curled-up dimensions. That's because there are lots of 6-dimensional manifolds that will do the job (the so-called "Calabi-Yau" manifolds). Different choices give different physics, and there is a lot of work to be done to pick the right one.

However, recently it's beginning to seem that all five of the basic sorts of string theory are beginning to look like different manifestations of the same theory in 11 dimensions... some monstrous thing called M-theory! Let me quote the following paper:

1) Kelly Jay Davis, "M-Theory and String-String Duality", 28 pages, available as `hep-th/9601102`, uses `harvmac.tex`.

The idea seems to be roughly that depending on how one compactifies the 11th dimension, one gets different 10-dimensional theories from M-theory:

> *"In the past year much has happened in the field of string theory. Old results relating the two Type II string theories and the two Heterotic string theories have been combined with newer results relating the Type II theory and the Heterotic theory, as well as the Type I theory and the Heterotic theory, to obtain a single"String Theory." In addition, there has been much recent progress in interpreting some, if not all, properties of String Theory in terms of an eleven-dimensional M-Theory. In this paper we will perform a self-consistency check on the various relations between M-Theory and String Theory. In particular, we will examine the relation between String Theory and M-Theory by examining its consistency with the string-string duality conjecture of six-dimensional String Theory. So, let us now take a quick look at the relations between M-Theory and String Theory some of which we will be employing in this article.*
>
> *In Witten's paper he established that the strong coupling limit of Type IIA string theory in ten dimensions is equivalent to eleven-dimensional supergravity on a "large" S^1. [Note: S^1 just means the circle — jb.] As the low energy limit of M-theory is eleven-dimensional supergravity, this relation states that the strong coupling limit of Type IIA string theory in ten-dimensions is equivalent to the low-energy limit of M-Theory on a "large" S^1. In the paper of Witten and Horava, they establish that the strong coupling limit of the ten-dimensional $E_8 \times E_8$ Heterotic string theory is equivalent to M-Theory on a "large" $S^1/\mathbb{Z}_2$.*
>
> *Recently, Witten, motivated by Dasgupta and Mukhi, examined M-Theory on a $\mathbb{Z}_2$ orbifold of the five-torus and established a relation between M-Theory on this orbifold and Type IIB string theory on $K3$. [Note: most of these undefined terms refer to various spaces; for example, the five-torus is the 5-dimensional version of a doughnut, while $K3$ is a certain 4-dimensional manifold — jb.] Also, Schwarz very recently looked at M-Theory and its relation to T-Duality.*
>
> *As stated above, M-Theory on a "large" S^1 is equivalent to a strongly coupled Type IIA string theory in ten-dimensions. Also, M-theory on a "large" $S^1/\mathbb{Z}_2$*

> *is equivalent to a strongly coupled $E_8 \times E_8$ Heterotic string theory in ten dimensions. However, the string-string duality conjecture in six dimensions states that the strongly coupled limit of a Heterotic string theory in six dimensions on a four-torus is equivalent to a weakly coupled Type II string theory in six-dimensions on $K3$. Similarly, it states that the strongly coupled limit of a Type II theory in six dimensions on K3 is equivalent to a weakly coupled Heterotic string theory in six-dimensions on a four-torus. Now, as a strongly coupled Type IIA string theory in ten-dimensions is equivalent to the low energy limit of M-Theory on a "large" S^1, the low energy limit of M-Theory on $S^1 \times K3$ should be equivalent to a weakly coupled Heterotic string theory on a four-torus by way of six-dimensional string-string duality. Similarly, as a strongly coupled $E_8 \times E_8$ Heterotic string theory in ten-dimensions is equivalent to the low energy limit of M-Theory on a "large" $S^1/\mathbb{Z}_2$, the low energy limit of M-Theory on $S^1/\mathbb{Z}_2 \times T^4$ should be equivalent to a weakly coupled Type II string theory on $K3$. The first of the above two consistency checks on the relation between M-Theory and String Theory will be the subject of this article. However, we will comment on the second consistency check in our conclusion."*

So, as you can see, there is a veritable jungle of relationships out there. But you must be wondering by now: *what's M-theory?* According to

2) Edward Witten, "Five-branes and M-Theory on an orbifold", available as hep-th/9512219.

the M stands for "magic", "mystery", or "membrane", according to taste. From a mathematical viewpoint a better term might be "murky", since apparently everything known about M-theory is indirect and circumstantial, except for the classical limit, in which it seems to act as a theory of 2-branes and 5-branes, where an "n-brane" is an n-dimensional analog of a membrane or surface.

Well, here I must leave off, for reasons of ignorance. I don't really understand the evidence for the existence of the M-theory. . . I can only await the day when the murk clears and it becomes possible to learn about this stuff a bit more easily. It has been suggested that string theory is a bit of 21st-century mathematics that accidentally fell into the 20th century. I think this is right, and that eventually much of this stuff will be seen as much simpler than it seems now.

Now let me briefly describe some papers I actually sort of understand.

3) Abhay Ashtekar, "Polymer geometry at Planck scale and quantum Einstein equations", available as hep-th/9601054.

   Roumen Borissov, Seth Major and Lee Smolin, "The geometry of quantum spin networks", available as gr-qc/9512043, 35 Postscript figures, uses `epsfig.sty`.

   Bernd Bruegmann, "On the constraint algebra of quantum gravity in the loop representation", available as gr-qc/9512036.

   Kiyoshi Ezawa, "Nonperturbative solutions for canonical quantum gravity: an overview", available as gr-qc/9601050

   Kiyoshi Ezawa, "A semiclassical interpretation of the topological solutions for canonical quantum gravity", available as gr-qc/9512017.

Jorge Griego, "Extended knots and the space of states of quantum gravity", available as gr-qc/9601007.

Seth Major and Lee Smolin, "Quantum deformation of quantum gravity", available as gr-qc/9512020.

Work on the loop representation of quantum gravity proceeds apace. The paper by Ashtekar and the first one by Ezawa review various recent developments and might be good to look at if one is just getting interested in this subject. Smolin has been pushing the idea of combining ideas about the quantum group $SU_q(2)$ with the loop representation, and his papers with Borissov and Major are about that. This seems rather interesting but still a bit mysterious to me. I suspect that what it amounts to is thinking of loops as excitations not of the Ashtekar-Lewandowksi vacuum state but the Chern-Simons state. I'd love to see this clarified, since these two states are two very important exact solutions of quantum gravity, and the latter has the former as a limit as the cosmological constant goes to zero. In the second paper listed, Ezawa gives semiclassical interpretations of these and other exact solutions of quantum gravity.

4) Thomas Kerler, "Genealogy of nonperturbative quantum-invariants of 3-Manifolds: the surgical family", available as q-alg/9601021.

Kerler brings a bit more order to the study of quantum invariants of 3-manifolds, in particular, the Reshetikhin-Turaev, Hennings-Kauffman-Radford, and Lyubashenko invariants. All of these are constructed using certain braided monoidal categories, like the category of (nice) representations of a quantum group. He describes how Lyubashenko's invariant specializes to the Reshetikhin-Turaev invariant for semisimple categories and to the Hennings-Kauffman-Radford invariant for Tannakian categories. People interested in extended TQFTs and 2-categories will find his work especially interesting, because he works with these invariants using these techniques. James Dolan and I have argued that it's only this way that one will really understand these TQFTs (see "Week 49").

In future editions of This Week's Finds I will say more about n-categories and topological quantum field theory. I have a feeling that while I've discussed these a lot, I have never really explained the basic ideas very well. As I gradually understand the basic ideas better, they seem simpler and simpler to me, so I think I should try to explain them.

Week 73

February 24, 1996

In this and future issues of This Week's Finds, I'd like to talk a bit more about higher-dimensional algebra, and how it should lead to many exciting developments in mathematics and physics in the 21st century. I've talked quite a bit about this already, but I hear from some people that the "big picture" remained rather obscure. The main reason, I suppose, is that I was just barely beginning to see the big picture myself! As Louis Crane noted, in this subject it often feels that we are unearthing the fossilized remains of some enormous prehistoric beast, still unsure of its extent or how it all fits together. Of course that's what makes it so exciting, but I'll try to make sense what we've found so far, and where it may lead. In the Weeks to come, I'll start out describing some basic stuff, and work my way up to some very new ideas.

However, before I get into that, I'd like to say a bit about something completely different: biology.

1) *Biological Asymmetry and Handedness*, Ciba Foundation Symposium **162**, John Wiley and Sons, 1991.

   D. K. Kondepudi and D. K. Nelson, "Weak neutral currents and the origins of molecular chirality", *Nature* **314**, pp. 438–441.

It's always puzzled me how humans and other animals could be consistently asymmetric. A 50-50 mix of two mirror-image forms could easily be explained by "spontaneously broken symmetry", but in fact there are many instances of populations with a uniform handedness. Many examples appear in Weyl's book "Symmetry" (see "Week 63"). To take an example close to home, the human brain appears to be lateralized in a fairly consistent manner; for example, most people have the speech functions concentrated in the left hemisphere of their cerebrum — even most, though not all, left-handers.

One might find this unsurprising: it just means that the asymmetry is encoded in the genes. But think about it: how are the genes supposed to tell the embryo to develop in an asymmetric way? How do they explain the difference between right and left? That's what intrigues me.

Of course, genes code for proteins, and most proteins are themselves asymmetric. Presumably the answer lurks somewhere around here. Indeed, even the amino acids of which the proteins are composed are asymmetric, as are many sugars and for that matter, the DNA itself, which is composed of two spirals, each of which has an intrinsic directionality and hence a handedness. The handedness of many of these basic biomolecules is uniform for all life on the globe, as far as I know.

In the conference proceedings on biological asymmetry, there is an interesting article on the development of asymmetry in *C. elegans*. Ever since the 1960s, this little nematode has been a favorite among biologists because of its simplicity, and because of the advantages understanding one organism thoroughly rather than many organisms in a sketchy way. I'm sure most of you know about the fondness geneticists have for the fruit fly, but Caenorhabditis elegans is a far simpler critter: it only has 959 cells, all of which have been individually named and studied! There are over 1000 people studying it by now, there is a journal devoted to it — The Worm Breeder's Gazette — and it has its own world-wide web server. Moreover, folks are busily sequencing not only the complete human genome but also all 100 million bases of the DNA of *C. elegans*.

But I digress! The point here is that *C. elegans* is asymmetric, and exhibits a consistent handedness. And the cool thing is that in the conference proceedings, Wood and Kevshan report on experiments where they artificially changed the handedness of *C. elegans* embryos when they consisted of only 6 cells! The embryos look symmetric when they have 4 cells; by the time they have 8 cells the asymmetry is marked. By moving some cells around at the 6-cell stage, Wood and Kevshan were able to create fully functional *C. elegans* having opposite the usual handedness.

The question of exactly how the embryo's asymmetry originates from some asymmetry at the molecular still seems shrouded in mystery. And there is another puzzle: how did the biomolecules choose their handedness in the first place? Here spontaneous symmetry breaking — an essentially random choice later amplified by selection — seems a natural hypothesis. But physicists should be interested to note that another alternative has been seriously proposed. Weak interactions violate parity and thus endow the laws of nature with an intrinsic handedness. This means there is a slight difference in energies between any biomolecule and its enantiomer, or mirror-image version. According to S. F. Mason's article in the conference proceedings, this difference indeed favors the observed forms of amino acids and sugars — the left-handed or "L" amino acids and the right-handed or "D" sugars. But the difference is is incredibly puny — typically it amounts to 10^{-14} joules per mole! How could such a small difference matter? Well, Kondepudi and Nelson have done calculations suggesting that in certain situations where there is both autocatalysis of both L and D forms of these molecules, and also competition between them, random fluctuations can be averaged out, while small energy level differences can make a big difference.

That would be rather satisfying to me: knowing that my heart is where it is for the same reason that neutrinos are left-handed. But in fact this theory is very controversial. . . . I mention it only because of its charm.

If we think of the universe as passing through the course of history from simplicity to complexity, from neutrinos to nematodes to humans, it's natural to wonder what's at the bottom, where things get very simple, where physics blurs into pure logic. . . . far from the "spires of form". Ironically, even the simplest things may be hard to understand, because they are so abstract.

Let's begin with the world of sets. In a certain sense, there is nothing much to a set except its cardinality, the number of elements it has. Of course, set theorists work hard to build up the universe of sets from the empty set, each set being a set of sets, with its own distinctive personality:

$$\{\},\ \{\{\}\},\ \{\{\{\}\}\},\ \{\{\},\{\{\}\}\},\ \{\{\},\{\{\{\}\}\}\},\ \{\{\},\{\{\}\},\{\{\},\{\{\}\}\}\}$$

and the like. But for many purposes, a one-to-one and onto function between two sets allows us to treat them as the same. So if necessary, we could actually get by with just one set of each cardinality. For example

$$\{\},\ \{\{\}\},\ \{\{\},\{\{\}\}\},\ \{\{\},\{\{\}\},\{\{\},\{\{\}\}\}\}$$

and so on. For short, people like to call these

$$0,\ 1,\ 2,\ 3$$

and so on. We could wonder what comes after all these finite cardinals, and what comes after that, and so on, but let's not. Instead, let's ponder what we've done so far. We started with the universe of sets — not exactly the set of all sets, but pretty close — but very soon we started playing with functions between sets. This is what allowed us to speak of two sets with the same cardinality as being isomorphic.

In short, we are really working with the *category* of sets. A category is something just as abstract as a set, but a bit more structured. It's not a mere collection of objects; there are also morphisms between objects, in this case the functions between sets.

Some of you might not know the precise definition of a category; let me state it just for completeness. A category consists of a collection of "objects" and a collection of "morphisms". Every morphism f has a "source" object and a "target" object. If the source of f is X and its target is Y, we write $f\colon X \to Y$. In addition, we have:

1) Given a morphism $f\colon X \to Y$ and a morphism $g\colon Y \to Z$, there is a morphism $fg\colon X \to Z$, which we call the "composite" of f and g.

2) Composition is associative: $(fg)h = f(gh)$.

3) For each object X there is a morphism $1_X\colon X \to X$, called the "identity" of X. For any $f\colon X \to Y$ we have $1_X f = f 1_Y = f$.

That's it.

(Note that we are writing the composite of $f\colon X \to Y$ and $g\colon Y \to Z$ as fg, which is backwards from the usual order. This will make life easier in the long run, though, since fg will mean "first do f, then g".)

Now, there are lots of things one can do with sets, which lead to all sorts of interesting examples of categories, but in a sense the primordial category is Set, the category of sets and functions. (One might try to make this precise, by trying to prove that every category is a subcategory of Set, or something like that. Actually the right way to say how Set is primordial is called the "Yoneda lemma". But to understand this lemma, one needs to understand categories a little bit.)

When we get to thinking about categories a lot, it's natural to think about the "category of all categories". Now just as it's a bit bad to speak of the set of all sets, it's bad to speak of the category of all categories. This is true, not only because Russell's paradox tends to ruin attempts at a consistent theory of the "thing of all things", but because, just as what really counts is the *category* of all sets, what really counts is the 2-*category* of all categories.

To understand this, note that there is a very sensible notion of a morphism between categories. It's called a "functor", and a functor $F\colon \mathcal{C} \to \mathcal{D}$ from a category $\mathcal{C}$ to a category $\mathcal{D}$ is just something that assigns to each object x of $\mathcal{C}$ an object $F(x)$ of $\mathcal{D}$, and to each morphism f of $\mathcal{C}$ a morphism $F(f)$ of $\mathcal{D}$, in such a way that "all structure in sight is preserved". More precisely, we want:

1) If $f\colon x \to y$, then $F(f)\colon F(x) \to F(y)$.

2) If $fg = h$, then $F(f)F(g) = F(h)$.

3) If 1_x is the identity morphism of x, then $F(1_x)$ is the identity morphism of $F(x)$.

It's good to think of a category as a bunch of dots — objects — and arrows going between them — morphisms. I would draw one for you if I could here. Category theorists love drawing these pictures. In these terms, we can think of the functor $F\colon \mathcal{C} \to \mathcal{D}$ as putting a little picture of the category $\mathcal{C}$ inside the category $\mathcal{D}$. Each dot of $\mathcal{C}$ gets drawn as a particular dot in $\mathcal{D}$, and each arrow in $\mathcal{C}$ gets drawn as a particular arrow in $\mathcal{D}$. (Two dots or arrows in $\mathcal{C}$ can get drawn as the same dot or arrow in $\mathcal{D}$, though.)

In addition, however, there is a very sensible notion of a "2-morphism", that is, a morphism between morphisms between categories! It's called a "natural transformation". The idea is this. Suppose we have two functors $F\colon \mathcal{C} \to \mathcal{D}$ and $G\colon \mathcal{C} \to \mathcal{D}$. We can think of these as giving two pictures of $\mathcal{C}$ inside $\mathcal{D}$. So for example, if we have any object x in $\mathcal{C}$, we get two objects in $\mathcal{D}$, $F(x)$ and $G(x)$. A "natural transformation" is then a gadget that draws an arrow from each dot like $F(x)$ to the dot like $G(x)$. In other words, for each x, the natural transformation T gives a morphism $T_x\colon F(x) \to G(x)$. But we want a kind of compatibility to occur: if we have a morphism $f\colon x \to y$ in $\mathcal{C}$, we want

$$\begin{array}{ccc} F(x) & \xrightarrow{F(f)} & F(y) \\ {\scriptstyle T_x}\downarrow & & \downarrow{\scriptstyle T_y} \\ G(x) & \xrightarrow[G(f)]{} & G(y) \end{array}$$

to commute; in other words, we want $T_x G(f) = F(f) T_y$.

This must seem very boring to the people who understand it and very mystifying to those who don't. I'll need to explain it more later. For now, let me just say a bit about what's going on. Sets are "zero-dimensional" in that they only consist of objects, or "dots". There is no way to "go from one dot to another" within a set. Nonetheless, we can go from one set to another using a function. So the category of all sets is "one-dimensional": it has both objects or "dots" and morphisms or "arrows between dots". In general, categories are "one-dimensional" in this sense. But this in turn makes the collection of all categories into a "two-dimensional" structure, a 2-category having objects, morphisms between objects, and 2-morphisms between morphisms.

This process never stops. The collection of all n-categories is an $(n+1)$-category, a thing with objects, morphisms, 2-morphisms, and so on all the way up to n-morphisms. To study sets carefully we need categories, to study categories well we need 2-categories, to study 2-categories well we need 3-categories, and so on... so "higher- dimensional algebra", as this subject is called, is automatically generated in a recursive process starting with a careful study of set theory.

If you want to show off, you can call the 2-category of all categories Cat, and more generally, you can call the $(n+1)$-category of all n-categories $n\mathsf{Cat}$. $n\mathsf{Cat}$ is the primordial example of an $(n+1)$-category!

Now, just as you might wonder what comes after $0, 1, 2, 3, \ldots$, you might wonder what comes after all these n-categories. The answer is "ω-categories".

What comes after these? Well, let us leave that for another time. I'd rather conclude by mentioning the part that's the most fascinating to me as a mathematical physicist. Namely, the various dimensions of category turn out to correspond in a very beautiful — but still incompletely understood — way to the various dimensions of spacetime. In other words, the study of physics in imaginary 2-dimensional spacetimes uses lots of 2-categories, the study of physics in a 3d spacetimes uses 3-categories, the study of physics in 4d spacetimes appears to use 4-categories, and so on. It's very surprising at first that something so simple and abstract as the process of starting with sets and recursively being led to study the $(n + 1)$-category of all n-categories could be related to the dimensionality of spacetime. In particular, what could possibly be special about 4 dimensions?

Well, it turns out that there *are* some special things about 4 dimensions. But more on that later.

To continue reading the "Tale of n-Categories", see "Week 74".

Addendum: Long after writing the above, I just saw an interesting article on chirality in biology:

2) N. Hirokawa, Y. Tanaka, Y. Okada and S. Takeda, "Nodal flow and the generation of left-right asymmetry", *Cell* **125** 1 (2006), 33–45.

It reports on detailed studies of how left-right asymmetry first shows in the development of animal embryos. It turns out this asymmetry is linked to certain genes with names like *Lefty-1*, *Lefty-2*, *Nodal* and *Pitx2*. About half of the people with a genetic disorder called Kartagener's Syndrome have their organs in the reversed orientation. These people also have immotile sperm and defective cilia in their airway. This suggests that the genes controlling left-right asymmetry also affect the development of cilia! And the link has recently been understood...

The first visible sign of left-right asymmetry in mammal embryos is the formation of a structure called the "ventral node" after the front-back (dorsal-ventral) and top-bottom (anterior-posterior) symmetries have been broken. This node is a small bump on the front of the embryo.

It has recently been found that cilia on this bump wiggle in a way that makes the fluid the embryo is floating in flow towards the *left*. It seems to be this leftward flow that generates many of the more fancy left-right asymmetries that come later.

How do these cilia generate a leftward flow? It seems they spin around *clockwise*, and are tilted in such a way that they make a leftward swing when they are near the surface of the embryo, and a rightward swing when they are far away. This manages to do the job... the article discusses the hydrodynamics involved.

I guess now the question becomes: why do these cilia spin clockwise?

Week 74

March 5, 1996

Before continuing my story about higher-dimensional algebra, let me say a bit about gravity. Probably far fewer people study general relativity than quantum mechanics, which is partially because quantum mechanics is more practical, but also because general relativity is mathematically more sophisticated. This is a pity, because general relativity is so beautiful!

Recently, I have been spending time on `sci.physics` leading an informal (nay, chaotic) "general relativity tutorial". The goal is to explain the subject with a minimum of complicated equations, while still getting to the mathematical heart of the subject. For example, what does Einstein's equation REALLY MEAN? It's been a lot of fun and I've learned a lot! Now I've gathered up some of the posts and put them on a web site:

1) John Baez et al, "General relativity tutorial", `gr/gr.html`

I hope to improve this as time goes by, but it should already be fun to look at.

Let me also list a couple new papers on the loop representation of quantum gravity, dealing with ways to make volume and area into observables in quantum gravity:

2) Abhay Ashtekar and Jerzy Lewandowski, "Quantum Theory of Geometry I: Area Operators", 31 pages in LaTeX format, to appear in Classical and Quantum Gravity, preprint available as `gr-qc/9602046`.

   Jerzy Lewandowski, "Volume and Quantizations", preprint available as `gr-qc/9602035`.

   Roberto De Pietri and Carlo Rovelli, "Geometry Eigenvalues and Scalar Product from Recoupling Theory in Loop Quantum Gravity", 38 pages, 5 Postscript figures, uses RevTeX 3.0 and `epsfig.sty`, preprint available as `gr-qc/9602023`.

I won't say anything about these now, but see "Week 55" for some information on area operators.

Okay, where were we? We had started messing around with sets, and we noted that sets and functions between sets form a category, called Set. Then we started messing around with categories, and we noted that not only are there "functors" between categories, there are things that ply their trade between functors, called "natural transformations". I then said that categories, functors, and natural transformations form a 2-category. I didn't really say what a 2-category is, except to say that it has objects, morphisms between objects, and 2-morphisms between morphisms. Finally, I said that this pattern continues: nCat forms an $(n+1)$-category.

By the way, I said last time that Set was "the primordial category". Keith Ramsay reminded me by email that this can be misleading. There are other categories that act a whole lot like Set and can serve equally well as "the primordial category". These are called topoi. Poetically speaking, we can think of these as alternate universes in which to do mathematics. For more on topoi, see "Week 68". All I meant by saying that Set was "the primordial category" is that, if we start from Set and various categories of structures built using sets — groups, rings, vector spaces, topological spaces, manifolds, and so on — we can then abstract the notion of "category", and thus obtain Cat. In the same sense, Cat is the primordial 2-category, and so on.

I mention this because it is part of a very important broad pattern in higher-dimensional algebra. For example, we will see that the complex numbers are the primordial Hilbert space, and that the category of Hilbert spaces is the primordial "2-Hilbert space", and that the 2-category of 2-Hilbert spaces is the primordial "3-Hilbert space", and so on. This leads to a quantum-theoretic analog of the hierarchy of n-categories, which plays an important role in mathematical physics. But I'm getting ahead of myself!

Let's start by considering a few examples of categories. I want to pick some examples that will lead us naturally to the main themes of higher-dimensional algebra. Beware: it will take us a while to get rolling. For a while — maybe a few issues of This Week's Finds — everything may seem somewhat dry, pointless and abstract, except for those of you who are already clued in. It has the flavor of "foundations of mathematics," but eventually we'll see these new foundations reveal topology, representation theory, logic, and quantum theory to be much more tightly interknit than we might have thought. So hang in there.

For starters, let's keep the idea of "symmetry" in mind. The typical way to think about symmetry is with the concept of a "group". But to get a concept of symmetry that's really up to the demands put on it by modern mathematics and physics, we need — at the very least — to work with a *category* of symmetries, rather than a group of symmetries.

To see this, first ask: what is a category with one object? It is a "monoid". The *usual* definition of a monoid is this: a set M with an associative binary product and a unit element 1 such that $a1 = 1a = a$ for all a in M. Monoids abound in mathematics; they are in a sense the most primitive interesting algebraic structures.

To check that a category with one object is "essentially just a monoid", note that if our category $\mathcal{C}$ has one object x, the set $\mathrm{Hom}(x, x)$ of all morphisms from x to x is indeed a set with an associative binary product, namely composition, and a unit element, namely 1_x. (Actually, in an arbitrary category $\mathrm{Hom}(x, y)$ could be a class rather than a set. But let's not worry about such nuances.) Conversely, if you hand me a monoid M in the traditional sense, I can easily cook up a category with one object x and $\mathrm{Hom}(x, x) = M$.

How about categories in which every morphism is invertible? We say a morphism $f\colon x \to y$ in a category has inverse $g\colon y \to x$ if $fg = 1_x$ and $gf = 1_y$. Well, a category in which every morphism is invertible is called a "groupoid".

Finally, a group is a category with one object in which every morphism is invertible. It's both a monoid and a groupoid!

When we use groups in physics to describe symmetry, we think of each element g of the group G as a "process". The element 1 corresponds to the "process of doing nothing at all". We can compose processes g and h — do h and then g — and get the product gh. Crucially, every process g can be "undone" using its inverse g^{-1}.

We tend to think of this ability to "undo" any process as a key aspect of symmetry. I.e., if we rotate a beer bottle, we can rotate it back so it was just as it was before. We don't tend to think of SMASHING the beer bottle as a symmetry, because it can't be undone. But while processes that can be undone are especially interesting, it's also nice to consider other ones... so for a full understanding of symmetry we should really study monoids as well as groups.

But we also should be interested in "partially defined" processes, processes that can be done only if the initial conditions are right. This is where categories come in! Suppose that we have a bunch of boxes, and a bunch of processes we can do to a bottle in one box to turn it into a bottle in another box: for example, "take the bottle out of box x, rotate it 90 degrees clockwise, and put it in box y". We can then think of the boxes as objects and the processes as morphisms: a process that turns a bottle in box x to a bottle in box y is a morphism $f\colon x \to y$. We can only do a morphism $f\colon x \to y$ to a bottle in box x, not to a bottle in any other box, so f is a "partially defined" process. This implies we can only compose $f\colon x \to y$ and $g\colon u \to v$ to get $fg\colon x \to v$ if $y = u$.

So: a monoid is like a group, but the "symmetries" no longer need be invertible; a category is like a monoid, but the "symmetries" no longer need to be composable!

Note for physicists: the operation of "evolving initial data from one spacelike slice to another" is a good example of a "partially defined" process: it only applies to initial data on that particular spacelike slice. So dynamics in special relativity is most naturally described using groupoids. Only after pretending that all the spacelike slices are the same can we pretend we are using a group. It is very common to pretend that groupoids are groups, since groups are more familiar, but often insight is lost in the process. Also, one can only pretend a groupoid is a group if all its objects are isomorphic. Groupoids really are more general.

Physicists wanting to learn more about groupoids might try:

3) Alan Weinstein, "Groupoids: unifying internal and external symmetry", available as `http://math.berkeley.edu/~alanw/Groupoids.ps` or `http://www.ams.org/notices/199607/weinstein.pdf`

So: in contrast to a set, which consists of a static collection of "things", a category consists not only of objects or "things" but also morphisms which can viewed as "processes" transforming one thing into another. Similarly, in a 2-category, the 2-morphisms can be regarded as "processes between processes", and so on. The eventual goal of basing mathematics upon ω-categories is thus to allow us the freedom to think of any process as the sort of thing higher-level processes can go between. By the way, it should also be very interesting to consider "$\mathbb{Z}$-categories" (where $\mathbb{Z}$ denotes the integers), having j-morphisms not only for $j = 0, 1, 2, \ldots$ but also for negative j. Then we may also think of any thing as a kind of process.

How do the above remarks about groups, monoids, groupoids and categories generalize to the n-categorical context? Well, all we did was start with the notion of category and consider two sorts of requirement: that the category have just one object, or that all morphisms be invertible.

A category with just one object — a monoid — could also be seen as a set with extra algebraic structure, namely a product and unit. Suppose we look at an n-category with just one object? Well, it's very similar: then we get a special sort of $(n-1)$-category, one with a product and unit! We call this a "monoidal $(n-1)$-category". I will explain this more thoroughly later, but let me just note that we can keep playing this game, and consider a monoidal $(n-1)$-category with just one object, which is a special sort of $(n-2)$-category, which we could call a "doubly monoidal $(n-2)$-category", and so on. This game must seem very abstract and mysterious when one first hears of it. But it turns out to yield a remarkable set of concepts, some already very familiar in mathematics, and it turns out to greatly deepen our notion of "commutativity". For now, let me simply display a chart of "k-tuply monoidal n-categories" for certain low values of n and k:

Table 1: 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	weakly involutory monoidal 2-categories
$k = 4$	" "	" "	strongly involutory monoidal 2-categories
$k = 5$	" "	" "	" "

The quotes indicate that each column "stabilizes" past a certain point. If you can't wait to read more about this, you might try "Week 49" for more, but I will explain it all in more detail in future issues.

What if we take an n-category and demand that all j-morphisms ($j > 0$) be invertible? Well, then we get something we could call an "n-groupoid". However, there are some important subtle issues about the precise sense in which we might want all j-morphisms to be invertible. I will have to explain that, too.

Let me conclude, though, by mentioning something the experts should enjoy. If we define n-groupoids correctly, and then figure out how to define ω-groupoids correctly, the homotopy category of ω-groupoids turns out to be equivalent to the homotopy category of topological spaces. The latter category is something algebraic topologists have spent decades studying. This is one of the main ways n-categories are important in topology. Using this correspondence between n-groupoid theory and homotopy theory, the "stabilization" property described above is then related to a subject called "stable homotopy theory", and "$\mathbb{Z}$-groupoids" are a way of talking about "spectra" — another important tool in homotopy theory.

The above paragraph is overly erudite and obscure, so let me explain the gist: there is a way to think of a topological space as giving us an ω-groupoid, and the ω-groupoid then captures all the information about its topology that homotopy theorists find interesting. (I will explain in more detail how this works later.) If this is *all* n-category theory did, it would simply be an interesting language for doing topology. But as we shall see, it does a lot more. One reason is that, not only can we use n-categories to think about spaces, we can also use them to think about symmetries, as described above. Of course, physicists are very interested in space and also symmetry. So from the viewpoint of a mathematical physicist, one interesting thing about n-categories is that they *unify* the study of space (or spacetime) with the study of symmetry.

I will continue along these lines next time and try to fill in some of the big gaping holes.

To continue reading the "Tale of n-Categories", see "Week 75".

Week 75

March 6, 1996

If you've been following my recent introduction to n-categories, you'll note that I haven't actually given the definition of n-categories! I've only defined categories, and *hinted* at the definition of 2-categories by giving an example, namely Cat. This is the 2-category whose objects are categories, whose morphisms are functors, and whose 2-morphisms are natural transformations.

The definition of n-categories — or maybe I should say the problem of defining n-categories — is actually surprisingly subtle. Since I want to proceed at a gentle pace here, I think I should first get everyone used to the 2-category Cat, then define 2-categories in general, then play around with those a bit, and then move on to n-categories for higher n.

So recall what the objects, morphisms and 2-morphisms in Cat are: they are categories, functors and natural transformations. A functor $F\colon \mathcal{C} \to \mathcal{D}$ assigns to each object x of $\mathcal{C}$ an object $F(x)$ of $\mathcal{D}$, and to each morphism f of $\mathcal{C}$ a morphism $F(f)$ of $\mathcal{D}$, and has

1. If $f\colon x \to y$, then $F(f)\colon F(x) \to F(y)$.
2. If $fg = h$, then $F(f)F(g) = F(h)$.
3. If 1_x is the identity morphism of x, then $F(1_x)$ is the identity morphism of $F(x)$.

Given two functors $F\colon \mathcal{C} \to \mathcal{D}$ and $G\colon \mathcal{C} \to \mathcal{D}$, a "natural transformation" $T\colon F \to G$ assigns to each object x of $\mathcal{C}$ a morphism $T_x\colon F(x) \to G(x)$, such that for any morphism $f\colon x \to y in C$, the diagram

$$\begin{array}{ccc} F(x) & \xrightarrow{F(f)} & F(y) \\ {\scriptstyle T_x}\big\downarrow & & \big\downarrow{\scriptstyle T_y} \\ G(x) & \xrightarrow[G(f)]{} & G(y) \end{array}$$

commutes.

Let me give a nice example. Let Top be the category with topological spaces and continuous functions between them as morphisms. Let Gpd be the category with groupoids as objects and functors between them as morphisms. (Remember from "Week 74" that a groupoid is a category with all morphisms invertible.) Then there is a functor

$$\Pi_1\colon \mathsf{Top} \to \mathsf{Gpd}$$

called the "fundamental groupoid" functor, which plays a very basic role in algebraic topology.

Here's how we get from any space X its "fundamental groupoid" $\Pi_1(X)$. (If perchance you already know about the "fundamental group", fear not, what we're doing now is very similar.) To say what the groupoid $\Pi_1(X)$ is, we need to say what its objects and morphisms are. The objects in $\Pi_1(X)$ are just the POINTS of X and the morphisms are just certain equivalence classes of PATHS in X. More precisely, a morphism $f\colon x \to y$ in $\Pi_1(X)$ is just an equivalence class of continuous paths from x to y, where two paths from x to y are decreed equivalent if one can be continuously deformed to the other while not moving the endpoints. (If this equivalence relation holds we say the two paths are "homotopic", and we call the equivalence classes "homotopy classes of paths.")

This is a truly wonderful thing! Recall the idea behind categories. A morphism $f\colon x \to y$ is supposed to be some abstract sort of "process going from x to y." The human mind often works by visual metaphors, and our visual image of a "process" from x to y is some sort of "arrow" from x to y:

$$x \xrightarrow{f} y.$$

That's why we write $f\colon x \to y$, of course. But now what we are doing is taking this visual metaphor literally! We have a space X, like the piece of the computer screen on which you are actually reading this text. The objects in $\Pi_1(X)$ are then points in X, and a morphism is basically just a path from x to y:

$$x \xrightarrow{f} y.$$

Well, not quite; it's a homotopy class of paths. But still, something very interesting is going on here: we are turning something "concrete", namely the space X, into a corresponding "abstract" thing, namely the groupoid $\Pi_1(X)$, by thinking of "concrete" paths as "abstract" morphisms. Here I am thinking of geometrical concepts as "concrete", and algebraic ones as "abstract".

You may wonder why we use homotopy classes of paths, rather than paths. One reason is that topologists want to use Π_1 to distill a very abstract "essence" of the topological space X, an essence that conveys only information that's invariant under "homotopy equivalence". Another reason is that we can easily get homotopy classes of paths to compose associatively, as they must if they are to be morphisms in a category. We simply glom them end to end:

$$x \xrightarrow{f} y \xrightarrow{g} z$$

and there is no problem with associativity, since we can easily reparametrize the paths to make $(fg)h = f(gh)$. (If you haven't thought about this, please do!) If we do not work with homotopy classes, it's a pain to define composition in such a way that $(fg)h = f(gh)$. Unless you are sneaky, you only get that $(fg)h$ is *homotopic* to $f(gh)$; there are ways to get composition to come out associative, but they are all somewhat immoral. As we shall see, if we want to preserve the "concreteness" of X as much as possible, and work with morphisms that are actual paths in X rather than homotopy equivalence classes, the best thing is to work with n-categories. More on that later.

Let's see; I claimed there is a functor $\Pi_1\colon \mathsf{Top} \to \mathsf{Gpd}$, but so far I have only defined Π_1 on the objects of Top; we also need to define it for morphisms. That's easy. A morphism $F\colon X \to Y$ in Top is a continuous map from the space X to the space Y; this is just what we need to take points in X to points in Y, and homotopy classes of paths in X to homotopy classes of paths in Y. So it gives us a morphism in Gpd from the fundamental groupoid $\Pi_1(X)$ to the fundamental groupoid $\Pi_1(Y)$. There are various things to check here, but it's not hard. We call this morphism $\Pi_1(F)\colon \Pi_1(X) \to \Pi_1(Y)$. With a little extra work, we can check that $\Pi_1\colon \mathsf{Top} \to \mathsf{Gpd}$, defined this way, is really a functor.

Perhaps you are finding this confusing. If so, it could easily be because there are several levels of categories and such going on here. For example, $\Pi_1(X)$ is a groupoid, and thus a category, so there are morphisms like $f\colon x \to y$ in it; but on the other hand Gpd itself is a category, and there are morphisms like $\Pi_1(F)\colon \Pi_1(X) \to \Pi_1(Y)$ in it, which are functors! If you find this confusing, take heart. Getting confused this way is crucial to learning n-category theory! After all, n-category theory is all about how every "process" is also a "thing" which can undergo higher-level "processes". Complex, interesting structures emerge from very simple ones by the interplay of these different levels. It takes work to mentally hop up and down these levels, and to weather the inevitable "level slips" one makes when one screws up. If you expect it to be easy and are annoyed when you mess up, you will hate this subject. When approached in the right spirit, it is very fun; it teaches one a special sort of agility. (We are, by the way, nowhere near the really tricky stuff yet.)

Okay, so we have seen an interesting example of a functor

$$\Pi_1\colon \mathsf{Top} \to \mathsf{Gpd}$$

. As I said, we can think of this as going from the concrete world of spaces to the abstract world of groupoids, turning concrete paths into abstract "morphisms". Wonderfully, there is a kind of "reverse" functor,

$$K\colon \mathsf{Gpd} \to \mathsf{Top}$$

which turns the abstract into the concrete, by making abstract morphisms into concrete paths! Basically, it goes like this. Say we have a groupoid G. We will build the space $K(G)$ out of simplices as follows. Start with one 0-simplex for each object in G. A 0-simplex is simply a point. We can draw the 0-simplex for the object x as follows:

$$x$$

Then put in one 1-simplex for each morphism in G. A 1-simplex is just a line segment. We can draw the 1-simplex for the morphism $f\colon x \to y$ as follows:

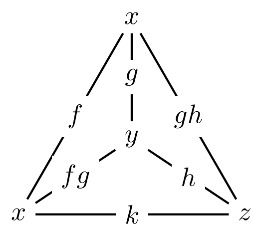
$$x \text{ --- } f \text{ --- } y$$

Really we should draw an arrow going from left to right, but soon things will get too messy if I do that, so I won't. Then, whenever we have $fg = h$ in the groupoid, we stick in a 2-simplex. A 2-simplex is just a triangle and we visualize it as follows:

```
        y
       / \
      f   g
     /     \
    x -- h -- z
```

$$f\colon x \to y$$
$$g\colon x \to z$$
$$h\colon y \to z$$

Then, whenever we have $fgh = k$ in the groupoid, we stick in a 3-simplex, which we can visualize as a tetrahedron like this



$$f\colon w \to x$$
$$g\colon x \to y$$
$$h\colon y \to z$$
$$k\colon w \to z$$

And so on... we do this forever and get a big "simplicial complex," which we can think of as the topological space KG. The reader might want to compare "Week 70", where do the same thing for a monoid instead of a groupoid. Really, one can do it for any category.

That's how we define K on objects; it's not hard to define K on morphisms too, so we get

$$K\colon \mathsf{Gpd} \to \mathsf{Top}$$

In case you study this in more detail at some point, I should add that folks often think of simplicial complexes as somewhat abstract combinatorial objects in their own right, and then they break down K into two steps: first they take the "nerve" of a groupoid and get a simplicial complex, and then they take the "geometrical realization" of the simplicial complex to get a topological space. For more on simplicial complexes and the like, try:

1) J. P. May, *Simplicial Objects in Algebraic Topology*, Van Nostrand, Princeton, 1968.

Now, in what sense is the functor $K\colon \mathsf{Gpd} \to \mathsf{Top}$ the "reverse" of the functor $\Pi_1\colon \mathsf{Top} \to \mathsf{Gpd}$? Is it just the "inverse" in the traditional sense? No! It's something more subtle. As we shall see, the fact that Cat is a 2-category means that a functor can have a more subtle and interesting sorts of "reverse" than one might expect if one were used to the simple "inverse" of a function. This is something I alluded to in "Week 74": inverses become subtler as we march up the n-categorical hierarchy.

I'll talk about this more later. But let me just drop a teaser... Quantum mechanics is all about Hilbert spaces and linear operators between them. Given any (bounded) linear operator $F\colon H \to H'$ from one Hilbert space to another, there is a subtle kind of "reverse" operator, called the "adjoint" of F and written $F^*\colon H' \to H$, defined by

$$\langle x, F^* y\rangle = \langle Fx, y\rangle$$

for all x in H and y in H'. This is not the same as the "inverse" of F; indeed, it exists even if F is not invertible. This sort of "reverse" operator is deeply related to the "reverse" functors I am hinting at above, and for this reason those "reverse" functors are also called "adjoints". This is part of a profound and somewhat mysterious relationship between quantum theory and category theory... which I eventually need to describe.

To continue reading the "Tale of n-Categories", see "Week 76".

Week 76

March 9, 1996

Yesterday I went to the oral exam of Hong Xiang, a student of Richard Seto who is looking for evidence of quark-gluon plasma at Brookhaven. The basic particles interacting via the strong force are quarks and gluons; these have an associated kind of "charge" known as color. Under normal conditions, quarks and gluons are confined to lie within particles with zero total color, such as protons and neutrons, and more generally the baryons and mesons seen in particle acccelerators — and possibly glueballs, as well. (See "Week 68" for more on glueballs.)

However, the current theory of the strong force — quantum chromodynamics — predicts that at sufficiently high densities and/or pressures, a plasma of protons and neutrons should undergo a phase transition called "deconfinement", past which the quarks and gluons will roam freely. At low densities, this is expected to happen at a temperature corresponding to about 200 MeV per nucleon (i.e., proton or neutron). If my calculation is right, this is about 2 trillion Kelvin! At low temperatures, it's expected to happen at about 5 to 20 times the density of an atomic nucleus. (Normal nuclear matter has about 0.16 nucleons per femtometer cubed.) For more on this, check out these:

1) Relativistic Heavy Ion Collider homepage, `http://www.bnl.gov/RHIC/`

   CERN Courier, "Phase diagram of nuclear matter", `http://www.cerncourier.com/main/article/40/5/17/1/cernquarks1_6-00`

The folks at Brookhaven are attempting to get high densities *and* temperatures by slamming two gold nuclei together. They are getting densities of about 9 times that of a nucleus... and I forget what sort of temperature, but there is reason to hope that the combined high density and pressure might be enough to cause deconfinement and create a "quark-gluon plasma". Colliding gold on gold at high energies produces a enormous spray of particles, but amidst this they are looking for a particular signal of deconfinement. They are looking for φ-mesons and looking to see if their lifetime is modified. A φ-meson is a spin-1 particle made of a strange quark / strange antiquark pair; strange quarks and antiquarks are supposed to be common in the quark-gluon plasma formed by the collision. Folks think the lifetime of a φ-meson will be affected by the medium it finds itself in, and that this should serve as a signature of deconfinement. In fact, they may have already seen this!

People might also enjoy looking at this review article:

2) Adriano Di Giacomo, "Mechanisms of colour confinement", preprint available as `hep-th/9603029`.

Okay, let me continue the tale of n-categories. I want to lead up to their role in physics, but to do it well, there are quite a few things I need to explain first. One of the important things about n-category theory is that they allow a much more fine-grained approach to the notion of "sameness" than we would otherwise be able to achieve.

In a bare set, two elements x and y are either equal or not equal; there is nothing much more to say.

In a category, two objects x and y can be equal or not equal, but more interestingly, they can be *isomorphic* or not, and if they are, they can be isomorphic in many different ways. An isomorphism between x and y is simply a morphism $f\colon x \to y$ which has an inverse $g\colon y \to x$. (For a discussion of inverse morphisms, see "Week 74".)

For example, in the category Set an isomorphism is just a one-to-one and onto function. If we know two sets x and y are isomorphic we know that they are "the same in a way", even if they are not equal. But specifying an isomorphism $f\colon x \to y$ does more than say x and y are the same in a way; it specifies a *particular way* to regard x and y as the same.

In short, while equality is a yes-or-no matter, a mere *property*, an isomorphism is a *structure*. It is quite typical, as we climb the categorical ladder (here from elements of a set to objects of a category) for properties to be reinterpreted as structures, or sometimes vice-versa.

What about in a 2-category? Here the notion of equality sprouts still further nuances. Since I haven't defined 2-categories in general, let me work with an example, Cat. This has categories as its objects, functors as its morphisms, and natural transformations as its 2-morphisms.

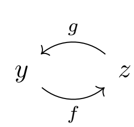
So... we can certainly speak, as before, of the *equality* of categories. We can also speak of the *isomorphism* of categories: an isomorphism between $\mathcal{C}$ and $\mathcal{D}$ is a functor $F\colon \mathcal{C} \to \mathcal{D}$ for which there is an inverse functor $G\colon \mathcal{D} \to \mathcal{C}$. I.e., FG is the identity functor on $\mathcal{C}$ and GF is the identity on $\mathcal{D}$, where we define the composition of functors in the obvious way. But because we also have natural transformations, we can also define a subtler notion, the *equivalence* of categories. An equivalence is a functor $F\colon \mathcal{C} \to \mathcal{D}$ together with a functor $G\colon \mathcal{D} \to \mathcal{C}$ and natural isomorphisms $a\colon FG \to 1_C$ and $b\colon GF \to 1_D$. A "natural isomorphism" is a natural transformation which has an inverse.

Abstractly, I hope you can see the pattern here: just as we can "relax" the notion of equality to the notion of isomorphism when we pass from sets to categories, we can relax the condition that FG and GF equal identity functors to the condition that they be isomorphic to identity functors when we pass from categories to the 2-category Cat. We need to have the natural transformations to be able to speak of functors being isomorphic, just as we needed functions to be able to speak of sets being isomorphic. In fact, with each extra level in the theory of n-categories, we will be able to come up with a still more refined notion of "n-equivalence" in this way. That's what "processes between processes between processes..." allow us to do.

But let me attempt to bring this notion of equivalence of categories down to earth with some examples. Consider first a little category $\mathcal{C}$ with only one object x and one morphism, the identity morphism $1_x\colon x \to x$. We can draw $\mathcal{C}$ as follows:

$$x$$

where we don't bother drawing the identity morphism 1_x. This category, by the way, is called the "terminal category". Next consider a little category $\mathcal{D}$ with two objects y and z and only four morphisms: the identities 1_y and 1_z, and two morphisms $f\colon y \to z$ and $g\colon z \to y$ which are inverse to each other. We can draw $\mathcal{D}$ as follows:



where again we don't draw identities.

So: $\mathcal{C}$ is a little world with only one object, while D is a little world with only two isomorphic objects... that are isomorphic in precisely one way! $\mathcal{C}$ and $\mathcal{D}$ are clearly not isomorphic, because for a functor $F\colon \mathcal{C} \to \mathcal{D}$ to be invertible it would need to be one-to-one and onto on objects, and also on morphisms.

However, $\mathcal{C}$ and $\mathcal{D}$ are equivalent. For example, we can let $F\colon \mathcal{C} \to \mathcal{D}$ be the unique functor with $F(x) = y$, and let $G\colon \mathcal{D} \to \mathcal{C}$ be the unique functor from $\mathcal{D}$ to $\mathcal{C}$. (There is only one functor from any category to $\mathcal{C}$, since $\mathcal{C}$ has only one object and one morphism; this is why we call $\mathcal{C}$ the terminal category.) Now, if we look at the functor $FG\colon \mathcal{C} \to \mathcal{C}$, it's not hard to see that this is the identity functor on $\mathcal{C}$. But the composite $GF\colon \mathcal{D} \to \mathcal{D}$ is not the identity functor on $\mathcal{D}$. Instead, it sends both y and z to y, and sends all the morphisms in $\mathcal{D}$ to 1_y. But while not *equal* to the identity functor on $\mathcal{D}$, the functor GF is *naturally isomorphic* to it. We can define a natural transformation $b\colon GF \to 1_D$ by setting $b_y = 1_y$ and $b_z = f$. Here some folks may want to refresh themselves on the definition of natural transformation, given in "Week 75", and check that b is really one of these, and that b is a natural isomorphism because it has an inverse.

The point is, basically, that having two uniquely isomorphic things with no morphisms other than the isomorphisms between them and the identity morphisms isn't really all that different from having one thing with only the identity morphism. Category theorists generally regard equivalent categories as "the same for all practical purposes". For example, given any category we can find an equivalent category in which any two isomorphic objects are equal. We call these "skeletal" categories because all the fat is gone and just the essential bones are left. For example, the category FinSet of finite sets, with functions between them as morphisms, is equivalent to the category with just the sets

$$0 = \{\}$$
$$0 = \{0\}$$
$$0 = \{0, 1\}$$
$$0 = \{0, 1, 2\}$$

etc., and functions between them as morphisms (see "Week 73"). Essentially all the mathematics that can be done in FinSet can be done in this skeletal category. This may seem shocking, but it's true.... Similarly, the category Set is equivalent to the category Card having one set of each cardinality. Also, the category Vect of complex finite–dimensional vector spaces, with linear functions between them as morphisms, is equivalent to a skeletal category where the only objects are those of the form $\mathbb{C}^n$. *This* example should not seem shocking; it's this fact which allows unsophisticated people to do linear algebra under the impression that all finite-dimensional vector spaces are of the form $\mathbb{C}^n$, and still manage to do all the practical computations that more sophisticated people can do, who know the abstract definition of vector space and thus know of lots more finite-dimensional vector spaces.

However, there is another thing we can do in Cat, another refinement of the notion of isomorphism, which I alluded to in "Week 75". This is the notion of "adjoint functor". Let me mention a few examples (in addition to the example given in "Week 75") and let the reader ponder them before giving the definition. Let Grp denote the category with groups as objects and homomorphisms as morphisms, a homomorphism $f\colon G \to H$ between groups being a function with $f(1) = 1$ and $f(gh) = f(g)f(h)$ for all g, h in G.

Then there is a nice functor

$$L\colon \mathsf{Set} \to \mathsf{Grp}$$

which takes any set S to the free group on S: this is the group $L(S)$ formed by all formal products of elements in S and inverses thereof, with no relations other than those in the definition of a group. For example, a typical element of the free group on $\{x, y, z\}$ is $xyzy^{-1}xxy$.

(It's easy to see that $f\colon S \to T$ is a function between sets, there is a unique homomorphism $L(f)\colon L(S) \to L(T)$ with $L(f)(x) = f(x)$ for all x in S, and that this makes L into a functor.)

There is also a nice functor

$$R\colon \mathsf{Grp} \to \mathsf{Set}$$

taking any group to its underlying set, and taking any homomorphism to its underlying function. We call this a "forgetful" functor since it simply amounts to forgetting that we are working with groups and just thinking of them as sets.

Now there is a sense in which L and R are reverse processes, but it is delicate. They certainly aren't inverses, and they aren't even part of an equivalence between Set and Grp. Nonetheless they are "adjoints". If the reader hasn't thought about this, she may enjoy figuring out what this might mean... perhaps keeping the adjoint operators mentioned in "Week 75" in mind.

To continue reading the "Tale of n-Categories", see "Week 77"

Week 77

March 23, 1996

I spent last week at Penn State visiting the CGPG — the Center for Gravitational Physics and Geometry. I like to visit this place whenever I can, because I've never found anywhere else that's as good for talking about quantum gravity.

The CGPG is run by Abhay Ashtekar, who introduced the "new variables" for general relativity (see "Week 7"). This formulation of general relativity allowed Carlo Rovelli and Lee Smolin to develop a new approach to quantum gravity, called the "loop representation". Smolin is at the CGPG, while Rovelli teaches at Pittsburgh, only a brief plane ride away: he was heading back just when I showed up. Jorge Pullin, who has done a lot of work on knot theory and quantum gravity, is also at the CGPG. Roger Penrose visits it regularly, and happened to be there last week. There is always a peppy bunch of grad students and postdocs wandering about the place, and some interesting mathematicians across the street. I have a particular interest in the work of Jean-Luc Brylinski, since he has thought a lot about the relationships between conformal field theory and category theory (see "Week 25").

You can find out more about the CGPG and the new variables at the following web sites:

1) Center for Gravitational Physics and Geometry (CGPG) home page, 'http://vishnu.nirvana.phys.psu.edu/"

   Reading list on the new variables: `http://vishnu.nirvana.phys.psu.edu/readinglist/readinglist.html`

I had two goals at the CGPG. One was to get people interested in the uses of higher-dimensional algebra in physics, and the other was to find out where folks were heading in quantum gravity. I made decent headway on the first front, but let me talk about the second one.

In the last few years, Abhay Ashtekar has been working hard with a bunch of collaborators on getting the loop representation set up on a mathematically rigorous basis, and making good progress. There is a natural order in which to set things up, and the next problem to deal with is the so-called Hamiltonian constraint (see "Week 43"). I have always been very worried about this, and I'm not alone, since this all the dynamics of quantum gravity is in this operator. Ashtekar and Lewandowski have a paper partially written in which they rigorously define an operator along these lines, using earlier ideas of Rovelli and Smolin. I have been hoping that this answer could be tested somehow... for example, checking out its commutation relations with the other constraints. It turns out that they have already done this to extent that seems possible. So then the question is, what next? March on, or continue trying to make sure the Hamiltonian constraint is right?

I should add that Pullin and Gambini have another proposal regarding the Hamiltonian constraint:

2) Rodolfo Gambini and Jorge Pullin, "The general solution of the quantum Einstein equations?", preprint in Revtex format, 7 figures included with `psfig`, available as `gr-qc/9603019`.

This is not as fully worked out, but it has a certain mathematical charm to it so far. Thus we may eventually be in a situation where there are various competing quantizations of gravity using the loop representation, differing mainly in their choice of Hamiltonian constraint. This suggests that we need further tests for what counts as the "right" Hamiltonian constraint.

When we spoke this time, Ashtekar was in favor of testing Hamiltonian constraints by seeing whether they implied the "Bekenstein bound". This bound says that the maximal entropy of a physical system is proportional to its surface area when we take quantum gravity into account. There are a number of heuristic derivations of this bound, so lots of people hope it would follow from any good theory of quantum gravity. Since the "physical states" of quantum gravity must be annihilated by the Hamiltonian constraint, and the maximal entropy of a system is just the logarithm of the number of physical states, the Hamiltonian constraint must have some interesting properties to get the Bekenstein bound to work out. So we can expect some work along these lines in the near future.

I also talked to Lee Smolin. He has been very interested in the relation between the loop representation and certain simplified versions of quantum gravity called topological quantum field theories (TQFTs). He has ideas on how to derive the Bekenstein bound using this relationship — see "Week 56" and "Week 57" for a description.

The funny thing is, some of the mathematics connecting TQFTs to the loop representation of quantum gravity also connects TQFTs to another well-known approach to quantum gravity — string theory! Smolin has been boning up on string theory lately, in part by giving a course on the subject, and presently he is eager to bring string theory and the loop representation closer together. So we can also expect to see more work on attempts to unify string and loops. (See "Week 18" for a bit more on strings and loops.)

So I left feeling reinvigorated and eager to continue my own work on higher-dimensional algebra and physics. . . which is what I have talking about here ever since "Week 73". In fact, I have been engaging in a lengthy warmup, a minicourse in category theory, with an eye to the basic themes of n-category theory. That way, when I get around to the really cool stuff, everyone out there will know what the heck I'm talking about. In theory, anyway. You gotta work a bit to wrap your mind around these concepts!

So, let's recall where we are in our tale of n-categories. We were studying increasingly subtle variations on the theme of identity and difference. Given two categories $\mathcal{C}$ and $\mathcal{D}$, we can ask if they are *equal* or not. We can also discuss *isomorphisms* between $\mathcal{C}$ and $\mathcal{D}$. An isomorphism is a functor $F\colon \mathcal{C} \to \mathcal{D}$ having an inverse: a functor $G\colon \mathcal{D} \to \mathcal{C}$ such that FG is equal to the identity functor on $\mathcal{D}$ and GF is equal to the identity on $\mathcal{C}$.

We can also discuss *equivalences* between $\mathcal{C}$ and $\mathcal{D}$. An equivalence is a functor $F\colon \mathcal{C} \to \mathcal{D}$ together with a functor $G\colon \mathcal{D} \to \mathcal{C}$ such that FG is naturally isomorphic to the identity functor on $\mathcal{D}$, and GF is naturally isomorphic to the identity functor on $\mathcal{C}$. Remember, two functors from one category to another are "naturally isomorphic" if there is a natural transformation from the first to the second, and that natural transformation has an inverse.

In math jargon we say it this way: two categories are equivalent if there is a functor from one to the other which is invertible "up to a natural isomorphism".

The most useful notion of categories being "the same" turns out to be not equality, or isomorphism, but this more supple notion of "equivalence"!

(As we shall see later, this is because Cat is a 2-category. Remember, an n-category is some sort of thing with objects, morphisms, 2-morphisms, and so on up to n-morphisms. One of the of the main themes of n-category theory is that we may regard two things are "the same", or "equivalent", if there is some sort of process to get from one to the other, and this process is invertible... up to equivalence! More precisely, we say an n-morphism is an equivalence if it's invertible, and then we work our way down, inductively defining a $(j-1)$-morphism to be an equivalence if it's invertible up to an equivalence. This downwards induction leaves off when we define equivalence for "0-morphisms", meaning objects.)

We have also begun talking about a curious situation where the categories $\mathcal{C}$ and $\mathcal{D}$ are not at all "the same," but there are "adjoint" functors $L\colon \mathcal{C} \to \mathcal{D}$ and $R\colon \mathcal{D} \to C$. Let me list some examples before defining the concept of adjoint functor and talking about it:

1. First for the one we discussed in "Week 76". Let Set be the category of sets, and Grp the category of groups. Let $L\colon$ Set $\to$ Grp be the functor taking each set S to the free group on S, and doing the obvious thing to morphisms. Let $R\colon$ Grp $\to$ Set be the functor taking each group to its underlying set.

2. Let Ab be the category of abelian (i.e., commutative) groups. Let $L\colon$ Set $\to$ Ab be the functor taking each set S to the free abelian group on S. The "free abelian group" on S is what we get by taking the free group on S and imposing commutativity relations like $xy = yx$ for all elements x, y in S. Let $R\colon$ Ab $\to$ Set be the functor taking each abelian group to its underlying set.

3. Let $L\colon$ Grp $\to$ Ab be the functor taking each group G to its "abelianization". The abelianization of G is what we get when we impose the extra relations $xy = yx$ for all elements x, y in G. Let $R\colon$ Ab $\to$ Grp be the functor taking each abelian group to its underlying group.

4. Let Mon be the category of monoids, where the objects are monoids and the morphisms are monoid homomorphisms. (Remember that a monoid is a set with an associative product and a unit; a monoid morphism $f\colon M \to N$ is a function between monoids such that $f(xy) = f(x)f(y)$ and $f(1) = 1$.) Let $L\colon$ Set $\to$ Mon be the functor taking each set S to the free monoid on S. This is simply the set of words whose letters are elements of S, with the product given by concatenation of words, and the unit being the empty word. Let $R\colon$ Mon $\to$ Set be the functor taking each monoid to its underlying set.

5. Let $L\colon$ Mon $\to$ Grp be the functor taking each monoid M to the group obtained by throwing in formal inverses for every element of M. The famous example of this is when $\mathbb{N} = \{0, 1, 2, ...\}$, which is a monoid whose "product" is addition. Then $L(\mathbb{N}) = \mathbb{Z}$, the integers, since we have thrown in the negative integers. Let $R\colon$ Grp $\to$ Mon be the functor taking each group to its underlying monoid. I.e., R simply forgets that our group has inverses and thinks of it as a monoid.

Note the common aspects of these examples! In most of them, $L\colon \mathcal{C} \to \mathcal{D}$ gives us a "free" object of $\mathcal{D}$ for every object of $\mathcal{C}$, while $R\colon \mathcal{D} \to \mathcal{C}$ gives us an "underlying" object of $\mathcal{C}$ for every object of $\mathcal{D}$. This is an especially good way to think about it when the objects of $\mathcal{D}$ are objects of $\mathcal{C}$ equipped with extra structure, as in examples 1, 2, 4, and 5. For example, a group is a set equipped with some extra structure, the group operations. In this case, the functor $L\colon \mathcal{C} \to \mathcal{D}$ turns an object of $\mathcal{C}$ into an object of $\mathcal{D}$ by "freely throwing in whatever extra stuff is necessary, without putting in any relations other than those needed to get an object of $\mathcal{D}$".

It's not quite the same when the objects of $\mathcal{D}$ are objects of $\mathcal{C}$ with extra *properties*, as in example 3. In this case, the functor $L\colon \mathcal{C} \to \mathcal{D}$ forces an object of $\mathcal{C}$ to have the properties needed to be an object of $\mathcal{D}$. It does so in as nonviolent a manner as possible.

In either of these situations, $R\colon \mathcal{D} \to \mathcal{C}$ has the flavor of what we call a "forgetful" functor. This is not a precisely defined term, but folks use it whenever we can simply "forget" something about an object of $\mathcal{D}$ and think of it as an object of $\mathcal{C}$. For example, we can take a group, and forget about the group operations, thinking of it as merely a set. Here we are forgetting extra structure; we can also forget extra properties.

The crucial thing here is that unlike in an equivalence, there is a built-in asymmetry here: L and R have very different flavors, and serve different mathematical purposes. We call L the "left adjoint" of R, and we call R the "right adjoint" of L.

There are situations where adjoint functors L and R aren't so immediately reminiscent of the concepts "free" and "underlying". But it's good to keep these ideas in mind when learning about adjoint functors. I used to have trouble remembering which was supposed to be the left adjoint and which was the right. The honest way to do this is to remember the definition (coming up soon), but for a cheap mnemonic, you can think of the L in a left adjoint as standing for "liberty" — that is, freedom!

So what's the definition of "adjoint"? Roughly speaking, it's that for any object c of $\mathcal{C}$ and any object d of $\mathcal{D}$, we have

$$\mathrm{Hom}(Lc, d) = \mathrm{Hom}(c, Rd).$$

Actually this is a slight exaggeration: we don't want these to be equal. The guy on the left is the set of morphisms from Lc to d in the category $\mathcal{D}$. The guy on the right is the set of morphisms from c to Rd in the category $\mathcal{C}$. So it's evil to want them to be *equal*. As you might guess, it's enough for them to be naturally isomorphic in some sense. Let's not worry about that too much yet, though. Let's get the basic idea here!

Consider example 1. Say S is a set and G is a group. Why is

$$\mathrm{Hom}(LS, G)$$

naturally isomorphic to

$$\mathrm{Hom}(S, RG)\,?$$

In other words, why is the set of homomorphisms from the free group on S to G naturally isomorphic to the set of functions from S to the underlying set of G?

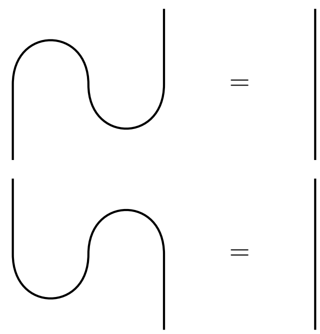
Well, say we have a homomorphism $f\colon LS \to G$. Since LS is a free group, we know f if we know what it does to each element of $S\ldots$ and it can do whatever it wants to these elements! So we can think of it as just a function from S to the underlying set of G. In other words, we can think of it as a function $f'\colon S \to RG$. Conversely, any function $f'\colon S \to RG$ gives us a homomorphism $f\colon LS \to G$.

So this is the idea. Say we have an object c of $\mathcal{C}$ and an object d of $\mathcal{D}$. Then:

> *"The set of morphisms from the free $\mathcal{D}$-object on c to d is naturally isomorphic to the set of morphisms from c to the underlying $\mathcal{C}$-object of d."*

Next time I will finish off the definition of adjoint functors, by making this "naturally isomorphic" stuff precise. I will also begin to explain what adjoint functors have to do with adjoint operators in quantum mechanics. Remember that an "observable" in quantum theory is an operator on a Hilbert space which is its own adjoint, while a "symmetry" in quantum theory is an operator whose adjoint is its inverse. I eventually hope to show that this, and many other shocking aspects of quantum theory, become less shocking when we think of the world in terms of categories (or n-categories) rather than sets. The way I think of it these days, the mysterious way quantum theory slammed into physics in the early 20th century was just nature's way of telling us we'd better learn n-category theory.

I'll also explain what adjoint functors have to do with the following topological equations:



To continue reading the "Tale of n-Categories", see "Week 78".

Week 78

March 28, 1996

Last Week I began explaining the concept of "adjoint functor". This Week I want to finish explaining it - or at least finish one round of explanation! Then we'll begin to be able to see the unity of category theory, topology, and quantum theory. These may seem rather distinct subjects, but they have an interesting tendency to blur together when one is doing topological quantum field theory. Part of the point of higher-dimensional algebra is to explain this.

So, remember the idea of adjoint functors. Say we have categories $\mathcal{C}$ and $\mathcal{D}$ and functors $L\colon \mathcal{C} \to \mathcal{D}$ and $R\colon \mathcal{D} \to \mathcal{C}$. Then we say L is the "left adjoint" of R, or that R is the "right adjoint" of L, if for any object c of $\mathcal{C}$ and object d of $\mathcal{D}$, there is a natural one-to-one correspondence between $\mathrm{Hom}(Lc, d)$ and $\mathrm{Hom}(c, Rd)$. An example to keep in mind is when $\mathcal{C}$ is the category of sets and $\mathcal{D}$ is the category of groups. Then L turns any set into the free group on that set, while R turns any group into the underlying set of that group. All sorts of other "free" and "underlying" constructions are also left and right adjoints, respectively.

Now the only thing I didn't make very precise is what I mean by "natural" in the above paragraph. Informally, the idea of a "natural" one-to-one correspondence is that doesn't depend on any arbitrary choices. The famous example is that if we have a finite-dimensional vector space V, it's always isomorphic to its dual V^*, but not naturally so: to set up an isomorphism we need to pick a basis e_i of V, and this gives a dual basis f^i of V^*, and then we get an isomorphism sending e_i to f^i, but this isomorphism depends on our choice of basis. But V is *naturally* isomorphic to its double dual $(V^*)^*$.

Now, it's hard to formalize the idea of "not depending on any arbitrary choices" directly, so one needs to reflect on why it's bad for an isomorphism to depend on arbitrary choices. The main reason is that the arbitrariness may break a useful symmetry. In fact, Eilenberg and Mac Lane invented category theory in order to formalize this idea of "naturality as absence of symmetry-breaking". Of course, they needed the notion of category to get a sufficiently general concept of "symmetry". They realized that a nice way to turn things in the category $\mathcal{C}$ into things in the category $\mathcal{D}$ is typically a functor $F\colon \mathcal{C} \to \mathcal{D}$. And then, if we have two functors $F, G\colon \mathcal{C} \to \mathcal{D}$, they defined a "natural transformation" from F to G to be a bunch of morphisms

$$T_c\colon F(c) \to G(c),$$

one for each object c of $\mathcal{C}$, such that the following diagram commutes for every morphism $f\colon c \to c'$ in $\mathcal{C}$:

$$\begin{array}{ccc} F(c) & \xrightarrow{F(f)} & F(c') \\ {\scriptstyle T_c}\downarrow & & \downarrow{\scriptstyle T_{c'}} \\ G(c) & \xrightarrow[G(f)]{} & G(c') \end{array}$$

This condition says that the transformation T gets along with all the "symmetries", or more precisely morphisms f, in the category $\mathcal{D}$. We can do it either before or after applying one of these symmetries, and we get the same result. For example, a vector space construction which depends crucially on a choice of basis will fail this condition if we take f to be a linear transformation corresponding to a change of basis.

A "natural isomorphism" is then just a natural transformation that's invertible, or in other words, one for which all the morphisms T_c are isomorphisms.

Okay. Hopefully that explains the idea of "naturality" a bit better. But right now we are trying to figure out what we mean by saying that $\mathrm{Hom}(Lc, d)$ and $\mathrm{Hom}(c, Rd)$ are naturally isomorphic. To do this, we need to introduce a couple more ideas: the product of categories, and the opposite of a category.

First, just as you can take the Cartesian product of two sets, you can take the Cartesian product of two categories, say $\mathcal{C}$ and $\mathcal{D}$. It's not hard. An object of $\mathcal{C}\times\mathcal{D}$ is just a pair of objects, one from $\mathcal{C}$ and one from $\mathcal{D}$. A morphism in $\mathcal{C}\times\mathcal{D}$ is just a pair of morphisms, one from $\mathcal{C}$ and one from $\mathcal{D}$. And everything works sort of the way you'd expect.

Second, if you have a category $\mathcal{C}$, you can form a new category $\mathcal{C}^{\mathrm{op}}$, the opposite of $\mathcal{C}$, which has the same objects as $\mathcal{C}$, and has the arrows in $\mathcal{C}$ turned around backwards. In other words, a morphism $f\colon x \to y in \mathcal{C}^{\mathrm{op}}$ is defined to be a morphism $f\colon y \to x in \mathcal{C}$, and the composite fg of morphisms in $\mathcal{C}^{\mathrm{op}}$ is defined to be their composite gf in $\mathcal{C}$. So $\mathcal{C}^{\mathrm{op}}$ is like a through-the-looking-glass version of $\mathcal{C}$ where they do everything backwards. A functor $F\colon \mathcal{C}^{\mathrm{op}} \to \mathcal{D}$ is also called a "contravariant" functor from $\mathcal{C}$ to $\mathcal{D}$, since we can think of it as a screwy functor from $\mathcal{C}$ to $\mathcal{D}$ with $F(fg) = F(g)F(f)$ instead of the usual $F(fg) = F(f)F(g)$. Whenever you see this perverse contravariant behavior going on, you should suspect an opposite category is to blame.

Now, it turns out that we can think of the "Hom" in a category $\mathcal{C}$ as a functor

$$\mathrm{Hom}(-,-)\colon \mathcal{C}^{\mathrm{op}}\times\mathcal{C} \to \mathsf{Set}$$

Here the $-$'s denote blanks to be filled in. Obviously, for any object (x, x') in $\mathcal{C}^{\mathrm{op}}\times\mathcal{C}$, there is a nice juicy set $\mathrm{Hom}(x, x')$, the set of all morphisms from $x to x'$. But what if we have a morphism

$$(f, f')\colon (x, x') \to (y, y')$$

in $\mathcal{C}^{\mathrm{op}}\times\mathcal{C}$? For $\mathrm{Hom}(-,-)$ to be a functor, we should get a nice juicy function

$$\mathrm{Hom}(f, f')\colon\ \mathrm{Hom}(x, x') \to \mathrm{Hom}(y, y').$$

How does this work? Well, remember that a morphism (f, f') as above is really just a pair consisting of a morphism $f\colon x \to y$ in $\mathcal{C}^{\mathrm{op}}$ and a morphism $f'\colon x' \to y'$ in $\mathcal{D}$. A morphism $f\colon x \to y$ in $\mathcal{C}^{\mathrm{op}}$ is just a morphism $f\colon y \to x$ in $\mathcal{D}$. Now say we have an unsuspecting element g of $\mathrm{Hom}(x, x')$ and we want to hit it with $\mathrm{Hom}(f, f')$ to get something in $\mathrm{Hom}(y, y')$. Here's how we do it:

$$\mathrm{Hom}(f, f')\colon g \mapsto f'gf$$

We compose it with f' on the left and f on the right! Composing on the left is a nice covariant thing to do, but composing on the right is contravariant, which is why we needed the opposite category $\mathcal{C}^{\mathrm{op}}$.

Okay, now back to our adjoint functors $L\colon \mathcal{C} \to \mathcal{D}$ and $R\colon \mathcal{D} \to \mathcal{C}$. Now we are ready to say what we mean by $\mathrm{Hom}(Lc, d)$ and $\mathrm{Hom}(c, Rd)$ being naturally isomorphic. Using the stuff we have set up, we can define two functors

$$\mathrm{Hom}(L-,-)\colon \mathcal{C}^{\mathrm{op}}\times\mathcal{D} \to \mathsf{Set}$$

and

$$\mathrm{Hom}(-,R-)\colon \mathcal{C}^{\mathrm{op}}\times\mathcal{D}\to \mathsf{Set}$$

and we are simply saying that for L and R to be adjoints, we demand the existence of a natural isomorphism between these functors!

Of course, this seems abstract, but if you work it out in some of the examples of adjoint functors given in "Week 76" you'll see it all makes good sense.

Now let me start explaining what this all has to do with quantum theory. (I'll put off the topology until next Week.) First of all, the "Hom functor" we introduced,

$$\mathrm{Hom}(-,-)\colon \mathcal{C}^{\mathrm{op}}\times\mathcal{C}\to \mathsf{Set}$$

should remind you a whole lot of the inner product on a Hilbert space H. The inner product is linear in one slot and conjugate-linear in the other, just like Hom is covariant in one slot and contravariant in the other. In fact, the inner product can be thought of as a bilinear map

$$\langle -,-\rangle\colon H^{\mathrm{op}}\times H\to\mathbb{C}$$

where H^{op}, the "opposite" Hilbert space, is like H but with a complex conjugate thrown into the definition of scalar multiplication, and here $\mathbb{C}$ denotes the complex numbers!

Second of all, the definition of adjoint functor, with $\mathrm{Hom}(Lc,d)$ and $\mathrm{Hom}(c,Rd)$ being naturally isomorphic, should remind you of adjoint linear operators on Hilbert spaces. If we have a linear operator $L\colon H\to K$ from a Hilbert space H to a Hilbert space K, its adjoint $R\colon K\to H$ is given by

$$\langle Lh,k\rangle=\langle h,Rk\rangle$$

for all h in H and k in K.

In fact, the whole situation with adjoint functors is a kind of "categorified" version of the situation with adjoint linear operators. Everything has been boosted up one notch on the n-categorical ladder. What I mean is this: the Hilbert spaces H and K above are *sets*, with *elements* h and k, while the categories $\mathcal{C}$ and $\mathcal{D}$ are *categories*, with *objects* c and d. The inner product of two elements of a Hilbert space is a *number*, while the hom of two objects in a category is a *set*. Most interesting, the definition of adjoint operators requires that $\langle Lh,k\rangle$ and $\langle h,Rk\rangle$ be *equal*, while the definition of adjoint functors requires only that $\langle Lc,d\rangle$ and $\langle c,Rd\rangle$ be *naturally isomorphic*.

So we can think of adjoints in category theory as a boosted-up version of the adjoints in quantum theory. But these days, I prefer to think of the adjoints in quantum theory as a watered-down or "decategorified" version of the adjoints in category theory. The reason is that categorification — as noted by Louis Crane, who I believe invented the term — is a risky, hit-or-miss business, while decategorification is much more systematic. Decategorification is the simply the process of neglecting the difference between isomorphism and equality. If we start with an n-category and then get lazy and decide to think of invertible n-morphisms as *equations* between the $(n-1)$-morphisms, we get an $(n-1)$-category. If we keep slacking off like this, before you know it we're doing set theory! The final stage of decategorification is when we get sloppy and instead of keeping track of *set*, we merely record the *number* of its elements.

It's amusing to imagine this process of decategorification as one of those elaborate Gnostic myths about the Fall. We start in the paradise of ω-categories (or perhaps even higher up), but by the repeated sin of confusing equality with isomorphism we fall all the way down the n-categorical ladder to the crude world of sets, or worse, simply numbers. But all this happened a long time ago: now we need to work our butt off to climb back up! In other words, historically our early ancestors dealt with finite sets by replacing them with something cruder: their numbers of elements. Counting is actually very handy, of course, but it can only tell if the cardinalities of two sets are *equal*; it doesn't address the problem of specific *isomorphisms* between sets. To climb back up the n-categorical ladder, we needed to start with the set $\mathbb{N}$ of natural numbers

$$0, 1, 2, 3, \ldots$$

and by dint of strenous mental effort realize that this is just the decategorification of the category FinSet of finite sets. (In fact, category-theorists routinely use 2 to stand for the 2-element set in the skeletal category equivalent to FinSet, and so on — see "Week 76".)

Now, you are certainly entitled to wonder if this elaborate mathematical-theological fantasy is just a joke or if it has some practical spinoffs. For example, is there anything we can *do* with the analogy between adjoint operators and adjoint functors? As it turns out, there is. The point is that the analogy is not quite precise. For example, every linear operator has an adjoint, but not every functor has an adjoint — nor need it be "linear" in any sense. If we endeavor to make the analogy precise, we will invent a special sort of category called a "2-Hilbert space" which is the precise categorified analog of a Hilbert space. And we will invent a nice sort of "linear" functor between these, and all such functors will have adjoints. Furthermore, in this situation all left adjoints will also be right adjoints... fixing another funny discrepancy. And these 2-Hilbert spaces turn out to be closely related to 2-dimensional topological quantum field theories (in general, n-Hilbert spaces appear to be related to n-dimensional TQFTs), as well as some interesting aspects of group representation theory.

I'm busily writing a paper on exactly this stuff, but I have not explained enough category theory here to describe it in detail yet. For now, let me just make the connection between the $\mathrm{Hom}(-,-)$ of category theory and the $\langle -,- \rangle$ of quantum theory more clear, and hopefully more plausible. If we have states h and h' in a Hilbert space, $\langle h, h' \rangle$ keeps track of the *amplitude* of getting from h to h'. (Often people will say "from h' to h", but here I think I really want to go the other way.) This is a mere *number*. If we have objects c and c' in a category, $\mathrm{Hom}(c, c')$ is the actual *set* of ways to get from c to c', that is, the set of morphisms from c to c'.

When one computes transition amplitudes by summing over paths, as in Feynman path integrals, one is in a sense decategorifying, that is, turning a set of ways to get from here to there into a number, the transition amplitude. However, one is not just counting the ways, one is counting them "with phase".... and I must admit that the role of the *complex numbers* in quantum theory is still puzzling from this viewpoint. For some food for thought, you might want to check out Dan Freed's work on torsors, which are a categorified version of phases:

1) "Higher algebraic structures and quantization", by Daniel Freed, *Commun. Math. Phys.* **159** (1994), 343–398, also available as `hep-th/9212115`.

To continue reading the "Tale of n-Categories", see "Week 79".

Week 79

April 1, 1996

Before I continue my tale of adjoint functors I want to say a little bit about icosahedra, buckyballs, and last letter Galois wrote before his famous duel. . . . all of which is taken from the following marvelous article:

1) Bertram Kostant, "The graph of the truncated icosahedron and the last letter of Galois", *Notices of the AMS* **42** (September 1995), 959–968. Also available at `http://www.ams.org/notices/199509/199509-toc.html`.

When I was a graduate student at MIT I realized that Kostant (who teaches there) was deeply in love with symmetry, and deeply knowledgeable about some of its more mysterious byways. Unfortunately I didn't dig too deeply into group theory at the time, and now I am struggling to catch up.

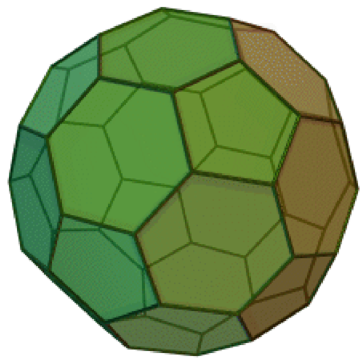
Let's start with the Platonic solids. Note that the cube and the octahedron are dual — putting a vertex in the center of each of the cube's faces gives you an octahedron, and vice versa. So every rotational symmetry of the cube can be reinterpreted as a symmetry of the octahedron, and vice versa. Similarly, the dodecahedron and the icosahedron are dual, while the tetrahedron is self-dual. So while there are 5 Platonic solids, there are really only 3 different symmetry groups here.

These 3 "Platonic groups" are very interesting. The symmetry group of the tetrahedron is the group A_4 of all *even* permutations of 4 things, since by rotating the tetrahedron we can achieve any even permutation of its 4 vertices. The symmetry group of the cube is S_4, the group of *all* permutations of 4 things. What are the 4 things here? Well, we can draw 4 line segments connecting opposite vertices of the cube; these are the 4 things! The symmetry group of the icosahedron is A_5, the group of even permutations of 5 things. What are the 5 things? It we take all the line segments connecting opposite vertices we get 6 things, not 5, but we can't get all even permutations of those by rotating the icosahedron. To find the *5* things is a bit trickier; I leave it as a puzzle here. See

2) John Baez, "Some thoughts on the number 6", `http://math.ucr.edu/home/baez/six.html`

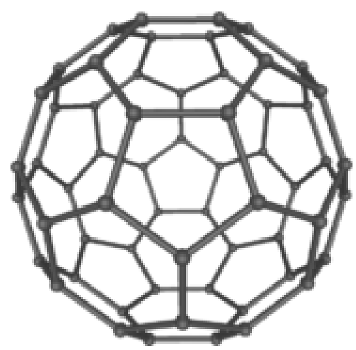
for an answer.

Once we convince ourselves that the rotational symmetry group of the icosahedron is A_5, it follows that it has $5!/2 = 60$ elements. But there is another nice way to see this. Take an icosahedron and chop off all 12 corners, getting a truncated icosahedron with 12 regular pentagonal faces and 20 regular hexagonal faces, with all edges the same length. It looks just like a soccer ball. It's called an Archimedean solid because, while not quite Platonic in its beauty, every face is a regular polygon and every vertex looks alike: two pentagons abutting one hexagon.



The truncated icosahedron has $5 \times 12 = 60$ vertices. Every symmetry of the icosahedron is a symmetry of the truncated icosahedron, so A_5 acts to permute these 60 vertices. Moreover, we can find an element of A_5 that moves a given vertex of the truncated icosahedron to any other one, since "every vertex looks alike". Also, there is a *unique* element of A_5 that does the job. So there must be precisely as many elements of A_5 as there are vertices of the truncated icosahedron, namely 60.

There is a lot of interest in the truncated icosahedron recently, because chemists had speculated for some time that carbon might form C_{60} molecules with the atoms at the vertices of this solid, and a while ago they found this was true. In fact, while C_{60} in this shape took a bit of work to get ahold of at first, it turns out that lowly soot contains lots of this stuff!



Since Buckminster Fuller was fond of using truncated icosahedra in his geodesic domes, C_{60} and its relatives are called fullerenes, and the shape is affectionately called a buckyball. For more about this stuff, try:

3) P. W. Fowler and D. E. Manolpoulos, *An Atlas of Fullerenes*, Oxford University Press, 1995.

M. S. Dresselhaus, G. Dresselhaus, and P. C. Eklund, *Science of Fullerenes and Carbon Nanotubules*, Academic Press, New York, 1994.

G. Chung, B. Kostant and S. Sternberg, "Groups and the buckyball", in *Lie Theory and Geometry*, eds. J.-L. Brylinski, R. Brylinski, V. Guillemin and V. Kac, Birkhauser, 1994.

In fact, for the person who has everything: you can now buy 99.5% pure C_{60} at the following site:

4) BuckyUSA homepage, `http://www.buckyusa.com/Fullerene%20C60.htm`

But I digress. Coming back to the 3 Platonic groups... there is much more that's special about them. Most of it requires a little knowledge of group theory to understand. For example, they are the 3 different finite subgroups of $\mathrm{SO}(3)$ having irreducible representations on $\mathbb{R}^3$. And they are nice examples of finite reflection groups. For more about them from this viewpoint, try "Week 62" and "Week 63". Also, via the McKay correspondence they correspond to the exceptional Lie groups E_6, E_7, and E_8 — see "Week 65" for an explanation of this!

Yet another interesting fact about these groups is buried in Galois' last letter, written to the mathematician Chevalier on the night before Galois' fatal duel. He was thinking about some groups we'd now call $\mathrm{PSL}(2, F)$. Here F is a field (for example, the real numbers, the complex numbers, or $\mathbb{Z}_p$, the integers $\mod p$ where p is prime). $\mathrm{PSL}(2, F)$ is a "projective special linear group over F." What does that mean? Well, first of all, $\mathrm{SL}(2, F)$ is the 2×2 matrices with entries in F having determinant equal to 1. These form a group under good old matrix multiplication. The matrices in $\mathrm{SL}(2, F)$ that are scalar multiples of the identity matrix form the "center" Z of $\mathrm{SL}(2, F)$ — the group of guys who commute with everyone else. We can form the quotient group $\mathrm{SL}(2, F)/Z$, and get a new group called $\mathrm{PSL}(2, F)$.

Now Galois was thinking about $\mathrm{PSL}(2, \mathbb{Z}_p)$ where p is prime. There's an obvious way to get this group to act as permutations of $p + 1$ things. Here's how! For any field F, the group $\mathrm{SL}(2, F)$ acts as linear transformations of the 2-dimensional vector space over F, and it thus acts on the set of lines through the origin in this vector space... which is called the "projective line" over F. But anything in $\mathrm{SL}(2, F)$ that's a scalar multiple of the identity doesn't move lines around, so we can mod out by the center and think of the quotient group $\mathrm{PSL}(2, F)$ as acting on projective line. (By the way, this explains the point of working with PSL instead of plain old SL.)

Now, an element of the projective line is just a line through the origin in F^2. We can specify such a line by taking any nonzero vector (x, y) in F^2 and drawing the line through the origin and this vector. However, (x', y') and (x, y) determine the same line if (x', y') is a scalar multiple of (x, y). Thus lines are in 1-1 correspondence with vectors of the form $(1, y)$ or $(x, 1)$. When our field F is $\mathbb{Z}_p$, there are just $p + 1$ of these. So $\mathrm{PSL}(2, \mathbb{Z}_p)$ acts naturally on a set of $p + 1$ things.

What Galois told Chevalier is that $\mathrm{PSL}(2, \mathbb{Z}_p)$ doesn't act nontrivially as permutation of any set with fewer than $p + 1$ elements if $p > 11$. This presumably means he knew that $\mathrm{PSL}(2, \mathbb{Z}_p)$ *does* act nontrivially on a set with only p elements if $p = 5$, 7, or 11. For example, $\mathrm{PSL}(2, 5)$ turns out to be isomorphic to A_5, which acts on a set of 5 elements in an obvious way. $\mathrm{PSL}(2, 7)$ and $\mathrm{PSL}(2, 11)$ act on a 7-element set and an 11-element set, respectively, in sneaky ways which Kostant describes.

These cases, $p = 5$, 7 and 11, are the the only cases where this happens and $\mathrm{PSL}(2, \mathbb{Z}_p)$ is simple. (See "Week 66" if you don't know what "simple" means.) In each case it is very amusing to look at how $\mathrm{PSL}(2, \mathbb{Z}_p)$ acts nontrivially on a set with p elements and consider the subgroup that doesn't move a particular element of this set. For example, when $p = 5$ we have $\mathrm{PSL}(2, 5) = A_5$, and if we look at the subgroup of even permutations of 5 things that leaves a particular thing alone, we get A_4. Kostant explains how if we play this game with $\mathrm{PSL}(2,7)$ we get S_4, and if we play this game with $\mathrm{PSL}(2,11)$ we get A_5. These are the 3 Platonic groups again!!

But notice an extra curious coincidence. A_5 is both $\mathrm{PSL}(2,5)$ and the subgroup of $\mathrm{PSL}(2,11)$ that fixes a point of an 11-element set. This gives a lot of relationships between A_5, $\mathrm{PSL}(2,5)$, and $\mathrm{PSL}(2,11)$. What Kostant does is take this and milk it for all it's worth! In particular, it turns out that one can think of A_5 as the vertices of the buckyball, and describe which vertices are connected by an edge using the embedding of A_5 in $\mathrm{PSL}(2,11)$. I won't say how this goes... read his paper!

This may even have some applications for fullerene spectroscopy, since one can use symmetry to help understand spectra of compounds. (Indeed, this is one way group theory entered chemistry in the first place.)

Now let me return to the tale of adjoint functors! I have been stressing the fact that two functors $L\colon \mathcal{C} \to \mathcal{D}$ and $R\colon \mathcal{D} \to \mathcal{C}$ are adjoint if there is a natural isomorphism between $\mathrm{Hom}(Lc,d)$ and $\mathrm{Hom}(c,Rd)$. We can say that an "adjunction" is a pair of functors $L\colon \mathcal{C} \to \mathcal{D}$ and $R\colon \mathcal{D} \to \mathcal{C}$ together with a natural isomorphism between $\mathrm{Hom}(Lc,d)$ and $\mathrm{Hom}(c,Rd)$. But there is another way to think about adjunctions which is also good.

In "Week 76" we talked about an "equivalence" of categories. We can summarize it this way: an "equivalence" of the categories $\mathcal{C}$ and $\mathcal{D}$ is a pair of functors $F\colon \mathcal{C} \to \mathcal{D}$ and $G\colon \mathcal{D} \to \mathcal{C}$ together with natural transformations $e : FG \Rightarrow 1_{\mathcal{D}}$ and $i\colon 1_{\mathcal{C}} \Rightarrow GF$ that are themselves invertible. (Note that we are now writing products of functors in the order that ordinary mortals typically do, instead of the backwards way we introduced in "Week 73". Sorry! It just happens to be better to write it this way now.) Now, the concept of "adjunction" is a cousin of the concept of "equivalence", and it's nice to have a definition of adjunction that brings out this relationship.

First, consider what happens in the definition of adjunction if we take $c = Rd$. Then we have a natural isomorphism between $\mathrm{Hom}(LRd,d)$ and $\mathrm{Hom}(Rd,Rd)$. Now there is a special element of $\mathrm{Hom}(Rd,Rd)$, namely the identity 1_{Rd}. This gives us a special element of $\mathrm{Hom}(LRd,d)$. Let's call this

$$e_d\colon LRd \to d.$$

What is this morphism like in an example? Say $L\colon \mathsf{Set} \to \mathsf{Grp}$ takes each set to the free group on that set, and $R\colon \mathsf{Grp} \to \mathsf{Set}$ takes each group to its underlying set. Then if d is a group, LRd is the free group on the underlying set of d. There's an obvious homomorphism from LRd to d, taking each word of elements in d and their inverses to their product in d. That's e_d. It goes from the free thing on the underlying thing of d to the thing d itself!

In fact, since everything in sight is natural, whenever we have an adjunction the morphisms e_d define a natural transformation

$$e\colon LR \Rightarrow 1_{\mathcal{D}}$$

Next, consider what happens in the definition of adjunction if we take $d = Lc$. Then we have a natural isomorphism between $\mathrm{Hom}(c,RLc)$ and $\mathrm{Hom}(Lc,Lc)$. Now there is a special element in $\mathrm{Hom}(Lc, Lc)$, namely the identity 1_{Lc}. This gives us a special element in $\mathrm{Hom}(c, RLc)$. Let's call this

$$i_c\colon c \to RLc.$$

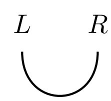
Again, it's good to consider the example of sets and groups: if c is a set, RLc is the underlying set of the free group on c. There is an obvious way to map c into RLc. That's i_c. It goes from the thing c to the underlying thing of the free thing on c.

As before, we get a natural transformation

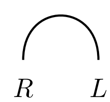
$$i : 1_{\mathcal{C}} \Rightarrow RL$$

So, as in an equivalence, when we have an adjunction we have natural transformations $e : LR \Rightarrow 1_{\mathcal{D}}$ and $i : 1_{\mathcal{C}} \Rightarrow RL$. Unlike in an equivalence, they needn't be natural *isomorphisms*, as the example of sets and groups shows. But they do have some cool properties, which are nice to draw using pictures.

First, we draw e as a U-shaped thing:



The idea here is that e goes from LR down to the identity $1_{\mathcal{D}}$, which we draw as "nothing". We can think of L and R as processes, and the U-shaped thing as the meta-process of L and R "colliding into each other and cancelling out", like a particle and antiparticle. (Lest you think that's just purple prose, wait and see! Eventually I'll explain what all this has to do with antiparticles!) Similarly, we draw i as an upside-down-U-shaped thing:

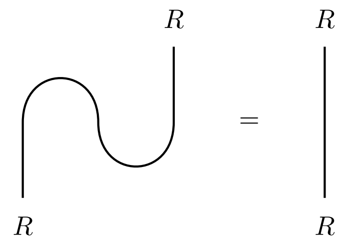


In other words, i goes from the identity $1_{\mathcal{C}}$ to RL.

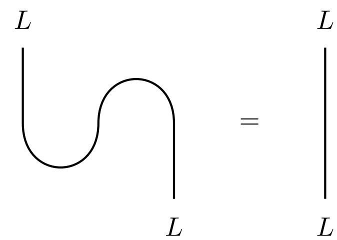
We can also use this sort of notation to talk about identity natural transformations. For example, if we have any old functor F, there is an identity natural transformation $1_F\colon F \Rightarrow F$, which we can draw as follows:

We draw it as a boring vertical line because "nothing is happening" as we go from F to F.

Now, I haven't talked much about the ways one can compose natural transformations like i and e, but remember that they are 2-morphisms, or morphisms-between-morphisms, in Cat (the 2-category of all categories). This means that they are inherently 2-dimensional, and in particular, one can compose them both "horizontally" and "vertically". I'll explain this more next time, but for now please take my word for it! Using these composition operations, one can make sense of the following equations involving i and e:



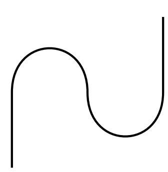
and



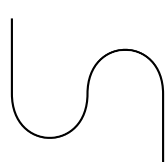
In the first equation we are asserting that a certain way of sticking together i and e and some identity natural transformations gives $1_R\colon R \Rightarrow R$. In the second we are asserting that some other way gives $1_L\colon L \Rightarrow L$.

I will explain these more carefully next time, but for now I mainly want to state that we can also *define* an adjunction to be a pair of functors $L\colon \mathcal{C} \to \mathcal{D}$ and $R\colon \mathcal{D} \to \mathcal{C}$ together with natural transformations $e\colon LR \Rightarrow 1_{\mathcal{D}}$ and $i\colon 1_{\mathcal{C}} \Rightarrow RL$ making the above 2 equations hold! This is the definition of "adjunction" that is the most similar to the definition of "equivalence".

Now, topologically, these 2 equations simply say that if you have a wiggly curve like



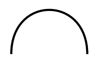
or



you can pull it tight to get

Thus, while

and



are not exactly "inverses", there is some subtler sense in which they "cancel out". This corresponds to the notion that while adjoint functors are not inverses, not even up to a natural isomorphism, they still are "like inverses" in a subtler sense.

Now this may seem like a silly game, drawing natural transformations as "string diagrams" and interpreting adjoint functors as wiggles in the string. But in fact this is part of a very big, very important, and very fun game: the relation between n-category theory and the topology of submanifolds of $\mathbb{R}^n$. Right now we are dealing with Cat, which is a 2-category, so we are getting into 2-dimensional pictures. But when we get into 3-categories we will get into 3-dimensional pictures, and knot theory… and what got me into this whole business in the first place: the relation between knots and physics. In higher dimensions it gets even fancier.

So I will continue next time and explain the recipes for composing natural transformations, and the associated string diagrams, more carefully.

To continue reading the "Tale of n-Categories", see "Week 80".

Week 80

April 20, 1996

There are a number of interesting books I want to mention.

Huw Price's book on the arrow of time is finally out! It's good to see a philosopher of science who not only understands what modern physicists are up to, but can occaisionally beat them at their own game.

Why is the future different from the past? This has been vexing people for a long time, and the stakes went up considerably when Boltzmann proved his "H-theorem", which seems at first to show that the entropy of a gas always increases, despite the time-reversibility of the laws of classical mechanics. However, to prove the H-theorem he needed an assumption, the "assumption of molecular chaos". It says roughly that the positions and velocities of the molecules in a gas are uncorrelated before they collide. This seems so plausible that one can easily overlook that it has a time-asymmetry built into it — visible in the word "before". In fact, we aren't getting something for nothing in the H-theorem; we are making a time-asymmetric assumption in order to conclude that entropy increases with time!

The "independence of incoming causes" is very intuitive: if we do an experiment on an electron, we almost always assume our choice of how to set the dials is not correlated to the state of the electron. If we drop this time-asymmetric assumption, the world looks rather different. . . but I'll let Price explain that to you.

Anyway, Price is an expert at spotting covertly time-asymmetric assumptions. you may remember from "Week 26" that he even got into a nice argument with Stephen Hawking about the arrow of time, thanks to this habit of his. you can read more about it in:

1) Huw Price, *Time's Arrow and Archimedes' Point: New Directions for a Physics of Time*, Oxford University Press, 1996.

Also, there is a new book out by Hawking and Roger Penrose on quantum gravity. First they each present their own ideas, and then they duke it out in a debate in the final chapter. This book is an excellent place to get an overview of some of the main ideas in quantum gravity. It helps if you have a little familiarity with general relativity, or differential geometry, or are willing to fake it.

There is even some stuff here about the arrow of time! Hawking has a theory of how it arose, starting from his marvelous "no-boundary boundary conditions", which say that the wavefunction of the universe is full of quantum fluctuations corresponding to big bangs which erupt and then recollapse in big crunches. The wavefunction itself has no obvious "time-asymmetry", indeed, time as we know it only makes sense *within* any one of the quantum fluctuations, one of which is presumably the world we know! But Hawking thinks that each of these quantum fluctuations, or at least most of them, should have an arrow of time. This is what Price raised some objections to. Hawking seems to argue that each quantum fluctuation should "start out" rather smooth near its big bang and develop more inhomogeneities as time passes, "winding up" quite wrinkly near its big crunch. But it's not at all clear what this "starting out" and "winding up" means. Possibly he is simply speaking vaguely, and all or most of the quantum fluctuations can be shown to have one smooth end and wrinkly at the other. That would be an adequate resolution to the arrow of time problem. But it's not clear, at least not to me, that Hawking really showed this.

Penrose, on the other hand, has some closely related ideas. His "Weyl curvature hypothesis" says that the Weyl curvature of spacetime goes to zero at initial singularities (e.g. the big bang) and infinity at final ones (e.g. black holes). The Weyl curvature can be regarded as a measure of the presence of inhomogeneity — the "wrinkliness" I alluded to above. The Weyl curvature hypothesis can be regarded as a time-asymmetric law built into physics from the very start.

To see them argue it out, read

2) Stephen Hawking and Roger Penrose, *The Nature of Space and Time*, Princeton University Press, 1996.

There are also a couple of more technical books on general relativity that I'd been meaning to get ahold of for a long time. They both feature authors of that famous book,

3) Charles Misner, Kip Thorne and John Wheeler, *Gravitation*, Freeman Press, 1973,

which was actually the book that made me decide to work on quantum gravity, back at the end of my undergraduate days. They are:

4) Ignazio Ciufolini and John Archibald Wheeler, *Gravitation and Inertia*, Princeton University Press, 1995.

and

5) Kip Thorne, Richard Price and Douglas Macdonald, eds., *Black Holes: The Membrane Paradigm*, 1986.

The book by Ciufolini and Wheeler is full of interesting stuff, but it concentrates on "gravitomagnetism": the tendency, predicted by general relativity, for a massive spinning body to apply a torque to nearby objects. This is related to Mach's old idea that just as spinning a bucket pulls the water in it up to the edges, thanks to the centrifugal force, the same thing should happen if instead we make lots of stars rotate around the bucket! Einstein's theory of general relativity was inspired by Mach, but there has been a long-running debate over whether general relativity is "truly Machian" — in part because nobody knows what "truly Machian" means. In any event, Ciufolini and Wheeler argue that gravitomagnetism exhibits the Machian nature of general relativity, and they give a very nice tour of gravitomagnetic effects.

That is fine in theory. However, the gravitomagnetic effect has never yet been observed! It was supposed to be tested by Gravity Probe B, a satellite flying at an altitude of about 650 kilometers, containing a superconducting gyroscope that should precess at a rate of 42 milliarcseconds per year thanks to gravitomagnetism. I don't know what ever happened with this, though: the following web page says "Gravity Probe B is expected to fly in 1995", but now it's 1996, right? Maybe someone can clue me in to the latest news.... I seem to remember some arguments about funding the program.

6) Gravity Probe B, `http://stugyro.stanford.edu/RELATIVITy/GPB/`

(Note added in 2002: now this webpage is gone; see `http://einstein.stanford.edu/` for the latest story.)

Kip Thorne's name comes up a lot in conjuction with black holes and the LIGO — or Laser-Interferometer Gravitational-Wave Observatory — project. As pairs of black holes or neutron stars spiral emit gravitational radiation, they should spiral in towards each other. In their final moments, as they merge, they should emit a "chirp" of gravitational radiation, increasing in frequency and amplitude until their ecstatic union is complete. The LIGO project aims to observe these chirps, and any other sufficiently strong gravitational radiation that happens to be passing by our way. LIGO aims to do this by using laser interferometry to measure the distance between two points about 4 kilometers apart to an accuracy of about 10^{-18} meters, thus detecting tiny ripples in the spaceteim metric. For more on LIGO, try

7) LIGO project home page, `http://www.ligo.caltech.edu/`

Thorne helped develop a nice way to think of black holes by envisioning their event horizon as a kind of "membrane" with well-defined mechanical, electrical and magnetic properties. This is called the membrane paradigm, and is useful for calculations and understanding what black holes are really like. The book "Black Holes: The Membrane Paradigm" is a good place to learn about this.

Now let me return to the tale of 2-categories. So far I've said only that a 2-category is some sort of structure with objects, morphisms between objects, and 2-morphisms between morphisms. But I have been attempting to develop your intuition for Cat, the primordial example of a 2-category. Remember, Cat is the 2-category of all categories! Its objects are categories, its morphisms are functors, and its 2-morphisms are natural transformations — these being defined in "Week 73" and again in "Week 75".

How can you learn more about 2-categories? Well, a really good place is the following article by Ross Street, who is one of the great gurus of n-category theory. For example, he was the one who invented ω-categories!

8) Ross Street, "Categorical structures", in *Handbook of Algebra*, vol. **1**, ed. M. Hazewinkel, Elsevier, 1996.

Physicists should note his explanation of the yang-Baxter and Zamolodchikov equations in terms of category theory. If you have trouble finding this, you might try

9) G. Maxwell Kelly and Ross Street, *Review of the elements of 2-categories*, Springer Lecture Notes in Mathematics **420**, Berlin, 1974, pp. 75–103.

I can't really compete with these for thoroughness, but at least let me give the definition of a 2-category. I'll give a pretty nuts-and-bolts definition; later I'll give a more elegant and abstract one. Readers who are familiar with Cat should keep this example in mind at all times!

This definition is sort of long, so if you get tired of it, concentrate on the pictures! They convey the basic idea. Also, keep in mind is that this is going to be sort of like the definition of a category, but with an extra level on top, the 2-morphisms.

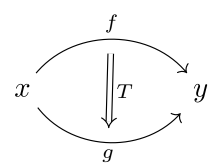
So: first of all, a 2-category consists of a collection of “objects” and a collection of “morphisms”. Every morphism f has a “source” object and a “target” object. If the source of f is x and its target is y, we write $f\colon x \to y$. In addition, we have:

1) Given a morphism $f\colon x \to y$ and a morphism $g\colon y \to Z$, there is a morphism $fg\colon x \to Z$, which we call the “composite” of f and g.
2) Composition is associative: $(fg)h = f(gh)$.
3) For each object x there is a morphism $1_x\colon x \to x$, called the “identity” of x. For any $f\colon x \to y$ we have $1_x f = f 1_y = f$.

you should visualize the composite of $f\colon x \to y$ and $g\colon y \to Z$ as follows:

$$x \xrightarrow{f} y \xrightarrow{g} Z.$$

So far this is exactly the definition of a category! But a 2-category ALSO consists of a collection of “2-morphisms”. Every 2-morphism T has a “source” morphism f and a target morphism g. If the source of T is f and its target is g, we write $T\colon f \Rightarrow g$. If $T\colon f \Rightarrow g$, we require that f and g have the same source and the same target; for example, $f\colon x \to y$ and $g\colon x \to y$. you should visualize T as follows:



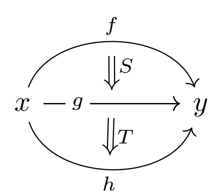
In addition, we have:

1’) Given a 2-morphism $S\colon f \Rightarrow g$ and a 2-morphism $T\colon g \Rightarrow h$, there is a 2-morphism $ST\colon f \Rightarrow h$, which we call the “vertical composite” of S and T.

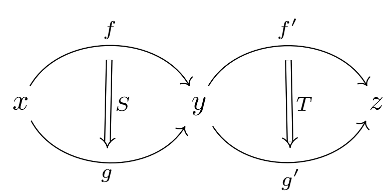
2’) Vertical composition is associative: $(ST)U = S(TU)$.

3’) For each morphism f there is a 2-morphism $1_f\colon f \Rightarrow f$, called the “identity” of f. For any $T\colon f \Rightarrow g$ we have $1_f T = T 1_g = T$.

Note that these are just like the previous 3 rules. We draw the vertical composite of $S\colon f \Rightarrow g$ and $T\colon g \Rightarrow h$ like this:



Now for a twist. We also require that we can “horizontally” compose 2-morphisms as follows:



So we also demand:

1'') Given morphisms $f, g\colon x \to y$ and $f', g'\colon y \to z$, and 2-morphisms $S\colon f \Rightarrow g$ and $T\colon f' \Rightarrow g'$, there is a 2-morphism $S \cdot T\colon ff' \Rightarrow gg'$, which we call the "horizontal composite" of S and T.

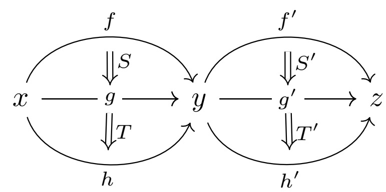
2'') Horizontal composition is associative: $(S \cdot T) \cdot U = S \cdot (T \cdot U)$.

3'') The identities for vertical composition are also the identities for horizontal composition. That is, given $f, g\colon x \to y$ and $T\colon f \Rightarrow g$ we have $1_{1_x} \cdot T = T \cdot 1_{1_y} = T$.

Finally, we demand the "exchange law" relating horizontal and vertical composition:

$$(ST) \cdot (S'T') = (S \cdot S')(T \cdot T')$$

This makes the following 2-morphism unambiguous:



We can think of it either as the result of first doing two vertical composites, and then one horizontal composite, or as the result of first doing two horizontal composites, and then one vertical composite!

Here we can really see why higher-dimensional algebra deserves its name. Unlike category theory, where we can visualize morphisms as 1-dimensional arrows, here we have 2-morphisms which are intrinsically 2-dimensional, and can be composed both vertically and horizontally.

Now if you are familiar with Cat, you may be wondering how we vertically and horizontally compose natural transformations, which are the 2-morphisms in Cat. Let me leave this as an exercise for now... there's a nice way to do it that makes Cat into a 2-category. This exercise is a good one to build up your higher-dimensional algebra muscles.

In fact, we could have invented the above definition of 2-category simply by thinking a lot about Cat and what you can do with categories, functors, and natural transformations. I'm pretty sure that's more or less what happened, historically! Thinking hard enough about nCat leads us on to the definition of $(n + 1)$-categories....

But that's enough for now. Typing those diagrams is hard work.

To continue reading the "Tale of n-Categories", see "Week 83".

I thank Keith Harbaugh for catching lots of typos and other mistakes in "Week 73" – "Week 80".

Week 81

May 12, 1996

I think I'll take a little break on the continuing saga of n-categories. Instead I'll talk about something which is secretly the very same subject, namely loop groups and their central extensions. This is important in string theory. But first I want to say a bit about some very high-energy physics.

1) D. J. Bird et al, "Detection of a cosmic ray with measured energy well beyond the expected spectral cutoff due to cosmic microwave radiation", preprint available as astro-ph/9410067

P. Bhattacharjee and G. Sigl, "Monopole annihilation and highest energy cosmic rays", preprint available as astro-ph/9412053.

R.J. Protheroe and P.A. Johnson, "Are topological defects responsible for the 300 EeV cosmic rays?", preprint available as astro-ph/9605006.

In 1994, folks at the Fly's Eye air shower detector in Utah observed a cosmic ray whose energy was about 320 EeV. In case you forget what an EeV is, it's a unit of energy equal to a billion GeV, and a Gev is equal to a billion ev (electron volts). Current particle accelerators routinely particles with energies about a hundred GeV, but a few hundred *EeV* is a whole different story! That's about 50 joules, the energy of a one-kilogram mass moving at 10 meters/second, all packed in one particle!

Nobody knows what would produce cosmic rays of this energy. To make the puzzle more mysterious, this energy is above the Greisen-Zatsepin- Kuz'min (or GZK) cutoff for cosmic rays produced at moderate extragalactic distances (30 megaparsecs). The idea of the GZK cutoff is that particles of extremely high energies whizzing through space would interact significantly with the cosmic microwave background radiation, losing energy to produce pions.

So it seems that something is producing really high energy particles, and this something is not too far away, by cosmic standards. Established mechanisms don't get energies that high. A possibility studied by various authors including P. Bhattacharjee and G. Sigl is that these super-energetic cosmic rays are produced by the decay of "topological defects". Various grand unified theories, or GUTs, predict that the strong, weak, and electromagnetic forces all act the the same at really high temperatures, while at low temperatures (like any sort of temperature you'd find around here) a "spontaneous symmetry breaking" occurs which makes them split up into their observed distinct personalities.

Mathematically this is a bit like how a magnet at low temperatures randomly picks out a certain axis of magnetization, breaking the rotational symmetry it possesses at high temperatures. And like in the case of a magnet, one would expect the possibility of "topological defects" where different regions of space pick different ways to break the symmetry, leaving nasty spots like lumps in the carpet that can't be straightened out. Ordinary magnets typically exhibit 2-dimensional "domain walls" between domains having different axes of magnetization. But in various GUTs folks have considered, one can also get 1-dimensional "cosmic strings" and 0-dimensional "topological solitons" including magnetic monopoles — particles with magnetic charge. None of these topological defects have ever been observed, despite a fair amount of searching. Could super-energetic cosmic rays be the result of a monopole-antimonopole collision?

Alas, Protheroe and Johnson's paper argues that in such decays lots of the energy would go into the production of high-energy γ rays... more than has been observed in the super-energetic cosmic ray showers. So maybe the puzzle has some other answer.

The weekend before last I went to the 11th Geometry Festival, which was held at the University of Maryland. Since I work on quantum gravity, I could be said to be a geometer of sorts — perhaps a quantum geometer. But geometry means a lot of different things to different people, and this conference concentrated on some aspects of geometry that I don't know much about. In particular, there were talks by Schmuel Weinberger, Bruce Kleiner and G. Wei on the implications of positive and negative curvature for Riemannian geometry.

A talk that was right up my alley was given by Jean-Luc Brylinski. It dealt with themes from his papers with McLaughlin:

2) Jean-Luc Brylinski and Dennis A. McLaughlin, "The geometry of degree four characteristic classes and of line bundles on loop spaces, I", *Duke Math. Journal* **75** (1994), 603–638. "II", *Duke Math. Journal* **83** (1996), 105–139.

Jean-Luc Brylinski, "Central extensions and reciprocity laws", preprint.

Jean-Luc Brylinski, "Coadjoint orbits of central extensions of gauge groups", preprint.

Jean-Luc Brylinski and Dennis A. McLaughlin, "The geometry of two dimensional symbols", preprint.

Let me say a bit about the math underlying these papers, the basic stuff that they build on. One hot topic in mathematical physics in the last decade has been the study of "loop groups". Say you take any Lie group G. Then the "loop group" LG is the set of smooth functions from the circle to G. This becomes a group with pointwise multiplication as the group operation. This sort of group shows up in 2-dimensional quantum field theory, where spacetime could be the cylinder. Then "space" is the circle, and if we are studying gauge theory with gauge group G, the group of gauge transformations over space would be the loop group LG. One main reason for being interested in 2-dimensional quantum field theory is string theory: here we think of the 2-dimensional worldsheet of the string as a spacetime in its own right, and we are often interested in doing gauge theory over this spacetime. For this reason, folks in string theory need to understand all they can about unitary representations of loop groups.

Actually they are interested in *projective* representations of loop groups. Remember, in quantum mechanics two vectors in a Hilbert space give the same expectation values for any observable if they differ only by a phase. So it is perfectly fine for a group representation R to satisfy the usual law

$$R(g)R(h) = R(gh)$$

where g, h are group elements, *only up to a phase*. So in the definition of a projective representation we weaken the above requirement to

$$R(g)R(h) = c(g,h)R(gh)$$

where $c(g,h)$ is a phase depending on g and h. Folks call $c(g,h)$ the "cocycle" of the projective representation.

A projective unitary representation of a group H can also be thought of as a representation of a bigger group $\widetilde{H}$ called a "central extension" of H. The idea is that this bigger group has a bunch of phases built into it to absorb the phase ambiguities in the projective representation of H. Let $\mathrm{U}(1)$ be the unit circle in the complex plane, a group under multiplication. This is the group of phases. We can think of $\widetilde{H}$ as $H \times \mathrm{U}(1)$ given a sneaky product designed to soak up the phases produced by the cocycle:

$$(g,a)(h,b) = (gh, abc(g,h)).$$

We can define a unitary representation S of $\widetilde{H}$ as follows:

$$S(g,a) = R(g)a.$$

It's then obvious that

$$S(g,a)S(h,b) = S((g,a)(h,b))$$

so S is really a representation. For this reason, if we are doing quantum theory and we don't like projective representations, it's okay as long as we understand the central extensions of our group of symmetries.

So, instead of thinking about projective representations of loop groups, we can think about central extensions of loop groups. How does one get ahold of these? There is a nice trick which Brylinski described in his talk. To get this trick, we need to assume a bit about the group G. Let's assume it's a connected and simply-connected simple Lie group. I'll explain that in a minute, but some good examples to keep in mind are $\mathrm{SU}(n)$ and $\mathrm{Spin}(n)$; see "Week 61" for the definitions and a bit of information about these groups.

Now remember that S^k stands for the k-dimensional sphere, and $\pi_k(X)$ of a topological space X stands for the set of continuous maps from S^k to X, modulo homotopy. In other words, two continuous maps from S^k to X define the same element of $\pi_k(X)$ if one can be continuously deformed to the other.

Saying that G is connected means that $\pi_0(G) = 0$. To understand this you need to realize that S^0 consists of two points. So $\pi_0(G) = 0$ means that G consists of one piece, any two points of which can be connected by a continuous path.

Saying that G is simply connected means that $\pi_1(G) = 0$. In other words, all loops in G can be "pulled tight". A good example of a group that's NOT simply connected is the group $\mathrm{SO}(n)$ of rotations in n dimensional space.. This flaw with $\mathrm{SO}(n)$ is why they needed to invent $\mathrm{Spin}(n)$; see "Week 61".

As it turns out, every Lie group has $\pi_2(G) = 0$. So all 2-spheres in G can be pulled tight too. Imagine taking a balloon and sticking it in G; then you can always shrink it down to a point in a continuous way without it getting stuck around a hole in G.

Saying that G is simple is an algebraic rather than topological condition, and I explained this condition in "Week 63". But it has topological ramifications. It implies, for example, that $\pi_3(G) = \mathbb{Z}$, the group of integers. So to each way of sticking a 3-sphere in G we can associate an integer. One way to compute this integer involves the Killing form on the Lie algebra of G. This is a special inner product on the Lie algebra of G. Using this inner product and the bracket in the Lie algebra we can convert 3 vectors u, v, and w in the Lie algebra into a number as follows:

$$W(u, v, w) = k\langle [u, v], w\rangle$$

Here k is a constant that will make life simpler later. The special property of the Killing form implies that W is totally antisymmetric, and we can use left multiplication to translate W all over the group G obtaining a closed 3-form on G. Call this 3-form W, too. Then, given any smooth function from S^3 into G we can pull back W to S^3 and integrate it over S^3. If we choose the constant k right, the result will be an integer — the integer we were looking for.

Hmm, this is getting technical. Well, it'll keep getting more technical. Just stop reading when it becomes unpleasant.

Okay, these topological facts about the group G have a bunch of cool consequences. One trick usually goes by the name of the "WZW action", which refers to Wess, Zumino, and Witten. Say we have smooth function f from S^2 to G. Since $\pi_2(G) = 0$ we can extend f to a smooth function F from the 3-dimensional ball, D^3, to G. (This is just another way of "pulling the balloon tight" as mentioned above.) Now we can use F to pull back the magic 3-form W to D^3, and then we can integrate the resulting 3-form over D^3 to get a number $S(f)$ called the Wess-Zumino-Witten action.

Unfortunately, this number depends on the choice of the function F extending f to the ball. Fortunately, it doesn't depend too much on F. Say we extended f to some other function F' from the ball to G. Then F together with F' define a map from S^3 to G, and we know from the previous stuff that the integral of the pullback of W over this S^3 is an integer. So changing our choice of an extension of f only changes $S(f)$ by an integer. This means that the exponential of the WZW action:

$$\exp(2\pi i S(f))$$

is completely well-defined. This is nice in quantum physics, where the exponential of the action is what really matters. Note also that this exponential is just a phase! So we are getting a function

$$A\colon \mathrm{Maps}(S^2, G) \to \mathrm{U}(1)$$

assigning a phase to any map f from S^2 to G.

Now $\mathrm{Maps}(S^2, G)$ is sort of like the loop group, since the loop group is just $\mathrm{Maps}(S^1, G)$. In particular, it too becomes a group by pointwise multiplication. A bit of calculation shows that A above is a group homomorphism:

$$A(f)A(g) = A(fg).$$

This homomorphism is the key to finding the central extension of the loop group. Here's how we do it. By now everyone but the experts has probably fallen asleep at the screen, so I can pull out all the stops.

Here's a useful way to think of a central extensions: a central extension $\widetilde{H}$ of the group H by the group $\mathrm{U}(1)$ is a special sort of short exact sequence of groups:

$$1 \to \mathrm{U}(1) \to \widetilde{H} \to H \to 1$$

By "short exact sequence of groups" I simply mean that $\mathrm{U}(1)$ is a subgroup of $\widetilde{H}$ and that $\widetilde{H}$ modulo $\mathrm{U}(1)$ is H. What's special about central extensions is that $\mathrm{U}(1)$ is in the *center* of $\widetilde{H}$. You can check that this definition of central extension matches up with our earlier more lowbrow definition.

Now how do we get this short exact sequence? Well, it comes from a short exact sequence of spaces:

$$\{*\} \to S^1 \to D^2 \to S^2 \to \{*\}$$

This diagram means simply that we can think of the circle as a subspace of the 2-dimensional disc D^2 in an obvious way, and then if we collapse this circle to a point the disc gets squashed down to a 2-sphere. Now, from this short exact sequence we get a short exact sequence of groups

$$1 \to \mathrm{Maps}(S^2, G) \to \mathrm{Maps}(D^2, G) \to \mathrm{Maps}(S^1, G) \to 1$$

In other words, $\mathrm{Maps}(S^2, G)$ is a normal subgroup of $\mathrm{Maps}(D^2, G)$, and if we mod out by this subgroup we get $\mathrm{Maps}(S^1, G)$. Now we can use the homomorphism $A\colon \mathrm{Maps}(S^2, G) \to \mathrm{U}(1)$ to get ourselves another exact sequence like this:

$$\begin{array}{ccccccccc}
1 & \longrightarrow & \mathrm{Maps}(S^2,G) & \xrightarrow{i} & \mathrm{Maps}(D^2,G) & \xrightarrow{j} & \mathrm{Maps}(S^1,G) & \longrightarrow & 1 \\
 & & \big\downarrow A & & \big\downarrow & & \big\downarrow 1 & & \\
1 & \longrightarrow & \mathrm{U}(1) & \xrightarrow{i} & \widetilde{L} & \xrightarrow{j} & \mathrm{Maps}(S^1,G) & \longrightarrow & 1
\end{array}$$

Remembering that $\mathrm{Maps}(S^1, G)$ is the loop group, $\widetilde{L}$ turns out to be the desired central extension! Concretely we can think of $\widetilde{L}$ as a quotient group of $\mathrm{Maps}(D^2, G) \times \mathrm{U}(1)$ by the subgroup of pairs of the form $(i(f), A(f))$ with f in $\mathrm{Maps}(S^2, G)$.

There is something fascinating about how spheres of different dimensions — S^0, S^1, S^2, and S^3 — conspire together with the topology of the group G to yield the central extension of the loop group LG. It appears that what we are really studying are the closely related cohomology groups:

- $H^0(\mathrm{Maps}(S^3, G))$ which is just another way of saying $\pi_3(G)$
- $H^1(\mathrm{Maps}(S^2, G))$ which describes homomorphisms from $\mathrm{Maps}(S^2, G)$ to $\mathrm{U}(1)$
- $H^2(\mathrm{Maps}(S^1, G))$ which describes central extensions of $\mathrm{Maps}(S^1, G)$
- $H^3(\mathrm{Maps}(S^0, G))$ which is just another way of saying $H^3(G)$, where W lives.

There is a fourth term in this series which I didn't get around to talking about; it's

- $H^4(\mathcal{B}G)$ where the degree 4 characteristic class for G-bundles, e.g. the 2nd Chern class for $\mathrm{SU}(n)$, lives.

Here $\mathcal{B}G$ is the "classifying space" of G. I would like to understand more deeply what's going on with this series, because the different terms have a lot to do with physics in different dimensions — dimensions 0 to 4, the "low dimensions" which are so specially interesting.

I should conclude by noting that while a lot of this appeared in Brylinski's talk, and a lot of it is probably common knowledge among topologists, it was in some conversations with James Dolan that we worked out some of the patterns I mention here.

Week 82

May 17, 1996

I will continue to take a break from the tale of n-categories. As the academic year winds to an end, an enormous pile of articles and books is building up on my desk. I can kill two birds with one stone if I list some of them while filing them. Here is a sampling:

1) *Advances in Applied Clifford Algebras*, ed. Jaime Keller. (Subscriptions are available from Mrs. Irma Aragon, F. Q., UNAM, Apartado 70-528, 04510 Mexico, D.F., MEXICO, for US $10 per year.)

This is a homegrown journal for fans of Clifford algebras. What are Clifford algebras? Well, let's start at the beginning, with the quaternions. . . .

As J. Lambek has pointed out, not many mathematicians can claim to have introduced a new kind of number. One of them was the Sir William Rowan Hamilton. He knew about the real numbers $\mathbb{R}$, of course, and also the complex numbers $\mathbb{C}$, which are the reals with a square root of -1, usually called i, thrown in. Why not try putting in another square root of -1? This might give a 3-dimensional algebra that'd help with 3-dimensional space as much as the complex numbers help with 2 dimensions. He tried this but couldn't get division to work out well. He struggled this for a long time. On the 16th of October, 1843, he was walking along the Royal Canal with his wife to a meeting of the Royal Irish Academy when he had a good idea: ". . . there dawned on me the notion that we must admit, in some sense, a fourth dimension of space for the purpose of calculating with triples . . . An electric circuit seemed to close, and a spark flashed forth." He carved the decisive relations

$$i^2 = j^2 = k^2 = ijk = -1$$

in the stone of Brougham Bridge as he passed it. This was bold: a *noncommutative* algebra, since $ij = -ji$, $jk = -kj$, and $ik = -ki$ follow from the above equations. These are the quaternions, which now we call $\mathbb{H}$ after Hamilton.

Hamilton wound up spending much of his time on quaternions. The lawyer and mathematician Arthur Cayley heard Hamilton lecture on quaternions and — I imagine — was influenced by this to invent his "octonions", an 8-dimensional nonassociative algebra in which division still works nicely. For more on quaternions, octonions, and the general subject of division algebras, try "Week 59" and "Week 61".

In 1845, two years after the birth of the quaternions, the visionary William Clifford was born in Exeter, England. He only lived to the age of 37: despite suffering from lung disease, he worked with incredible intensity, and his closest friend wrote that "He could not be induced, or only with the utmost difficulty, to pay even moderate attention to the cautions and observances which are commonly and aptly described as 'taking care of one's self' ". But in his short life, he pushed quite far into the mathematics that would become the physics of the 20th century. He studied the geometry of Riemann and prophetically envisioned general relativity in 1876, in the following famous remarks:

> *"Riemann has shown that as there are different kinds of lines and surfaces, so there are different kinds of space of three dimensions; and that we can only find out by experience to which of these kinds the space in which we live belongs. I hold in fact*

(1) *That small portions of space* are *in fact of a nature analogous to little hills on a surface which is on the average flat; namely, that the ordinary laws of geometry are not valid for them.*

(2) *That this property of being curved or distorted is continually being passed on from one portion of space to another after the manner of a wave.*

(3) *That this variation of the curvature of space is what really happens in that phenomenon which we call the* motion of matter*, whether ponderable or etherial.*

(4) *That in the physical world nothing else takes place but this variation, subject (possibly) to the law of continuity.*

He also substantially generalized Hamilton's quaternions, dropping the condition that one have a division algebra, and focusing on the aspects crucial to n-dimensional geometry. He obtained what we call the Clifford algebras.

What's a Clifford algebra? Well, there are various flavors. But one of the nicest — let's call it C_n — is just the associative algebra over the real numbers generated by n anticommuting square roots of -1. That is, we start with n fellows called

$$e_1, \ldots, e_n$$

and form all formal products of them, including the empty product, which we call 1. Then we form all real linear combinations of these products, and then we impose the relations

$$e_i^2 = -1$$
$$e_i e_j = -e_j e_i.$$

What are these algebras like? Well, C_0 is just the real numbers, since none of these e_i's are thrown into the stew. C_1 has one square root of -1, so it is just the complex numbers. C_2 has two square roots of -1, e_1 and e_2, with

$$e_1 e_2 = -e_2 e_1.$$

Thus C_2 is just the quaternions, with e_1, e_2, and $e_1 e_2$ corresponding to Hamilton's i, j, and k.

How about the C_n for larger values of n? Well, here is a little table up to $n = 8$:

C_0	$\mathbb{R}$
C_1	$\mathbb{C}$
C_2	$\mathbb{H}$
C_3	$\mathbb{H} + \mathbb{H}$
C_4	$\mathbb{H}(2)$
C_5	$\mathbb{C}(4)$
C_6	$\mathbb{R}(8)$
C_7	$\mathbb{R}(8) + \mathbb{R}(8)$
C_8	$\mathbb{R}(16)$

What do these entries mean? Well, $\mathbb{R}(n)$ means the $n \times n$ matrices with real entries.

Similarly, $\mathbb{C}(n)$ means the $n \times n$ complex matrices, and $\mathbb{H}(n)$ means the $n \times n$ quaternionic matrices. All these become algebras with the usual matrix addition and matrix multiplication. Finally, if A is an algebra, $A + A$ means the algebra consisting of pairs of guys in A, with the obvious rules for addition and multiplication:

$$(a, a') + (b, b') = (a + b, a' + b')$$
$$(a, a')(b, b') = (ab, a'b')$$

You might enjoy checking some of these descriptions of the Clifford algebras C_n for n up to 8. You have to find the "isomorphism" — the correspondence between the Clifford algebra and the algebra I claim is really the same. This gets pretty tricky when n gets big.

How about n larger than 8? Well, here a remarkable fact comes into play. Clifford algebras display a certain sort of "period 8" phenomenon. Namely, C_{n+8} consists of 16×16 matrices with entries in C_n! For a proof you might try

2) H. Blaine Lawson, Jr. and Marie-Louise Michelson, *Spin Geometry*, Princeton U. Press, Princeton, 1989.

or

3) Dale Husemoller, *Fibre Bundles*, Springer-Verlag, Berlin, 1994.

These books also describe some of the amazing consequences of this periodicity phenomenon. The topology of n-dimensional manifolds is very similar to the topology of $(n+8)$-dimensional manifolds in some subtle but important ways! I should describe this "Bott periodicity" sometime, but for now let me leave it as a tantalizing mystery.

I will also have to take a rain check when it comes to describing the importance of Clifford algebras in physics... let me simply note that they are crucial for understanding spin-$1/2$ particles. I talked a bit about this in "Week 61".

The "Spin Geometry" book goes into a lot of detail on Clifford algebras, spinors, the Dirac equation and more. The "Fibre Bundles" book concentrates on the branch of topology called K-theory, and uses this together with Clifford algebras to tackle various subtle questions, such as how many linearly independent vector fields you can find on a sphere.

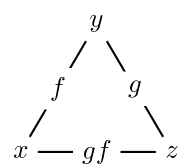
4) Ralph L. Cohen, John D. S. Jones, and Graeme B. Segal, "Morse theory and classifying spaces", preprint as of Sept. 13, 1991.

This is a nice way to think about what's really going on with Morse theory. In Morse theory we study the topology of a compact Riemannian manifold by putting a "Morse function" on it: a real-valued smooth function with only nondegenerate critical points. The gradient of this function defines a vector field and we use the way points flow along this vector field to chop the manifold up into convenient pieces or "cells". A while back, Witten discovered, or rediscovered, a very cute way to compute a topological invariant called the "homology" of the invariant using Morse theory. (I've heard that this was previously known and then largely forgotten.)

Here the authors refine this construction. They cook up a category $\mathcal{C}$ from the Morse function: the objects of $\mathcal{C}$ are critical points of the Morse function, and the morphisms are piecewise gradient flow lines. This is a topological category, meaning that for any pair of objects x and y the morphisms in $\mathrm{Hom}(x, y)$ form a topological space, and composition is a continuous map. There is a standard recipe to construct the "classifying space" of any topological category, invented by Segal in the following paper:

5) Graeme B. Segal, "Classifying spaces and spectral sequences", *Pub. IHES* **34** (1968), 105–112.

I described classifying spaces for discrete groups in "Week 70", and the more general case of discrete groupoids in "Week 75". The construction for topological categories is similar: we make a big space by sticking in one point for each object, one edge for each morphism, one triangle for each composable pair of morphisms:



$$f : x \to y$$
$$g : y \to z$$
$$gf : x \to z$$

and so on. The only new trick is to make sure this space gets a topology in the right way using the topologies on the spaces $\mathrm{Hom}(x, y)$.

Anyway, if we form this classifying space from the topological category $\mathcal{C}$ coming from the Morse function on our manifold M, we get a space homotopic to M! In other words, for many topological purposes the category $\mathcal{C}$ is just as good as the manifold M itself.

6) Ross Street, "Descent theory", preprint of talks given at Oberwolfach, Sept. 17–23, 1995.

   Ross Street, "Fusion operators and cocycloids in monoidal categories", preprints.

Street is one of the gurus of n-category theory, which he notes "might be called post-modern algebra (or even 'post-modern mathematics' since geometry and algebra are handled equally well by higher categories)." His paper on "Descent theory" serves as a rapid introduction to n-categories. But the real point of the paper is to explain the role n-categories play in cohomology theory: in particular, how to do cohomology with coefficients in an ω-category!

7) Viqar Husain, "Intersecting-loop solutions of the hamiltonian constraint of quantum general relativity", *Nucl. Phys.* **B313** (1989), 711–724.

   Viqar Husain and Karel V. "Kuchar, General covariance, new variables, and dynamics without dynamics", *Phys. Rev. D* **42** (1990), 4070–4077.

   Viqar Husain, "Einstein's equations and the chiral model", to appear in *Phys. Rev.* D, preprint available as gr-qc/9602050.

Viqar is one of the excellent younger folks at the Center for Gravitational Physics and Geometry at Penn State; I only had a bit of time to speak with him during my last visit there, but I got some of his papers. The first paper is from the good old days when folks were just beginning to find explicit solutions of the constraints of quantum gravity using the loop representation — it's still worth reading! The second introduced a field theory now called the Husain-Kuchar model, which has the curious property of resembling gravity without the dynamics. The third studies 4-dimensional general relativity assuming there are two commuting spacelike Killing vector fields. Here he reduces the theory to a 2-dimensional theory which appears to be completely integrable — though it has not been proved to be so in the sense of admitting a complete set of Poisson-commuting conserved quantities.

8) *The Interface of Knots and Physics*, ed. Louis H. Kauffman, Proc. Symp. Appl. Math. **51**, American Mathematical Society, Providence, Rhode Island, 1996.

This slim volume contains the proceedings of an AMS "short course" on knots and physics held in San Francisco in January 1995, namely:

- Louis H. Kauffman, "Knots and statistical mechanics"
- Ruth J. Lawrence, "An introduction to topological field theory"
- Dror Bar-Natan, "Vassiliev and quantum invariants of braids"
- Samuel J. Lomonaco, "The modern legacies of Thomson's atomic vortex theory in classical electrodynamics"
- John C. Baez, "Spin networks in nonperturbative quantum gravity"

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William Kingon Clifford
Born May 4th, 1845
Died March 3rd, 1879

I was not, and was conceived
I loved, and did a little work
I am not, and grieve not.

And

Lucy, his wife
Died April 21st, 1929

Oh, two such silver currents when they join
Do glorify the banks that bound them in.
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Week 83

June 10, 1996

I'll return to the tale of n-categories this week, and continue to explain the mysteries of duals and inverses. But first let me describe two new papers by Connes.

1) Alain Connes, "Gravity coupled with matter and the foundation of non-commutative geometry", preprint available as hep-th/9603053.

   Ali H. Chamseddine and Alain Connes, "The spectral action principle", preprint available as hep-th/9606001.

The second paper here fills in details that are missing from the first. Hopefully lots of you know that Connes is the wizard of operator theory who turned to inventing a new branch of geometry, "noncommutative geometry". The idea of algebraic geometry is that we can study a space by studying the functions on that space — which typically form some kind of commutative algebra. If we let the algebra become noncommutative, it is no longer functions on some space, but we can pretend it is nonetheless, and do geometry by analogy with the commutative case. This is very much based on the philosophy of quantum mechanics, where the observables form a noncommutative algebra, yet are analogous to the commutative algebras of observables of classical mechanics, these commutative algebras consisting simply of functions on the classical space states.

In quantum mechanics, the failure of two observables to commute implies that they cannot always be simultaneously measured with arbitrary accuracy; there is a very precise mathematical statement of Heisenberg's uncertainty principle that makes this quantitative. We can thus think of noncommutative geometry as "quantum geometry", geometry where the uncertainty principle of quantum mechanics has infected the very notion of space itself! In noncommutative geometry it impossible to simultaneously measure all the coordinates of a point with arbitrary accuracy, because they do not commute!

For the definitive introduction to noncommutative geometry, see Connes' book "Noncommutative Geometry", reviewed in "Week 39". Already in this book Connes, working with Lott, was beginning to explore the idea that the geometry of our physical universe is noncommutative. Actually, they used ideas from noncommutative geometry to study a weird kind of commutative geometry in which spacetime is "two-sheeted" - two copies of standard 4-dimensional spacetime, very close together. In normal geometry it doesn't even make sense to speak of two separate copies of spacetime being "close together", since there is no way to get from one to the other! Tricks from noncommutative geometry allow it to make sense. They found something amazing: if you do $\mathrm{U}(1) \times \mathrm{SU}(2)$ Yang-Mills theory on this spacetime, you get the Higgs particle for free!

Sorry for the jargon. What it means is this: in the Standard Model of particle physics we describe the electromagnetic force and the weak nuclear force in a unified way using a theory called "$\mathrm{U}(1) \times \mathrm{SU}(2)$ Yang-Mills theory", but then we postulate an extra particle, the Higgs particle, which has the effect of making the electromagnetic force work quite differently from the weak force. We say it "breaks the symmetry" between the two forces. It has not yet been observed, though particle physicists hope to see it (or not!) in experiments coming up fairly soon. It is a rather puzzling, ad hoc element of the

Standard Model. The amazing thing about the Connes-Lott model is that it arises in a natural way from the fact that spacetime has two sheets.

Connes and Lott also studied the strong force, but now Connes has introduced gravity into his model. I haven't had time to absorb this new work yet, so let me simply say what his current model of spacetime is, and list some of the concrete predictions the new theory makes. His spacetime is the noncommutative algebra consisting of smooth functions on good old 4-dimensional Minkowski spacetime, taking values in the algebra A given by the direct sum

$$A = \mathbb{C} + \mathbb{H} + M_3(\mathbb{C})$$

where $\mathbb{C}$ is the complex numbers, $\mathbb{H}$ is the quaternions, and $M_3(\mathbb{C})$ is the 3×3 complex matrices. (Exercise: redo Connes' model, replacing $M_3(\mathbb{C})$ with the octonions. Hint: develop nonassociative geometry and use Geoffrey Dixon's theory relating the electromagnetic, weak, and strong forces to the complex numbers, quaternions, and octonions, respectively. See "Week 59" for references to Dixon's work, and an explanation of quaternions and octonions.)

The Chamseddine-Connes model predicts that the sine squared of the Weinberg angle — an important constant in the theory of the electroweak force — is between $.206$ and $.210$. Unfortunately this disagrees with the experimental value of $.2325$, but it's sort of surprising that they can derive something this close, since in the Standard Model the Weinberg is just an arbitrary parameter. They also derive a Higgs mass of 160–180 GeV, and expect accuracy comparable to their prediction of the Weinberg angle (about 10%).

Well worth pondering!

There is an interesting analogy between the dual of a vector space and the inverse of a number which I would like to explain now. I assume you know that multiplying numbers is a lot like tensoring vector spaces. For example, just as multiplication distributes over addition, tensoring distributes over direct sums. Also, just as there is a number called 1 which is the unit for multiplication, there is a 1-dimensional vector space, the ground field itself, which is the unit for tensoring. Let me take the unusual liberty of writing tensor products by juxtaposition, so that xy is the tensor product of the vector space x and the vector space y, and let me call the 1-dimensional vector space that's the unit for tensoring simply "1".

Now, if a number x has an inverse y, we have

$$yx = 1$$

and

$$1 = xy.$$

Similarly, if a vector space x has a dual y, we have linear maps

$$e\colon yx \to 1$$

and

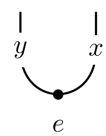
$$i\colon 1 \to xy$$

What are these linear maps? Well, the whole point of the dual vector space y is that a vector in y is a linear functional from x to 1. This "dual pairing" between vectors in y and those in x defines a linear map $e\colon yx \to 1$, often called the "counit". On the other hand, the space xy can be thought of as the space of linear transformations of x. The linear map $i\colon 1 \to xy$ sends any scalar (i.e., any vector in 1) to the corresponding scalar multiple of the identity transformation of x.

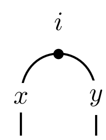
So we see that dual vector spaces are a bit like inverse numbers, except that we don't have $yx = 1$ and $1 = xy$, and we don't even have that yx is *isomorphic* to 1 and 1 is *isomorphic* to xy. We just have some maps going from yx to 1, and from 1 to xy.

These maps satisfy two equations, though. Here's the first. We start with x, use the obvious isomorphism to map to $1x$, then use $i\colon 1 \to xy$ to map this to xyx, then use $e\colon yx \to 1$ to map this to $x1$, and then use the other obvious isomorphism to map back to x. This composite of maps should be the identity on x. What this says is that the identity linear transformation of x really acts as the identity!

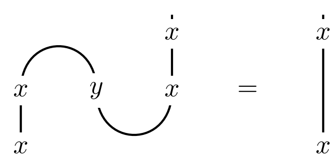
Stealing a trick from "Week 79", we can draw this as follows. Draw the counit $e\colon yx \to 1$ as follows:



and draw the unit $i\colon 1 \to xy$ as follows:

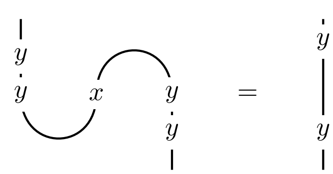


Then the above equation says that



Here the left side, which we read from top to bottom, corresponds to the composite $x \to 1x \to xyx \to x1 \to x$. (The factors of 1 are invisible in the picture, since they don't do much.) The left side corresponds to the identity map $x \to x$.

The second equation goes like this. We start with y, use the obvious isomorphism to map to $y1$, then use the unit to map this to yxy, then use the counit to map this to $1y$, and then use the other obvious isomorphism to map back to y. This composite should be the identity on y. What this says is that the identity linear transformation of x also acts dually as the identity on y! We can draw this as follows:



If you now steal a peek at "Week 79", you'll see that these two equations are just the same equations used to define adjoint functors in category theory! What's going on? Well, dual vector spaces are analogous to adjoint functors, clearly. But more deeply, what we have is an analogy between duals in any category with tensor products — or "monoidal category" — and adjoints in any 2-category.

What's a monoidal category, exactly? Roughly it's a category with some sort of "tensor product" and "unit object". But we can precisely define the so-called "strict" monoidal categories as follows: they are simply 2-categories with one object. (Turn to "Week 80" for a definition of 2-categories.) A 2-category has objects, morphisms, and 2-morphisms, but if there is only one object, we can do the following relabelling trick:

$$\begin{aligned} \text{2-morphisms} &\mapsto \text{morphisms} \\ \text{morphisms} &\mapsto \text{objects} \\ \text{objects} &\mapsto \end{aligned}$$

Namely, we can forget about the object, call the morphisms "objects", and call the 2-morphisms "morphisms". But since all the new "objects" were really morphisms from the original single object to itself, they can all be composed, or "tensored". That's why we get a category with "tensor product", and similarly, a "unit object".

So, just as a category with one object is just a monoid, a 2-category with one object is a monoidal category! This is one instance of a trick that I sketched many more cases of in "Week 74".

Now, in "Week 79" I defined left and right adjoints of functors between categories. Here the only thing I really needed about category theory was that Cat is a 2-category with categories as its objects, functors as its morphisms, and natural transformations as its 2-morphisms. So we can define left and right adjoints of morphisms in any 2-category by analogy as follows:

Suppose a and b are objects in a 2-category. Then we say that the morphism

$$L\colon a \to b$$

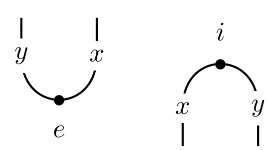
is a "left adjoint" of the morphism

$$R\colon b \to a$$

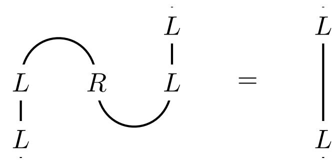
(and R is a "right adjoint" of L) if there are 2-morphisms

$$\begin{aligned} e&\colon RL \Rightarrow 1_b \\ i&\colon 1_a \Rightarrow LR \end{aligned}$$

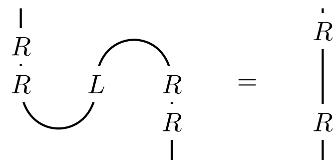
satisfying two magic equations. If we draw e and i as we did above,



then the two magic equations are



and



Alternatively, we can state these equations using the 2-categorical notation described in "Week 80", by saying that the following vertical composites of 2-morphisms are identity morphisms:

$$L = 1_a L \xRightarrow{i \cdot 1_L} LRL \xRightarrow{1_L \cdot e} L1_a = L$$

and

$$R = R1_a \xRightarrow{1_R \cdot i} RLR \xRightarrow{e \cdot 1_R} 1_b R = R$$

where $\cdot$ denotes the horizontal composite. If you look at these, and compare them to the graphical notation above, you'll see they are really saying the same thing.

The punchline is, when our 2-category has one object, we can think of it as a monoidal category, and then these equations are the definition of "duals" — one example being our earlier definition of dual vector spaces in the monoidal category Vect of vector spaces!

So adjoint functors and dual vector spaces are both instances of the general notion of adjoint 1-morphisms in a 2-category. Adjointness is a very basic concept.

I hope all that made some sense.

If this category theory stuff seems confusing, maybe you should read a 3-volume book about it! I can see you smiling, but seriously, I find the following reference very useful (despite a certain number of annoying errors). You can find a lot of good stuff about adjoint functors, monoidal categories, and much much more in here:

2) Francis Borceux, *Handbook of Categorical Algebra*, Cambridge U. Press 1994. *Volume 1: Basic Category Theory*. *Volume 2: Categories and Structure*. *Volume 3: Categories of Sheaves*.

To continue reading the "Tale of n-Categories", see "Week 84".

Week 84

June 27, 1996

While I try to limit myself to mathematical physics in This Week's Finds, I can't always keep it from spilling over into other subjects... so if you're not interested in computers, just skip down to reference 8 below. A while back on `sci.physics` Matt McIrvin pointed out that the closest thing we have to the computer of old science fiction — the underground behemoth attended by technicians in white lab coats that can answer any question you type in — is AltaVista. I agree wholeheartedly.

In case you are a few months or years behind on the technological front, let me explain: these days there is a vast amount of material available on the World-Wide Web, so that the problem has become one of locating what you are interested in. You can do this with programs known as search engines. There are lots of search engines, but my favorite these days is AltaVista, which is run by DEC, and seems to be especially comprehensive. So these days if you want to know, say, the meaning of life, you can just go to

1) AltaVista, `http://www.altavista.digital.com/`

type in "meaning of life", and see what everyone has written about it. You'll be none the wiser, of course, but that's how it always worked in those old science fiction stories, too.

The intelligence of AltaVista is of course far less than that of a fruit fly. But Matt's comment made me think about how now, as soon as we develop even a rudimentary form of artificial intelligence, it will immediately have access to vast reams of information on the Web... and may start doing some surprising things.

An example of what I'm talking about is the CYC project, Doug Lenat's $35 million project, begun in 1984, to write a program with common sense. In fact, the project plans to set CYC loose on the web once it knows enough to learn something from it.

2) CYC project homepage, `http://www.cyc.com/`

The idea behind CYC is to encode "common sense" as about half a million rules of thumb, declarative sentences which CYC can use to generate inferences. To have any chance of success, these rules of thumb must be organized and manipulated very carefully. One key aspect of this is CYC's ontology — the framework that lets it know, for example, that you can eat 4 sandwiches, but not 4 colors or the number 4. Most of the CYC code is proprietary, but the ontology will be made public in July of this year, they say. One can already read about aspects of it in

3) Douglas B. Lenat and R.V. Guha, *Building Large Knowledge-Based Systems: Representation and Inference in the Cyc Project*, Addison-Wesley, Reading, Mass., 1990.

My network of spies informs me that many hackers are rather suspicious of CYC. For an interesting and somewhat critical account of CYC at one stage of its development, see

4) Vaughan Pratt, "CYC Report", `http://boole.stanford.edu/pub/cyc.report`

Turning to something that's already very practical, I was very pleased when I found one could use AltaVista to do "backlinks". Certainly the World-Wide Web is in part inspired by Ted Nelson's visionary but ill-starred Xanadu project.

5) Project Xanadu, `http://xanadu.net/the.project`

Backlinking is one of the most tricky parts of Ted Nelson's vision, one often declared infeasible, but one upon which he has always insisted. Basically, the idea is that you should always be able to find all the documents pointing *to* a given document, as well as those to which it points. This allows **commentary** or **annotation**: if you read something, you can read what other people have written about it. My spies inform me that the World-Wide Web Committee is moving in this direction, but it is exciting that one can already do "backlinks browsing" with the help of a program written by Ted Kaehler:

6) Ted Kaehler's backlinks browser, `http://www.foresight.org/backlinks1.3.1.html`

Go to this page at the start of your browsing session. Follow the directions and let it create a new Netscape window for you to browse in. Whenever you want backlinks, click in the original page, and click "Links to Other Page". This launches an AltaVista search for links to the page you were just looking at.

It works quite nicely. I hope you try it, because with backlinking the Web will become a much more interesting and useful place, and the more people who know about it, the sooner it will catch on. For more discussion of backlinking, see

7) Backlinking news at the Foresight Institute, `http://www.foresight.org/backlinks.news.html`

   Robin Hanson's ideas on backlinking, `http://www.hss.caltech.edu/~hanson/findcritics.html`

I thank my best buddy Bruce Smith for telling me about CYC, Project Xanadu, and Ted Kaehler's backlinks browser.

Now let me turn to some mathematics and physics.

8) Francesco Fucito, Maurizio Martellini and Mauro Zeni, "The BF formalism for QCD and quark confinement", preprint available as hep-th/9605018.

9) Ioannis Tsohantjis, Alex C Kalloniatis, and Peter D. Jarvis, "Chord diagrams and BPHZ subtractions", preprint available as hep-th/9604191.

These two papers both treat interesting relationships between topology and quantum field theory — not the "topological quantum field theory" beloved of effete mathematicians such as myself, but the pedestrian sort of quantum field theory that ordinary working physicists use to study particle physics. So we are seeing an interesting cross-fertilization here: first quantum field theory got applied to topology, and now the resulting ideas are getting applied back to particle physics.

Why don't we see quarks roaming the streets freely at night? Because they are confined! Confined to the hadrons in which they reside, that is. We mainly see two sorts of hadrons: baryons made of three quarks, like the proton and neutron, and mesons made of a quark and an antiquark, like the pion or kaon. Why are the quarks confined in hadrons? Well, roughly it's because if you grab a quark inside a hadron and try to pull it out, the force needed to pull it doesn't decrease as you pull it farther out; instead, it remains about constant. Thus the energy grows linearly with the distance, and eventually you have put enough energy into the hadron to create another quark-antiquark pair, and *pop* — you find you are holding not a single quark but a quark together with a newly born antiquark, that is, a meson! What's left is a hadron with a newly born quark as the replacement for the one you tried to pull out!

That's a pretty heuristic description. In fact, particle physicists do not usually grab hadrons and try to wrest quarks from them with their bare hands. Instead they smash hadrons and other particles at each other and study the debris. But as a rough sketch of the theory of quark confinement, the above description is not *completely* silly.

There are various interesting things left to do, though. One is to try to get those quarks out by means of sneaky tricks. The only way known is to *heat* a bunch of hadrons to ridiculously high temperatures, preferably at ridiculously high pressures. I'm talking temperatures like 2 trillion degrees, and densities comparable to that of nuclear matter! This should yield a "quark-gluon plasma" in which quarks can zip around freely at enormous energies. That's what the folks at the Relativistic Heavy Ion Collider are doing — see "Week 76" for more.

This should certainly keep the experimentalists entertained. On the other hand, theorists can have lots of fun trying to understand more deeply why quarks are confined. We'd like best to derive confinement in some fairly clear and fairly rigorous way from quantum chromodynamics, or QCD — our current theory of the strong force, the force that binds the quarks together. Unfortunately, mathematical physicists are still struggling to formulate QCD in a rigorous way, so they can't yet turn to the exciting challenge of proving that confinement follows from QCD. And we certainly don't expect any simple way to "exactly solve" QCD, since it is complicated and highly nonlinear. So what some people do instead is computer simulations of QCD, in which they approximate spacetime by a lattice and do a lot of number-crunching. They usually use a fairly measly-sounding grid of something like 16 x 16 x 16 x 16 sites or so, since currently calculations take too long when the lattice gets much bigger than that.

Numerical calculations like these have a lot of potential. In "Week 68", for example, I talked about how people found numerical evidence for the existence of a "glueball" — a hadron made of no quarks, just gluons, the gluon being the particle that carries the strong force. This glueball candidate seems to match the features of an observed particle! Also, people have put a lot of work into computing the masses of more familiar hadrons. So far I believe they have concentrated on mesons, which are simpler. Eventually we should in principle be able to calculate things like the mass of the proton and neutron — which would be really thrilling, I think. Numerical calculations have also yielded a lot of numerical evidence that QCD predicts confinement.

Still, one would very much like some conceptual explanation for confinement, even if it's not quite rigorous. One way people try to understand it is in terms of "dual superconductivity". In certain superconductors, magnetic fields can only penetrate as long narrow tubes of magnetic flux. (For example, this happens in neutron stars - see "Week 37".) Now, just as electromagnetism consists of an "electric" part and a "magnetic" part, so does the strong force. But the idea is that confinement is due to the *electric* part of the strong force only being able to penetrate the vacuum in the form of long narrow tubes of field lines. The electric and magnetic fields are "dual" to each other in a precise mathematical sense, so this is referred to as dual superconductivity. Quarks have the strong force version of electric charge — called "color" — and when we try to pull two quarks apart, the strong electric field gets pulled into a long tube. This is why the force remains constant rather than diminishing as the distance between the quarks increases.

A while back, 't Hooft proposed an idea for studying confinement in terms of dual superconductivity and certain "order" and "disorder" observables. It seems this idea has languished to some extent due to a lack of necessary mathematical infrastructure. For the last couple of years, Martellini has been suggesting to use ideas from topological quantum field theory to serve this role. In particular, he suggested treating Yang-Mills theory as a perturbation of BF theory, and applying some of the ideas of Witten and Seiberg (who related confinement in the supersymmetric generalization of Yang-Mills theory to Donaldson theory). In the paper with Fucito and Zeni, they make some of these ideas precise. There are still some potentially serious loose ends, so I am very interested to hear the reaction of others working on confinement.

I have not studied the paper of Tsohantjis, Kalloniatis, and Jarvis in any detail, but people studying Vassiliev invariants might find it interesting, since it claims to relate the renormalization theory of φ^3 theory to the mathematics of chord diagrams.

10) Masaki Kashiwara and Yoshihisa Saito, "Geometric construction of crystal bases", `q-alg/9606009`.

The "canonical" and "crystal" bases associated to quantum groups, studied by Kashiwara, Lusztig, and others, are exciting to me because they indicate that the quantum groups are just the tip of a still richer structure. Whenever you see an algebraic structure with a basis in which the structure constants are nonnegative integers, you should suspect that you are really working with a category of some sort, but in boiled-down or "decategorified" form.

Consider for example the representation ring $R(G)$ of a group G. This is a ring whose elements are just the isomorphism classes of finite- dimensional representations of G. Addition in $R(G)$ corresponds to taking the direct sum of representations, while multiplication corresponds to taking the tensor product. Thus for example if x and y are irreducible representations of G — or "irreps" for short — and $[x]$ and $[y]$ are the corresponding basis elements of R(G), the product $[x][y]$ is equal to a linear combination of the irreps appearing in $x \otimes y$, with the coefficients in the linear combination being the *multiplicities* with which the various irreps appear in $x \otimes y$. These coefficients are therefore nonnegative integers. They are an example of what I'm calling "structure constants".

What's happening here is that the ring $R(G)$ is serving as a "decategorified" version of the category $\mathsf{Rep}(G)$ of representations of the group G. Alsmost everything about $R(G)$ is just a decategorified version of something about $\mathsf{Rep}(G)$. For example, $R(G)$ is a monoid under multiplication, while $\mathsf{Rep}(G)$ is a monoidal category under tensor product. $R(G)$ is actually a commutative monoid, while $\mathsf{Rep}(G)$ is a symmetric monoidal category — this being jargon for how the tensor product is "commutative" up to a nice sort of isomorphism. In $R(G)$ we have addition, while in $\mathsf{Rep}(G)$ we have direct sums, which category theorists would call "biproducts". And so on. The representation ring is a common tool in group theory, but a lot of the reason for working with $R(G)$ is simply that we don't know enough about category theory and are too scared to work directly with $\mathsf{Rep}(G)$. There are also *good* reasons for working with $R(G)$, basically *because* it is simpler and contains less information than $\mathsf{Rep}(G)$.

We can imagine that if someone handed us a representation ring $R(G)$ we might eventually notice that it had a nice basis in which the structure constants were nonnegative integers. And we might then realize that lurking behind it was a category, $\mathsf{Rep}(G)$. And then all sorts of things about it would become clearer. . . .

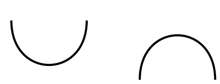
Something similar like this seems to be happening with quantum groups! Ignoring a lot of important technical details, let me just say that quantum groups turn out have nice bases in which the structure constants are nonnegative integers, and the reason is that lurking behind the quantum groups are certain categories. What sort of categories? Categories of "Lagrangian subvarieties of the cotangent bundles of quiver varieties". Yikes! I don't think I'll explain *that* mouthful! Let me just note that a quiver is itself a cute little category that you cook up by taking a graph and thinking of the vertices as objects and the edges as morphisms, like this:

$$\bullet \to \bullet \to \bullet \to \bullet \to \bullet$$

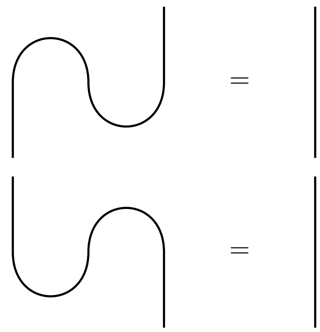
If you do this to a graph that's the Dynkin diagram of a Lie group — see "Week 62" and the weeks following that — then the fun starts! Dynkin diagrams give Lie groups, but also quantum groups, and now it turns out that they also give rise to certain categories of which the quantum groups are decategoried, boiled-down versions. . . . I don't understand all this, but I certainly intend to, because it's simply amazing how a world of complex symmetry emerges from these Dynkin diagrams.

For more on this stuff try the paper by Crane and Frenkel referred to in "Week 38" and "Week 50". It suggests some amazing relationships between this stuff and 4-dimensional topology. . . .

Let me conclude by reminding you where I am in "the tale of n-categories" and where I want to go next. So far I have spoken mainly of 0-categories, 1-categories, and 2-categories, with lots of vague allusions as to how various patterns generalize to higher n. Also, I have concentrated mainly on the related notions of equality, isomorphism, equivalence, and adjointness. Equality, isomorphism and equivalence are the most natural notions of "sameness" when working in 0-categories, 1-categories, and 2-categories, respectively. Adjointness is a closely related but more subtle and exciting concept that you can only start talking about once you get to the level of 2-categories. People got tremendously excited by it when they started working with the 2-category Cat of all small categories, because it explained a vast number of situations where you have a way to go back and forth between two categories, without the categories being "the same" (or equivalent). Another exciting thing about adjointness is that it really highlights the relation between 2-categories and 2-dimensional topology — thus pointing the way to a more general relation between n-categories and n-dimensional topology. From this point of view, adjointness is all about "folds":



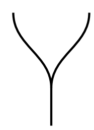
and their ability to cancel:



This concept of "doubling back" or "backtracking" is a very simple and powerful one, so it's not surprising that it is prevalent throughout mathematics and physics. It is an essentially 2-dimensional phenomenon (though it occurs in higher dimensions as well), so it can be understood most generally in the language of 2-categories.

(In physics, "doubling back" is related to the notion of antiparticles as particle moving backwards in time, and appears in the Feynman diagrams for annihilation and creation of particle/antiparticle pairs. For those familiar with the category-theoretic approach to Feynman diagrams, the stuff in "Week 83" about dual vector spaces should suffice to make this connection precise.)

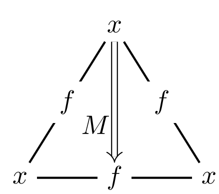
Next I will talk about another 2-dimensional concept, the concept of "joining" or "merging":



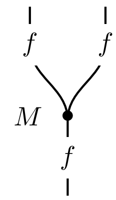
This is probably even more powerful than the concept of "folding": it shows up whenever we add numbers, multiply numbers, or in many other ways combine things. The 2-categorical way to understand this is as follows. Suppose we have an object x in a 2-category, and a morphism $f\colon x \to x$. Then we can ask for a 2-morphism

$$M\colon f^2 \Rightarrow f.$$

If we have such a thing, we can draw it as a traditional 2-categorical diagram:



or dually as a "string diagram"



Regardless of how you draw it, the 2-morphism $M\colon f^2 \Rightarrow f$ represents a process turning two copies of f into one. And as we'll see, all sorts of fancy ways mathematicians have of studying this sort of process — "monoids", "monoidal categories", and "monads" — are special cases of this sort of situation.

To continue reading the "Tale of n-Categories", see "Week 89".

Week 85

July 14, 1996

I'm spending this month at the Erwin Schroedinger Institute in Vienna, where Abhay Ashtekar and Peter Aichelburg are running a workshop called Mathematical Problems of Quantum Gravity.

Ashtekar is one of the founders of an approach to quantizing gravity called the loop representation. I've explained this approach in "Week 7", "Week 43", and other places, but let me just remind you of the basic idea. In the traditional approach to reconciling general relativity with quantum theory, excitations of the gravitational field were described by small ripples in the geometry of flat spacetime, or "gravitons". In the loop representation, they are instead described by collections of loops, which we can think of as "flux tubes of area" floating in an otherwise utterly featureless void. More recently, the loop approach has been supplemented by a technical device known as "spin networks": roughly speaking, a spin network is a graph whose edges are labelled by spins $0, 1/2, 1, 3/2, \ldots$ with an edge of spin j corresponding to a flux tube carrying area equal to $\sqrt{j(j+1)}$ times the square of the Planck length — the fundamental length scale in quantum gravity, about 10^{-35} meters. For more on spin networks, try "Week 55".

Quantum gravity has always been a tough subject. After a lot of work, a lot of people concluded that the traditional approach to quantum gravity didn't make sense, mathematically. This led to string theory, an attempt to quantize gravity together with all the other forces and particles. But in the late 1980s, Rovelli and Smolin revived hopes of quantizing gravity alone by introducing the loop representation.

One doesn't expect the loop representation to describe much real physics until one introduces other forces and particles. Pure gravity is just a warm-up exercise — but it's not at all easy! When the loop representation was born, it was rather sketchy at many points. A lot of mathematical problems had to be overcome to make it as precise as it is now.... and there are a lot of formidable difficulties left, any one of which could spell doom for the theory. Luckily, progress has been rapid. Many of the problems which seemed hopelessly beyond our reach a few years ago can now be formulated precisely, and maybe even solved. The idea of this workshop is to start tackling these problems.

A lot has been going on! People give talks at 11 in the morning, while afternoons are devoted to more informal discussions in small groups. There are general introductory talks on Tuesdays, more technical talks on Thursdays, and short talks on research in progress on some other days.

To give a bit of the flavor of the workshop, let me describe things day by day. I'll need to describe some days very sketchily, though, or I'll never finish writing this!

- **Wednesday, July 3** — Rodolfo Gambini spoke on gauge-invariance in the extended loop representation. The idea of the loop representation is to study the gravitational vector potential by studying certain integrals of it around loops. Mathematicians call this the trace of the holonomy, and physicists call it a Wilson loop or the trace of a path-ordered exponential. In the loop representation, states of quantum gravity are described by certain functions that eat Wilson loops and spit out complex numbers... i.e., that assign an "amplitude" to each Wilson loop.

In quantum field theory you often need to average a quantum field over some 3-dimensional region of space or 4-dimensional region of spacetime to get a well-defined operator. Wilson loops are rather singular because a loop is a one-dimensional object. On the other hand, they are nice because they are gauge-invariant: they don't change when we do a gauge transformation to the vector potential.

In the "extended" loop representation one tries to make the integral less singular by not dealing with actual loops, but certain analogous integrals over all 3-dimensional space. Heuristic calculations suggest that they are gauge-invariant, but Troy Schilling noticed a while ago that they aren't always *really* gauge-invariant — basically because the the path-ordered exponential is given by a certain Taylor series, and nasty things can happen when you manipulate infinite series without checking if your manipulations are legitimate! See:

1) Troy Schilling, "Non-covariance of the generalized holonomies: Examples", preprint available as gr-qc/9503064.

There has been a certain amount of competition between the extended loop representation, developed by Gambini and various coauthors, and Ashtekar's approach. Thus Schilling's result was seen as a blow against the extended loop representation. In Gambini's talk, he argued that gauge-invariance is rigorously maintained by certain extended loops, e.g. those for which the power series has finitely many terms. The most famous examples of functions of extended loops with only finitely many terms are the Vassiliev invariants, which come up in knot theory (see "Week 3"). Gambini and Pullin have claimed that certain Vassiliev invariants are states of quantum gravity, so these are of special interest.

The feeling was that we needed to compare these different loop representations more carefully because they both have advantages.

Also, Renate Loll spoke about "Lattice Gravity". See "Week 55" for a bit more on this. Her talk led to an interesting discussion of the meaning of the limit, as the lattice spacing goes to zero, of quantum gravity as done on a lattice. Does it make sense? One needs, apparently, to look at ones formula for the Hamiltonian constraint on the lattice, and see if it depends on the Planck length in a manner *other than* having the Planck length as an overall prefactor. Various people tried to do the calculation on the spot, and got mixed up.

- **Thursday, July 4 —** Thomas Thiemann spoke on "The Hamiltonian Constraint for Lorentzian Canonical Quantum Gravity". This was a big bombshell. The Hamiltonian constraint in quantum gravity is one of the biggest, baddest problems we are facing. It's the analog of Schrodinger's equation in quantum mechanics, but it's a constraint:

$$H\psi = 0.$$

All the dynamics of the theory is contained in this equation, yet we only roughly understand how to define it in a rigorous way. Thiemann, a student of Ashtekar who is now a postdoc at Harvard, had put the following 5 papers on the general relativity preprint server right before the workshop. The first one gives a rigorous definition of the Hamiltonian constraint!

2) Thomas Thiemann, "Quantum Spin Dynamics (QSD)", preprint available as gr-qc/9606089.

Thomas Thiemann, "Quantum Spin Dynamics (QSD) II", preprint available as gr-qc/9606090.

Thomas Thiemann, "Anomaly-free formulation of non-perturbative, four-dimensional Lorentzian quantum gravity", *Phys. Lett. B* **380** (1996) 257–264, preprint available as gr-qc/9606088.

Thomas Thiemann, "Closed formula for the matrix elements of the volume operator in canonical quantum gravity", preprint available as gr-qc/9606091.

Thomas Thiemann, "A length operator for canonical quantum gravity", preprint available as gr-qc/9606092.

It is interesting to compare "Quantum Spin Dynamics" to the paper by Ashtekar and Lewandowksi, so far available only in draft form to a select few, in which they gave a rigorous definition of the square root of the Hamiltonian constraint. The advantage of "QSD" is that it deals directly with the Hamiltonian constraint, rather than its square root, and that it does this using some ingenious formulas for the Hamiltonian constraint of Lorentzian gravity in terms of the Hamiltonian constraint for Riemannian gravity and the total volume and total extrinsic curvature of the universe (which we assume is compact).

You see, quantum gravity comes in two flavors, Lorentzian and Riemannian, depending on whether we work with real time — the obviously sensible thing to do — or imaginary time — not at all obviously sensible, but with a curious mathematical charm to it, which has won many hearts. The interplay between these two has long been a bugaboo of the loop representation. The Lorentzian theory is harder to work with, so lots of people cheat and study the Riemannian theory. Sometimes they do this covertly, with a guilty conscience, so in some papers it's left unclear which theory the author is actually working with! Thiemann's work, however, seems to exploit the interplay between the theories in a benign way — related to earlier ideas of Ashtekar, but different. I would like to understand this interplay more deeply.

Due to jetlag I woke up at 4 am on the morning of this talk, and I couldn't get back to sleep, so I read his paper. When I came to the Institute at 9 am — a shockingly early hour for people working on quantum gravity — I was sure nobody would be there yet. But as I entered I bumped into Carlo Rovelli. It turned out he had stayed up all night reading Thiemann's paper, too excited to sleep!

After this talk everyone was busily trying to learn Thiemann's stuff, trying to figure out if it is physically correct, and trying to figure out what to do next.

- **Tuesday, July 9** — Abhay Ashtekar gave a general talk on the "Quantum Theory of Geometry". Most of it was well-known stuff to fans of the loop representation, but one new tidbit concerned the noncommutativity of area operators. Since the area of surfaces in space depends only on the metric on space, not on its first time derivative, one might expect their quantum analogs to commute, since the metric and its first time derivative are analogous to position and momentum in quantum mechanics. But they don't commute! In a later talk, Ashtekar explained that this is not really a strange new feature of quantum gravity, but one which has its classical analog.

- **Wednesday, July 10 —** Kirill Krasnov gave a talk on a paper we started working on together just recently, "Quantization of diffeomorphism invariant theories with fermions". Kirill is a young Ukrainian physicist whom I first met last summer in Warsaw; he had written a nice paper on the loop representation of quantum gravity coupled to electromagnetism and fermions:

  3) Kirill Krasnov, "Quantum loop representation for fermions coupled to Einstein-Maxwell field", *Phys. Rev.* **D53** (1996), 1874; preprint available as gr-qc/9506029.

  When I met him again here, it turned out he was continuing this work, and also making it more rigorous. Now, I had for some time been meaning to write something with Hugo Morales-Tecotl showing that a slight generalization of spin network states form a basis of states for such theories. These states had already appeared, for example, in his work with Rovelli:

  4) Carlo Rovelli and Hugo Morales-Tecotl, "Fermions in quantum gravity", *Phys. Rev. Lett.* **72** (1994), 3642–3645.
  Carlo Rovelli and Hugo Morales-Tecotl, *Nucl. Phys.* **B451** (1995), 325, preprint available as gr-qc/9401011.

  But we had never gotten around to it. So, I decided to team up with Kirill and write a paper on this stuff.

Week 86

August 6, 1996

Let me continue my reportage of what happened at the Mathematical Problems of Quantum Gravity workshop in Vienna. I will only write about quantum gravity aspects here. I had an interesting conversation with Bertram Kostant in which he explained to me the deep inner secrets of the exceptional Lie group E_8. However, my writeup of that has grown to the point where I will save it for some other week.

By the way, my course on n-category theory is not over! I'm merely taking a break from it, and will return to it after this workshop.

So. . .

- **Wednesday, July 10th —** Jerzy Lewandowski gave a talk on the "Spectrum of the Area Operator". As I've mentioned a few times before, one of the exciting things about the loop representation of quantum gravity is that the spectrum of the area operator associated to any surface is discrete. In other words, area is quantized!

  Let me describe the area operator a bit more precisely. Before I tell you what the area operator is, I have to tell you what it operates on. Remember from "Week 43" that there are various Hilbert spaces floating around in the canonical quantization of gravity. First there is the "kinematical state space". In the old-fashioned metric approach to quantum gravity, known as geometrodynamics, this was supposed to be $L^2(\mathrm{Met})$, where Met is the space of Riemannian metrics on space. (We take as space some 3-manifold S, which for simplicity we assume is compact). The problem was that nobody knew how to rigorously define this Hilbert space $L^2(\mathrm{Met})$. In the "new variables" approach to quantum gravity, the kinematical state space is taken instead to be $L^2(\mathcal{A})$, where $\mathcal{A}$ is the space of connections on space on some trivial $\mathrm{SU}(2)$ bundle over S. This *can* be defined rigorously.

  Here's the idea, roughly. Fix any graph g, with finitely many edges and vertices, embedded in S. Let $\mathcal{A}_g$, the space of connections on that graph, be $\mathrm{SU}(2)^n$ where n is the number of edges in e. Thus a connection on a graph tells us how to parallel transport things along each edge of that graph — an idea familiar from lattice gauge theory. $L^2(\mathcal{A}_g)$ is well-defined because $\mathrm{SU}(2)$ has a nice measure on it, namely Haar measure, so $\mathcal{A}_g$ gets a nice measure on it as well.

  Now if one graph g is contained in another graph h, the space $L^2(\mathcal{A}_g)$ is contained in the space $L^2(\mathcal{A}_h)$ in an obvious way. So we can form the union of all the Hilbert spaces $L^2(\mathcal{A}_g)$ and get a big Hilbert space $L^2(\mathcal{A})$. Mathematicians would say that $L^2(\mathcal{A})$ is the "projective limit" of the Hilbert spaces $L^2(\mathcal{A}_g)$, but let's not worry about that.

  So that's how we get the space of "kinematical states" in the loop representation of quantum gravity. The space of physical states is then obtained by imposing constraints: the Gauss law constraint (i.e., gauge-invariance), the diffeomorphism constraint (i.e., invariance under diffeomorphisms of S) and the Hamiltonian constraint (i.e., invariance under time evolution). States in the physical state space are supposed to only contain information that's invariant under all coordinate transformations and gauge transformations — the really physical information.

I explained these constraints to some extent in "Week 43", and I don't really want to worry about them here. But let me just mention that the Gauss law constraint is easy to impose in a mathematically rigorous way. The diffeomorphism constraint is harder but still possible, and the Hamiltonian constraint is the big thorny question plaguing quantum gravity — see "Week 85" for some recent progress on this. The area operators I'll be talking about are self-adjoint operators on the space of kinematical states, $L^2(\mathcal{A})$, and are a preliminary version of some related operators one hopes eventually to get on the physical state space, after much struggle and sweat.

To define an operator on $L^2(\mathcal{A})$ it's enough to define it on $L^2(\mathcal{A}_g)$ for every graph g and then check that these definitions fit together consistently to give an operator on the big space $L^2(\mathcal{A})$. So let's take a graph g and a surface s in space. The area operator we're after is supposed to be the quantum analog of the usual classical formula for the area of s. The usual classical area is a function of the metric on space; similarly, the quantum area is an operator on the space $L^2(\mathcal{A})$.

The area operator only cares about the points where the graph intersects the surface. We assume that there are only finitely many points where it does so, apart from points where the edges are tangent to the surface. (To make this assumption reasonable, we need to assume, e.g., that the space S has a real-analytic structure and the surface and graph are analytic — an annoying technicality that I have been seeking to eliminate.)

The area operator is built using three operators on $L^2(\mathrm{SU}(2))$ called J_1, J_2, and J_3, the self-adjoint operators corresponding to the 3 generators of $\mathrm{SU}(2)$ — which often show up in physics as the three components of angular momentum! Alternatively, we can think of all three together as one vector-valued operator J, the "angular momentum operator". Note that $L^2(\mathcal{A}_g)$ is just the tensor product of one copy of the Hilbert space $L^2(\mathrm{SU}(2))$ for each edge of our graph g. Thus for any edge e we get an angular momentum operator $J(e)$ that acts on the copy of $L^2(\mathrm{SU}(2))$ corresponding to the edge e in question, leaving the other copies alone.

This, then, is how we define the operator on $L^2(\mathcal{A}_g)$ corresponding to the area of the surface s. Pick an orientation for the surface s. For any point where the graph g intersects s, let $J(\mathrm{in})$ denote the sum of the angular momentum operators of all edges intersecting s at the point in question and pointing "inwards" relative to our chosen orientation. Similarly, let $J(\mathrm{out})$ be the sum of the angular momentum operators of edges intersecting s at the point in question and pointing "outwards". (Note: edges tangent to the surface contribute neither to $J(\mathrm{in})$ nor $J(\mathrm{out})$.) Now sum up, over all points where the graph intersects the surface, the following quantity:

$$\sqrt{(J(\mathrm{in}) - J(\mathrm{out})) \cdot ((J(\mathrm{in}) - J(\mathrm{out}))}$$

where the dot denotes the obvious sort of dot product of vector-valued operators. Multiply by half the Planck length squared and you've got the area operator!

This very beautiful and simple formula was derived by Ashtekar and Lewandowski, but the first people to try to quantize the area operator were Rovelli and Smolin; see

1) "Discreteness of area and volume in quantum gravity", by Carlo Rovelli and Lee Smolin, 36 pages in LaTeX format, 13 figures uuencoded, available as gr-qc/9411005.

   Abhay Ashtekar and Jerzy Lewandowski, "Quantum theory of geometry I: area operators", 31 pages in LaTeX format, to appear in the *Classical and Quantum Gravity* special issue dedicated to Andrzej Trautman, preprint available as gr-qc/9602046.

In his talk Jerzy showed how to work the spectrum of the area operator (which is discrete) and showed how it could depend on whether the surface s cuts space into 2 parts or not.

Later that day, Mike Reisenberger, Matthias Blau, Carlo Rovelli and I talked about the relation between string theory and the loop representation of quantum gravity.

Mike has been working on a very interesting "state sum model" for quantum gravity; that is, a discretized model in which spacetime is made of 4-simplices (the 4d version of tetrahedra), fields are thought of ways of labelling the faces, edges and so on by spins, elements of $\mathrm{SU}(2)$ and the like, and the path integral is replaced by a sum over these labellings. This works out quite nicely for quantum gravity in 3 dimensions — see "Week 16" — but it's much more challenging in 4 dimensions.

One nice feature of these state sum models for quantum gravity is that they may be reinterpreted as sums over "worldsheets" — surfaces mapped into spacetime. Since the spacetime is discrete, so are these surfaces — they're made of lots of triangles — but apart from that, having a path integral that's a sum over worldsheets is pleasantly reminscent of string theory. Indeed, once upon a time I proposed that the loop representation of quantum gravity and string theory were two aspects of some theory still waiting to be fully understood:

2) John Baez, "Strings, loops, knots, and gauge fields", in *Knots and Quantum Gravity*, ed. J. Baez, Oxford U. Press, Oxford, 1994, preprint available in LaTeX form as hep-th/9309067, 34 pages.

The problem was getting a concrete way to relate the Lagrangian for the string theory to the Lagrangian for gravity (or whatever gauge theory one started with). Iwasaki tackled this problem was tackled in 3d quantum gravity using state sum models:

3) Junichi Iwasaki, "A reformulation of the Ponzano-Regge quantum gravity model in terms of surfaces", University of Pittsburgh, 11 pages in LaTeX format available as gr-qc/9410010.

Later, Reisenberger extended this approach to deal with certain 4d theories which are simpler than quantum gravity, like BF theory:

4) Michael Reisenberger, "Worldsheet formulations of gauge theories and Gravity", University of Utrecht preprint, 1994, available as gr-qc/9412035.

In all of these theories, one computes the action for the worldsheets by summing something over places where they intersect. In other words, they "interact" at intersections.

But the really exciting thing would be to do something like this for Mike's new state sum model for 4d quantum gravity. And the real challenge would be to relate this — if possible! — to conventional string theory. In a coffeeshop I suggested that one might do this by using the usual formula for the action in (bosonic) string theory. This is simply the *area* of the string worldsheet with respect to some background metric. The loop representation of quantum gravity doesn't make reference to any background metric; the closest approximation to a classical metric is a "weave" state in which space is tightly packed with lots of loops or spin networks. From the 4d point of view, we'd expect this to correspond to a spacetime packed with lots of worldsheets. Now, given the relation between area and intersection number in the loop representation (see above!), one might expect the area of a given worldsheet to be roughly proportional to the number of its intersections with the other worldsheets in this "weave". But this is what one would expect in any theory where the worldsheets interact at intersections. So, one could hope that Mike's state sum model would be approximately equivalent to a string theory of the sort string theorists study.

There are lots of obvious problems with this idea, but it led to an interesting conversation, and I am still not convinced that it is crazy.

- **Thursday, July 11th —** Jorge Pullin spoke on skein relations and the Hamiltonian constraint in lattice quantum gravity. His idea was that the Hamiltonian constraint contains a "topological factor" which serves as a skein relation on loop states.

- **Friday, July 12th —** Abhay Ashtekar gave a talk on "Noncommutativity of Area Operators". This explained how the rather shocking fact that the area operators for two intersecting surfaces needn't commute actually has a perfect analog in classical general relativity.

  Mike Reisenberger spoke on "Euclidean Simplicial GR". This presented the details of his state sum model. Since he hasn't published this yet, and since I am getting a bit tired out, I won't describe it here.

- **Monday, July 15th —** Renate Loll gave a talk on the volume and area operators in lattice gravity. I wrote a bit about her work on the volume operator in "Week 55", and more can be found in:

  5) Renate Loll, "The volume operator in discretized quantum gravity", preprint available as gr-qc/9506014, 15 pages.
  Renate Loll, "Spectrum of the volume operator in quantum gravity", preprint available as gr-qc/9511030, 14 pages.

Also, Jerzy Lewandowski spoke on his work with Ashtekar on the volume operator in the continuum theory:

6) Jerzy Lewandowski, "Volume and quantizations", preprint available as gr-qc/9602035, 8 pages.

Abhay Ashtekar and Jerzy Lewandowski, "Quantum theory of geometry II: volume operators", manuscript in preparation.

The volume operator is more tricky than the area operator, and various proposed formulas for it do not agree. This is summarized quite clearly in Jerzy's paper.

In fact, I have already left Vienna by now. I was too busy there to keep up with This Week's Finds, but my life is a bit calmer now and I will try to finish these reports soon.

Week 87

August 20, 1996

Let me continue summarizing what happened during July at the Mathematical Problems of Quantum Gravity workshop in Vienna. The first two weeks concentrated on the foundations of the loop representation of quantum gravity; the next week was all about black holes!

- **Tuesday, July 16th —** Ted Jacobson gave an overview of "Issues of Black Hole Thermodynamics". There is a lot to say about this subject and I won't try to repeat his marvelous talk here. Let me just mention a very interesting technical point he made. The original Bekenstein-Hawking formula for the entropy of a black hole is

  $$S = A/(4\hbar G)$$

  where A is the area of the event horizon, $\hbar$ is Planck's constant, and G is Newton's constant. One way to try to derive this is from the partition function of a quantum field theory involving gravity and other fields. Jacobson sketched a heuristic calculation along these lines. When you do this calculation it's natural to worry why the other fields, representing various forms of matter, don't seem to contribute to the answer above. Also, when we do quantum field theory, there is often a difference between the "bare" coupling constants we put into the theory and the "renormalized" coupling constants that are what the theory predicts we'll observe experimentally. So it's natural to worry about whether it's the bare or renormalized Newton's constant G that enters the above formula — even though quantum gravity is so unlike most other quantum field theories that it's unclear that this worry makes sense, ultimately.

  Anyway, the nice thing is that these two worries cancel each other out. In other words: yes, it's the renormalized Newton's constant G — the physically measured one — that enters the above formula. But at least to first order in $\hbar$, the difference between the bare G and the renormalized G is precisely due to the interactions between gravity and the matter fields (including the self-interaction of the gravitational field). In other words, the matter fields really *do* contribute to the black hole entropy, but this contribution is absorbed into the definition of the renormalized G.

  In the most extreme case, the bare $1/G$ is zero, and the renormalized $1/G$ is entirely due to interactions between matter and gravity. This is Andrei Sakharov's theory of "induced gravity". According to Jacobson, in this case all of the black hole entropy is "entanglement entropy" — this being standard jargon for the way that two parts of a quantum system can each have entropy due to correlations, even though the whole system has zero entropy. Unfortunately my notes do not allow me to reconstruct the wonderful argument whereby he showed this. (See "Week 27" for a more detailed explanation of entanglement entropy.)

- **Wednesday July 17th —** There was a talk on "Colombeau theory" by a mathematician whose name I unfortunately failed to catch. Colombeau theory is a theory that allows you to multiply distributions, just like they said in school that you weren't allowed to do. So if for example you want to square the Dirac delta function, you can do it in the context of Colombeau theory. There has been a certain amount of debate, however, on whether Colombeau theory allows you to this multiplication in a *useful* way. There were a lot of physicists at this talk who would be willing and eager to master Colombeau theory if it let one solve the physics problems they wanted to solve. However, after much discussion, it appears that they didn't buy it. I believe that at best Colombeau theory provides a useful framework for understanding the ambiguities one encounters when multiplying distributions.

I say "ambiguities" rather than "disasters" because while the square of the Dirac delta function has no sensible interpretation as a distribution, there are many cases, such as when you try to multiply the Dirac delta function and the Heaviside function, where you can interpret the result as a distribution in a variety of ways. These ambiguous cases are the ones of greatest interest in physics. A nice place to see this in quantum field theory is in

1) G. Scharf, *Finite quantum electrodynamics: the causal approach*, Springer-Verlag, Berlin, 1995.

If you want to learn about Colombeau theory you can try:

2) J. F. Colombeau, *Multiplication of Distributions: a Tool in Mathematics, Numerical Engineering, and Theoretical Physics*, Lecture Notes in Mathematics **1532**, Springer, Berlin, 1992.

Later that day I had nice conversation with Jerzy Lewandowski on the approach to the loop representation where one uses smooth, rather than analytic, loops. (See "Week 55" for more on this issue.)

- **Thursday, July 18th —** Carlo Rovelli spoke on "Black Hole Entropy", reporting some work he did with Kirill Krasnov. They have a nice approach to computing the black hole entropy using the loop representation of quantum gravity. A common goal among quantum gravity folks is to recover the Bekenstein-Hawking formula from some full-fledged theory of quantum gravity — the original derivation being a curious "semiclassical" hybrid of quantum and classical reasoning. In a statistical mechanical approach, entropy should be the logarithm of the number of microstates some system can have in a given macrostate. So one wants to count states somehow. Basically what Rovelli and Krasnov do is count the number of ways a surface can be pierced by a spin network so as to give it a certain area. (This uses the formula for the area operator I descrbed in "Week 86".) They get an entropy proportional to the area, but not with the same constant as in the Bekenstein-Hawking formula.

  There were some hopes that taking matter fields into account might give the right constant. But since everyone had been to Ted Jacobson's talk, this led to much interesting wrangling over whether Rovelli and Krasnov were using the bare or renormalized Newton's constant G, and whether the concept of bare and renormalized G even makes sense, ultimately! Also, there are some extremely important puzzles about what the right way to count states is, in these loop representation computations.

For more, try:

3) Carlo Rovelli, "Loop quantum gravity and black hole physics", preprint available as gr-qc/9608032.

   Kirill Krasnov, "The Bekenstein bound and non-perturbative quantum gravity", preprint available as gr-qc/9603025.

   Kirill Krasnov, "On statistical mechanics of gravitational systems", preprint available as gr-qc/9605047.

- **Friday, July 19th** — Don Marolf spoke on "Black hole entropy in string theory". He attempted valiantly to describe the string-theoretic approach to computing black hole entropy to an audience only generally familiar with string theory. I will not try to summarize his talk, except to note that he mainly discussed the case of a black hole in 5 dimensions, which was really a "black string" in 6 dimensions — a solution with translational symmetry in the 6th dimension, but where the extra 6th dimension is so tiny that ordinary 5-dimensional folks think they've just got a black hole. (By the way, even the 6-dimensional approach is really just a way of talking about a string theory that fundamentally lives in 10 dimensions. This stuff is not for the faint-hearted.)

  Here are a few papers on this subject by Marolf and Horowitz:

4) Gary Horowitz, "The origin of black hole entropy in string theory", preprint available as gr-qc/9604051.

   Gary T. Horowitz and Donald Marolf, "Counting states of black strings with traveling waves", preprint available as hep-th/9605224.

   Gary T. Horowitz and Donald Marolf, "Counting states of black strings with traveling waves II", preprint available as hep-th/9606113.

- **Monday, July 22nd** — Kirill Krasnov spoke on "The Einstein-Maxwell Theory of Black Hole Entropy". This was a report on attempts to see how his calculations of the black entropy in the loop representation changed when he took the electromagnetic field into account. The calculations were very tentative, for certain technical reasons I won't go into here, but they made even clearer the importance of the issue of how one counts states when computing entropy in this approach.

  Later, I had a nice conversation with Carlo Rovelli about my hopes for thinking of fermions (e.g., electrons) as the ends of wormholes in the loop representation of quantum gravity. We came up with a nice heuristic argument to get the right Fermi statistics for these wormhole ends. Hopefully we can make this all more precise at some later date.

- **Tuesday, July 23rd** — Ted Jacobson gave informal talks on two subjects, the first of which was "Transplanckian puzzle: origin of outgoing black hole modes." This dealt with the puzzling fact that in the standard computation of Hawking radiation, the rather low-frequency radiation which leaves the hole is the incredibly redshifted offspring of high-frequency modes which swung past the horizon shortly after the hole's formation — modes whose wavelength is far smaller than the Planck length!

What if spacetime is "grainy" in some way at the Planck scale? Jacobson studied this using an analogy introduced by Unruh. If you have fluid flowing down a narrowing pipe, and at some point the velocity of the fluid flow exceeds the speed of sound in the fluid, there will be a "sonic horizon". In other words, there is a line where the fluid flow exceeds the speed of sound, and no sound can work its way upstream across that line. Now if you quantize the theory of sound in a simple-minded way you get "phonons" — which have indeed been observed in solid-state physics. Unruh showed that in the case at hand you would get "Hawking radiation" of phonons from the sonic horizon, going upstream and getting shifted to lower frequencies as they go.

Jacobson considered what would happen if you actually took into account the graininess of the fluid. (He considered the theory of liquid helium, to be specific.) The graininess at the molecular scale means that the group velocity of waves drops at very high frequencies. So what happens instead of "Hawking radiation" is something rather different. Start with a high-frequency wave attempting to go upstream, starting from upstream of the sonic horizon. Its group velocity is very slow so it fails miserably and gets swept toward the sonic horizon, like a hapless poor swimmer getting pulled to the edge of a waterfall despite trying to swim upstream. But as it gets pulled near the horizon its wavelength increases, and thus group velocity increases, thus allowing it to shoot upstream at the last minute! (An analogous process is apparently familiar in plasma physics under the name of "mode conversion".) In this scenario, the Hawking radiation winds up resulting from incoming modes through this process of mode conversion — modes that have short wavelength, but not as short as the intermolecular spacing (or Planck length, in the gravitational case.)

Ted Jacobson's second talk was even more interesting to me, but I'll postpone that for next Week.

Here, by the way, is a paper related to the talk by Pullin discussed in "Week 86":

5) Hugo Fort, Rodolfo Gambini and Jorge Pullin, "Lattice knot theory and quantum gravity in the loop representation", preprint available as gr-qc/9608033.

Week 88

August 26, 1996

This issue concludes my report of what happened at the Mathematical Problems of Quantum Gravity workshop in Vienna. I left the workshop at the end of July, so my reportage ends there, but the workshop went on for a few more weeks after that. I'll be really bummed out if I find out that they solved all the problems with quantum gravity after I left.

Before I launch into my day-by-day account of what happened, let me note that I've written a little introduction to Thiemann's work on the Hamiltonian constraint, which he presented at the workshop (see "Week 85"):

1) John Baez, "The Hamiltonian constraint in the loop representation of quantum gravity", available at `http://math.ucr.edu/home/baez/hamiltonian/`

A less technical version of this appears in Jorge Pullin's newsletter *Matters of Gravity*, issue 8, at `http://www.phys.lsu.edu//mog/mog8/node7.html`

Okay... I'll start out simple today since there is something nice and simple to ponder:

- **Tuesday, July 23rd —** Ted Jacobson spoke on the "Geometry and Evolution of Degenerate Metrics". One of the interesting things about Ashtekar's reformulation of general relativity is that it extends general relativity to the case of degenerate metrics, that is, metrics where there are vectors whose dot product with all other vectors is zero. However, one needs to be very careful because different versions of Ashtekar's formulation give *different* ways of handling degenerate metrics.

  To see why in a simple example, remember that the usual metric on Minkowski spacetime is nondegenerate and in nice coordinates looks like

  $$-dt^2 + dx^2 + dy^2 + dz^2$$

  Here we are setting the speed of light equal to 1. In general relativity, one way people describe the metric is using a tensor g_{ab}, where the indices a and b go from 0 to 3. In Minkowski space this tensor equals

  $$\begin{pmatrix} -1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 1 \end{pmatrix}$$

  What this tensor means is that if you have two vectors v and w, their dot product is $g_{ab}v^a w^b$, where as usual we multiply the entries of the metric tensor and the vectors v and w as indicated, and then sum over repeated indices. So, for example, the dot product of the vector

  $$v = (1, 1, 0, 0)$$

  with itself is 0, though its dot product with other vectors needn't be zero. There is a bunch of vectors whose dot products with themselves are zero, and these are called lightlike vectors, because light travels in these directions, moving one unit in space for each unit in time. There is actually a cone of lightlike vectors, called the lightcone.

One can imagine a world where the metric g_{ab} is

$$\begin{pmatrix} -1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & k & 0 \\ 0 & 0 & 0 & k \end{pmatrix}$$

for some $k > 0$. This world isn't really so different from Minkowski space, because you can also think of it as Minkowski space described in screwy coordinates where you are measuring distances in the y and z directions in different units than the x direction. When k gets small, you can check that the lightcone gets stretched out in the y and z directions. Alternatively, when k gets big, the lightcone gets squashed in the y and z directions.

Another way to formulate general relativity uses the inverse metric g^{ab}. This is just the inverse of the matrix g_{ab}, which is indeed invertible when the metric is nondegenerate. So for example in the above case the inverse metric g^{ab} is

$$\begin{pmatrix} -1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & K & 0 \\ 0 & 0 & 0 & K \end{pmatrix}$$

where $K = 1/k$. You can think of K as the speed of light in the y and z directions, which is different from the speed of light in the x direction.

Now there are two different limiting cases we can consider, depending on whether we work with the metric or the inverse metric. If we work with the metric, we can let $k = 0$. This corresponds to making the speed of light in the y and z directions *infinite*, so that information can go as fast as it likes in those directions and the lightcone gets completely stretched out in those directions. Note that now the metric g_{ab} is

$$\begin{pmatrix} -1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \end{pmatrix}$$

so the inverse metric doesn't even make sense — you can't invert this matrix. If we extend general relativity to degenerate metrics, we are allowing ourselves to study weird worlds like this. Why we'd want to — well, that's another matter.

If we work with the inverse metric, we can't let $k = 0$, but we can let $K = 0$. This corresponds to making the speed of light in the y and z directions *zero*, so that information can't go at all in those directions: the lightcone is squashed down onto the t-x plane. Now it's the inverse metric that equals

$$\begin{pmatrix} -1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \end{pmatrix}$$

and the metric doesn't even make sense.

Ted Jacobson's talk was about doing general relativity in weird worlds like this, where the inverse metric is degenerate. Here information flows only along surfaces, like the x-t plane in the example above, and these different surfaces don't really talk to each other very much. It's as if the world was split up (or in math jargon, foliated) into lots of different 2-dimensional worlds, which didn't know about each other. Jacobson showed that in this case, the equations of general relativity (extended in a certain way to degenerate inverse metrics) boil down to saying that there are two kinds of massless spin-$1/2$ particle living on all these 2-dimensional worlds.

This got me quite excited because it reminded me of string theory, which is all about massless particles (or in physics jargon, conformal fields) living on the 2-dimensional string worldsheet. I am always hunting around for relationships between string theory and the loop representation of quantum gravity, and I think this is an important clue. The reason is that I think the loop representation can be thought of as a quantum version of the theory of degenerate solutions of general relativity where the metric is *zero* most places and less degenerate (but still degenerate) on certain surfaces. When you slice one of these surfaces with the hyperplane $t = 0$ you get a bunch of loops (or more generally a graph), and these are the loops of the loop representation. Jacobson's talk may give a way to understand the conformal field theory living on these surfaces, which one needs if one wants to think of these surfaces as the "string worldsheets" of string theory fame. Anyway, I am busily thrashing this stuff out and trying to write a paper on it, but it may or may not hang together.

Jacobson's talk is based on a short paper he'd just been editing the galley proofs for; so it should come out soon:

2) Ted Jacobson, "1+1 sector of 3+1 gravity", *Class. Quant. Grav.* **13** (1996), L1–L6.

Now around this time the Erwin Schroedinger Institute, where the workshop was being held, moved from its comfortable old spot on Pasteurgasse to a more spacious location on Boltzmanngasse, near the physics department. (In Germany the word "Gasse" means "alley", and one might find it disrespectful that Pasteur and Boltzmann have mere alleys named after them, but in Vienna even lots of large streets are called "Gasse", when in Germany they'd be called "Strasse". But then even the word for potato is different in Austria; it's all part of the charm of the place.) The move disrupted the schedule of the talks a bit, and it also seems to have disrupted my note-taking, which gets more sketchy from here on out. Some of the dates below might be a bit off.

- **Thursday, July 25th —** I spoke on "Topological Quantum Field Theory". I am always talking about this on This Week's Finds so I won't bore you with the details. Basically I summarized what is known about BF theory (a particular topological quantum field theory) in dimensions 2, 3, and 4, and the discrete formulation of BF theory where you chop spacetime into simplices and label the edges and so on with spins and the like — so-called "state sum models". You can read more about this in "Week 38".

Later that day, Jerzy Lewandowski spoke on "Degenerate Metrics". Being somewhat less degenerate than Ted Jacobson, he spoke about extending general relativity to cases where the inverse metric looks like

$$\begin{pmatrix} -1 & 0 & 0 & 0 \\ 0 & 1 & 0 & 0 \\ 0 & 0 & 1 & 0 \\ 0 & 0 & 0 & 0 \end{pmatrix}$$

In other words, where the speed of light is zero only in the z direction. Basically what happens is that spacetime gets foliated with a lot of 3-dimensional slices, and on each one you get the equations of 3-dimensional general relativity.

- **Friday, July 26th** — Thomas Strobl spoke on 2-dimensional gravity. I don't understand his work well enough yet to have anything much to say, but the most interesting thing about it to *me* is that it allows one to see how quantum groups emerge from the G/G gauged Wess-Zumino-Witten model (a certain 2-dimensional topological quantum field theory), by describing this theory as the quantization of a Poisson σ-model — a field theory where the fields take values in a Poisson manifold. For more, try:

  3) Peter Schaller and Thomas Strobl, A brief introduction to Poisson σ-models, preprint available as hep-th/9507020.

     Peter Schaller and Thomas Strobl, Poisson σ-models: a generalization of 2d gravity-Yang-Mills systems, preprint available as hep-th/9411163.

  Later, I had a great conversation with Mike Reisenberger and Carlo Rovelli on reformulating the loop representation of quantum gravity in terms of surfaces embedded in spacetime. This again touched upon my interest in relating string theory and the loop representation. They are writing a paper on this which should be on the preprint servers pretty soon, so I'll wait until then to talk about it.

- **Saturday, July 27th** — Carlo Rovelli explained some things about the problem of time to me.

- **Monday, July 30th** — I spoke about relative states and entanglement entropy in two-part quantum systems (see "Week 27" and the applications of these ideas to topological quantum field theory and quantum gravity. A lot of this came from my attempts to understand the relation between quantum gravity and Chern-Simons theory, and Lee Smolin's work where he tries to use this relation to derive the Bekenstein bound on the entropy of a system in terms of its surface area (see "Week 56").

  An interesting little fact that I needed to use is that if you have a two-part quantum system in a pure state — a state of zero entropy — the two parts, regarded individually, can themselves have entropy, but the entropies of the two parts are equal. I worked this out using the symmetry of the situation but Walter Thirring, who attended the talk, pointed out that it can also be derived from a wonderful general fact: the triangle inequality! Namely, if your two-part system has entropy S, and the two parts individually have entropies S_1 and S_2, then S can never be less than $|S_1 - S_2|$ or greater than $S_1 + S_2$. (In classical mechanics it's also true that S can never be less than *either* S_1 *or* S_2, but this fails in quantum mechanics, where for example you can have S be zero but $S1 = S2 > 0$.)

- **Wednesday, August 1st —** Full of excitement and new ideas, I somewhat regretfully left the workshop and flew to London. Then I spent most of August working at Imperial College, thanks to a kind offer of office space from Chris Isham. I had some nice talks with Isham and his students on quantum gravity and the decoherent histories approach to quantum mechanics. I'll say a bit about this in a while, but next Week I am going to talk about triality and the secret inner meaning of E_8.

Week 89

September 17, 1996

This week I want to return to the tale of n-categories, from which I have been taking a break during summer vacation. But first, here are a few things about quantum gravity. Last time I mentioned Jorge Pullin's newsletter on general relativity, "Matters of Gravity". I am pleased to report that it is now available on the world-wide web:

1) Jorge Pullin, ed., *Matters of Gravity*, first 8 issues now available at `http://www.phys.lsu.edu//mog`, or latest issue in LaTeX form as gr-qc/9609008.

Anyone who wants to keep up with the latest news on general relativity should certainly read "Matters of Gravity" and MacCallum's list. MacCallum's list? Yes, I should've mentioned it earlier: it's a mailing list where you can find out where the general relativity conferences are, where the postdoctoral positions are, what the latest books are, and so on.

2) MacCallum's gravity mailing list: to subscribe send polite email to `M.A.H.MacCallum@qmw.ac.uk`

By the way, a bunch of math and physics preprints are available from the Schroedinger Institute, including a lot of new stuff on quantum gravity that came out of that workshop I've been talking about:

3) Erwin Schroedinger Institute preprint archive, available at `http://www.esi.ac.at/ESI-Preprints.html`. Recent preprints include:

   Abhay Ashtekar and Alejandro Corichi, "Photon inner-product and the Gauss linking number".

   Abhay Ashtekar, Donald Marolf, Jose Mourao and Thomas Thiemann, "$\mathrm{SU}(N)$ quantum Yang-Mills theory in 2 dimensions: a complete solution".

   Hugo Fort, Rodolfo Gambini and Jorge Pullin, "Lattice knot theory and quantum gravity in the loop representation", also available as gr-qc/9608033.

   Michael Reisenberger, "A left-handed simplicial action for Euclidean GR".

   Carlo Rovelli, "Loop quantum gravity and black hole physics".

I described the ideas behind some of these papers in "Week 85 – "Week 88". I didn't mention the paper by Ashtekar and Corichi. It gives nice formula for the inner product in the Hilbert space for photons in terms of the Gauss linking number — a thing that counts how many times one knot links another.

In its simplest form, the formula goes like this: say you have two knots, and you do a line integral of the electric field around one of them, and of the magnetic field around the other. You get two observables which in the *quantum* theory of electromagnetism do not commute. So the uncertainty principle says you can't measure them both exactly at once. In fact, the uncertainty in one times the uncertainty in the other can't be less than $\hbar/2$ times the absolute value of the Gauss linking number of the two knots! A nice blend of quantum theory and topology! This winds up also being relevant to the photon inner product, because, as the experts out there should know, the canonical commutation relations in a free field theory always come from the imaginary part of the inner product in the single-particle Hilbert space.

In "Week 88" I also mentioned a talk by Jerzy Lewandowski, which has now appeared as a preprint:

4) Jerzy Lewandowski and Jacek Wilsniewski, "2+1 sector of 3+1 gravity", preprint available as gr-qc/9609019.

Also, Lee Smolin has written a paper arguing that Thiemann's work has trouble squaring with the positivity of energy and the existence of long-range correlations (i.e., massless gravitons) that one might expect from semi-classical approaches to quantum gravity.

5) Lee Smolin, "The classical limit and the form of the Hamiltonian constraint in nonperturbative quantum gravity", preprint available as gr-qc/9609034.

This paper has sparked some controversy in the loop representation community. Its arguments are heuristic rather than mathematically rigorous, so one can certainly imagine ways to wriggle out of the conclusions it tries to draw. Nonetheless I think it does a good service by focusing attention on down-to-earth physical issues. If the more mathematically inclined quantum gravity folks are able either to prove *or* refute Smolin's ideas, we'll have made lots of progress.

Smolin has also written a paper relating the loop representation to string theory:

6) Lee Smolin, "Three dimensional strings as collective coordinates of four dimensional quantum gravity", preprint available as gr-qc/9609031.

This paper really freaks me out, because it attempts to relate the loop representation of quantum gravity in 4-dimensional spacetime to string theory in *3-dimensional* spacetime. That's an idea that never would have occurred to me. Smolin suggests it might possibly be related to how supergravity in 11 dimensions is related to string theory in 10 dimensions, but unfortunately I don't know enough about all that to know where to go with it. I need to learn more about this string theory duality stuff — see "Week 72" for my pathetic attempts so far to understand it. I haven't read this yet, but I should:

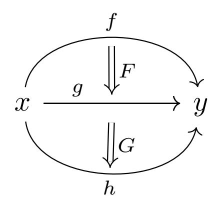
7) Michael Dine, "String theory dualities", preprint available as hep-th/9609051.

It's an expository article.

Okay, now let's go back to the tale of n-categories. As promised, I will tell you all about monads, monoids, monoid objects, and monoidal categories.

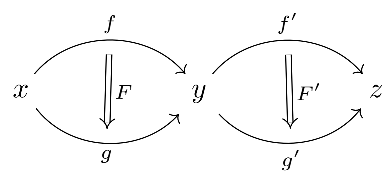
You may or may not remember, but in "Week 80" I explained the idea of a "2-category" pretty precisely. This is a gadget with a bunch of objects, a bunch of morphisms going from one object to another, and a bunch of 2-morphisms going from one morphism to another. We write $f\colon x \to y$ to denote a morphism f from the object x to the object y, and we write $F\colon f \Rightarrow g$ to denote a 2-morphism F from the morphism f to the morphism g.

Just as in a category, in a 2-category we can compose a morphism $f\colon x \to y$ with a morphism $g\colon y \to z$ to get a morphism $fg\colon x \to z$. (Note that I write fg instead of gf; I'm going to use this ordering most of the time, though I may occaisionally change my mind just to confuse you more.) Similarly, we can compose a 2-morphism $F\colon f \Rightarrow g$ with a 2-morphism $G\colon g \Rightarrow h$ to get a 2-morphism $FG\colon f \Rightarrow h$. This is called "vertical composition" of 2-morphisms. We can visualize FG like this:



We stick F on top of G to get FG, which is why it's called "vertical" composition.

Also, if we have morphisms $f, g\colon x \to y$ and $f', g'\colon y \to z$, and 2-morphisms $F\colon f \Rightarrow g$ and $F'\colon f' \Rightarrow g'$, we can "horizontally compose" F and F' to get $F \cdot F'\colon ff' \Rightarrow gg'$. It looks like this:

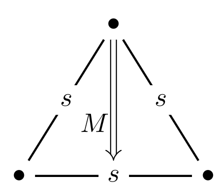


There are some axioms all this stuff has to satisfy, which I described in "Week 80", but I won't repeat them here. The main thing to keep in mind is that a 2-category is like an abstract 2-dimensional world… and the axioms for a 2-category are algebraic distillations of the rules for putting things together in 2 dimensions. In particular, you can put the 2-morphisms together side by side (horizontally) or one on top of the other (vertically), if they fit.

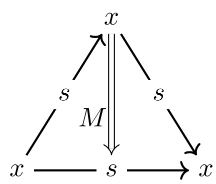
Later I'll say more about what 2-categories have to do with 2-dimensional physics, but right now I want to do something more fundamental. I want to show how all sorts of concepts of "multiplication" or "combination" fit nicely into the framework of 2-categories. The basic idea is really simple: we often think of multiplication as some sort of function

$$M\colon s \times s \to s$$

where we take two elements a and b from some set s, and "multiply" them to get a new one $M(a, b)$. But we can visualize this as follows:



I've drawn a triangular shaped gadget that takes two "inputs" from the two slanted edges labelled s, and spits out one "output" from the horizontal edge labelled s on the bottom. It's clear from the geometry here that M is something 2-dimensional — hence, a 2-morphism — and that s is 1-dimensional — hence, a morphism. Let's label the corners too:



to make it clear that s is a morphism from x to itself. Here x, being 0-dimensional, is an object.

This hocus-pocus may seem mystifying, but if you bear with me and work at it you'll see what I'm up to. I'm saying that essence of "multiplication" can be described very generally in a situation where you have a 2-category with an object x in it, a morphism $s\colon x \to x$, and a 2-morphism $M\colon ss \Rightarrow s$. Often we are interested in situations like this where the "multiplication" M is associative, meaning that the composite

$$sss \xRightarrow{M\cdot 1_s} ss \xRightarrow{M} s$$

equals

$$sss \xRightarrow{1_s\cdot M} ss \xRightarrow{M} s$$

(Here $1_s\colon s \Rightarrow s$ is the identity 2-morphism from s to itself... the axioms for a 2-category say that this exists.) Also, we're often interested in situations where there is a "multiplicative unit", that is, a 2-morphism $I\colon 1_x \to s$ for which

$$s = 1_x s \xRightarrow{I\cdot 1_s} ss \xRightarrow{M} s$$

equals 1_s, and so does

$$s = s1_x \xRightarrow{1_s\cdot I} ss \xRightarrow{M} s$$

If we have a 2-category with stuff in it satisfying these rules, we say we have a "monad" in that 2-category.

What is an example of a monad? Well, consider our original example where s is a set and M is a function. We can think of this as living in a 2-category as follows. Our 2-category will have only one object, x. The morphisms of this 2-category are sets, and composing morphisms corresponds to taking the Cartesian product of sets. The 2-morphisms of this 2-category are functions between sets.

What does a monad amount to in this case? Well, work it out! The multiplicative unit 1_x must corresponds to the one-element set; s is some set; the 2-morphism $I\colon 1_x \Rightarrow s$ is a function from the one-element set to s, which picks out a special *element* of s; the 2-morphism $M\colon ss \Rightarrow s$ is our multiplication operation. The axioms of a monad I gave then say that this multiplication is associative and that the special element of s is the multiplicative unit... that is, it serves as the left and right identity for multiplication.

So we have a set with an associative multiplication and a unit for this multiplication. That's what folks call a "monoid" — see "Week 74" for more on these. So a monoid is a special sort of monad!

The point, however, is that there are lots of other kinds of monads, and this 2-categorical nonsense unifies the study of all of them. Consider, for example, that trick we played of turning the category Set into a 2-category with just one object x. It's a very versatile trick. In general, a 2-category with just one object is called a "monoidal category", because you can do this relabelling trick:

$$\begin{aligned} \text{2-morphisms} &\mapsto \text{morphisms} \\ \text{morphisms} &\mapsto \text{objects} \\ \text{objects} &\mapsto \end{aligned}$$

You take the 2-category with just one object, forget the object, call the morphisms "objects" and the 2-morphisms "morphisms", and you've got a category! But one where you can compose or "multiply" or "tensor" objects, because they were secretly morphisms from x to itself. For example, Set is a monoidal category where we can multiply objects (i.e., sets) with the Cartesian product.

However, there are lots of other interesting monoidal categories. For example, Vect (the category of vector spaces) becomes a monoidal category if we multiply vector spaces by tensoring them. Top (the category of topological spaces) becomes a monoidal category if we multiply spaces by taking their Cartesian product with the usual product topology. Mon (the category of monoids) becomes a monoidal category if we multiply groups by taking their direct product. And so on. . . .

Because a monoidal category is a 2-category with one object, we can talk about monads in any monoidal category. These are usually called "monoid objects", because they are like a monoid living in the category in question. For example, a monoid object in Vect is an associative algebra. A monoid object in Top is a topological monoid.

Sometimes funny things happen: for example, a monoid object in Mon is a commutative monoid! This "birth of commutativity" illustrates something called the "Eckmann-Hilton principle". Some more sophisticated ramifications of this principle are discussed in the following paper:

8) John Baez and Martin Neuchl, "Higher-dimensional algebra I: braided monoidal 2-categories", *Adv. Math.* **121** (1996), 196–244. Also available as `arXiv:q-alg/9511013`.

We can get into some curious self-referential loops, too: the category having (small) categories as objects and functors as morphisms becomes a monoidal category with the "Cartesian product" of categories as the way to multiply objects. . . and a monoid object in this is a (small) monoidal category! Try wrapping your brain around that! A monoid object is something you define in a monoidal category, but a monoidal category is itself a kind of monoid object! This illustrates something that James Dolan and I call it the "microcosm principle". I should note at this point — I should have noted it before — that most of this stuff about category theory is stuff I learned from Dolan. We are writing a paper in which we give a general definition of n-categories, and explain this "microcosm principle".

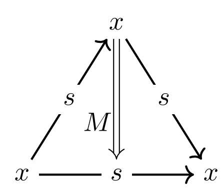
Anyway, some of the most interesting monads live not in monoidal categories but 2-categories with lots of objects. The primordial 2-category is Cat, which has (small) categories as objects, functors as morphisms and *natural transformations* as 2-morphisms.

(A minute ago I gave a way to think of Cat as a monoidal category. That was a bit different than this!) Monads in Cat are the first monads anyone called "monads", I believe. You can read a bunch about them in the bible of category theory:

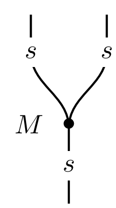
9) *Categories for the Working Mathematician*, by Saunders Mac Lane, Springer, Berlin, 1988.

Believe or not, monads in Cat are nice way to think about *algebraic theories* — a branch of logic perhaps pioneered by the theory of "univeral algebra". (My knowledge of the history here is sort of fuzzy.) It would take me a while to explain this so I'll put it off for next Week.

Let me just wrap up by saying that we can take this picture



and draw a "dual" picture like this:



which illustrates perhaps more vividly how M is the process of two copies of s getting squashed down into one copy. This sort of picture is called a "string diagram", and it's literally the Poincare dual of the earlier picture, meaning that stuff that was k-dimensional is now drawn as $(2 - k)$-dimensional. (The 0-dimensional object x is now the 2-dimensional "background.") For more on string diagrams, see:

10) Ross Street, "Categorical structures", in *Handbook of Algebra*, vol. **1**, ed. M. Hazewinkel, Elsevier, 1996.

This diagram may also remind physicists (if any of them are still reading this) of a Feynman diagram, in particular a 3-gluon vertex in QCD. It's no coincidence! I'll have to say more about that later, though.

To continue reading the "Tale of n-Categories", see "Week 92".

Week 90

September 30, 1996

If you've been following This Week's Finds, you know that I'm in love with symmetry. Lately I've been making up for my misspent youth by trying to learn more about simple Lie groups. They are, roughly speaking, the basic building blocks of the symmetry groups of physics.

In trying to learn about them, certain puzzles come up. In July I asked Bertram Kostant about one that's been bugging me for years: "Why does E_8 exist?" In a word, his answer was: "Triality!" This was incredibly exciting to me; it completely blew my mind. But I should start at the beginning. . . .

In my youth, I found the classification of simple Lie groups to be unintuitive and annoying. I still do, but over the years I've realized that suffering through this classification theorem is the necessary entrance fee to a whole world of symmetry. I gave a tour of this world in "Week 62" – "Week 65", but here I want to make everything as simple as possible, so I won't assume you've read that stuff. Experts should jump directly to the end of this article and read backwards until it becomes boring.

A Lie group is a group that can be given coordinates for which all the group operations are infinitely differentiable. A good example is the group $\mathrm{SO}(n)$ of rotations in n-dimensional Euclidean space. You can multiply rotations by doing first one and then the other, or mathematically by doing matrix multiplication. Every rotation has an inverse, given mathematically by the inverse matrix. Since matrices are just bunches of numbers, you can coordinatize $\mathrm{SO}(n)$, at least locally, and in terms of these coordinates the operations of multiplication and taking inverses are infinitely differentiable, or "smooth", so $\mathrm{SO}(n)$ is a Lie group.

Using the magic of calculus, we can think of tangent vectors at the identity element of $\mathrm{SO}(n)$ as "infinitesimal rotations". So for example, taking $n = 3$, let's start with the rotation by the angle t about the z axis, given by the matrix:

$$\begin{pmatrix} \cos t & -\sin t & 0 \\ \sin t & \cos t & 0 \\ 0 & - & 1 \end{pmatrix}$$

Then we can differentiate this and set $t = 0$ to get an "infinitesimal rotation about the z axis":

$$\begin{pmatrix} 0 & -1 & 0 \\ 1 & 0 & 0 \\ 0 & - & 1 \end{pmatrix}$$

Let's call this J_z, since it's very related to angular momentum about the z axis. (Folks often throw in a factor of $-i$ when they define J_z in quantum mechanics, but let's not bother with that here.)

Similarly we have J_x and J_y. Now rotations about different axes don't commute, so these infinitesimal rotations don't either. In fact, we have

$$J_x J_y - J_y J_x = J_z,$$
$$J_y J_z - J_z J_y = J_x,$$
$$J_z J_x - J_x J_z = J_y.$$

If you have never done it, there are few things in life as rewarding at this point as computing J_x and J_y for yourself and checking the above "commutation relations".

Folks usually write the "commutators" on the left hand side using brackets, like this:

$$[J_x, J_y] = J_z,$$
$$[J_y, J_z] = J_x,$$
$$[J_z, J_x] = J_y.$$

These relations are lurking in the definition of quaternions and also the vector cross product. Quaternions and cross products are good for understanding rotations in 3-dimensional space; they let us describe infinitesimal rotations and their failure to commute. Here we are calling a spade a spade and working directly with the algebra of infinitesimal rotations, which folks call $\mathfrak{so}(3)$. (For related stuff, see "Week 5".)

Okay. The point is, we can do this trick for any Lie group! The space of "infinitesimal group elements", or more precisely tangent vectors at the identity element of a Lie group, is called the "Lie algebra of the group". It's a vector space whose dimension is the dimension of the group, and it always has a bracket operation on it satisfying certain axioms (listed in "Week 3").

The classification of Lie groups can be reduced to the classification of Lie algebras, because the Lie algebra almost determines the Lie group. More precisely, every Lie algebra is the Lie algebra of a unique Lie group that is "simply connected" — i.e., one for which every loop in it can be continuously shrunk to a point. People understand how to get from any Lie group to a simply connected one (called its "universal cover"), so if we understand simply connected Lie groups, we pretty much understand all Lie groups. See "Week 61" for an instance of this philosophy.

Now classifying Lie algebras is just a matter of heavy-duty linear algebra. Let me explain what the "simple" Lie algebras are; you'll have to take my word for it that understanding these is a big step towards understanding all Lie algebras.

At one extreme in the world of Lie groups are the commutative, or "abelian" Lie groups. Here multiplication is commutative, so $[x, y] = 0$ for all x and y in the Lie algebra of the group. At the other extreme are the "semisimple" Lie groups. Here every element in the Lie algebra is of the form $[x, y]$ for some x and y: roughly, if we bracket the whole Lie algebra with itself, we get itself back again. The semisimple Lie algebras turn out to be incredibly important in physics, where they are the typical "gauge groups" of field theories.

The "simple" Lie algebras are the building blocks of the semisimple ones: every semisimple Lie algebra can be broken down into pieces that are simple. (Technically, we say it's a "direct sum" of simple Lie algebras). We say a Lie group is simple if its Lie algebra is simple.

So: what are the simple Lie algebras? They were classified, thanks to some heroic work by Killing and Cartan, in the early part of the 20th century. To keep life simple (ahem) I'll only give the classification of those simple Lie algebras whose corresponding Lie groups are *compact* — meaning roughly that they are finite in size. (For example, $\mathrm{SO}(n)$ is compact.) It turns out that if we understand the compact ones, we can understand the noncompact ones too.

So, here are the Lie algebras of the compact simple Lie groups! There are 4 straightforward infinite families and 5 delightful and puzzling exceptions. The 4 infinite families are easy to understand and are called "classical groups". They are the workhorses of mathematics and physics. The other 5 are called "exceptional groups". They have always seemed very mysterious to me.

The 4 infinite families are:

- A_n: This is the Lie algebra of $\mathrm{SU}(n)$, the group of $n \times n$ complex matrices that preserve lengths (i.e., are unitary) and have determinant 1. This Lie algebra is also called $\mathfrak{su}(n)$.
- B_n: This is the Lie algebra of $\mathrm{SO}(2n+1)$, the group of $(2n+1) \times (2n+1)$ real matrices that preserve lengths (i.e., are orthogonal) and have determinant 1. This Lie algebra is also called $\mathfrak{so}(2n+1)$.
- C_n: This is the Lie algebra of $\mathrm{Sp}(n)$, the group of $n \times n$ quaternionic matrices that preserve lengths. This Lie algebra is also called $\mathfrak{sp}(n)$.
- D_n: This is the Lie algebra of $\mathrm{SO}(2n)$, the group of $2n \times 2n$ real matrices that preserve lengths and have determinant 1. This Lie algebra is also called $\mathfrak{so}(2n)$.

You may justly wonder why the heck they are called A_n, B_n, C_n, and D_n, and why we separated out the even and odd cases of $\mathrm{SO}(n)$ as we did! This is explained in "Week 64", and I don't want to worry about it here. Anyway, glossing over some nuances, we see that these guys are all pretty much just groups of rotations in real, complex, and quaternionic vector spaces.

The 5 exceptions are as follows:

- F_4: A 52-dimensional Lie algebra.
- G_2: A 14-dimensional Lie algebra.
- E_6: A 78-dimensional Lie algebra.
- E_7: A 133-dimensional Lie algebra.
- E_8: A 248-dimensional Lie algebra.

Here I am being rather reticent about what these Lie algebras — or the corresponding Lie groups, which go by the same names — actually ARE! The reason is that it's not so easy to explain. One can certainly describe the exceptional Lie groups as groups of matrices with certain complicated properties, but often this is done in a way that leaves one utterly puzzled as to the real reason why these simple Lie groups exist.

Of course, the answer to "why" a mathematical object exists is a matter of taste. You may feel satisfied if you can easily construct it from other objects you know and love, or you may feel satisfied once it is so tightly woven into your overall scheme of things that you can't imagine life without it.

In any case, I have long been asking people why the exceptional Lie groups exist, but without much luck. Until recently I only felt happy about one of them, the one called G_2: it's the group of rotations of the octonions! The real numbers, complex numbers, quaternions and octonions are the only "normed division algebras" — a property which makes it easy to define rotation groups — but the octonions are weirder than the other three because, unlike the others, they are not associative. (See "Week 59" and "Week 61" for details.) One might expect a series of simple Lie groups coming from rotations in octonionic vector spaces, like the other classical series... but there isn't one! The only simple Lie group like this is the group of rotations of a ONE-dimensional octonionic vector space, G_2. (More precisely, we say that G_2 is the group of automorphisms of the octonions, that is, the linear transformations that preserve the octonion product. These all preserve lengths.)

The idea that the exceptional groups are all related to octonions is sort of pleasing, because one might easily *expect* that the reals, complexes and quaternions give nice infinite series of "classical" Lie groups, while the octonions, being much more bizarre, give only 5 bizarre "exceptional" Lie groups. Indeed, in "Week 64" I described how F_4 and E_6 are related to the octonions... but in a pretty complicated way! As for E_7 and E_8, here until recently I had always been completely in the dark. This is all the more irksome because the biggest, most mysterious exceptional Lie group of all, E_8, plays an important role in string theory!

Luckily, on Thursday July 11th I ran into Bertram Kostant, who had been attending the previous workshop here at the Erwin Schroedinger Institute. As I described in "Week 79", Kostant is one of the expert's experts on group theory. So I got up my nerve and asked him, "Why does E_8 exist?" And he told me! Best of all, he explained both E_8 and F_4 in terms of a principle that I knew was crucial for understanding G_2 and the octonions ... the principle of triality!

I sketched a description of triality in "Week 61". Let me just summarize the idea here. One of the main way to understand Lie algebras is to understand their "representations". A representation of a Lie algebra is simply a function from it to the space of $n \times n$ matrices that preserves the bracket operation. (The $n \times n$ matrices form a Lie algebra with the commutator as the bracket operation.) For example, $\mathfrak{so}(n)$ has a representation where we map each element to an $n \times n$ matrix in the most utterly obvious way: each element IS an $n \times n$ matrix, so don't do anything to it! This is called the "vector" representation, because this is how we do infinitesimal rotations to vectors. But $\mathfrak{so}(n)$ also has representations called "spinor" representations. In physics, the vector representation describes spin-1 particles, while the spinor representations describe spin-$1/2$ particles.

Spinor representations work differently depending on whether the dimension n is even or odd. (This is one reason why people distinguish the even and odd n case of $\mathfrak{so}(n)$ in that classification of simple Lie algebras above!) When n is odd there is one spinor representation. That's why in ordinary 3-dimensional space there is just one kind of spinor to worry about, as you learn when you learn about spin-$1/2$ particles in undergraduate quantum mechanics. When n is even there are two different spinor representations, called the "left-handed" and "right-handed" spinor representations. This shows up when you do quantum mechanics taking special relativity — and 4-dimensional spacetime — into account. For example, the way neutrinos transform under rotations is described by the left-handed spinor representation, while anti-neutrinos are described by right-handed spinors.

When n is even, both the spinor representations of $\mathfrak{so}(n)$ are of dimension $2^{n/2-1}$. That is, they are functions from $\mathfrak{so}(n)$ to the space of $2^{n/2-1} \times 2^{n/2-1}$ matrices. Now something marvelous happens when $n = 8$. Namely, $2^{n/2-1} = n$, so the spinor representations are just as big as the vector representation. This might lead one to hope that in some sense they are "the same" as the vector representation. This is actually true, but in a subtle way. . . . they are not "equivalent" representations in the standard sense of Lie algebra theory, but something sneakier is true.

The Lie algebra $\mathfrak{so}(8)$ has interesting symmetries! It has a little symmetry group with 6 elements, the same as the symmetries of a equilateral triangle, and using these 6 symmetries we can permute the vector, left-handed spinor, and right-handed spinor representations into each other however we please!

For example, one of these symmetries switches the left-handed and right-handed spinor representations, but leaves the vector representation alone. Actually, this symmetry works in any even dimension, not just dimension 8. Its analogue in 4-dimensional spacetime is called "parity", a symmetry that turns left-handed particles into right-handed ones and vice versa. The fact that there are no right-handed neutrinos means that the laws of nature do not actually have this symmetry. . . but it's still very important in math and physics.

What's special about dimension 8 is that there are symmetries switching the vector representation and the spinor representations. For example: if we take an element x of $\mathfrak{so}(8)$, apply the right symmetry of $\mathfrak{so}(8)$ to turn it into another element of $\mathfrak{so}(8)$, and then use the right-handed spinor representation to it to turn it into a matrix, we get the same thing as if we just used the vector representation to turn x into a matrix.

Now $\mathfrak{so}(8)$ is the Lie algebra of the Lie group $\mathrm{SO}(8)$, but $\mathrm{SO}(8)$ is not "simply connected" in the sense defined above. The simply connected group whose Lie algebra is $\mathrm{SO}(n)$ is called $\mathrm{Spin}(n)$. I gave an introduction to these "spin groups" in "Week 61", and I don't want to say much about them here, except for this: the triality symmetries of $\mathfrak{so}(8)$ do not give symmetries of $\mathrm{SO}(8)$, but they do give symmetries of $\mathrm{Spin}(8)$. Experts say the group of outer automorphisms modulo inner automorphisms of $\mathrm{SO}(8)$ is S_3 (the group of permutations of 3 things).

Pretty sneaky, how a group of symmetries can have its own group of symmetries, no? As we'll now see, this is what gives birth to G_2, F_4, E_8, and the octonions.

To get G_2 is pretty simple; we look at those elements of $\mathrm{Spin}(8)$ that are fixed (i.e., unaffected) by all the triality symmetries, and these form a subgroup, which is G_2.

For the rest, we need one more fact: there is a way to "multiply" a left-handed spinor and a right-handed spinor and get a vector. This is true in all even dimensions, not just $n = 8$, so in particular it is familiar to particle theorists who live in 4-dimensional spacetime. As I noted, what happens to a neutrino when you rotate (or Lorentz transform) it is described using left-handed spinors, while anti-neutrinos are described by right-handed spinors. Similarly, photons are described by vectors. So as far as *rotational* properties go, one could think of a photon as a bound state of a neutrino and an antineutrino. This led Schroedinger (or someone) to propose at one point that photons were actually neutrino-antineutrino pairs. Subsequent experiments showed this theory has lots of problems, and nobody sane believes it any more. Still, it's sort of cute.

Now, in 8 dimensions, it shouldn't be surprising that we can also multiply a left-handed spinor and a vector to get a right-handed spinor, and so on. The point is, you can just use triality to permute the three representations whichever way you please. . . they are not really all that different.

So in particular, you can multiply two 8-dimensional vectors and get another vector. And this gives us the octonions!

Now how about F_4 and E_8? This is the cool stuff Kostant told me about. Here I will describe the Lie algebras, not the Lie groups.

Let's call the right-handed and left-handed spinor representations S_+ and S_-, respectively. (Us left-handers are always getting shafted, being "sinister" rather than "dextrous" and all that, so we get S_- rather than S_+.) And let's call the vector representation V. And let's be sloppy, the way people usually are, and also use these letters to stand for the 8-dimensional vector spaces on which $\mathfrak{so}(8)$ acts as transformations.

Now let's form the direct sum of vector spaces

$$\mathfrak{so}(8) \oplus S_+ \oplus S_- \oplus V$$

A vector in this vector space is just a list consisting of a guy in $\mathfrak{so}(8)$, a guy in S_+, a guy in S_-, and a guy in $V. The dimension of this vector space is therefore

$$28 + 8 + 8 + 8 = 52$$

since it takes $n(n-1)/2$ numbers to describe a rotation in n dimensions. Hey! Look! 52 is the dimension of F_4! So maybe this thing is F_4.

Yes, it is! Here's how it works. To make this gadget into a Lie algebra — which turns out to be F_4 — we need a way to take the "bracket" of any two elements in it. We already know how to take the bracket of two guys in $\mathfrak{so}(8)$, so that's no problem. Since $\mathfrak{so}(8)$ acts as transformations of S_+ and S_- and V, we also know how to multiply a guy in $\mathfrak{so}(8)$ by one of these other guys. We also know how to multiply a guy in S_+ by a guy in S_- to get a guy in V, and so on. Finally, we can multiply two guys in V to get a guy in $\mathfrak{so}(8)$ as follows: two vectors determine an infinitesimal rotation which starts rotating the first vector in the direction of the second. (More technically, we say that $\mathfrak{so}(8)$ is isomorphic to the second exterior power of V, so we can multiply two guys in V to get a guy in $\mathfrak{so}(8)$ using the wedge product.) Using triality, we can equally well multiply two guys in S_+ to get a guy in $\mathfrak{so}(8)$, or multiply two guys in S_- to get a guy in $\mathfrak{so}(8)$.

So taking all these multiplication operations together, we can cook up a way to take the bracket of any two guys in $\mathfrak{so}(8) \oplus S_+ \oplus S_- \oplus V$ and get another such guy. If you do it right — I've been pretty vague, so I leave it to you to fill in the details — you can get this bracket to satisfy the Lie algebra axioms, and you get F_4!

Emboldened with our success, we now look at the vector space

$$\mathfrak{so}(8) \oplus \mathfrak{so}(8) \oplus \mathrm{End}(S_+) \oplus \mathrm{End}(S_-) \oplus \mathrm{End}(V).$$

Here $\mathrm{End}(S_+)$ is the space of all linear transformations of the vector space S_+, so if you like, it's just the space of 8×8 matrices. Similarly for $\mathrm{End}(S_-)$ and $\mathrm{End}(V)$. Now the dimension of this space is

$$28 + 28 + 64 + 64 + 64 = 248$$

Hey! This is just the dimension of E_8! Maybe this space is E_8!

Yes indeed. Again, you can cook up a bracket operation on this space using all the stuff we've got. Here's the basic idea. $\mathrm{End}(S_+)$, $\mathrm{End}(S_-)$, and $\mathrm{End}(V)$ are already Lie algebras, where the bracket of two guys x and y is just the commutator $[x, y] = xy - yx$, where we multiply using matrix multiplication. Since $\mathfrak{so}(8)$ has a representation as linear transformations of V, it has two representations on $\mathrm{End}(V)$, corresponding to left and right matrix multiplication; glomming these two together we get a representation of $\mathfrak{so}(8) \oplus \mathfrak{so}(8)$ on $\mathrm{End}(V)$. Similarly we have representations of $\mathfrak{so}(8) \oplus \mathfrak{so}(8)$ on $\mathrm{End}(S_+)$

and $\mathrm{End}(S_-)$. Putting all this stuff together we get a Lie algebra, if we do it right — and it's E_8. At least that's what Kostant said; I haven't checked it.

So now we see, at least roughly, how triality gives birth to the octonions, G_2, F_4, and E_8. That leaves E_8's "little brothers" E_6 and E_7. These are contained in E_8 as Lie subalgebras, but apart from that I don't know any especially beautiful way to get ahold of them, except for the way to get E_6 from 3x3 matrices of octonions, which I described in "Week 64".

For some references to this stuff, try:

1) Claude C. Chevalley, *The algebraic theory of spinors*, Columbia University Press, New York, 1954.

2) F. Reese Harvey, *Spinors and calibrations*, Perspectives in Mathematics, **9**, Academic Press, Inc., Boston, MA, 1990.

3) Ian R. Porteous, *Topological geometry*, 2nd ed., Cambridge University Press, Cambridge, 1981.

4) Ian R. Porteous, *Clifford algebras and the classical groups*, Cambridge University Press, Cambridge, 1995.

5) Hans Freudenthal and H. de Vries, *Linear Lie groups*, Academic Press, New York, 1969.

6) Alex J. Feingold, Igor B. Frenkel, and John F. X. Rees, "Spinor construction of vertex operator algebras", triality, and $\mathrm{E}_8^{(1)}$, *Contemp. Math.* **121**, AMS, Providence Rhode Island.

I haven't looked at all these books lately, and the only source I *know* contains the above construction of E_8 from triality is the last one, by Feingold, Frenkel, and Rees.

Now let me allow myself to get a bit more technical.

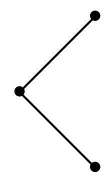
I am still not entirely happy, by any means, because what I'd really like would be a simple explanation of why these exceptional simple Lie algebras arise from triality, *and no others*. In other words, I'd like a classification of the simple Lie algebras that proceeded not by the usual exhaustive (and exhausting) case-by-case study of Dynkin diagrams, but by some less combinatorial and more "synthetic" approach. For example, it would be nice to really see a good explanation of how the reals, the complexes, the quaternions and octonions each give rise to a family of simple Lie algebras, and one gets *all* of them this way.

On the other hand, don't think I'm knocking the Dynkin diagram stuff. As I explained in "Week 62" – "Week 64", what's really fundamental to the Dynkin diagram approach seems to be the not the Lie algebras themselves but their root lattices. Taking lattices as fundamental to the study of symmetry *does* seem to be a good idea, since it gets you to not just the simple Lie algebras described above, but also the "Kac-Moody algebras" so important in string theory and other forms of 2-dimensional physics, as well as marvelous things like the Leech lattice and the Monster group.

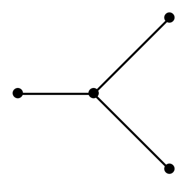
The Dynkin diagram approach also makes it clear *why* triality exists: symmetries of Dynkin diagrams always give outer automorphisms of the corresponding Lie algebras, and as you examine the Dynkin diagrams of D_n, you get

$\mathrm{D}_2 = \mathfrak{so}(4) \ =$

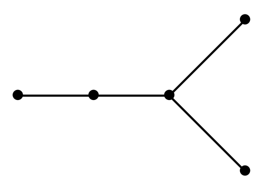
$\mathrm{D}_3 = \mathfrak{so}(6) \ =$



$\mathrm{D}_4 = \mathfrak{so}(8) \ =$



$\mathrm{D}_6 = \mathfrak{so}(10) \ =$



and you can just *see* how when you get to $\mathfrak{so}(8)$ there is that amazing triality symmetry, flashing briefly into being before reverting to the boring old duality symmetry which only interchanges the left-handed and right-handed spinor representations, corresponding to the two dots on the far right of the Dynkin diagram. (The dot on the far left corresponds to the vector representation.)

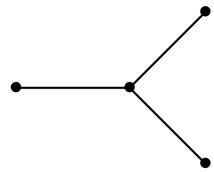
Of course, people don't usually talk about D_2 or D_3, because D_2 is two copies of A_1, and D_3 is the same as A_3. However, there is no shame in doing so, and indeed a lot of insight to be gained: the fact that D_2 consists of two copies of A_1 corresponds to the isomorphism

$$\mathfrak{so}(4) = \mathfrak{su}(2) \oplus \mathfrak{su}(2),$$

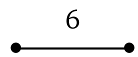
while the fact that D_3 is the same as A_3 corresponds to the isomorphism

$$\mathfrak{so}(6) = \mathfrak{su}(4).$$

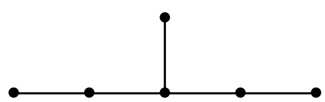
Each of these could easily serve as the springboard for a very long and interesting discussion. However, I will refrain. Here let me simply note that you can always "fold" a Dynkin diagram using one of its symmetries, and if you do this to D_4 using triality you go from

$D_4 \quad =$ 

down to

$G_2 \quad =$ 

(Here the number 6 means that the two roots are at an angle of $\pi/6$ from each other. People usually just draw a triple line to indicate this. The arrow points from the long root to the shorter root.) This corresponds to how G_2 is the subgroup of $\mathrm{Spin}(8)$ consisting of elements that are invariant under triality. You can also go from

$E_6 \quad =$ 

down to

$F_4 \quad =$

by folding along the reflection symmetry. And Friedrich Knop told me a neat way to get triality symmetry *from* F_4, if you happen to have F_4 around: the long roots of F_4 form a root system of type D_4, which defines an embedding of $\mathrm{Spin}(8)$ into the Lie group F_4 (more precisely, the compact real form). On the other hand, the two short simple roots define an embedding of $\mathrm{SU}(3)$ in F_4. The Weyl group of $\mathrm{SU}(3)$ is S_3 and can be lifted to $\mathrm{SU}(3)$, so we have an S_3 subgroup of F_4. This acts by conjutation on the $\mathrm{Spin}(8)$ subgroup, implementing the triality symmetries!

But I digress. My main point is, the Dynkin diagram symmetries do give a nice way to understand outer automorphisms of simple Lie groups, and these provide some important ties between simple Lie algebras, including triality, which links the "classical" world to the "exceptional" world. But it is also nice to try to understand these in a somewhat more "conceptual" way. This is one of the reasons I'm interested in 2-Hilbert spaces... they seem to help one understand this stuff from a new angle. But more on those, later. They tie into the n-category stuff I'm always talking about. I will return to that tale soon, and I'll keep building up some of the tools we need, until we are ready to launch into a description of 2-Hilbert spaces.

In writing this Week's Finds, I benefitted greatly from email correspondence with Robt Bryant, Christopher Henrich, Geoffrey Mess, Friedrich Knop, and others.

Week 91

October 6, 1996

For a while now I've been meaning to finish talking about monads and adjunctions, and explain what that has to do with the 4-color theorem. But first I want to say a little bit more about "triality", which was the subject of "Week 90".

Triality is a cool symmetry of the infinitesimal rotations in 8-dimensional space. It was only last night, however, that I figured out what triality has to do with 3 dimensions. Since it's all about the number *three* obviously triality should originate in the symmetries of *three*-dimensional space, right? Well, maybe it's not so obvious, but it does. Here's how.

Take good old three-dimensional Euclidean space with its usual basis of unit vectors i, j, and k. Look at the group of all permutations of $\{i, j, k\}$. This is a little 6-element group which people usually call S_3, the "symmetric group on 3 letters".

Every permutation of $\{i, j, k\}$ defines a linear transformation of three-dimensional Euclidean space in an obvious way. For example the permutation p with

$$p(i) = j, \quad p(j) = k, \quad p(k) = i$$

determines a linear transformation, which we'll also call p, with

$$p(ai + bj + ck) = aj + bk + ci.$$

In general, the linear transformations we get this way either preserve the cross product, or switch its sign. If p is an even permutation we'll get

$$p(v) \times p(w) = p(v \times w)$$

while if p is odd we'll get

$$p(v) \times p(w) = -p(v \times w) = p(w \times v).$$

That's where triality comes from. But now let's see what it has to do with *four*-dimensional space. We can describe four-dimensional space using the quaternions. A typical quaternion is something like

$$a + bi + cj + dk$$

where a, b, c, d are real numbers, and you multiply quaternions by using the usual rules together with the rules

$$i^2 = j^2 = k^2 = -1$$
$$ij = k, \quad jk = i, \quad ki = j,$$
$$ji = -k, \quad kj = -i, \quad ik = -j.$$

Now, any permutation p of $\{i, j, k\}$ also determines a linear transformation of the quaternions, which we'll also call p. For example, the permutation p I gave above has

$$p(a + bi + cj + dk) = a + bj + ck + di.$$

The quaternion product is related to the vector cross product, and so one can check that for any quaternions q and q' we get

$$p(qq') = p(q)p(q')$$

if p is even, and

$$p(q'q) = p(q')p(q)$$

if p is odd. So we are getting triality to act as some sort of symmetries of the quaternions.

Now sitting inside the quaternions there is a nice lattice called the "Hurwitz integral quaternions". It consists of the quaternions $a + bi + cj + dk$ for which either a, b, c, d are all integers, or all half-integers. Here I'm using physics jargon, and referring to any number that's an integer plus $1/2$ as a "half-integer". A half-integer is *not* any number that's half an integer!

You can think of this lattice as the 4-dimensional version of all the black squares on a checkerboard. One neat thing is that if you multiply any two guys in this lattice you get another guy in this lattice, so we have a "subring" of the quaternions. Another neat thing is that if you apply any permutation of $\{i, j, k\}$ to a guy in this lattice, you get another guy in this lattice — this is easy to see. So we are getting triality to act as some sort of symmetries of this lattice. And *that* is what people *usually* call triality.

Let me explain, but now let me use a lot of jargon. (Having shown it's all very simple, I now want to relate it to the complicated stuff people usually talk about. Skip this if you don't like jargon.) We saw how to get S_3 to act as automorphisms and antiautomorphisms of $\mathbb{R}^3$ with its usual vector cross product… or alternatively, as automorphisms and antiautomorphisms of the Lie algebra $\mathfrak{so}(3)$. From that we got an action as automorphisms and antiautomorphisms of the quaternions and the Hurwitz integral quaternions. But the Hurwitz integral quaternions are just a differently coordinatized version of the 4-dimensional lattice D_4! So we have gotten triality to act as symmetries of the D_4 lattice, and hence as automorphisms of the Lie algebra D_4, or in other words $\mathfrak{so}(8)$, the Lie algebra of infinitesimal rotations in 8 dimensions. (For more on the D_4 lattice see "Week 65", where I describe it using different, more traditional coordinates.)

Actually I didn't invent all this stuff, I sort of dug it out of the literature, in particular:

1) John H. Conway and Neil J. A. Sloane, *Sphere Packings, Lattices and Groups*, second edition, Grundlehren der mathematischen Wissenschaften **290**, Springer-Verlag, 1993.

and

2) Frank D. (Tony) Smith, "Sets and C^n; quivers and A-D-E; triality; generalized supersymmetry; and D_4-D_5-E$_6$", preprint available as hep-th/9306011.

But I've never quite seen anyone come right out and admit that triality arises from the permutations of the unit vectors i, j, and k in 3d Euclidean space.

I should add that Tony Smith has a bunch of far-out stuff about quaternions, octonions, Clifford algebras, triality, the D_4 lattice — you name it! — on his home page:

3) Tony Smith's home page, http://valdostamuseum.org/hamsmith/

He engages in more free association than is normally deemed proper in scientific literature — you may raise your eyebrows at sentences like "the Tarot shows the Lie algebra structure of the D_4-D_5-E_6 model, while the I Ching shows its Clifford algebra structure" — but don't be fooled; his mathematics is solid. When it comes to the physics, I'm not sure I buy his theory of everything, but that's not unusual: I don't think I buy *anyone's* theory of everything!

Let me wrap up by passing on something he told me about triality and the exceptional groups. In "Week 90" I described how you could get the Lie groups G_2, F_4 and E_8 from triality. I didn't know how E_6 and E_7 fit into the picture. He emailed me, saying:

> *"Here is a nice way: Start with $D_4 = \mathrm{Spin}(8)$:*
>
> $$28 = 28 + 0 + 0 + 0 + 0 + 0 + 0$$
>
> *Add spinors and vector to get F_4:*
>
> $$52 = 28 + 8 + 8 + 8 + 0 + 0 + 0$$
>
> *Now, "complexify" the $8 + 8 + 8$ part of F_4 to get E_6:*
>
> $$78 = 28 + 16 + 16 + 16 + 1 + 0 + 1$$
>
> *Then, "quaternionify" the $8 + 8 + 8$ part of F_4 to get E_7:*
>
> $$133 = 28 + 32 + 32 + 32 + 3 + 3 + 3$$
>
> *Finally, "octonionify" the $8 + 8 + 8$ part of F_4 to get E_8:*
>
> $$248 = 28 + 64 + 64 + 64 + 7 + 14 + 7$$
>
> *This way shows you that the "second" $\mathrm{Spin}(8)$ in E_8 breaks down as $28 = 7 + 14 + 7$ which is globally like two 7-spheres and a G_2, one S_7 for left-action, one for right-action, and a G_2 automorphism group of octonions that is needed to for "compatibility" of the two S_7s. The $3+3+3$ of E_7, the $1+0+1$ of E_6, and the $0 + 0 + 0$ of F_4 and D_4 are the quaternionic, complex, and real analogues of the $7 + 14 + 7$."*

When I asked him where he got this, he said he cooked it up himself using the construction of E_8 that I learned from Kostant together with the Freudenthal-Tits magic square. He gave some references for the latter:

4) Hans Freudenthal, *Adv. Math.* **1** (1964) 143.

5) Jacques Tits, *Indag. Math.* **28** (1966) 223–237.

6) Kevin McCrimmon, "Jordan Algebras and their applications", *Bull. AMS* **84** (1978) 612–627, at pp. 620-621. Available at http://projecteuclid.org

I would describe it here, but I'm running out of steam, and it's easy to learn about it from his web page:

7) Tony Smith, Freudenthal-Tits magic square, `http://valdostamuseum.org/hamsmith/FTsquare.html`

"I regret that it has been necessary for me in this lecture to administer such a large dose of four-dimensional geometry. I do not apologise, because I am not really responsible for the fact that nature in its most fundamental aspect is four-dimensional"

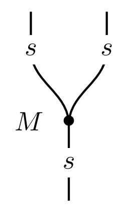
— *Albert North Whitehead.*

Week 92

October 17, 1996

I'm sure most of you have lost interest in my "tale of n-categories", because it takes a fair amount of work to keep up with all the abstract concepts involved. However, we are now at a point where we can have some fun with what we've got, even if you haven't really followed all the previous stuff. So what follows is a rambling tour through monads, adjunctions, the 4-color theorem and the large-N limit of $\mathrm{SU}(N)$ gauge theory. . . .

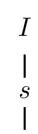
Okay, so in "Week 89" we defined a gadget called a "monad". Using the string diagrams we talked about, you can think of a monad as involving a process like this:



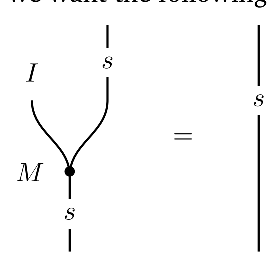
which we read downwards as describing the "fusion" of two copies of something called s into one copy of the same thing s. The fusion process itself is called M.

I can hear you wonder, what exactly *is* this thing s? What *is* this process M? Well, I gave the technical answer in "Week 89" — but the point is that n-category theory is deliberately designed to be so general that it covers pretty much anything you could want! For example, s could be the set of real numbers and M could be multiplication of real numbers, which is a function from $s \times s$ to s. Or we could be doing topology in the plane, in which case the picture above stands for exactly what it looks like: two lines merging to form one line! These and many other situations are analogous, and the formalism allows us to treat them all at once. Here I will not review all the rules of the game. If you just play along and trust me everything will be all right. If you don't trust me, go back and check the definitions.

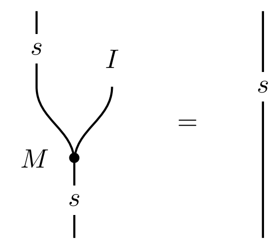
Let me turn to the axioms for a monad. In addition to the multiplication M we want to have a "multiplicative identity", I, looking like this:



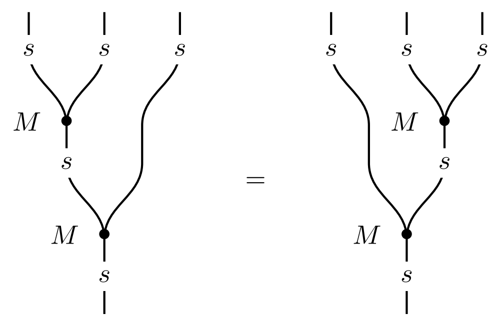
Here nothing is coming in, and a copy of s is going out. Because ordinary multiplication has $1x = x$ and $x1 = x$ for all x, we want the following axioms to hold:



and



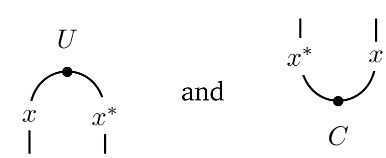
Also, since ordinary multiplication has $(xy)z = x(yz)$, we want the following associativity law to hold, too:



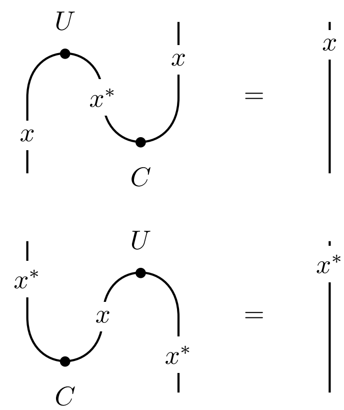
These rules are a translation of the rules given in "Week 89" into string diagram form.

If you are a physicist, you can think of these diagrams as being funny Feynman diagrams where you've got some kind of particle s and two processes M and I. Then M is a bit like what you'd call a "cubic self-interaction", where two particles combine to form a third. These interactions show up in simple textbook theories like the "φ^3 theory" and, more importantly, in nonabelian gauge field theories like quantum chromodynamics, where the gauge bosons have cubic self-interactions. On the other hand, I is a bit like what you'd usually call a "source" or an "external potential", some sort of field imposed from outside that can create particles of type s. You shouldn't take the analogy with Feynman diagrams too seriously yet, because the context we're working in is so general, and the most interesting physics theories don't correspond to monads but to more elaborate setups. However, we could flesh out the analogy to make it very precise and accurate if we wanted, and this is especially important in topological quantum field theory. More later about that.

Now in "Week 83" I discussed a different sort of gadget, called an "adjunction". Here you have two guys x and x^*, and two processes U and C called the "unit" and "counit", which look like this:

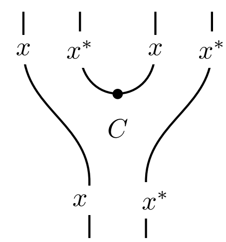


They satisfy the following axioms:



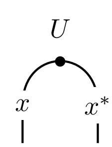
Physically, we can think of x^* as the antiparticle of x, and then U is the process of creation of a particle-antiparticle pair, while C is the process of annihilation. The axioms just say that for a particle or antiparticle to "double back in time" by means of these processes isn't really different than for it to march obediently along forwards. Mathematically, one nice example of an adjunction involves a vector space x and its dual vector space x^*. This is really the same example, since if the behavior of a particle under symmetry transformations is described by some group representation, its antiparticle is described by the dual representation. For more details on the math, see "Week 83".

Now, let's see how to get a monad from an adjunction! We need to get s, M, and I from x, x^*, U, and C. To do this, we first define s to be xx^*. Then define M to be



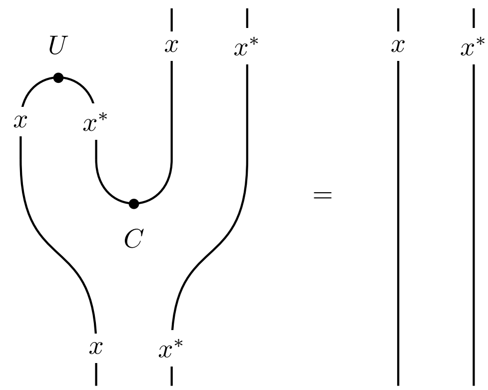
Again, to really understand the rules of the game you need to learn a bit about string diagrams and 2-categories, but the basic idea is supposed to be simple: we can get two xx^*'s to turn into one xx^* by letting an x^* and x annihilate each other!

Finally, we define I to be

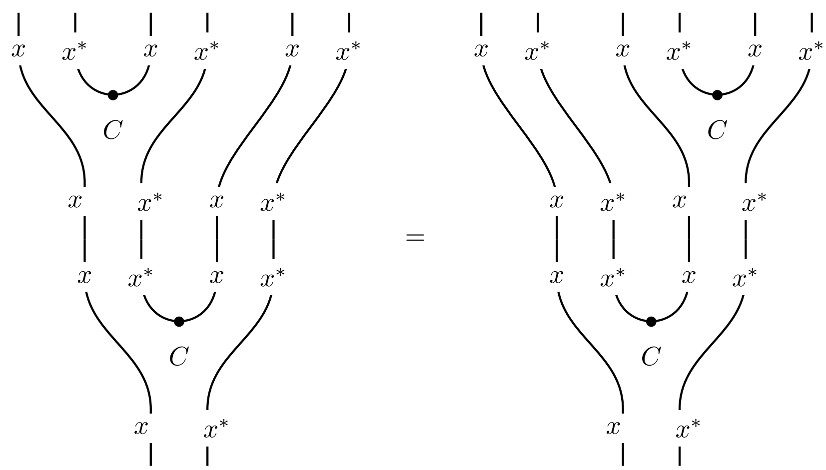


In other words, an xx^* can be created out of nothing since it's a "particle/antiparticle pair".

Now one can check that all the axioms for a monad hold. You really need to know a bit about 2-categories to do it carefully, but basically you just let yourself deform the pictures, in part with the help of the axioms for an adjunction, which let you straighten out curves that "double back in time." So for example, we can prove the identity law



by canceling the U and the C on the left using one of the axioms for an adjunction. Similarly, associativity holds because the following two pictures are topologically the same:



Whew! Drawing these is tough work.

Now, as I said, an example of an adjunction is a vector space x and its dual x^*. What monad do we get in this case? Well, the vector space x tensored with x^* is just the vector space of linear transformations of x, so that's our monad in this case. In less high-brow terms, we've proven that matrices form an algebra when you define matrix multiplication in the usual way! In particular, the above picture serves as a diagrammatic proof that matrix multiplication is associative.

Of course, people didn't invent all this fancy-looking (but actually very basic) stuff just to deal with matrix multiplication! Or did they? Well, actually, Penrose *did* invent a diagrammatic notation for tensors which is just a slightly souped-up version of the above stuff. You can find it in:

1) "Applications of negative dimensional tensors", by Roger Penrose, in *Combinatorial Mathematics and its Applications*, ed. D. J. A. Welsh, Academic Press, 1971.

But most of the work on this sort of thing has been aimed at applications of other sorts.

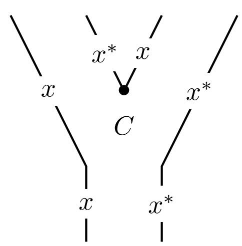
Now let me drift over to a related subject, the large-N limit of $\mathrm{SU}(N)$ gauge theory. Quantum chromodynamics, or QCD, is an $\mathrm{SU}(N)$ gauge theory with $N = 3$, but it turns out that things simplify a lot in the limit as $N \to \infty$, and one gets some nice qualitative insight into the strong force by considering this simplified theory. One can even treat the number 3 as a small perturbation around the number ∞ and get some decent answers! A good introduction to this appears in Coleman's delightful book, essential reading for anyone learning particle physics:

2) Sidney Coleman, *Aspects of Symmetry*, Cambridge University Press, Cambrdige, 1989.

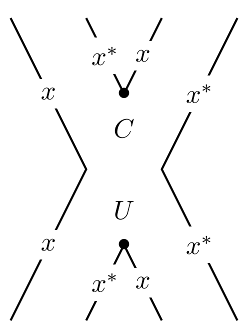
Check out section 8.3.1, entitled "the double line representation and the dominance of planar graphs". Coleman considers Yang-Mills theories, like QCD, but many of the same ideas apply to other gauge theories.

The idea is that if we start out studying the Feynman diagrams for a gauge field theory with gauge group $\mathrm{SU}(N)$, and see how much various diagrams contribute to any process for large N, the diagrams that contribute the most are those that can be drawn on a plane without any lines crossing. Technically, the reason is that diagrams that can only be drawn on a surface of genus g grow like N^{2-2g} as N increases. This number $2 - 2g$ is called the Euler characteristic and it's biggest when your surface has no handles.

Even better, in the $N \to \infty$ limit we can think of the Feynman diagrams using diagrams like the ones above. For example, we can think of the cubic self-interaction in Yang-Mills theory as simply matrix multiplication:



and the quartic self-interaction as something a wee bit fancier:



Apparently these ideas have spawned a whole field of physics called "matrix models".

These ideas work not only for Yang-Mills theory but also for Chern-Simons theory, which is a topological quantum field theory: a theory that doesn't require any metric on spacetime to make sense. Here they have been exploited by Dror Bar-Natan to come up with a new formulation of the famous 4-color theorem:

3) Dror Bar-Natan, "Lie algebras and the four color theorem", preprint available as q-alg/9606016.

As I explained in "Week 8" and "Week 22", there is a way to formulate about the 4-color theorem as a statement about trivalent graphs. In particular, Penrose invented a little recipe that lets us calculate an invariant of trivalent graphs, which is zero for some *planar* graph only if some corresponding map can't be 4-colored. This recipe involves the vector cross product, or equivalently, the Lie algebra of the group $\mathrm{SU}(2)$. You can generalize it to work for $\mathrm{SU}(N)$. And if you then consider the $N \to \infty$ limit, you get the above stuff! (The point is that the above stuff also gives a rule for computing a number from any trivalent graph.)

Now as I said, in the $N \to \infty$ limit all the nonplanar Feynman diagrams give negligible results compared to the planar ones. So another way to state the 4-color theorem is this: if the $\mathrm{SU}(2)$ invariant of a trivalent graph is zero, the $\mathrm{SU}(N)$ invariant is negligible in the $N \to \infty$ limit.

This doesn't yet give a new proof of the 4-color theorem. But it makes it into sort of a *physics* problem: a problem about the relation of $\mathrm{SU}(2)$ Chern-Simons theory and the $N \to \infty$ limit of Chern-Simons theory.

Now, the 4-color theorem is one of the two deep mysteries of 2-dimensional topology — a subject too often considered trivial. The other mystery is the Andrews-Curtis conjecture, discussed in "Week 23". Often a problem is hard or unsolvable until you get the right tools. Topological quantum field theory is a new tool in topology, so one could hope it'll shed some light on these problems. Bar-Natan's paper is a tantalizing piece of evidence that maybe, just maybe, it will.

One can't really tell yet.

Anyway, I don't really care much about the 4-color theorem per se. If I ever need to color a map I'll hire a cartographer. It's the connections between seemingly disparate subjects that I find interesting. 2-categories are a very abstract formalism developed to describe 2-dimensional ways of glomming things together. Starting from the study of 2-categories, we very naturally get the notions of "monad" and "adjunction". And before we know it, this leads us to some interesting questions about 2-dimensional quantum field theory: for really, the dominance of planar diagrams in the $N \to \infty$ limit of gauge theory is saying that in this limit the theory becomes essentially a 2-dimensional field theory, in some funny sense. And then, lo and behold, this turns out to be related to the 4-color theorem!

By the way, I guess you all know that the 4-color theorem was proved using a computer, by breaking things down into lots of separate cases. (See "Week 22" for references.) Well, there's a new proof out, which also uses a computer, but is supposed to be simpler:

4) Neil Robertson, Daniel P. Sanders, Paul Seymour, and Robin Thomas, "A new proof of the four-colour theorem", *Electronic Research Announcements of the Amer-*

ican Mathematical Society **2** (1996), 17–25. Available at `http://www.ams.org/journals/era/1996-02-01/`

I'm still hoping for the 2-page "physicist's proof" using path integrals!

To continue reading the "Tale of n-Categories", see "Week 99".

For more on adjunctions and monoid objects, try "Week 173" and especially "Week 174".

Week 93

October 27, 1996

Lately I've been trying to learn more about string theory. I've always had grave doubts about string theory, but it seems worth knowing about. As usual, when I'm trying to learn something I find it helpful to write about it — it helps me remember stuff, and it points out gaps in my understanding. So I'll start trying to explain some string theory in this and forthcoming Week's Finds.

However: watch out! This isn't going to be a systematic introduction to the subject. First of all, I don't know enough to do that. Secondly, it will be very quirky and idiosyncratic, because the aspects of string theory I'm interested in now aren't necessarily the ones most string theorists would consider central. I've been taking as my theme of departure, "What's so great about 10 and 26 dimensions?" When one reads about string theory, one often hears that it only works in 10 or 26 dimensions — and the obvious question is, why?

This question leads one down strange roads, and one runs into lots of surprising coincidences, and spooky things that sound like coindences but might NOT be coincidences if we understood them better.

For example, when we have a string in 26 dimensions we can think of it as wiggling around in the 24 directions perpendicular to the 2-dimensional surface the string traces out in spacetime (the "string worldsheet"). So the number 24 plays an especially important role in 26-dimensional string theory. It turns out that

$$1^2 + 2^2 + 3^2 + \ldots + 24^2 = 70^2.$$

In fact, 24 is the *only* integer $n > 1$ such that the sum of squares from 1^2 to n^2 is itself a perfect square. Is this a coincidence? Probably not, as I'll eventually explain! This is just one of many eerie facts one meets when trying to understand this stuff.

For starters I just want to explain why dimensions of the form $8k + 2$ are special. Notice that if we take $k = 0$ here we get 2, the dimension of the string worldsheet. For $k = 1$ we get 10, the dimension of spacetime in "supersymmetric string theory". For $k = 3$ we get 26, the dimension of spacetime in "purely bosonic string theory". So these dimensions are important. What about $k = 2$ and the dimension 18, I hear you ask? Well, I don't know what happens there yet... maybe someone can tell me! All I want to do now is to explain what's good about $8k + 2$.

But I need to start by saying a bit about fermions.

Remember that in the Standard Model of particle physics — the model that all fancier theories are trying to outdo — elementary particles come in 3 basic kinds. There are the basic fermions. In general a "fermion" is a particle whose angular momentum comes in units of Planck's constant $\hbar$ times $1/2$, $3/2$, $5/2$, and so on. Fermions satisfy the Pauli exclusion principle — you can't put two identical fermions in the same state. That's why we have chemistry: the electrons stack up in "shells" at different energy levels, instead of all going to the lowest-energy state, because they are fermions and satisfy the exclusion principle. In the Standard Model the fermions go like this:

Leptons		**Quarks**	
electron	electron neutrino	down quark	up quark
muon	muon neutrino	strange quark	charm quark
tauon	tauon neutrino	bottom quark	top quark

There are three "generations" here, all rather similar to each other.

There are also particles in the Standard Model called "bosons" having angular momentum in units of $\hbar$ times 0,1,2, and so on. Identical bosons, far from satisfying the exclusion principle, sort of like to all get into the same state: one sees this in phenomena such as lasers, where lots of photons occupy the same few states. Most of the bosons in the Standard Model are called "gauge bosons". These carry the different forces in the standard model, by which the particles interact:

Electromagnetic force	Weak force	Strong force
photon	W_+, W_-, Z	8 gluons

Finally, there is also a bizarre particle in the Standard Model called the "Higgs boson". This was first introduced as a rather ad hoc hypothesis: it's supposed to interact with the forces in such a way as to break the symmetry that would otherwise be present between the electromagnetic force and the weak force. It has not yet been observed; finding it would would represent a great triumph for the Standard Model, while *not* finding it might point the way to better theories.

Indeed, while the Standard Model has passed many stringent experimental tests, and successfully predicted the existence of many particles which were later observed (like the W, the Z, and the charm and top quarks), it is a most puzzling sort of hodgepodge. Could nature really be this baroque at its most fundamental level? Few people seem to think so; most hope for some deeper, simpler theory.

It's easy to want a "deeper, simpler theory", but how to get it? What are the clues? What can we do? Experimentalists certainly have their work cut out for them. They can try to find or rule out the Higgs. They can also try to see if neutrinos, assumed to be massless in the Standard Model, actually have a small mass — for while the Standard Model could easily be patched if this were the case, it would shed interesting light on one of the biggest mysteries in physics, namely why the fermions in nature seem not to be symmetric under reflection, or "parity". Right now, we believe that neutrinos only exist in a left-handed form, rotating one way but not the other around the axis they move along. This is intimately related to their apparent masslessness. In fact, for reasons that would take a while to explain, the lack of parity symmetry in the Standard Model forces us to assume all the observed fermions acquire their mass only through interaction with the Higgs particle! For more on the neutrino mass puzzle, try:

1) Paul Langacker, Implications of Neutrino Mass, `http://dept.physics.upenn.edu/neutrino/jhu/jhu.html`

And, of course, experimentalists can continue to do what they always do best: discover the utterly unexpected.

Theorists, on the other hand, have been spending the last couple of decades poring over the standard model and trying to understand what it's telling us. It's so full of suggestive patterns and partial symmetries! First, why are there 3 forces here? Each force goes along with a group of symmetries called a "gauge group", and electromagnetism corresponds to $\mathrm{U}(1)$, while the weak force corresponds to $\mathrm{SU}(2)$ and the strong force corresponds to $\mathrm{SU}(3)$. (Here $\mathrm{U}(n)$ is the group of $n \times n$ unitary complex matrices, while $\mathrm{SU}(n)$ is the subgroup consisting of those with determinant equal to 1.) Well, actually the Standard Model partially unifies the electromagnetic and weak force into the "electroweak force", and then resorts to the Higgs to explain why these forces are so different in practice. Various "grand unified theories" or "GUTs" try to unify the forces further by sticking the group $\mathrm{SU}(3) \times \mathrm{SU}(2) \times \mathrm{U}(1)$ into a bigger group — but then resort to still more Higgses to break the symmetry between them!

Then, there is the curious parallel between the leptons and quarks in each generation. Each generation has a lepton with mass, a massless or almost massless neutrino, and two quarks. The massive lepton has charge -1, the neutrino has charge 0 as its name suggests, the "down" type quark has charge $-1/3$, and the "up" type quark has charge $2/3$. Funny pattern, eh? The Standard Model does not really explain this, although it would be ruined by "anomalies" — certain nightmarish problems that can beset a quantum field theory — if one idly tried to mess with the generations by leaving out a quark or the like. It's natural to try to "unify" the quarks and leptons, and indeed, in grand unified theories like the $\mathrm{SU}(5)$ theory proposed in 1974 of Georgi and Glashow, the quarks and leptons are treated in a unified way.

Another interesting pattern is the repetition of generations itself. Why is there more than one? Why are there three, almost the same, but with the masses increasing dramatically as we go up? The Standard Model makes no attempt to explain this, although it does suggest that there had better not be more than 17 quarks — more, and the strong force would not be "asymptotically free" (weak at high energies), which would cause lots of problems for the theory. In fact, experiments strongly suggest that there are no more than 3 generations. Why?

Finally, there is the grand distinction between bosons and fermions. What does this mean? Here we understand quite a bit from basic principles. For example, the "spin-statistics theorem" explains why particles with half-integer spin should satisfy the Pauli exclusion principle, while those with integer spin should like to hang out together. This is a very beautiful result with a deep connection to topology, which I try to explain in

2) John Baez, Spin, statistics, CPT and all that jazz, `http://math.ucr.edu/home/baez/spin.stat.html`

But many people have tried to bridge the chasm between bosons and fermions, unifying them by a principle called "supersymmetry". As in the other cases mentioned above, when they do this, they then need to pull tricks to "break" the symmetry to get a theory that fits the experimental fact that bosons and fermions are very different. Personally, I'm suspicious of all these symmetries postulated only to be cleverly broken; this approach was so successful in dealing with the electroweak force — modulo the missing Higgs! - that it seems to have been accepted as a universal method of having ones cake and eating it too.

Now, string theory comes in two basic flavors. Purely bosonic string theory lives in 26 dimensions and doesn't have any fermions in it. Supersymmetric string theories live in

10 dimensions and have both bosons and fermions, unified via supersymmetry. To deal with the fermions in nature, most work in physics has focused on the supersymmetric case. Just for completeness, I should point out that there are 5 different supersymmetric string theories: type I, type IIA, type IIB, $\mathrm{E}_8 \times \mathrm{E}_8$ heterotic and $\mathrm{SO}(32)$ heterotic. For more on these, see "Week 72". We won't be getting into them here. Instead, I just want to explain how fermions work in different dimensions, and why nice things happen in dimensions of the form $8k + 2$. Most of what I say is in Section 3 of

3) John H. Schwarz, "Introduction to supersymmetry", in *Superstrings and Supergravity, Proc. of the 28th Scottish Universities Summer School in Physics*, ed. A. T. Davies and D. G. Sutherland, University Printing House, Oxford, 1985.

but mathematicians may also want to supplement this with material from the book "Spin Geometry" by Lawson and Michelson, cited in "Week 82".

To understand fermions in different dimensions we need to understand Clifford algebras. As far as I know, when Clifford originally invented these algebras in the late 1800s, he was trying to generalize Hamilton's quaternion algebra by considering algebras that had lots of different anticommuting square roots of -1. In other words, he considered an associative algebra generated by a bunch of guys $e_1, \ldots, e_n$, satisfying

$$e_i^2 = -1$$

for all i, and

$$e_i e_j = -e_j e_i$$

whenever i is not equal to j. I discussed these algebras in "Week 82" and I said what they all were — they all have nice descriptions in terms of the reals, the complexes, and the quaternions.

These original Clifford algebras are great for studying rotations in n-dimensional Euclidean space — please take my word for this for now. However, here we want to study rotations and Lorentz transformations in n-dimensional Minkowski spacetime, so we need to work with a slightly Different kind of Clifford algebra, which was probably invented by Dirac. In n-dimensional Euclidean space the metric (used for measuring distances) is

$$dx_1^2 + dx_2^2 + \ldots + dx_n^2$$

while in n-dimensional Minkowski spacetime it is

$$dx_1^2 + dx_2^2 + \ldots - dx_n^2$$

or if you prefer (it's just a matter of convention), you can take it to be

$$-dx_1^2 - dx_2^2 - \ldots + dx_n^2$$

So it turns out that we need to switch some signs in the definition of the Clifford algebra when working in Minkowski spacetime.

In general, we can define the Clifford algebra $C_{p,q}$ to be the algebra generated by a bunch of elements e_i, with p of them being square roots of -1 and q of them being square roots of 1. As before, we require that they anticommute:

$$e_i e_j = -e_j e_i$$

when i and j are different. Physicists usually call these guys "gamma matrices". For n-dimensional Minkowski space we can work either with $Cn-1,1$ or $C_{1,n-1}$, depending on our preference. As Cecile DeWitt has pointed out, it *does* make a difference which one we use.

With some work, one can check that these algebras go like this:

$C_{0,1}$	$\mathbb{R}+\mathbb{R}$	$C_{1,0}$	$\mathbb{C}$
$C_{1,1}$	$\mathbb{R}(2)$	$C_{1,1}$	$\mathbb{R}(2)$
$C_{2,1}$	$\mathbb{C}(2)$	$C_{1,2}$	$\mathbb{R}(2)+\mathbb{R}(2)$
$C_{3,1}$	$\mathbb{H}(2)$	$C_{1,3}$	$\mathbb{R}(4)$
$C_{4,1}$	$\mathbb{H}(2)+\mathbb{H}(2)$	$C_{1,4}$	$\mathbb{C}(4)$
$C_{5,1}$	$\mathbb{H}(4)$	$C_{1,5}$	$\mathbb{H}(4)$
$C_{6,1}$	$\mathbb{C}(8)$	$C_{1,6}$	$\mathbb{H}(4)+\mathbb{H}(4)$
$C_{7,1}$	$\mathbb{R}(16)$	$C_{1,7}$	$\mathbb{H}(8)$

I've only listed these up to 8-dimensional Minkowski spacetime, and the cool thing is that after that they sort of repeat — more precisely, $C_{n+8,1}$ is just the same as 16×16 matrices with entries in $C_{n,1}$, and $C_{1,n+8}$ is just 16×16 matrices with entries in $C_{1,n}$! This "period-8" phenomenon, sometimes called Bott periodicity, has implications for all sorts of branches of math and physics. This is why fermions in 2 dimensions are a bit like fermions in 10 dimensions and 18 dimensions and 26 dimensions. . . .

In physics, we describe fermions using "spinors", but there are different kinds of spinors: Dirac spinors, Weyl spinors, Majorana spinors, and even Majorana-Weyl spinors. This is a bit technical but I want to dig into it here, since it explains what's special about $8k+2$ dimensions and especially 10 dimensions.

Before I get technical, though, let me just summarize the point for those of you who don't want all the gory details. "Dirac spinors" are what you use to describe spin-$1/2$ particles that come in both left-handed and right-handed forms and aren't their own antiparticle — like the electron. Weyl spinors have half as many components, and describe spin-$1/2$ particles with an intrinsic handedness that aren't their own antiparticle — like the neutrino. "Weyl spinors" are only possible in even dimensions!

Both these sorts of spinors are "complex" — they have complex-valued components. But there are also real spinors. These are used for describing particles that are their own antiparticle, because the operation of turning a particle into an antiparticle is described mathematically by complex conjugation. "Majorana spinors" describe spin-$1/2$ particles that come in both left-handed and right-handed forms and are their own antiparticle. Finally, "Majorana-Weyl spinors" are used to describe spin-$1/2$ particles with an intrinsic handedness that are their own antiparticle.

As far as we can tell, none of the particles we've seen are Majorana or Majorana-Weyl spinors, although if the neutrino has a mass it might be a Majorana spinor. Majorana and Majorana-Weyl spinors only exist in certain dimensions. In particular, Majorana-Weyl spinors are very finicky: they only work in dimensions of the form $8k+2$. This is part of what makes supersymmetric string theory work in 10 dimensions!

Now let me describe the technical details. I'm doing this mainly for my own benefit; if I write this up, I'll be able to refer to it whenever I forget it. For those of you who stick with me, there will be a little reward: we'll see that a certain kind of supersymmetric gauge theory, in which there's a symmetry between gauge bosons and fermions, only works in dimensions 3, 4, 6, and 10. Perhaps coincidentally — I don't understand this stuff well enough to know — these are also the dimensions when supersymmetric string theory works classically. (It's the quantum version that only works in dimension 10.)

So: part of the point of these Clifford algebras is that they give representations of the double cover of the Lorentz group in different dimensions. In "Week 61" I explained this double cover business, and how the group $\mathrm{SO}(n)$ of rotations of n-dimensional Euclidean space has a double cover called $\mathrm{Spin}(n)$. Similarly, the Lorentz group of n-dimensional Minkowski space, written $\mathrm{SO}(n-1,1)$, has a double cover we could call $\mathrm{Spin}(n-1,1)$. The spinors we'll discuss are all representations of this group.

The way Clifford algebras help is that there is a nice way to embed $\mathrm{Spin}(n-1,1)$ in either $C_{n-1,1}$ or $C_{1,n-1}$, so any representation of these Clifford algebras gives a representation of $\mathrm{Spin}(n-1,1)$. We have a choice of dealing with real representations or complex representations. Any complex representation of one of these Clifford algebras is also a representation of the *complexified* Clifford algebra. What I mean is this: above I implicitly wanted $C_{p,q}$ to consist of all *real* linear combinations of products of the e_i, but we could have worked with *complex* linear combinations instead. Then we would have "complexified" $C_{p,q}$. Since the complex numbers include a square root of minus 1, the complexification of $C_{p,q}$ only depends on the dimension p + q, not on how many minus signs we have.

Now, it is easy and fun and important to check that if you complexify $\mathbb{R}$ you get $\mathbb{C}$, and if you complexify $\mathbb{C}$ you get $\mathbb{C}+\mathbb{C}$, and if you complexify $\mathbb{H}$ you get $\mathbb{C}(2)$. Thus from the above table we get this table:

dimension n	complexified Clifford algebra
1	$\mathbb{C}+\mathbb{C}$
2	$\mathbb{C}(2)$
3	$\mathbb{C}(2)+\mathbb{C}(2)$
4	$\mathbb{C}(4)$
5	$\mathbb{C}(4)+\mathbb{C}(4)$
6	$\mathbb{C}(8)$
7	$\mathbb{C}(8)+\mathbb{C}(8)$
8	$\mathbb{C}(16)$

Notice this table is a lot simpler — complex Clifford algebras are "period-2" instead of period-8.

Now the smallest complex representation of the complexified Clifford algebra in dimension n is what we call a "Dirac spinor". We can figure out what this is using the above table, since the smallest complex representation of $\mathbb{C}(n)$ or $\mathbb{C}(n)+\mathbb{C}(n)$ is on the n-dimensional complex vector space $\mathbb{C}^n$, given by matrix multiplication. Of course, for $\mathbb{C}(n)+\mathbb{C}(n)$ there are *two* representations depending on which copy of $\mathbb{C}(n)$ we use, but these give equivalent representations of $\mathrm{Spin}(n-1,1)$, which is what we're really interested in, so we still speak of "the" Dirac spinors.

So we get:

dimension n	Dirac spinors
1	$\mathbb{C}$
2	$\mathbb{C}(2)$
3	$\mathbb{C}(2)$
4	$\mathbb{C}(4)$
5	$\mathbb{C}(4)$
6	$\mathbb{C}(8)$
7	$\mathbb{C}(8)$
8	$\mathbb{C}(16)$

The dimension of the Dirac spinors doubles as we go to each new even dimension.

We can also look for the smallest real representation of $C_{n-1,1}$ or $C_{1,n-1}$. This is easy to work out from our tables using the fact that the algebra $\mathbb{R}$ has its smallest real representation on $\mathbb{R}$, while for $\mathbb{C}$ it's on $\mathbb{R}^2$ and for $\mathbb{H}$ it's on $\mathbb{R}^4$.

Sometimes this smallest real representation is secretly just the Dirac spinors *viewed as a real representation* — we can view $\mathbb{C}^n$ as the real vector space $\mathbb{R}^{2n}$. But sometimes the Dirac spinors are the *complexification* of the smallest real representation — for example, $\mathbb{C}^n$ is the complexification of $\mathbb{R}^n$. In this case folks call the smallest real representation "Majorana spinors".

When we are looking for the smallest real representations, we get different answers for $C_{n-1,1}$ and $C_{1,n-1}$. Here is what we get:

n	$C_{n-1,1}$	smallest $\mathbb{R}$ rep.	M.s?	$C_{1,n-1}$	smallest $\mathbb{R}$ rep.	M.s?
1	$\mathbb{R}+\mathbb{R}$	$\mathbb{R}$	✓	$\mathbb{C}$	$\mathbb{R}^2$	
2	$\mathbb{R}(2)$	$\mathbb{R}^2$	✓	$\mathbb{R}(2)$	$\mathbb{R}^2$	✓
3	$\mathbb{C}(2)$	$\mathbb{R}^4$		$\mathbb{R}(2)+\mathbb{R}(2)$	$\mathbb{R}^2$	✓
4	$\mathbb{H}(2)$	$\mathbb{R}^8$		$\mathbb{R}(4)$	$\mathbb{R}^4$	✓
5	$\mathbb{H}(2)+\mathbb{H}(2)$	$\mathbb{R}^8$		$\mathbb{C}(4)$	$\mathbb{R}^8$	
6	$\mathbb{H}(4)$	$\mathbb{R}^{16}$		$\mathbb{H}(4)$	$\mathbb{R}^{16}$	
7	$\mathbb{C}(8)$	$\mathbb{R}^{16}$		$\mathbb{H}(4)+\mathbb{H}(4)$	$\mathbb{R}^{16}$	
8	$\mathbb{R}(16)$	$\mathbb{R}^{16}$	✓	$\mathbb{H}(8)$	$\mathbb{R}^{32}$	

I've noted when the representations are Majorana spinors. Everything repeats with period 8 after this, in an obvious way.

Finally, sometimes there are "Weyl spinors" or "Majorana-Weyl" spinors. The point is that sometimes the Dirac spinors, or Majorana spinors, are a *reducible* representation of $\mathrm{Spin}(1, n-1)$. For Dirac spinors this happens in every even dimension, because the Clifford algebra element

$$\Gamma = e_1 \dots e_n$$

commutes with everything in $\mathrm{Spin}(1, n-1)$ and Γ^2 is 1 or -1, so we can break the space of Dirac spinors into the two eigenspaces of Γ, which will be smaller reps of $\mathrm{Spin}(1, n-1)$ — the "Weyl spinors". Physicists usually call this Γ thing "γ_5", and it's an operator that represents parity transformations. We get "Majorana-Weyl" spinors only when we have Majorana spinors, n is even, and $\Gamma^2 = 1$, since we are then working with real numbers and -1 doesn't have a square root. You can work out Γ^2 for either $C_{n-1,1}$ or $C_{1,n-1}$, and see that we'll only get Majorana-Weyl spinors when $n = 8k + 2$.

Whew! Let me summarize some of our results:

n	Dirac	Majorana	Weyl	Majorana-Weyl
1	$\mathbb{C}$	$\mathbb{R}$		
2	$\mathbb{C}^2$	$\mathbb{R}^2$	$\mathbb{C}$	$\mathbb{R}$
3	$\mathbb{C}^2$	$\mathbb{R}^2$		
4	$\mathbb{C}^4$	$\mathbb{R}^4$	$\mathbb{C}^2$	
5	$\mathbb{C}^4$			
6	$\mathbb{C}^8$		$\mathbb{C}^4$	
7	$\mathbb{C}^8$			
8	$\mathbb{C}^{16}$	$\mathbb{R}^{16}$	$\mathbb{C}^8$	

When there are blanks here, the relevant sort of spinor doesn't exist. Here I'm not distinguishing Majorana spinors that come from $C_{n-1,1}$ and those that come from $C_{1,n-1}$; you can do that with the previous table. Again, things continue for larger n in an obvious way.

Now, let's imagine a theory that has a supersymmetry between a gauge bosons and a fermion. We'll assume there are as many physical degrees of freedom for the gauge boson as there are for the fermion. Gauge bosons have $n-2$ physical degrees of freedom in n dimensions: for example, in dimension 4 the photon has 2 degrees of freedom, the left and right polarized states. So we want to find a kind of spinor that has $n-2$ physical degrees of freedom. But the number of physical degrees of freedom of a spinor field is half the number of (real) components of the spinor, since the Dirac equation relates the components. So we are looking for a kind of spinor that has $2(n-2)$ real components. This occurs in only 4 cases:

- $n = 3$: then $2(n-2) = 2$, and Majorana spinors have 2 real components
- $n = 4$: then $2(n-2) = 4$, and Majorana or Weyl spinors have 4 real components
- $n = 6$: then $2(n-2) = 8$, and Weyl spinors have 8 real components
- $n = 10$: then $2(n-2) = 16$, and Majorana-Weyl spinors have 16 real components

Note we count complex components as two real components. And note how dimension 10 works: the dimension of the spinors grows pretty fast as n increases, but the Majorana-Weyl condition reduces the dimension by a factor of 4, so dimension 10 just squeaks by!

Here John Schwarz explains how nice things happen in the same dimensions for superstring theory:

4) John H. Schwarz, "Introduction to superstrings", in *Superstrings and Supergravity, Proc. of the 28th Scottish Universities Summer School in Physics*, ed. A. T. Davies and D. G. Sutherland, University Printing House, Oxford, 1985.

He also makes a tantalizing remark: perhaps these 4 cases correspond somehow to the reals, complexes, quaternions and octonions. Note: $3 = 1 + 2$, $4 = 2 + 2$, $6 = 4 + 2$ and $10 = 8 + 2$. You can never tell with this stuff... everything is related.

I thank Joshua Burton for helping me overcome my fear of Majorana spinors, and for correcting a number of embarrassing errors in the first version of this article.

Addendum: In July 2001, long after the above article was written, Lubos Motl explained where the number 18 shows up in string theory:

> *Today we know that the two heterotic string theories are related by various dualities. For example, in 17+1 dimension, the lattices Γ_{16} and $\Gamma_8 + \Gamma_8$, with an added Lorentzian $\Gamma_{1,1}$, become isometric. There is a single even self-dual lattice in 17+1 dimensions, $\Gamma_{17,1}$. This is the reason why two heterotic string theories are T-dual to each other. The compactification on a circle adds two extra $\mathrm{U}(1)$s (from Kaluza-Klein graviphoton and the B-field), and with appropriate Wilson lines, a compactification of one heterotic string theory on radius R is equivalent to the other on radius $1/R$, using correct units.*

Also, in "Week 104", and especially in the Addendum written by Robert Helling, we'll see that it's *not* a coincidence that super-Yang-Mills theory works nicely in dimensions that are 2 more than the dimensions of the reals, complex numbers, quaternions and octonions.

Since the mathematicians have grabbed ahold of the theory of relativity, I no longer understand it. — Albert Einstein

Week 94

November 11, 1996

Today I want to talk a bit about asymptotic freedom.

First of all, remember that in quantum field theory, studying very small things is the same as studying things at very high energies. The reason is that in quantum mechanics you need to collide two particles at a large relative momentum p to make sure the distance x between them gets small, thanks to the uncertainty principle. But in special relativity the energy E and momentum p of a particle of mass m are related by

$$E^2 = p^2 + m^2,$$

in God's units, where the speed of light is 1. So small x also corresponds to large E.

"Asymptotic freedom" refers to fact that some forces become very weak at high energies, or equivalently, at very short distances. The most interesting example of this is the so-called "strong force", which holds the quarks together in a hadron, like a proton or neutron. True to its name, it is very strong at distances comparable to the radius of proton, or at energies comparable to the mass of the proton (where if we don't use God's units, we have to use $E = mc^2$ to convert units of mass to units of energy). But if we smash protons at each other at much higher energies, the constituent quarks act almost as free particles, indicating that the strong force gets weak when the quarks get really close to each other.

Now in "Week 76" and "Week 84" I talked about another phenomenon, called "confinement". This simply means that at lower energies, or larger distance scales, the strong force becomes so strong that it is *impossible* to pull a quark out of a hadron. Asymptotic freedom and confinement are two aspects of the same thing: the dependence of the strength of the strong force on the energy scale. Asymptotic freedom is better understood, though, because the weaker a force is, the better we can apply the methods of perturbation theory — a widely used approach where we try to calculate everything as a Taylor series in the "coupling constant" measuring the strength of the force in question. This is often successful when the coupling constant is small, but not when it's big.

The interesting thing is that in quantum field theory the coupling constants "run". This is particle physics slang for the fact that they depend on the energy scale at which we measure them. "Asymptotic freedom" happens when the coupling constant runs down to zero as we move up to higher and higher energy scales. If you want to impress someone about your knowledge of this, just mutter something about the "beta function" being negative — this is a fancy way of saying the coupling constant decreases as you go to higher energies. You'll sound like a real expert.

Now, Frank Wilczek is one of the original discoverers of asymptotic freedom. He *is* a real expert. He recently won a prize for this work, and he gave a nice talk which he made into a paper:

1) Frank Wilczek, "Asymptotic freedom", preprint available as `hep-th/9609099`.

Among other things, he gives a nice summary of the work of Nielsen and Hughes, which gave the first really easy to understand explanation of asymptotic freedom. For the original work, try:

2) N. K. Nielsen, *Am. J. Phys.* **49**, 1171 (1981).

3) R. J. Hughes, *Nucl. Phys. B186*, **376** (1981).

Why would a force get weak at short distance scales? Actually it's easier to imagine why it would get *strong* — and sometimes that is what happens. Of course there are lots of forces that decrease with distance like $1/r^2$, but I'm talking about something more drastic: I'm talking about "screening".

For example, say you have an electron in some water. It'll make an electric field, but this will push all the other negatively charged particles little bit *away* from your electron and pull all the positively charged ones a little bit *towards* your electron:

```
                  -
                   +

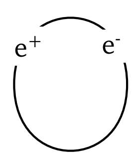
    your electron: -       +-

                         +
                          -
```

In other words, it will "polarize" all the neighboring water molecules. But this will create a counteracting sort of electric field, since it means that if you draw any sphere around your electron, there will be a bit more *positively* charged other stuff in that sphere than negatively charged other stuff. The bigger the sphere is, the more this effect occurs — though there is a limit to how much it occurs. We say that the further you go from your electron, the more its electric charge is "screened", or hidden, behind the effect of the polarization.

This effect is very common in materials that don't conduct electricity, like water or plastics or glass. They're called "dielectrics", and the dielectric constant, ε, measures the strength of this screening effect. Unlike in math, this ε is typically bigger than 1. If you apply an electric field to a dielectric material, the electric field inside the material is only $1/\varepsilon$ as big as you'd expect if this polarization wasn't happening.

What's cool is that according to quantum field theory, screening occurs even in the vacuum, thanks to "vacuum polarization". One can visualize it rather vaguely as due to a constant buzz of virtual particle-antiparticle pairs getting created and then annihilating — called "vacuum bubbles" in the charming language of Feynman diagrams, because you can draw them like this:



Here I've drawn a positron-electron pair getting created and then annihilating as time passes.

There is a lot I should say about virtual particles, and how despite the fact that they aren't "real" they can produce very real effects like vacuum polarization. A strong enough electric field will even "spark the vacuum" and make the virtual particles *become* real! But discussing this would be too big of a digression. Suffice it to say that you have to learn quantum field theory to see how something that starts out as a kind of mathematical book-keeping device — a line in a Feynman diagram — winds up acting a bit like a real honest particle. It's a case of a metaphor gone berserk, but in an exceedingly useful way.

Anyway, so much for screening. Asymptotic freedom requires something opposite, called "anti-screening"! That's why it's harder to understand.

Nielsen and Hughes realized that anti-screening is easier to understand using magnetism than electricity. In analogy to dielectrics, there are some materials that screen magnetic fields, and these are called "diamagnetic" — for example, one of the strongest diamagnets is bismuth. But in addition, there are materials that "anti-screen" magnetic fields — the magnetic field inside them is stronger than the externally applied magnetic field — and these are called "paramagnetic". For example, aluminum is paramagnetic. People keep track of paramagnetism using a constant called the magnetic permeability, μ. Just to confuse you, this works the opposite way from the dielectric constant. If you apply a magnetic field to some material, the magnetic field inside it is μ times as big as you'd expect if there were no magnetic effects going on.

The nice thing is that there are lots of examples of paramagnetism and we can sort of understand it if we think about it. It turns out that paramagnetism in ordinary matter is due to the spin of the electrons in it. The electrons are like little magnets — they have a little "magnetic moment" pointing along the axis of their spin. Actually, purely by convention it points in the direction opposite their spin, since for some stupid reason Benjamin Franklin decided to decree that electrons were *negative*. But don't worry about this — it doesn't really matter. The point is that when you put electrons in a magnetic field, their spins like to line up in such a way that their magnetic field points the same way as the externally applied magnetic field, just like a compass needle does in the Earth's magnetic field. So they *add* to the magnetic field. Ergo, paramagnetism.

Now, spin is a form of angular momentum intrinsic to the electron, but there is another kind of angular momentum, namely orbital angular momentum, caused by how the electron (or whatever particle) is moving around in space. It turns out that orbital angular momentum also has magnetic effects, but only causes diamagnetism. The idea that when you apply a magnetic field to some material, it can also make the electrons in it tend to move in orbits perpendicular to the magnetic field, and the resulting current creates a magnetic field. But this magnetic field must *oppose* the external magnetic field. Ergo, diamagnetism.

Why does orbital angular momentum work one way, while spin works the other way? I'll say a bit more about that later. Now let me get back to asymptotic freedom.

I've talked about screening and antiscreening for both electric and magnetic fields now. But say the "substance" we're studying is the *vacuum*. Unlike most substances, the vacuum doesn't look different when we look at it from a moving frame of reference. We say it's "Lorentz-invariant". But if we look at an electric field in a moving frame of reference, we see a bit of magnetic field added on, and vice versa. We say that the electric and magnetic fields transform into each other... they are two aspects of single thing, the electromagnetic field. So the amount of *electric* screening or antiscreening in the vacuum has to equal the amount of the *magnetic* screening or antiscreening. In other words, thanks to the silly way we defined ε differently from μ, we must have

$$\varepsilon = 1/\mu$$

in the vacuum.

Now the cool thing is that the Yang-Mills equations, which describe the strong force, are very similar to Maxwell's equations. In particular, the strong force, also known as the "color" force, consists of two aspects, the "chromoelectric" field and "chromomagnetic" field. Moreover, the same argument above applies here: the vacuum must give the same antiscreening for the chromoelectric field as it does for the chromomagnetic field, so $\varepsilon = 1/\mu$ here too.

So to understand asymptotic freedom it is sufficient to see why the vacuum acts like a paramagnet for the strong force! This depends on a big difference between the strong force and electromagnetism. Just as the electromagnetic field is carried by photons, which are spin-1 particles, the strong force is carried by "gluons", which are also spin-1 particles. But while the photon is electrically uncharged, the gluon is charged as far as the strong force goes: we say it has "color".

The vacuum is bustling with virtual gluons. When we apply a chromomagnetic field to the vacuum, we get two competing effects: paramagnetism thanks to the *spin* of the gluons, and diamagnetism due to their *orbital angular momentum*. But — the spin effect is stronger. The vacuum acts like a paramagnet for the strong force. So we get asymptotic freedom!

That's the basic idea. Of course, there are some loose ends. To see why the spin effect is stronger, you have to calculate a bit. At least I don't know how to see it without calculating — but Wilczek sketches the calculation, and it doesn't look too bad. It's also true in most metals that the spin effect wins, so they are paramagnetic.

You might also wonder why spin and orbital angular momentum work oppositely as far as magnetism goes. Unfortunately I don't have any really simple slick answer. One thing is that it seems any answer must involve quantum mechanics. [Note: later I realized some very basic things about this, which I append below.] In volume II of his magnificent series:

4) Richard Feynman, Robert Leighton, and Matthew Sands, *The Feynman Lectures on Physics*, Addison-Wesley, Reading, Mass., 1964.

Feynman notes: "It is a consequence of classical mechanics that if you have any kind of system — a gas with electrons, protons, and whatever — kept in a box so that the whole thing can't turn, there will be no magnetic effect. [. . . .] The theorem then says that if you turn on a magnetic field and wait for the system to get into thermal equilibrium, there will be no paramagnetism or diamagnetism — there will be no induced magnetic moment. Proof: According to statistical mechanics, the probability that a system will have any given state of motion is proportional to $\exp(-U/kT)$, where U is the energy of that motion. Now what is the energy of motion. For a particle moving in a constant magnetic field, the energy is the ordinary potential energy plus $mv^2/2$, with nothing additional for the magnetic field. (You know that the forces from electromagnetic fields are $q(E + v \times B)$, and that the rate of work $F \cdot v$ is just $qE \cdot v$, which is not affected by the magnetic field.) So the energy of a system, whether it is in a magnetic field or not, is always given by the kinetic energy plus the potential energy. Since the probability of any motion depends only on the energy — that is, on the velocity and position — it is the same whether or not there is a magnetic field. For *thermal* equilibrium, therefore, the magnetic field has no effect."

So to understand magnetism we really need to work quantum-mechanically. Laurence Yaffe has brought to my attention a nice path-integral argument as to why orbital angular momentum can only yield diamagnetism; this can be found in his charming book:

5) Barry Simon, *Functional Integration and Quantum Physics*, Academic Press, 1979.

This argument is very simple if you know about path integrals, but I think there should be some more lowbrow way to see it, too. I think it's good to make all this stuff as simple as possible, because the phenomena of asympotic freedom and confinement are very important and shouldn't only be accessible to experts.

I'd like to thank Douglas Singleton, Matt McIrvin, Mike Kelsey, and Laurence Yaffe for some posts on sci.physics.research that helped me understand this stuff.

Addendum *(November 13, 1996)*. Thanks to emails from Yehuda Naveh and Bruce Smith I'm beginning to understand this stuff at the 13-year-old level it deserves. If you want to jump to the punchline, skip down to the stuff between double lines — that's the part I should have known ages ago!

Here's the deal. Feynman's theorem deals with classical systems made only of a bunch of electrically charged point particles. Remember how it goes: A magnetic field can never do work on such a system, because it always exerts a force perpendicular to the velocity of an electrically charged particle. So the energy of such a system is independent of the externally applied magnetic field. Now, in statistical mechanics the equilibrium state of a system depends only on the energy of each state, since the probability of being in a state with energy E is proportional to $\exp(-E/kT)$. So an external magnetic field doesn't affect the equilibrium state of this sort of system. So there can't be anything like paramagnetism or diamagnetism, where the equilibrium state is affected by an external magnetic field.

But suppose instead we allowed an extra sort of building block of our system, in addition to electrically charged particles. Suppose we allow little "current loops". We take these as "primitives", in the sense that we don't ask how or why the current keeps flowing around the loop, we just assume it does. We just *define* one of these "current loops" to be a little circle of stuff with a constant mass per unit length, with a constant current that flows around it. This may or may not be physically reasonable, but we're gonna do it anyway!

Note: If we tried to make a current loop out of classical electrically charged point particles, the current loop would tend to fall apart! A loop is not going to be the equilibrium state of a bunch of charged particles. So we are going to get around this by taking current loops as new primitives — simply *assuming* they exist and have the properties given above.

If we build our system out of current loops and point particles, Feynman's theorem no longer applies. Why? Well, a constant magnetic field exerts a force perpendicular to the direction of the current, and this applies a *torque* to the current loop — no net force, just a torque. But since the current loop is made out of stuff that has a constant mass per unit length, when the current loop is rotating it will have kinetic energy. So by applying a torque to the current loop, the magnetic field does *work* on the current loop. Thus Feynman's reasoning no longer applies to this case.

In particular, what happens is just what we expect. The torque on the little current loops makes them want to line up with the external magnetic field. In other words, they will have less energy when they are lined up like this. In particular, the energy of the system *does* depend on the external magnetic field, and the equilibrium state will tend to have more little current loops lined up with the field than not.

Now if we keep track of the magnetic field produced by these current loops, we see it points the same way as the externally applied field. So we get paramagnetism.

Now, even without doing a detailed quantum-mechanical treatment of this problem, we see what's special about spin: a particle with spin is a bit like one of our imaginary "primitive current loops". This is how spin can give paramagnetism.

Great. But what had always been bugging me is this! If you put a charged particle in a constant magnetic field, it moves in a circular or spiral orbit. For simplicity let's say it moves in a circle. You can think of this, if you like, as a kind of current loop — but a very different sort of current loop than the one we've just been considering! In particular, if you work it out, this particle circling around will produce a magnetic field that *opposes* the external magnetic field. On the other hand, our primitive current loops are in the state of least energy when they're lined up to produce a magnetic field that *goes with* the external field.

What's the deal? Well, it's just something about how the vector cross product works; you gotta work it out yourself to believe it. All you need to know is that the force on a charged particle is $qv \times B$. It boils down to this:

> *A positively charged particle orbiting in a magnetic field pointing along the z-axis will orbit CLOCKWISE in the x-y plane. However, a primitive current loop in a magnetic field pointing along the z-axis will be in its state of least energy when the current runs COUNTERCLOCKWISE in the x-y plane.*

I'm sure this is what was nagging at me. It's just one of those basic funny little things. If I'm still mixed up, someone had better let me know.

There are a couple other things perhaps worth saying about this:

1. In our calculation of the energy of the system, we have been neglecting the energy due to the electric and magnetic fields *produced* by our point particles and current loops. A more careful analysis would take these into account. In particular, the reason ferromagnets prefer to have lots of "domains" than to have all their little current loops lined up, is to keep the energy due to the magnetic field produced by these loops from getting too big.

2. A little current loop acts like a magnetic dipole. We'd also get interesting effects if we had magnetic monopoles. Here I simply assume that, just as an electric field

exerts a force on a electrically charged particle equal to qE, a magnetic field exerts a force on a magnetically charged particle equal to mB, where m is the magnetic charge. A magnetic field would then be able to do work on a magnetic monopole, and again Feynman's theorem would not apply. So it's perhaps not so surprising that Feynmans' theorem fails when we have magnetic dipoles as primitive constituents of our system, too (although these dipoles had better not be points — they need a moment of inertia for a torque on them to do work).

Week 95

November 26, 1996

Last week I talked about asymptotic freedom — how the "strong" force gets weak at high energies. Basically, I was trying to describe an aspect of "renormalization" without getting too technical about it. By coincidence, I recently got my hands on a book I'd been meaning to read for quite a while:

1) Laurie M. Brown, ed., *Renormalization: From Lorentz to Landau (and Beyond)*, Springer-Verlag, New York, 1993.

It's a nice survey of how attitudes to renormalization have changed over the years. It's probably the most fun to read if you know some quantum field theory, but it's not terribly technical, and it includes a "Tutorial on infinities in QED", by Robert Mills, that might serve as an introduction to renormalization for folks who've never studied it.

Okay, on to some new stuff. . . .

It's a bit funny how one of the most curious features of bosonic string theory in 26 dimensions was anticipated by the number theorist Edouard Lucas in 1875. I assume this is the same Lucas who is famous for the Lucas numbers: 1,3,4,7,11,18,. . . , each one being the sum of the previous two, after starting off with 1 and 3. They are not quite as wonderful as the Fibonacci numbers, but in a study of pine cones it was found that while *most* cones have consecutive Fibonacci numbers of spirals going around clockwise and counterclockwise, a small minority of deviant cones use Lucas numbers instead.

Anyway, Lucas must have liked playing around with numbers, because in one publication he challenged his readers to prove that: "A square pyramid of cannon balls contains a square number of cannon balls only when it has 24 cannon balls along its base". In other words, the only integer solution of

$$1^2 + 2^2 + \ldots + n^2 = m^2,$$

is the solution $n = 24$, not counting silly solutions like $n = 0$ and $n = 1$.

It seems that Lucas didn't have a proof of this; the first proof is due to G. N. Watson in 1918, using elliptic functions. Apparently an elementary proof appears in the following ridiculously overpriced book:

2) W. S. Anglin, *The Queen of Mathematics: An Introduction to Number Theory*, Kluwer, Dordrecht, 1995.

For more historical details, see the review in

3) Jet Wimp, "Eight recent mathematical books", *Math. Intelligencer* **18** (1996), 72–79.

Unfortunately, I haven't seen these proofs of Lucas' claim, so I don't know why it's true. I do know a little about its relation to string theory, so I'll talk about that.

There are two main flavors of string theory, "bosonic" and "supersymmetric". The first is, true to its name, just the quantized, special-relativistic theory of little loops made of some abstract string stuff that has a certain tension — the "string tension". Classically this theory would make sense in any dimension, but quantum-mechanically, for reasons that I want to explain *someday* but not now, this theory works best in 26 dimensions. Different modes of vibration of the string correspond to different particles, but the theory is called "bosonic" because these particles are all bosons. That's no good for a realistic theory of physics, because the real world has lots of fermions, too. (For a bit about bosons and fermions in particle physics, see "Week 93".)

For a more realistic theory people use "supersymmetric" string theory. The idea here is to let the string be a bit more abstract: it vibrates in "superspace", which has in addition to the usual coordinates some extra "fermionic" coordinates. I don't want to get too technical here, but the basic idea is that while the usual coordinates commute as usual:

$$x_i x_j = x_j x_i$$

the fermionic coordinates "anticommute"

$$y_i y_j = -y_j y i$$

while the bosonic coordinates commute with fermionic ones:

$$x_i y_j = y_j x_i.$$

If you've studied bosons and fermions this will be sort of familiar; all the differences between them arise from the difference between commuting and anticommuting variables. For a little glimpse of this subject try:

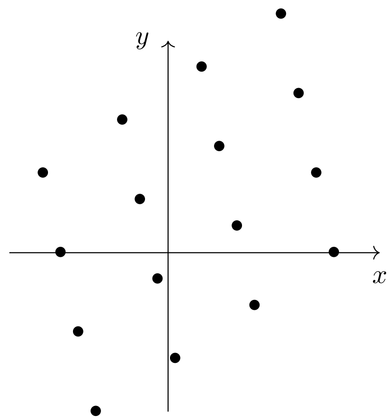
4) John Baez, Spin and the harmonic oscillator, `http://math.ucr.edu/home/baez/harmonic.html`

As it so happens, supersymmetric string theory — often abbreviated to "superstring theory" — works best in 10 dimensions. There are five main versions of superstring theory, which I described in "Week 74". The type I string theory involves open strings — little segments rather than loops. The type IIA and type IIB theories involve closed strings, that is, loops. But the most popular sort of superstring theories are the "heterotic strings". A nice introduction to these, written by one of their discoverers, is:

5) David J. Gross, 'The heterotic string', in *Workshop on Unified String Theories*, eds. M. Green and D. Gross, World Scientific, Singapore, 1986, pp. 357–399.

These theories involve closed strings, but the odd thing about them, which accounts for the name "heterotic", is that vibrations of the string going around one way are supersymmetric and act as if they were in 10 dimensions, while the vibrations going around the other way are bosonic and act as if they were in 26 dimensions!

To get this string with a split personality to make sense, people cleverly think of the 26 dimensional spacetime for the bosonic part as a 10-dimensional spacetime times a little 16-dimensional curled-up space, or "compact manifold". To get the theory to work, it seems that this compact manifold needs to be flat, which means it has to be a torus - a 16-dimensional torus. We can think of any such torus as 16-dimensional Euclidean space "modulo a lattice". Remember, a lattice in Euclidean space is something that looks sort of like this:



Mathematically, it's just a discrete subset L of $\mathbb{R}^n$ (n-dimensional Euclidean space, with its usual coordinates) with the property that if x and y lie in L, so does $jx + ky$ for all integers j and k. When we form n-dimensional Euclidean space "modulo a lattice", we decree two points x and y to be the same if $x - y$ is in L. For example, all the points labelled x in the figure above count as the same when we "mod out by the lattice"... so in this case, we get a 2-dimensional torus.

For more on 2-dimensional tori and their relation to complex analysis, you can read "Week 13". Here we are going to be macho and plunge right into talking about lattices and tori in arbitrary dimensions.

To get our 26-dimensional string theory to work out nicely when we curl up 16-dimensional space to a 16-dimensional torus, it turns out that we need the lattice L that we're modding out by to have some nice properties. First of all, it needs to be an "integral" lattice, meaning that for any vectors x and y in L the dot product $x \cdot y$ must be an integer. This is no big deal — there are gadzillions of integral lattices. In fact, sometimes when people say "lattice" they really mean "integral lattice". What's more of a big deal is that L must be "even", that is, for any x in L the inner product $x \cdot x$ is even. This implies that L is integral, by the identity

$$(x + y) \cdot (x + y) = x \cdot x + 2x \cdot y + y \cdot y.$$

But what's really a big deal is that L must also be "unimodular". There are different ways to define this concept. Perhaps the easiest to grok is that the volume of each lattice cell — e.g., each parallelogram in the picture above — is 1. Another way to say it is this. Take any basis of L, that is, a bunch of vectors in L such that any vector in L can be uniquely expressed as an integer linear combination of these vectors. Then make a matrix with the components of these vectors as rows. Then take its determinant. That should equal plus or minus 1. Still another way to say it is this. We can define the "dual" of L, say L^*, to be all the vectors x such that $x \cdot y$ is an integer for all y in L. An integer lattice is one that's contained in its dual, but L is unimodular if and only if $L = L^*$. So people also call unimodular lattices "self-dual". It's a fun little exercise in linear algebra to show that all these definitions are equivalent.

Why does L have to be an even unimodular lattice? Well, one can begin to understand this a litle by thinking about what a closed string vibrating in a torus is like. If you've ever studied the quantum mechanics of a particle on a torus (e.g. a circle!) you may know that its momentum is quantized, and must be an element of L^*. So the momentum of the center of mass of the string lies in L^*.

On the other hand, the string can also wrap around the torus in various topologically different ways. Since two points in Euclidean space correspond to the same point in the torus if they differ by a vector in L, if we imagine the string as living up in Euclidean space, and trace our finger all around it, we don't necesarily come back to the same point in Euclidean space: the same point *plus* any vector in L will do. So the way the string wraps around the torus is described by a vector in L. If you've heard of the "winding number", this is just a generalization of that.

So both L and L^* are really important here (which has to do with the fashionable subject of "string duality"), and a bunch more work shows that they *both* need to be even, which implies that L is even and unimodular.

Now something cool happens: even unimodular lattices are only possible in certain dimensions — namely, dimensions divisible by 8. So we luck out, since we're in dimension 16.

In dimension 8 there is only *one* even unimodular lattice (up to isometry), namely the wonderful lattice E_8! The easiest way to think about this lattice is as follows. Say you are packing spheres in n dimensions in a checkerboard lattice — in other words, you color the cubes of an n-dimensional checkerboard alternately red and black, and you put spheres centered at the center of every red cube, using the biggest spheres that will fit. There are some little hole left over where you could put smaller spheres if you wanted. And as you go up to higher dimensions, these little holes gets bigger! By the time you get up to dimension 8, there's enough room to put another sphere OF THE SAME SIZE AS THE REST in each hole! If you do that, you get the lattice E_8. (I explained this and a bunch of other lattices in "Week 65", so more info take a look at that.)

In dimension 16 there are only *two* even unimodular lattices. One is $\mathrm{E}_8 \oplus \mathrm{E}_8$. A vector in this is just a pair of vectors in E_8. The other is called D_{16}^+, which we get the same way as we got E_8: we take a checkerboard lattice in 16 dimensions and stick in extra spheres in all the holes. More mathematically, to get E_8 or D_{16}^+, we take all vectors in $\mathbb{R}^8$ or $\mathbb{R}^{16}$, respectively, whose coordinates are either *all* integers or *all* half-integers, for which the coordinates add up to an even integer. (A "half-integer" is an integer plus $1/2$.)

So $\mathrm{E}_8 \oplus \mathrm{E}_8$ and D_{16}^+ give us the two kinds of heterotic string theory! They are often called the $\mathrm{E}_8 \oplus \mathrm{E}_8$ and $\mathrm{SO}(32)$ heterotic theories.

In "Week 63" and "Week 64" I explained a bit about lattices and Lie groups, and if you know about that stuff, I can explain why the second sort of string theory is called "SO(32)". Any compact Lie group has a maximal torus, which we can think of as some Euclidean space modulo a lattice. There's a group called E_8, described in "Week 90", which gives us the E_8 lattice this way, and the product of two copies of this group gives us $\mathrm{E}_8 \oplus \mathrm{E}_8$. On the other hand, we can also get a lattice this way from the group $\mathrm{SO}(32)$ of rotations in 32 dimensions, and after a little finagling this gives us the D_{16}^+ lattice (technically, we need to use the lattice generated by the weights of the adjoint representation and one of the spinor representations, according to Gross). In any event, it turns out that these two versions of heterotic string theory act, at low energies, like gauge field theories with gauge group $\mathrm{E}_8 \times \mathrm{E}_8$ and $\mathrm{SO}(32)$, respectively! People seem especially optimistic that the $E_8 \times E_8$ theory is relevant to real-world particle physics; see for example:

6) Edward Witten, "Unification in ten dimensions", in *Workshop on Unified String Theories*, eds. M. Green and D. Gross, World Scientific, Singapore, 1986, pp. 438–456.

Edward Witten, "Topological tools in ten dimensional physics", with an appendix by R. E. Stong, in *Workshop on Unified String Theories*, eds. M. Green and D. Gross, World Scientific, Singapore, 1986, pp. 400–437.

The first paper listed here is about particle physics; I mention the second here just because E_8 fans should enjoy it — it discusses the classification of bundles with E_8 as gauge group.

Anyway, what does all this have to do with Lucas and his stack of cannon balls?

Well, in dimension 24, there are *24* even unimodular lattices, which were classified by Niemeier. A few of these are obvious, like $E_8 \oplus E_8 \oplus E_8$ and $E_8 \oplus D_{16}^+$, but the coolest one is the "Leech lattice", which is the only one having no vectors of length 2. This is related to a whole WORLD of bizarre and perversely fascinating mathematics, like the "Monster group", the largest sporadic finite simple group — and also to string theory. I said a bit about this stuff in "Week 66", and I will say more in the future, but for now let me just describe how to get the Leech lattice.

First of all, let's think about Lorentzian lattices, that is, lattices in Minkowski spacetime instead of Euclidean space. The difference is just that now the dot product is defined by

$$(x_1, \ldots, x_n) \cdot (y_1, \ldots, y_n) = -x_1 y_1 + x_2 y_2 + \ldots + x_n y_n$$

with the first coordinate representing time. It turns out that the only even unimodular Lorentzian lattices occur in dimensions of the form $8k + 2$. There is only *one* in each of those dimensions, and it is very easy to describe: it consists of all vectors whose coordinates are either all integers or all half-integers, and whose coordinates add up to an even number.

Note that the dimensions of this form: 2, 10, 18, 26, etc., are precisely the dimensions I said were specially important in "Week 93" for some *other* string-theoretic reason. Is this a "coincidence"? Well, all I can say is that I don't understand it.

Anyway, the 10-dimensional even unimodular Lorentzian lattice is pretty neat and has attracted some attention in string theory:

7) Reinhold W. Gebert and Hermann Nicolai, "$E_1 0$ for beginners", preprint available as hep-th/9411188

but the 26-dimensional one is even more neat. In particular, thanks to the cannonball trick of Lucas, the vector

$$v = (70, 0, 1, 2, 3, 4, \ldots, 24)$$

is "lightlike". In other words,

$$v \cdot v = 0.$$

What this implies is that if we let T be the set of all integer multiples of v, and let S be the set of all vectors x in our lattice with $x \cdot v = 0$, then T is contained in S, and S/T is a 24-dimensional lattice — the Leech lattice!

Now *that* has all sorts of ramifications that I'm just barely beginning to understand. For one, it means that if we do bosonic string theory in 26 dimensions on $\mathbb{R}^{26}$ modulo the 26-dimensional even unimodular lattice, we get a theory having lots of symmetries related to those of the Leech lattice. In some sense this is a "maximally symmetric" approach to 26-dimensional bosonic string theory:

8) Gregory Moore, "Finite in all directions", preprint available as hep-th/9305139.

Indeed, the Monster group is lurking around as a symmetry group here! For a physics-flavored introduction to that aspect, try:

9) Reinhold W. Gebert, "Introduction to vertex algebras, Borcherds algebras, and the Monster Lie algebra", preprint available as hep-th/9308151

and for a detailed mathematical tour see:

10) Igor Frenkel, James Lepowsky, and Arne Meurman, *Vertex Operator Algebras and the Monster*, Academic Press, 1988.

Also try the very readable review articles by Richard Borcherds, who came up with a lot of this business:

11) Richard Borcherds, "Automorphic forms and Lie algebras".

Richard Borcherds, "Sporadic groups and string theory".

These and other papers available at `http://www.pmms.cam.ac.uk/Staff/R.E.Borcherds.html`; click on the personal home page.

Well, there is a lot more to say, but I need to go home and pack for my Thanksgiving travels. Let me conclude by answering a natural followup question: how many even unimodular lattices are there in 32 dimensions? Well, one can show that there are AT LEAST 80 MILLION!

Some of you may have wondered what's happened to the "tale of n-categories". I haven't forgotten that! In fact, earlier this fall I finished writing a big fat paper on 2-Hilbert spaces (which are to Hilbert spaces as categories are to sets), and since then I have been struggling to finish another big fat paper with James Dolan, on the general definition of "weak n-categories". I want to talk about this sort of thing, and other progress on n-categories and physics, but I've been so busy *working* on it that I haven't had time to *chat* about it on This Week's Finds. Maybe soon I'll find time.

Addenda: Robin Chapman pointed out that Anglin's proof also appears in the American Mathematical Monthly, February 1990, pp. 120–124, and that another elementary proof has subsequently appeared in the Journal of Number Theory. David Morrison pointed out in email that since the sum

$$1^2 + 2^2 + \ldots + n^2 = m^2$$

is $n(n+1)(2n+1)/6$, this problem can be solved by finding all the rational points (n, m) on the elliptic curve

$$\frac{n^3}{3} + \frac{n^2}{2} + \frac{n}{6} = m^2$$

which is the sort of thing folks know how to do.

Also, here's something Michael Thayer wrote on one of the newsgroups, and my reply:

```
> John Baez wrote:

>> In particular, thanks to the cannonball trick of Lucas,
>> the vector
>>
>>                v = (70,0,1,2,3,4,...,24)
>>
>> is "lightlike".  In other words,
>>
>>                     v.v = 0

> I don't see what is so significant about the vector v.
> For instance, the 10 dimensional vector
> (3,1,1,1,1,1,1,1,1,1) is also light like, and you make
> no big deal about that. Is there some reason why the
> ascending values in v are important?
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Yikes! Thanks for catching that massive hole in the exposition.

You're right that there's no shortage of lightlike vectors in the even unimodular Lorentzian lattices of other dimensions $8n + 2$; there are also lots of other lightlike vectors in the 26-dimensional one. Any one of these gives us a lattice in $8n$-dimensional Euclidean space. In fact, we can get all 24 even unimodular lattices in 24-dimensional Euclidean space by suitable choices of lightlike vector. The lightlike vector you wrote down happens to give us the E_8 lattice in 8 dimensions.

So what's so special about I wrote, which gives the Leech lattice? Of course the Leech lattice is itself special, but what does this have to do with the nicely ascending values of the components of v?

Alas, I don't know the real answer. I'm not an expert on this stuff; I'm just explaining it in order to try to learn it. Let me just say what I know, which all comes from Chap. 27 of Conway and Sloane's book "Sphere Packings, Lattices, and Groups".

If we have a lattice, we say a vector r in it is a "root" if the reflection through r is a symmetry of the lattice. Corresponding to each root is a hyperplane consisting of all vectors perpendicular to that root. These chop space into a bunch of "fundamental regions". If we pick a fundamental region, the roots corresponding to the hyperplanes that form the walls of this region are called "fundamental roots". The nice thing about the fundamental roots is that the reflection through any root is a product of reflections through these fundamental roots.

[For more stuff on reflection groups and lattices see "Week 62" and the following weeks.]

In 1983 John Conway published a paper where he showed various amazing things; this is now Chapter 27 of the above book. First, he shows that the fundamental roots of the even unimodular Lorentzian lattices in dimensions 10, 18, and 26 are the vectors r with $r \cdot r = 2$ and $r \cdot v = -1$, where the "Weyl vector" v is

$$(28, 0, 1, 2, 3, 4, 5, 6, 7, 8)$$

$$(46, 0, 1, 2, 3, \ldots, 16)$$

and

$$(70, 0, 1, 2, 3, \ldots, 70)$$

respectively.

They all have this nice ascending form but only in 26 dimensions is the Weyl vector lightlike!

Howerver, Conway doesn't seem to explain *why* the Weyl vectors have this ascending form. So I'm afraid I really don't understand how all the pieces fit together. All I can say is that for some reason the Weyl vectors have this ascending form, and the fact that the Weyl vector is also lightlike makes a lot of magic happen in 26 dimensions. For example, it turns out that in 26 dimensions there are *infinitely many* fundamental roots, unlike in the two lower dimensional cases.

Just to add mystery upon mystery, Conway notes that in higher dimensions there is no vector v for which all the fundamental roots r have $r \cdot v$ equal to some constant. So the pattern above does not continue.

I find this stuff fascinating, but it would drive me nuts to try to work on it. It's as if God had a day off and was seeing how many strange features he could build into mathematics without actually making it inconsistent.

Yet another addendum (August 2001): now, with the rise of interest in 11-dimensional physics, there is even a paper on E_{11}:

12) P. West, E_{11} and M-theory, available as hep-th/0104081.

Week 96

December 16, 1996

Lots of cool papers have been appearing which I've been neglecting in my attempts to write expository stuff about string theory, lattices, category theory, and all that. It's time to start catching up!

Let me start with the following book:

1) J. Scott Carter, Daniel E. Flath and Masahico Saito, *The Classical and Quantum $6j$-Symbols*, Princeton University Press, Princeton, 1995. ISBN 0-691-02730-7.

Ever since Jones discovered the Jones polynomial invariant of knots, an amazing story has been unfolding about the relation between algebra and 3-dimensional topology. Some key players in this story are the "quantum groups": certain noncommutative algebras analogous to the commutative algebras of functions on groups. In fact, not merely are they analogous, they depend on a parameter, usually called Planck's constant or $\hbar$, and in the classical limit where $\hbar \to 0$ they actually reduce to algebras of functions on familiar groups. The simplest case is "quantum $\mathrm{SU}(2)$", which reduces in the classical limit to the group $\mathrm{SU}(2)$ of 2×2 unitary matrices with determinant 1. Ironically, it's good old "classical $\mathrm{SU}(2)$" that governs the quantum mechanical theory of angular momentum. Quantum $\mathrm{SU}(2)$ was first discovered by people working on physics in 2-dimensional spacetime, where when you quantize certain systems you also need to quantize their group of symmetries!

Nowadays, mathematicians find it simpler to work with the closely related "quantum $\mathrm{SL}(2)$", a quantization of the the group $\mathrm{SL}(2)$ of all 2×2 complex matrices with determinant 1. The above book is largely about quantum $\mathrm{SL}(2)$ and its applications to topology.

All quantum groups give rise to invariants of knots, links, and tangles. They also give rise to 3-dimensional topological quantum field theories of "Turaev-Viro type". This is a kind of quantum field theory you can define on a 3-dimensional spacetime that you've triangulated, i.e., chopped up into tetrahedra. One of the main things you want to compute in a quantum field theory is the "partition function", and we say the Turaev-Viro theories are "topological" because you get the same answer for the partition function no matter how you triangulate the 3-dimensional manifold corresponding to your spacetime: the partition function only depends on the topology of the manifold. The $\mathrm{SU}(2)$ Turaev-Viro theory, the first one to be discovered, is also one of the most interesting because, modulo a few subtle points, this theory is just quantum gravity in 3 dimensions (see "Week 16"). The basic idea, though, is that you compute the partition function by summing over all ways of labelling the edges of your tetrahedra by "spins" $j = 0, 1/2, 1, 3/2, \ldots$. Ponzano and Regge had tried to set up 3-dimensional quantum gravity this way previously, but there were problems getting the sum to converge. The neat thing about the quantum group is that you only sum over spins less than some fixed spin depending on the value of $\hbar$. Since the sums are finite, they automatically converge.

It turns out that in these Turaev-Viro theories you are not actually taking advantage of all the structure of the quantum group. Using the extra structure, you can also use quantum groups to define certain *4-dimensional* topological quantum field theories, those of "Crane-Yetter-Broda" type. Here you triangulate a 4-dimensional manifold and, in the $\mathrm{SU}(2)$ case, you label both the 2d faces the 3d tetrahedra with spins. Actually, lots of people think the Crane-Yetter-Broda theories are boring, because they look sort of boring if you only examine their implications for 4-dimensional topology. However, they become interesting when you realize that, like all topological quantum field theories defined using triangulations, they are "extended topological quantum field theories". Roughly speaking this means that they have implications for all dimensions below the dimension they live in.

In particular, the Crane-Yetter-Broda theories spawn 3-dimensional topological quantum field theories of "Chern-Simons-Reshetikhin-Turaev" type, and most people agree that *these* are interesting. I like to emphasize, however, that a deep understanding of these 3-dimensional progeny requires an understanding of their seemingly innocuous 4-dimensional ancestors. Also, there are a lot of interesting relationships between the $\mathrm{SU}(2)$ Crane-Yetter-Broda model and quantum gravity in 4 dimensions, which we are just beginning to understand. See "Week 56" for a bit about this.

If you haven't yet joined the fun, Carter, Saito, and Flath's book is a great place to start learning about the marvelous interplay between algebra, topology, and physics in 3 and 4 dimensions. Needless to say, it doesn't cover all the ground I've sketched above. Instead, it focuses on a rather specific and concrete aspect: the $6j$ symbols. This should make it especially handy for beginners who aren't familiar with category theory, path integrals, and all that jazz.

What are the $6j$ symbols, anyway? Here I need to get a wee bit more technical. The "classical" $6j$ symbols are important in the representation theory of plain old classical $\mathrm{SU}(2)$, while the "quantum" ones are analogous gadgets applicable to quantum $\mathrm{SU}(2)$. In either case the idea is the same. $\mathrm{SU}(2)$, classical or quantum, has different representations corresponding to different spins $j = 0, 1/2, 1, 3/2, \ldots$. (If you don't know what I mean by this, try "Week 5".) If we take three representations j_1, j_2, and j_3, we can tensor them either like this:

$$(j_1 \otimes j_2) \otimes j_3$$

or like this

$$j_1 \otimes (j_2 \otimes j_3)$$

The tensor product is associative, but that doesn't mean that the above two representations are *equal*. They are only *isomorphic*. This *isomorphism* can be thought of as just a big fat matrix, and the entries in this matrix are a bunch of numbers, the $6j$ symbols.

Turaev and Viro used the quantum $6j$ symbols to define the original Turaev-Viro model. It goes like this: first you chop your 3-dimensional manifold up into tetrahedra, and then you consider all possible ways of labelling the edges with spins. Each tetrahedron gets labelled with 6 spins since it has 6 edges, and from these spins we can compute a number: the $6j$ symbol. Then we multiply all these together, one for each tetrahedron, and finally we sum over labellings to get the partition function. Marvelously, the identities satisfied by the $6j$ symbols are precisely what's needed to make the result independent of the triangulation! See "Week 38" for an explanation of this seeming miracle: it's actually no miracle at all.

2) E. Guadagnini, L. Pilo, "Three-manifold invariants and their relation with the fundamental group", 22 pages in LaTeX available as `hep-th/9612090`.

Fans of topological field theory may like this one, though I must admit I haven't gotten around to doing more than reading the abstract yet. In this paper the authors give evidence for the conjecture that among 3-manifolds M for which the Chern-Simons invariant $\mathrm{CS}(M)$ is nonzero, the absolute value $|\mathrm{CS}(M)|$ only depends on the fundamental group of M. Chern-Simons theory depends on a choice of group; they prove the conjecture for certain manifolds ("lens spaces") when the group is $\mathrm{SU}(2)$, and give numerical evidence when the gauge group is $\mathrm{SU}(3)$.

What's interesting about this to me is that $|\mathrm{CS}(M)|^2$ is just the Turaev-Viro theory partition function, so this conjecture is saying that the Turaev-Viro theories discussed above have a tendency to notice only the fundamental group.

3) Michael Reisenberger and Carlo Rovelli, " 'Sum over surfaces' form of loop quantum gravity", preprint available as gr-qc/9612035.

This wonderful paper should really push forwards our understanding of the loop representation of quantum gravity. I talked a little bit about the basic idea in "Week 86". In the loop representation, a state of quantum gravity at a given moment is represented by a bunch of knotted loops or "spin networks" in space. What's the spacetime picture? Well, if you have a surface in spacetime and look at it at one moment of time, it typically looks like a bunch of loops… so maybe the spacetime picture of quantum gravity is that spacetime is packed with 2-dimensional surfaces, all tangled up. Interestingly, this is also very reminiscent of the picture of quantum gravity in string theory!

I've been working on this sort of idea ever since I wrote a paper suggesting that the loop representation and string theory might be two faces of the same ideas (see "Week 18"). Since then, most of the time I've been trying to understand how these ideas relate to the Crane-Yetter-Broda theories, and trying to set up the necessary *algebra* (n-category theory) to deal nicely with surfaces in 4-dimensional spacetime.

But there are many other angles from which one can attack this problem, and one of the best is to start directly from Einstein's equations for general relativity, try to quantize them using the path-integral approach, and see how the path integral can be written as a sum over surfaces. Reisenberger has already begun work on this in the context of "simplicial quantum gravity" — where you chop spacetime up into the 4-dimensional analog of tetrahedra. But during the Vienna workshop on canonical quantum gravity this summer, we talked about a different, still more direct approach (see "Week 89"). The idea is to copy standard quantum field theory, write the propagator describing time evolution as a time-ordered exponential, and interpret the terms in the resulting sum as surfaces in spacetime. It's all very analogous to traditional Feynman diagrams, where you write the propagator as a sum over diagrams, but now the "Feynman diagrams" are 2-dimensional surfaces. (Again, this is reminiscent of string theory — but with many important differences.)

There is much more to say, but I think I'll leave it at that…. Over in the world of n-categories there is also some very interesting stuff happening, which I will discuss more next week. I'm almost done writing a paper with James Dolan on the definition of n-categories, but in the meantime some other folks have been coming up with other definitions of n-categories, so we will soon be in the position to compare definitions and see how similar or different they are, and start erecting the formalism needed to deal with all these topological quantum field theories and "sums over surfaces" in a really elegant way! Everything looks like its fitting together. At least, that's my momentary optimistic feeling. Perhaps it's just the fact that classes are over that is making me so happy. Yes, it's probably just that.

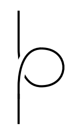
Week 97

February 8, 1997

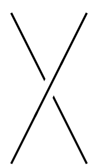
I've taken a break from writing This Week's Finds in order to finish up a paper with James Dolan in which we give a definition of "weak n-categories" for all n. This paper is now available on my website, and I'm immodestly eager to talk about it, and I will, but a lot of stuff has accumulated in the meantime which I want to discuss first.

First, I'm sure you remember a while back when atoms were first coaxed to form true Bose-Einstein condensates. The basic idea is that particles come in two basic kinds, fermions and bosons, and while the fermions have half-integer spin and obey the Pauli exclusion principle saying that no two identical fermions can be in the same state at the same time, bosons have integer spin and are gregarious: they *love* to be in the same state at the same time.

Why is spin related to what happens when you try to put a bunch of particles in the same state? Well, it all has to do with the relation between twisting something around:



and switching two things:



To understand this, try

1) Spin, statistics, CPT and all that jazz, `http://math.ucr.edu/home/baez/spin.stat.html`

But let's consider some examples. Since photons have spin 1 they are bosons. In laser light one has a bunch of photons all in the same state. Thanks to the Heisenberg uncertainty principle, of course, we can't know both their position and momentum. In a laser we don't know the position of the photons: each photon is all over the laser beam in a spread-out sort of way. However, we do know the momentum of the photons and they all have the same momentum. This means that we have "coherent light" in which all the photons are like waves wiggling perfectly in phase. One can demonstrate this by interfering two beams of laser light and seeing beautifully perfect interference fringes, bright and dark stripes in places where the two beams are either in phase with each other and adding up, or out of phase and cancelling out.

Now, other particles are bosons as well, and they can do similar tricks. Bose and Einstein predicted that when any gas of noninteracting bosons gets sufficiently cold, all — or at least a sizeable fraction — of them will be found in the same state: the state of least possible energy. Unfortunately, when things get cold they are usually liquids or solids rather than a gas, and the particles in a liquid or gas interact a lot, so true Bose-Einstein condensation is hard to achieve.

Some related things have been studied for decades. If you get an even number of fermions together they act approximately like a boson, at least if the density is not too high. Helium stays liquid at temperatures arbitrarily close to absolute zero, when the pressure is low enough. Since helium 4 has 2 protons, 2 neutrons, and 2 electrons, and all these particles are fermions, helium 4 acts like a boson. At really low temperatures, helium 4 becomes "superfluid" — a substantial fraction of the atoms fall into the same state and the liquid acquires shocking properties, like zero viscosity. Similarly, in certain metals at low temperatures electrons will, by a subtle mechanism, form "Cooper pairs", and these act like bosons. When a bunch of these fall into the same state, you have a "superconductor".

But neither of these is a Bose-Einstein condensate in the technical sense of the term, because the helium atoms interact a lot in superfluid helium, and the Cooper pairs interact a lot in a superconductor. Only recently have people been able to get dilute gases of bosonic atoms cold enough to study true Bose-Einstein condensation.

The fist team to do it, the "JILA" team in Boulder, Colorado got a Bose-Einstein condensate of about 2000 rubidium atoms to form in a magnetic trap at less than 2×10^{-7} degrees above absolute zero. A team at Rice University did it with lithium soon after, followed by a team at MIT, who did it with sodium.

Check out:

2) Physicists create new state of matter, `http://jilav1.colorado.edu/www/bose-ein.html`

   Atomcool home page, `http://atomcool.rice.edu/`

   Neutral sodium ion trap at MIT, `http://bink.mit.edu/dallin/nat.html`

So what's the news? Well, recently the team at MIT, led by Wolfgang Ketterle, made two blobs of Bose-Einstein condensate out of sodium atoms. Ramming these into each other, they were able to see interference fringes just as in a laser! In other words, they is seeing interference of matter waves, just as quantum mechanics predicts, but involving lots of atoms in a coherent state rather than a single electron as in the famous double slit experiment. For pictures and even movies, try:

3) Matter-wave interference of two Bose condensates, `http://bink.mit.edu/dallin/news.html#matterwave`

In honor of this event, I hereby present the following limerick composed by the poet Lisa Raphals, with myself serving as science consultant. It may aid your appreciation if I note first that "Squantum" is an actual town in Massachusetts. With no further ado:

> *A metaphysician from Squantum Was asked, what's the state of the quantum?*
> *It's all reciprocity: Position, velocity — They're never both there when you want 'em!*

Now on to some more technical stuff. . . .

I am now visiting the Center for Gravitational Physics and Geometry here at Penn State, which is a delightful place for people interested in the loop representation of quantum gravity (see "Week 77"). Right now everyone is working on using the loop representation to derive Hawking's formula which says that the entropy of a black hole is proportional to the surface area of its event horizon.

When I arrived, Jorge Pullin handed me a copy of his book:

4) Rodolfo Gambini and Jorge Pullin, *Loops, knots, gauge theories, and quantum gravity*, Cambridge U. Press, Cambridge, 1996.

Here is the table of contents:

1. Holonomies and the group of loops
2. Loop coordinates and the extended group of loops
3. The loop representation
4. Maxwell theory
5. Yang-Mills theories
6. Lattice techniques
7. Quantum gravity
8. The loop representation of quantum gravity
9. Loop representation: further developments
10. Knot theory and physical states of quantum gravity
11. The extended loop representation of quantum gravity
12. Conclusions, present status and outlook

This is presently the most complete introduction available to the "loop representation" concept, as applied to electromagnetism, Yang-Mills theory, and quantum gravity. Gambini was one of the original inventors of this notion, and this book covers the whole sweep of its ramifications, with a special emphasis on a particular technical form, the "extended loop representation", which he has been developing with Pullin and other collaborators.

What the heck is the loop representation, anyway? Well, all the forces we know are described by gauge theories, and gauge theories all describe the "phase", or generalization thereof, that a particle acquires when you carry it around a loop. In the case of electromagnetism, for example, a charged quantum particle carried around a loop in space acquires a phase equal to

$$\exp(-iqB/\hbar)$$

where q is the particle's charge, $\hbar$ is Planck's constant, and B is the magnetic flux through the loop: i.e., the integral of the magnetic field over any surface spanning the loop. Knowing these phase for all loops is the same as knowing the magnetic field. Similarly, if we knew the phase for all loops in SPACETIME instead of just space, we would know both the electric and magnetic fields throughout spacetime.

General relativity is similar except that instead of a phase one gets a rotation, or more generally a Lorentz transformation, when one parallel transports a little arrow around a loop.

The theories of the electroweak and strong forces are similar but the analog of the "phase" is a bit more abstract: an element of the group $\mathrm{SU}(2) \times \mathrm{U}(1)$ or $\mathrm{SU}(3)$, respectively.

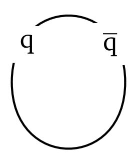
The idea of the loop representation is to take these "phases acquired around loops" as basic variables for describing the laws of physics.

That's the idea in a nutshell. It turns out, not surprisingly, that there are many interesting relationships with such topics involving loops, such as string theory and knot theory.

Gambini and Pullin's book develops this theme in many directions. Let me say a bit about one fascinating topic that they mention, which I would like to understand better: Gerard 't Hooft's work on confinement in chromodynamics using his "order and disorder operators".

I explained some basic ideas about confinement and asymptotic freedom in "Week 84" and "Week 94", so I'll assume you've read that stuff. Remember, the basic idea of confinement is that if you take a meson and try to pull the quark and antiquark it contains apart, the force required does not decrease with distance like $1/r^2$, because the chromoelectric field — the strong force analog of the electric field — does not spread out in all directions like an ordinary electric field does. Instead, all the field lines are confined to a "flux tube", so the force is roughly independent of the distance.

This means that the energy is roughly proportional to the distance. Since action has dimensions of energy times time, this means that if we consider the creation and subsequent annihilation of a virtual quark-antiquark pair:



the total action is proportional to the *area* of the loop traced out in spacetime. Here I am neglecting the action due to the kinetic energy of the quark and antiquark, and only worrying about the potential energy due to the flux tube joining them. This amounts to treating the quark and antiquark as "test particles" to study the behavior of the strong force.

Now, when we study quantum physics using Euclidean path integrals the basic principle is that the probability of the occurence of any process is proportional to

$$\exp(-S/\hbar)$$

where S is the action of that process and $\hbar$ is Planck's constant again. So in this framework the *probability* of a particular virtual quark-antiquark pair tracing out a loop like the above one is proportional to

$$\exp(-cA)$$

where $c > 0$ is some constant and A is the area of the loop. This "area law" was first proposed by Kenneth Wilson in his pioneering work on confinement; he proposed it as a way to tell, mathematically, if confinement was happening in some theory. Just compute the probability of a virtual quark-antiquark pair tracing out a particular loop and see if it decreases exponentially with the area!

Deriving confinement from chromodynamics is something that people have worked on for quite a while, and it's not easy: there is still no rigorous proof, even though there are a bunch of heuristic arguments for it, and computer simulations seem to demonstrate that it's bound to occur. One approach to studying the puzzle is due to 't Hooft and involves "order" and "disorder" operators.

I'll explain what these are, and what they have to do with knot theory, but not how 't Hooft actually uses them in his argument for confinement. For the actual argument, try Gambini and Pullin's book, or else 't Hooft's paper:

5) Gerard 't Hooft, *Nucl. Phys.* **B138**, (1978) 1.

Let us work in space at a given time, rather than in the Euclidean path integral approach. We'll do "canonical quantization", meaning that now observables will be operators on some Hilbert space.

If we have any loop g in space, there is an observable called the "Wilson loop" $W(g)$, which is the trace of the holonomy of the connection around g. The precise way of stating Wilson's area law for confinement in this context is that

$$\langle W(g)\rangle \sim \exp(-cA)$$

where $\langle W(g)\rangle$ is the vacuum expectation value of the Wilson loop, and A is the area spanned by the loop g. The point is that $\langle W(g)\rangle$ is the same as what I was (a bit sloppily) calling the probability of the quark-antiquark pair tracing out the loop g.

't Hooft calls the Wilson loops "order operators". We don't really need to worry why he calls them this, but if you know how physicists think, you may know that the Wilson loops are keeping track of a kind of "order parameter" of the vacuum state. Anyway, his idea was to study the Wilson loops by introducing some other operators he called "disorder operators".

Chromodynamics is an $\mathrm{SU}(3)$ gauge theory but it's a little clearer if we work with any $\mathrm{SU}(N)$ gauge theory. Notice that the center of the group $\mathrm{SU}(N)$ consists of the matrices of the form

$$\exp(2\pi i n/N)$$

where n is an integer. So if we have a loop h, we can imagine an operator that does the following thing: it modifies the connection, or vector potential, in such a way that if you do parallel transport around a tiny loop linking h once, the holonomy changes to $\exp(2\pi i/N)$ times what it had been. Note: this is a gauge-invariant thing to do, because that $\exp(2\pi i/N)$ is in the center of $\mathrm{SU}(N)$! So just as the Wilson loop observables are gauge-invariant, we can hope for some some "disorder operators" $V(h)$ that modify the connection in this way.

If you think about it, what this means is that the following commutation relations hold:

$$W(g)V(h) = V(h)W(g)\exp(2\pi i L(g,h)/N)$$

where $L(g, h)$ is the linking number of the loops g and h, which counts how many times g wraps around h.

There is an obvious symmetry or "duality" between the V's and the W's going on here. In fact, just as W's satisfy an area law where there is confinement of chromoelectric field lines into flux tubes, I believe the V's satisfy an area law when there is confinement of chromomagnetic field lines into flux tubes. The simplest case of this kind of thing occurs in plain old electromagnetism, where plain old magnetic field lines are confined into flux tubes in type II superconductors. For this reason confinement of electric field lines is sometimes called "dual superconductivity".

Perhaps the simplest way of beginning to understand this stuff more deeply is to understand the wonderful formula proved by Ashtekar and Corichi in the following paper:

6) Abhay Ashtekar and Alejandro Corichi, "Gauss linking number and electro-magnetic uncertainty principle", preprint available as hep-th/9701136.

This formula applies to plain old electromagnetism, or more precisely, quantum electrodynamics. If we work in units where $\hbar = 1$, and consider a particle of charge 1, the Wilson loop operator $W(g)$ in electromagnetism is just

$$W(g) = \exp(-iB(g))$$

where B is the magnetic flux flowing through the loop g. But instead we can just work with $B(g)$ directly. Similarly, instead of $V(h)$'s we can work with the operator $E(h)$ corresponding to the electric flux through the loop h. Then we have

$$B(g)E(h) - E(h)B(g) = iL(g, h).$$

In other words, the electric and magnetic fields don't commute in quantum electrodynamics, and the Heisenberg uncertainty of the electric field flowing through a loop g and the magnetic field flowing through a loop h is proportional to the linking number of g and h!

Quantum mechanics, electromagnetism, and knot theory are clearly quite tangled up here. Since the linking number was first discovered by Gauss in his work on magnetism, it's all quite fitting.

And that leads me to the last paper I want to mention this week. It should be of great interest to Vassiliev invariant fans; see "Week 3" if you don't know what a Vassiliev invariant is.

7) Dror Bar-Natan and Alexander Stoimenow, "The fundamental theorem of Vassiliev invariants", preprint available as q-alg/9702009.

Let me just quote the abstract here:

The "fundamental theorem of Vassiliev invariants" says that every weight system can be integrated to a knot invariant. We discuss four different approaches to the proof of this theorem: a topological/combinatorial approach following M. Hutchings, a geometrical approach following Kontsevich, an algebraic approach following Drinfel'd's theory of associators, and a physical approach coming from the Chern-Simons quantum field theory. Each of these approaches is unsatisfactory in one way or another, and hence we argue that we still don't really understand the fundamental theorem of Vassiliev invariants.

Week 98

February 27, 1997

I feel guilty for slacking off on This Week's Finds, so I should explain the reason. Lots of things have been building up that I'm dying to talk about, but new ones keep coming in at such a rapid rate that I never feel I have time!

I will have to be ruthless, and face up to the fact that a quick and imperfect exposition is better than none.

First of all, here at the Center for Gravitational Physics and Geometry there are a lot of interesting attempts going on to compute the entropy of black holes from first principles. Bekenstein, Hawking and many others have used a wide variety of semiclassical arguments to argue that black holes satisfy

$$S = A/4$$

where S is the entropy and A is the area of the event horizon, both measured in Planck's units, where $G = c = \hbar = 1$.

For example, using purely classical reasoning (general relativity, but no quantum theory) one can prove the "2nd law of black hole thermodynamics", which says that A always increases. As Bekenstein noted, this suggests that the area of the event horizon is somehow analogous to entropy. However, by itself this does not determine the magic number $1/4$, which can only be derived using quantum theory (as one can see by simple dimensional analysis).

By semiclassical reasoning — studying quantum electrodynamics in the Schwarzschild metric used to describe black holes — Hawking showed that black holes should radiate as if they had a temperature inversely proportional to their mass:

$$T = \frac{1}{8\pi M}.$$

This made the analogy between entropy and event horizon area much more than an analogy, because it meant that one could assign a temperature to black holes and see if they satisfy the laws of thermodynamics. It turns out that if you consider $A/4$ to be the entropy of a black hole, you can eliminate seeming violations of the 2nd law that otherwise arise in thought experiments where you get rid of entropy by throwing it into a black hole. In other words, if you throw something with entropy S into a black hole, calculations seem to show that the area of the event horizon always increases by at least $4S$!

So far nothing I've said is related to full-fledged quantum gravity, because in the semiclassical arguments one is still working in the approximation where the gravitational field is treated classically. An interesting test of any theory of quantum gravity is whether can use it to derive $S = A/4$. In a subject with no real experimental evidence, this is the closest we have to an "experimental result" that our theory should predict.

Recently the string theorists have done some calculations claiming to show that string theory predicts $S = A/4$. Personally I feel that while these calculations are interesting they are far from definitive. For example, they all involve taking calculations done using perturbative string theory on *flat* spacetime and extrapolating them drastically to the regime in which string theory approximates general relativity. One typically uses ideas from supersymmetry to justify such extrapolations; however, these ideas only seem to apply to "extremal black holes", having the maximum possible charge for a black hole of a given mass and angular momentum. Realistic black holes are far from extremal. In short, while exciting, these calculations still need to be taken with a grain of salt.

Of course, I am biased because I am interested in another approach to quantum gravity, the loop representation of quantum gravity, which folks are working on here at the CGPG, among other places. This is in many ways a more conservative approach. The idea is to simply take Einstein's equation for general relativity and quantize it, rather than trying to develop a theory of *all* particles and forces as in string theory. Of course, for various reasons it is not so easy to quantize Einstein's equation. String theorists think it's *impossible* without dragging in all sorts of other forces and particles, but folks working on the loop representation are more optimistic. This is an ongoing argument, but certainly a good test of the loop representation would be to try to use it to derive Hawking's formula S = A/4. If the loop representation is really any good, this should be possible, because many different lines of reasoning using only general relativity and quantum theory lead to this formula.

I've already mentioned a few attempts to do this in "Week 56", "Week 57", and "Week 87". These were promising, but they didn't get the magical number $1/4$. Also, they are rather rough, in that they do computations on some region with boundary, but don't use anything that ensures the boundary is an event horizon.

Recently Kirill Krasnov has made some progress:

1) Kirill Krasnov, "On statistical mechanics of Schwarzschild black hole", preprint available as gr-qc/9605047.

This paper still doesn't get the magic number $1/4$, and Krasnov later realized it has a few mistakes in it, but it does something very cool. It notes that the boundary conditions holding on the event horizon of a Schwarzschild black hole are closely related to Chern-Simons theory. Now is not the time for me to go into Chern-Simons theory, but basically, it lets you apply a lot of neat mathematics to calculate everything to your heart's content, very much as Carlip did on his work on the toy model of a 2+1-dimensional black hole (see "Week 41"). Also, it sheds new light on the relationship between topological quantum field theory and quantum gravity, something I am always trying to understand better.

While I'm at it, I should note the existence of a paper that reworks Carlip's calculation from a slightly different angle:

2) Maximo Banados and Andres Gomberoff, "Black hole entropy in the Chern-Simons formulation of 2+1 gravity", preprint available as gr-qc/9611044.

2+1-dimensional quantum gravity is very simple compared to the 3+1-dimensional quantum gravity we'd really like to understand: in a sense it's "exactly solvable". But there are still some puzzling things about Carlip's computation of the entropy of a black hole in 2+1 dimensions which need figuring out, so every paper on the subject is worth looking at, if you're interested in black hole entropy.

Speaking of topological quantum field theory and quantum gravity, I just finished a paper on these topics:

3) John Baez, "Degenerate solutions of general relativity from topological field theory", preprint available as gr-qc/9702051 or in Postscript form at `http://math.ucr.edu/home/baez/deg.ps`.

Let me just summarize the basic idea, resisting the temptation to become insanely technical.

A while ago Rovelli and Smolin introduced Penrose's notion of "spin network" into the loop representation of quantum gravity. I described spin networks pretty carefully in "Week 43", but here let me just say that they are graphs embedded in space with edges labelled by spins $j = 0, 1/2, 1, 3/2, \ldots$, just as in the quantum mechanics of angular momentum, and with vertices labelled by "intertwining operators", which are other gadgets that come up in the study of angular momentum. In the loop representation these spin networks form a basis of states. Geometrical observables like the area of surfaces and the volumes of regions have been quantized and their matrix elements computed in the spin network basis, giving us a nice picture of "quantum 3-geometries", that is, the possible geometries of space in the context of quantum gravity. In this picture, the edges of spin networks play the role of quantized flux tubes of area, much as the magnetic field comes in quantized flux tubes in a type II superconductor. To work out the area of a surface in some spin network state, you just total up contributions from each edge of the spin network that pokes through the surface. An edge labelled with spin j carries an area equal to $\sqrt{j(j+1)}$ times the Planck length squared. What's cool is that this is not merely postulated, it's derived from fairly standard ideas about how you turn observables into operators in quantum mechanics.

However, the dynamics of quantum gravity is more obscure. Technical issues aside, the main problem is that while we have a nice picture of quantum 3-geometries, we don't have a similar picture of the *4-dimensional*, or *spacetime*, aspects of the theory. To represent a physical state of quantum gravity, a spin network state (or linear combination thereof) has to satisfy something called the Wheeler-DeWitt equation. This is sort of the quantum gravity analog of the Schrodinger equation. There is a lot of controversy over the Wheeler-DeWitt equation and what's the right way to write it down in the loop representation. The really annoying thing, however, is that even if you feel you know how to write it down — for example, Thomas Thiemann has worked out one way (see "Week 85") — and can find solutions, you still don't necessarily have a good intuition as to what the solutions *mean*. For example, almost everyone seems to agree that spin networks with no vertices should satisfy the Wheeler-DeWitt equation. These are just knots or links with edges are labelled by spins. We know these states are supposed to represent "quantum 4-geometries" satisfying the quantized Einstein equations. But how should we visualize these states in 4-dimensional terms?

In search of some insight into the 4-dimensional interpretation of these states, I turn to classical general relativity. In my paper, I construct solutions of the equations of general relativity which at a typical fixed time look like "flux tubes of area" reminiscent of the loop states of quantum gravity. These are "degenerate solutions", meaning that the "3-metric", the tensor you use to measure distances in 3-dimensional space, is zero in lots of regions of space. Here I should warn you that ordinary general relativity doesn't allow degenerate metrics like this. The loop representation works with an extension of general relativity that covers the case of degenerate metrics; for more on this, see "Week 88".

More precisely, if you look at these "flux tube" solutions at a typical time, the 3-metric vanishes outside a collection of solid tori embedded in space, while inside any of these solid tori the metric is degenerate in the longitudinal direction, but nondegenerate in the two transverse directions.

Now since these are classical solutions — no quantum theory in sight! — there is no problem with understanding what they do as time passes. We can solve Einstein's equation and get a 4-metric, a metric on spacetime. The 4-dimensional picture is as follows: given any surface Σ embedded in spacetime, I get solutions for which the 4-metric vanishes outside a neighborhood of Σ. Inside this neighborhood, the 4-metric is zero in the two directions tangent to Σ but nondegenerate in the two transverse directions. In the 4-geometry defined by one of these solutions, the area of a typical surface Σ' intersecting Σ in some isolated points is a sum of contributions from the points where Σ and Σ' intersect.

The solutions I study are inspired by the work of Mike Reisenberger, who studied a solution for which the metric vanishes outside a neighborhood of a sphere embedded in $\mathbb{R}^4$. I consider more general surfaces embedded in more general 4-manifolds, so I need to worry a lot more about topological issues. Also, I allow the possibility of a nonzero cosmological constant (this being a parameter in Einstein's equation that determines the energy density of the vacuum). A lot of the most interesting stuff happens for nonzero cosmological constant, and this case actually helps one understand the case of vanishing cosmological constant as a kind of limiting case.

It turns out that the interesting degrees of freedom of the metric living on the surface Σ in spacetime are described by fields living on this surface. In fact, these fields are solutions of a 2-dimensional topological field theory called BF theory. To prove this, I take advantage of the relation between general relativity and BF theory in 4 dimensions, together with the fact that BF theory behaves in a simple manner under dimensional reduction.

Another neat thing is that to get a solution of general relativity this way, we need to pick a "framing" of Σ. Roughly speaking, this means we need to pick a way of thickening up the surface Σ to a neighborhood that looks like $\Sigma \times D^2$, where D^2 is the 2-dimensional disc. This is precisely the 4-dimensional analog of a framing of a knot or link in 3-dimensions. People who know about topological quantum field theory know that framings are very important. In fact, I can show that my solutions of general relativity are closely related to Chern-Simons theory, a 3-dimensional topological field theory famous for giving invariants of framed knots and links. What's beginning to emerge is a picture that makes the *spacetime* aspects of framings easier to understand.

Now before I plunge into some even more esoteric stuff, let me briefly return to reality and answer the question you've all been secretly dying to ask: how does general relativity impact the world of big business?

In plain terms: is all this fancy physics just an excuse to have fun visualizing evolving spin networks in terms of quantum field theories on surfaces embedded in 4-dimensional spacetime, etcetera etcetera... or does it actually contribute to the well-being of the corporations upon which we depend?

Well, you may be surprised to know that general relativity plays an significant role in a $200-million business. Surprised? Read on! What follows is taken from the latest issue of "Matters of Gravity", the newsletter put out by Jorge Pullin. More precisely, it's from:

4) Neil Ashby, "General relativity in the global positioning system", in *Matters of Gravity*, ed. Jorge Pullin, no. **9**, available at `http://www.phys.lsu.edu//mog/mog9/node9.html`.

I will simply quote some excerpts from this fascinating article:

"The Global Position System (GPS) consists of 24 earth-orbiting satellites, each carrying accurate, stable atomic clocks. Four satellites are in each of six different orbital planes, of inclination 55 degrees with respect to earth's equator. Orbital periods are 12 hours (sidereal), so that the apparent position of a satellite against the background of stars repeats in 12 hours. Clock-driven transmitters send out synchronous time signals, tagged with the position and time of the transmission event, so that a receiver near the earth can determine its position and time by decoding navigation messages from four satellites to find the transmission event coordinates, and then solving four simultaneous one-way signal propagation equations. Conversely, γ-ray detectors on the satellites could determine the space-time coordinates of a nuclear event by measuring signal arrival times and solving four one-way propagation delay equations.

Apart possibly from high-energy accelerators, there are no other engineering systems in existence today in which both special and general relativity have so many applications. The system is based on the principle of the constancy of c in a local inertial frame: the Earth-Centered Inertial or ECI frame. Time dilation of moving clocks is significant for clocks in the satellites as well as clocks at rest on earth. The weak principle of equivalence finds expression in the presence of several sources of large gravitational frequency shifts. Also, because the earth and its satellites are in free fall, gravitational frequency shifts arising from the tidal potentials of the moon and sun are only a few parts in 10^16 and can be neglected.

[. . .]

At the time of launch of the first NTS-2 satellite (June 1977), which contained the first Cesium clock to be placed in orbit, there were some who doubted that relativistic effects were real. A frequency synthesizer was built into the satellite clock system so that after launch, if in fact the rate of the clock in its final orbit was that predicted by GR, then the synthesizer could be turned on bringing the clock to the coordinate rate necessary for operation. The atomic clock was first operated for about 20 days to measure its clock rate before turning on the synthesizer. The frequency measured during that interval was +442.5 parts in 10^{12} faster than clocks on the ground; if left uncorrected this would have resulted in timing errors of about 38,000 nanoseconds per day. The difference between predicted and measured values of the frequency shift was only 3.97 parts in 10^{12}, well within the accuracy capabilities of the orbiting clock. This then gave about a 1% validation of the combined motional and gravitational shifts for a clock at 4.2 earth radii.

[. . .]

> *This system was intended primarily for navigation by military users having access to encrypted satellite transmissions which are not available to civilian users. Uncertainty of position determination in real time by using the Precise Positioning code is now about 2.4 meters. Averaging over time and over many satellites reduces this uncertainty to the point where some users are currently interested in modelling many effects down to the millimeter level. Even without this impetus, the GPS provides a rich source of examples for the applications of the concepts of relativity.*
>
> *New and surprising applications of position determination and time transfer based on GPS are continually being invented. Civilian applications include for example, tracking elephants in Africa, studies of crustal plate movements, surveying, mapping, exploration, salvage in the open ocean, vehicle fleet tracking, search and rescue, power line fault location, and synchronization of telecommunications nodes. About 60 manufacturers now produce over 350 different commercial GPS products. Millions of receivers are being made each year; prices of receivers at local hardware stores start in the neighborhood of $200."*

Pretty cool, eh?

Okay, now for something completely different — homotopy theory! Well, everything I write about is actually secretly part of my grand plan to see how everything interesting is related to everything else, but let me not delve into how homotopy theory is related to topological quantum field theory and thus quantum gravity. Let me simply mention the existence of this great book:

5) *Handbook of Algebraic Topology*, ed. I. M. James, North-Holland, the Netherlands, 1995, 1324 pages.

Occasionally you come across a book that you wish you just download into your brain; for me this is one of those books. It is probably not a good idea to read it if you are just wanting to get started on algebraic topology; it assumes you are pretty familiar with the basic ideas already, and it goes into a lot of depth, mainly in hardcore homotopy theory. A lot of it is too technical for me to appreciate, but let me list a few chapters that I can understand and like.

- Chapter 1, "Homotopy types" by Hans-Joachim Baues, is a great survey of different models of homotopy types. Remember, we say two topological spaces X and Y are homotopy equivalent if there are continuous functions $f\colon X \to Y$ and $g\colon Y \to X$ that are inverses "up to homotopy". In other words, we don't require that fg and gf are *equal* to identity functions, but merely that they can both be *continuously deformed* to identity functions. So for example the circle and an annulus are homotopy equivalent, and we say therefore that they represent the same "homotopy type".

  The cool thing is that there turn out to be very elegant algebraic and combinatorial ways of describing homotopy types that don't mention topology at all. Perhaps the most beautiful way of all is a way that in a sense hasn't been fully worked out yet: namely, thinking of homotopy types as "ω-groupoids". The idea is this. An "ω-category" is something that has

- objects like x
- morphisms between objects like $f\colon x \to y$
- 2-morphisms between morphisms like $F\colon f \to g$
- 3-morphisms between 2-morphisms like $T\colon F \to G$
- ...

and so on ad infinitum. There should be some ways of composing these, and these should satisfy some axioms, and that of course is the tricky part. But the basic idea is that if you hand me a topological space X, I can cook up an ω-category whose

- objects are points in X
- morphisms are paths between points in X
- 2-morphisms are continuous 1-parameter families of paths in X, i.e. "paths of paths" in X
- 3-morphisms are "paths of paths of paths" in X
- ...

and so on. This is better than your garden-variety ω-category because all the morphisms and 2-morphisms and 3-morphisms and so on have inverses, at least "up to homotopy". We call it an "ω-groupoid". This ω-groupoid keeps track of the homotopy type of X in a very nice way. (If this "ω" stuff is too mind-boggling, you may want to start by reading a bit about plain old categories and groupoids in "Week 74".)

Conversely, given any ω-groupoid there should be a nice way to cook up a homotopy corresponding to it. This is just the infinite-dimensional generalization of something I described in "Week 75". There, I showed how you could get a groupoid from a "homotopy 1-type" and vice versa. Here there 1-morphisms but no interesting 2-morphisms, 3-morphisms, and so on, because the topology of a "homotopy 1-type" is boring in dimensions greater than 1. (In case any experts are reading this, what I mean is that its higher homotopy groups are trivial; its higher homology and cohomology groups can be very interesting.)

So we can — and should — think of homotopy theory as, among other things, the study of ω-groupoids, and thus a very useful warmup to the study of ω-categories. In my occasional series on This Week's Finds called "the tale of n-Categories", I have tried to explain why n-categories, and ultimately ω-categories, should serve as a powerful unifying approach to lots of mathematics and physics. In trying to understand this subject, I find time and time again that homotopy theorists are the ones to listen to.

- Chapter 2, "Homotopy theories and model categories", by W. G. Dwyer and J. Spalinski, is a nice introduction to the formal idea of using different "models" for homotopy types. For example, above I was sketching how one might do homotopy theory using the "model category" of ω-groupoids. Other model categories include gadgets like Kan complexes, CW complexes, simplicial complexes, and so on.

- Chapter 6, "Modern foundations for stable homotopy theory", by A. D. Elmendorf, I. Kriz, M. Mandell and J. P. May describes a very nice approach to spectra. Loosely speaking, we can think of a spectrum as a $\mathbb{Z}$-groupoid, where $\mathbb{Z}$ denotes the integers. In other words, in addition to j-morphisms for all natural numbers j, we also have j-morphisms for negative j! This may seem bizarre, but it's a lot like how in homology theory one is interested in chain complexes that extend in both the positive and negative directions. In fact, we can think of a chain complex as a very special sort of $\mathbb{Z}$-groupoid or spectrum. The study of spectra is called stable homotopy theory.

- Chapter 13, "Stable homotopy and iterated loop spaces", by G. Carlsson and R. J. Milgram, is packed with handy information about stable homotopy theory.

- Chapter 21, "Classifying spaces of compact Lie groups and finite loop spaces", by D. Notbohm, is a good source of heavy-duty information on classifying spaces of your favorite Lie groups. To study vector bundles and the like one really needs to become comfortable with classifying spaces, and I'm finally doing this, and I hope eventually I'll be comfortable enough with them to really understand all these results.

There is a lot more, but I will stop here.

Week 99

March 15, 1997

Life here at the Center for Gravitational Physics and Geometry is tremendously exciting. In two weeks I have to return to U. C. Riverside and my mundane life as a teacher of calculus, but right now I'm still living it up. I'm working with Ashtekar, Corichi, and Krasnov on computing the entropy of black holes using the loop representation of quantum gravity, and also I'm talking to lots of people about an interesting 4-dimensional formulation of the loop representation in terms of "spin foams" — roughly speaking, soap-bubble-like structures with faces labelled by spins.

Here are some papers I've come across while here:

1) Lee Smolin, "The future of spin networks", in *The Geometric Universe: Science, Geometry, and the Work of Roger Penrose*, eds. S. Hugget, Paul Tod, and L.J. Mason, Oxford University Press, 1998. Also available as gr-qc/9702030.

I've spoken a lot about spin networks here on This Week's Finds. They were first invented by Penrose as a radical alternative to the usual way of thinking of space as a smooth manifold. For him, they were purely discrete, purely combinatorial structures: graphs with edges labelled by "spins" $j = 0, 1/2, 1, 3/2, \ldots$, and with three edges meeting at each vertex. He showed how when these spin networks become sufficiently large and complicated, they begin in certain ways to mimic ordinary 3-dimensional Euclidean space. Interestingly, he never got around to publishing his original paper on the subject, so it remains available only if you know someone who knows someone who has it:

2) Roger Penrose, "Theory of quantized directions", unpublished manuscript.

In case you're wondering, I don't have a copy. Someone here has an nth-generation xerox copy, which I read, but n was sufficiently large that the $(n + 1)$st generation copy would have been unreadable. I will get ahold of it somehow, though!

Anyway, Smolin's paper is a kind of tribute to Penrose, and it traces the curiously twisting history of spin networks from their origin up to the present day, where they play a major role in topological quantum field theory and the loop representation — now more appropriately called the spin network representation! — of quantum gravity. (See "Week 55" for more on spin networks.)

Note however that the title of the paper refers to the *future* of spin networks. Smolin argues that spin networks are a major clue about the future of physics, and he paints a picture of what this future might be... which I urge you to look at.

For more on this, try:

3) Fotini Markopoulou and Lee Smolin, "Causal evolution of spin networks", preprint available as gr-qc/9702025.

Fotini Markopoulou is a student of Chris Isham at Imperial College, but now she's visiting the CGPG and working with Lee Smolin on spin networks. In this paper they describe some theories in which spin networks evolve in time in discrete steps. The evolution is "local" in the sense that in a given step, any vertex of the spin network changes in a way that only depends on its immediate neighbors — vertices connected to it by an edge. It is also "causal" in the sense that history of spin network evolving according to their rules gives a causal set, i.e. a set equipped with a partial ordering which we think of as saying which points come "before" which other points. This ties their work to the work of Rafael Sorkin on causal sets, e.g.:

4) Luca Bombelli, Joohan Lee, David Meyer and Rafael D. Sorkin, "Space-time as a causal set", *Phys. Rev. Lett.* **59** (1987), 521.

Unlike the related work of Reisenberger and Rovelli (see "Week 96"), Markopolou and Smolin do not attempt to "derive" their rules from general relativity by standard quantization techniques. Instead, they hope that some theory of the sort they consider will approximate general relativity in the large-scale limit. To check this will require some new techniques akin to the "renormalization group" approach to studying the large-scale limits of statistical mechanical systems defined on a lattice. This is a bit daunting, but it seems likely that no matter how one proceeds to pursue a spin-network-based theory of quantum gravity, one will need to develop such techniques at some point.

Now I'd like to switch gears and return to. . .

THE TALE OF n-CATEGORIES!

Recall that in our last episode, in "Week 92", we had worked our way up to 2-categories, and we were beginning to see what they had to do with 2-dimensional physics and toplogy. I described how to get monads from adjunctions, and what this has to do with matrix multiplication, Yang-Mills theory, and the 4-color theorem.

Next week I want to get serious and start talking about n-categories for arbitrary n. One reason is that at the end of this month there's a conference on n-categories and physics that I want to report on:

5) *Workshop on Higher Category Theory and Physics*, March 28-30, 1997, Northwestern University, Evanston, Illinois. Organized by Ezra Getzler and Mikhail Kapranov; program available at `http://math.nwu.edu/~getzler/conf97.html`

But before doing this, I want to say a bit about what category theory has to do with quantum mechanics!

First remember the big picture: n-category theory is a language to talk about processes that turn processes into other processes. Roughly speaking, an n-category is some sort of structure with objects, morphisms between objects, 2-morphisms between morphisms, and so on up to n-morphisms. A 0-category is just a set, with its objects usually being called "elements". Things get trickier as n increases. For a precise definition of n-categories for $n = 1$ and 2, see "Week 73" and "Week 80", respectively.

Most familiar mathematical gadgets are sets equipped with extra bells and whistles: groups, vector spaces, Hilbert spaces, and so on all have underlying sets. This is why set theory plays an important role in mathematics. However, we can also consider fancier gadgets that are *categories* equipped with extra bells and whistles. Some of the most interesting examples are just "categorifications" of well-known gadgets.

For example, a "monoid" is a simple gadget, just a set equipped with an associative product and multiplicative identity. An example we all know and love is the complex numbers: the product is just ordinary multiplication, and the multiplicative identity is the number 1.

We may categorify the notion of "monoid" and define a "monoidal category" to be a *category* equipped with an associative product and multiplicative identity. I gave the precise definition back in "Week 89"; the point here is that while they may sound scary, monoidal categories are actually very familiar. For example, the category of Hilbert spaces is a monoidal category where the product of Hilbert spaces is the tensor product and the multiplicative identity is $\mathbb{C}$, the complex numbers.

If one systematically studies categorification one discovers an amazing fact: many deep-sounding results in mathematics are just categorifications of stuff we all learned in high school. There is a good reason for this, I believe. All along, mathematicians have been unwittingly "decategorifying" mathematics by pretending that categories are just sets. We "decategorify" a category by forgetting about the morphisms and pretending that isomorphic objects are equal. We are left with a mere set: the set of isomorphism classes of objects.

I gave an example in "Week 73". There is a category FinSet whose objects are finite sets and whose morphisms are functions. If we decategorify this, we get the set of natural numbers! Why? Well, two finite sets are isomorphic if they have the same number of elements. "Counting" is thus the primordial example of decategorification.

I like to think of it in terms of the following fairy tale. Long ago, if you were a shepherd and wanted to see if two finite sets of sheep were isomorphic, the most obvious way would be to look for an isomorphism. In other words, you would try to match each sheep in herd A with a sheep in herd B. But one day, along came a shepherd who invented decategorification. This person realized you could take each set and "count" it, setting up an isomorphism between it and some set of "numbers", which were nonsense words like "one, two, three, four,..." specially designed for this purpose. By comparing the resulting numbers, you could see if two herds were isomorphic without explicitly establishing an isomorphism!

According to this fairy tale, decategorification started out as the ultimate stroke of mathematical genius. Only later did it become a matter of dumb habit, which we are now struggling to overcome through the process of "categorification".

Okay, so what does this have to do with quantum mechanics?

Well, a Hilbert space is a set with extra bells and whistles, so maybe there is some gadget called a "2-Hilbert space" which is a *category* with analogous extra bells and whistles. And maybe if we figure out this notion we will learn something about quantum mechanics.

Actually the notion of 2-Hilbert space didn't arise in this simple-minded way. It arose in some work by Daniel Freed on topological quantum field theory:

5) "Higher algebraic structures and quantization", by Dan Freed, *Comm. Math. Phys.* **159** (1994), 343–398, preprint available as `hep-th/9212115`; see also "Week 48".

Later, Louis Crane adopted this notion as part of his program to reduce quantum gravity to n-category theory:

6) Louis Crane: "Clock and category: is quantum gravity algebraic?", *Jour. Math. Phys.* **36** (1995), 6180–6193, preprint available as gr-qc/9504038.

These papers made is clear that 2-Hilbert spaces are interesting things and that one should go further and think about "n-Hilbert spaces" for higher n, too. However, neither of them gave a precise definition of 2-Hilbert space, so at some point I decided to do this. It took a while for me to learn enough category theory, but eventually I wrote something about it:

7) John Baez, "Higher-dimensional algebra II: 2-Hilbert spaces", to appear in *Adv. Math.*, preprint available as q-alg/9609018 or at http://math.ucr.edu/home/baez/

To understand this requires a little category theory, so let me explain the basic ideas here.

I'll concentrate on finite-dimensional Hilbert spaces, since the infinite-dimensional case introduces extra complications. To define 2-Hilbert spaces we need to start by categorifying the various ingredients in the definition of Hilbert space. These are:

1. the zero element,
2. addition,
3. subtraction,
4. scalar multiplication, and
5. the inner product.

The first four have well-known categorical analogs. The fifth one, which is really the essence of a Hilbert space, may seem a bit more mysterious at first, but as we shall see, it's really the key to the whole business.

1) The analog of the zero vector is a 'zero object'. A zero object in a category is an object that is both initial and terminal. That is, there is exactly one morphism from it to any object, and exactly one morphism to it from any object. Consider for example the category Hilb having finite-dimensional Hilbert spaces as objects, and linear maps between them as morphisms. In Hilb, any zero-dimensional Hilbert space is a zero object.

   Note: there isn't really a unique zero object in the "strict" sense of the term. Instead, any two zero objects are canonically isomorphic. The reason is that if you have two zero objects, say 0 and $0'$, there is a unique morphism $f\colon 0 \to 0'$ and a unique morphism $g\colon 0' \to 0$. These morphisms are inverses of each other so they are isomorphisms. Why are they inverses? Well, $fg\colon 0 \to 0'$ must be the identity morphism $1_0\colon 0 \to 0$, because there is only one morphism from 0 to 0! Similarly, gf is the identity on $0'$. (Note that I am using category theorist's notation, where the composite of $f\colon x \to y$ and $g\colon y \to z$ is denoted $fg\colon x \to z$.)

   This is typical in category theory. We don't expect stuff to be unique; it should only be unique up to a canonical isomorphism.

2) The analog of adding two vectors is forming the "coproduct" of two objects. Coproducts are just a fancy way of talking about direct sums. Any decent quantum mechanic knows about the direct sum of Hilbert spaces. But in fact, we can define this notion very generally in any category, where it goes under the name of a "coproduct". (I give the definition below; if I gave it here it would scare people away.) As with zero objects, coproducts are typically not unique, but they are always unique up to canonical isomorphism, which is what matters. It's a good little exercise to show this.

3) The analog of subtracting vectors is forming the "cokernel" of a morphism $f\colon x \to y$. If x and y are Hilbert spaces, the cokernel of f is just the orthogonal complement of the range of f. In other words, for Hilbert spaces we have "direct differences" as well as direct sums. However, the notion of cokernel makes sense in any category with a zero object. I won't burden you with the precise definition here.

An important difference between zero, addition and subtraction and their categorical analogs is that these operations represent extra *structure* on a set, while having a zero object, coproducts of two objects, or cokernels of morphisms is merely a *property* of a category. Thus these concepts are in some sense more intrinsic to categories than to sets. On the other hand, we've seen one pays a price for this: while the zero element, sums, and differences are unique in a Hilbert space, the zero object, coproducts, and cokernels are typically unique only up to canonical isomorphism.

4) The analog of multiplying a vector by a complex number is tensoring an object by a Hilbert space. Besides its additive properties (zero object, binary coproducts, and cokernels), Hilb is also a monoidal category: we can multiply Hilbert space by tensoring them, and there is and a multiplicative identity, namely the complex numbers $\mathbb{C}$. In fact, Hilb is a "ring category", as defined by Laplaza and Kelly.

We expect Hilb to play a role in 2-Hilbert space theory analogous to the role played by the ring $\mathbb{C}$ of complex numbers in Hilbert space theory. Thus we expect 2-Hilbert spaces to be "module categories" over Hilb, as defined by Kapranov and Voevodsky.

An important part of our philosophy here is that $\mathbb{C}$ is the primordial Hilbert space: the simplest one, upon which the rest are modelled. By analogy, we expect Hilb to be the primordial 2-Hilbert space. This is part of a general pattern pervading higher-dimensional algebra; for example, there is a sense in which the $(n+1)$-category of all (small) n-categories, nCat, is the primordial $(n+1)$-category. The real significance of this pattern remains mysterious.

5) Finally, what is the categorification of the inner product in a Hilbert space? It's the 'Hom functor'! The inner product in a Hilbert space eats two vectors v and w and spits out a complex number

$$\langle v, w \rangle$$

Similarly, given two objects v and w in a category, the Hom functor gives a *set*

$$\mathrm{Hom}(x, y)$$

namely the set of morphisms from x to y. Note that the inner product $\langle v, w \rangle$ is linear in w and conjugate-linear in y, and similarly, the Hom functor $\mathrm{Hom}(x, y)$ is

> covariant in y and contravariant in x. This hints at the category theory secretly underlying quantum mechanics. In quantum theory the inner product $\langle v, w\rangle$ represents the *amplitude* to pass from v to w, while in category theory $\mathrm{Hom}(x, y)$ is the *set* of ways to get from x to y. In Feynman path integrals, we do an integral over the set of ways to get from here to there, and get a number, the amplitude to get from here to there. So when physicists do Feynman path integration — just like a shepherd counting sheep — they are engaged in a process of decategorification!

To understand this analogy better, note that any morphism $f\colon x \to y$ in Hilb can be turned around or "dualized" to obtain a morphism $f^*\colon y \to x$. This is usually called the "adjoint" of f, and it satisfies

$$\langle fv, w\rangle = \langle v, f^*w\rangle$$

for all v in x, and w in y. This ability to dualize morphisms is crucial to quantum theory. For example, observables are represented by self-adjoint morphisms, while symmetries are represented by unitary morphisms, whose adjoint equals their inverse.

However, it should now be clear — at least to the categorically minded — that this sort of adjoint is just a decategorified version of the "adjoint functors" so important in category theory. As I explained in "Week 79", a functor $F^*\colon \mathcal{D} \to \mathcal{C}$ is a "right adjoint" of $F\colon \mathcal{C} \to \mathcal{D}$ if there is, not an equation, but a natural isomorphism

$$\mathrm{Hom}(Fc, d) \cong \mathrm{Hom}(c, F^*d)$$

for all objects c in $\mathcal{C}$, and $d in $\mathcal{D}$.

Anyway, in the paper I proceed to use these ideas to give a precise definition of 2-Hilbert spaces, and then I prove all sorts of stuff which I won't describe here.

Let me wrap up by explaining the definition of "coproduct". This is one of those things they should teach all math grad students, but for some reason they don't. It's a bit dry but it'll be good for you. A coproduct of the objects x and y is an object $x + y$ equipped with morphisms

$$i\colon x \to x + y$$

and

$$j\colon y \to x + y$$

that is universal with respect to this property. In other words, if we have any *other* object, say z, and morphisms

$$i'\colon x \to z$$

and

$$j'\colon y \to z$$

then there is a unique morphism $f\colon x + y \to z$ such that

$$i' = if$$

and

$$j' = jf.$$

This kind of definition automatically implies that the coproduct is unique up to canonical isomorphism. To understand this abstract nonsense, it's good to check that the coproduct of sets or topological spaces is just their disjoint union, while the coproduct of vector spaces or Hilbert spaces is their direct sum.

To continue reading the "Tale of n-Categories", see "Week 100".

Week 100

March 23, 1997

Pretty much ever since I started writing "This Week's Finds" I've been trying to get folks interested in n-categories and other aspects of higher-dimensional algebra. There is really an enormous world out there that only becomes visible when you break out of "linear thinking" — the mental habits that go along with doing math by writing strings of symbols in a line. For example, when we write things in a line, the sums $a + b$ and $b + a$ look very different. Then we introduce a profound and mysterious equation, the "commutative law":

$$a + b = b + a$$

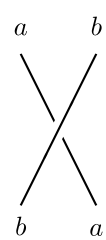
which says that actually they are the same. But in real life, we prove this equation using higher-dimensional reasoning:

$$a + b = {}^{a} + {}_{b} = \begin{matrix} a \\ + \\ b \end{matrix} = {}_{b} + {}^{a} = b + a$$

If this seems silly, think about explaining to a kid what $9 + 17$ means, and how you could prove that $9 + 17 = 17 + 9$. You might take a pile of 9 rocks and set it to the left of a pile of 17 rocks, and say "this is 9+17 rocks". Alternatively, you might put the pile of 9 rocks to the right of the pile of 17 rocks, and say "this is 17+9 rocks". Thus to prove that $9 + 17 = 17 + 9$, you would simply need to *switch* the two piles by moving one around the other.

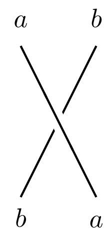
This is all very simple. Historically, however, it took people a long to really understand. It's one of those things that's too simple to take seriously until it turns out to have complicated ramifications. Now it goes by the name of the "Eckmann-Hilton theorem", which says that "a monoid object in the category of monoids is a commutative monoid". You practically need a PhD in math to understand *that*! However, lest you think that Eckmann and Hilton were merely dressing up the obvious in fancy jargon, it's important to note that what they did was to figure out a *framework* that turns the above "picture proof" that $a + b = b + a$ into an actual rigorous proof! This is one of the goals of higher-dimensional algebra.

The above proof that $a + b = b + a$ uses 2-dimensional space, but if you really think about it also uses a 3rd dimension, namely time: the time that passes as you move "a" around "b". If we draw this 3rd dimension as space rather than time we can visualize the process of moving a around b as follows:



This picture is an example of what mathematicians call a "braid". This particular one is a boring little braid with only two strands and one place where the two strands cross.

It illustrates another major idea behind higher-dimensional algebra: equations are best thought of as summarizing "processes" (or technically, "isomorphisms"). The equation $a+b=b+a$ is a summary of the process of switching a and b. There is more information in the process than in the mere equation $a+b=b+a$, because in fact there are two *different* ways to switch a and b: the above way and



If one has a bunch of objects one can switch them around in a lot of ways, getting lots of different braids.

In fact, the mathematics of braids, and related things like knots, is crucially important for understanding quantum gravity in 3-dimensional spacetime. Spacetime is really 4-dimensional, of course, but quantum gravity in 4-dimensional spacetime is awfully difficult, so in the late 1980s people got serious about studying 3-dimensional quantum gravity as a kind of warmup exercise. It turned out that the math required was closely related to some mysterious new mathematics related to knots and "braidings". At first this must sound bizarre: a deep relationship between knots and 3-dimensional quantum gravity! However, after you fight your way through the sophisticated mathematical physics that's involved, it becomes clear why they are related: both rely crucially on "3-dimensional algebra", the algebra describing how you can move things around in 3-dimensional spacetime.

However, there is more to the story, because knot theory also seems deeply related to *4-dimensional* quantum gravity. Here the knots arise as "flux tubes of area" living in 3-dimensional space at a given time. Recent work on quantum gravity suggests that as time passes these knots (or more generally, "spin networks") move around and change topology as time passes.

To really understand this, we probably need to understand "4-dimensional algebra". Unfortunately, not enough is known about 4-dimensional algebra. The problem is that we don't know much about 4-categories! To do n-dimensional algebra in a really nice way, you need to know about n-categories. Roughly speaking, an n-category is an algebraic structure that has a bunch of things called "objects", a bunch of things called "morphisms" that go between objects, and similarly 2-morphisms going between morphisms, 3-morphisms going between 2-morphisms, and so on up to the number n. You can think of the objects as "things" of whatever sort you like, the morphisms as processes going from one thing to another, the 2-morphisms as meta-processes going from one process to another, and so on. Depending on how you play the n-category game, there are either no morphisms after level n, or only simple and bland ones playing the role of "equations". The idea is that in the world of n-categories, one keeps track of things, processes, meta-processes, and so on to the nth level, but after that one calls it quits and uses equations.

So what is the definition of 4-categories? Well, Eilenberg and Mac Lane defined 1-categories, or simply "categories", in a paper that was published in 1945:

1) S. Eilenberg and S. Mac Lane, "General theory of natural equivalences", *Trans. Amer. Math. Soc.* **58** (1945), 231–294.

Benabou defined 2-categories — though actually he called them "bicategories" — in a 1967 paper:

2) J. Benabou, *Introduction to bicategories*, Springer Lecture Notes in Mathematics **47**, New York, 1967, pp. 1–77.

Gordon, Power, and Street defined 3-categories — or actually "tricategories" — in a paper that came out in 1995:

3) R. Gordon, A. J. Power, and R. Street, "Coherence for tricategories", *Memoirs Amer. Math. Soc.* **117** (1995) Number 558.

This step took a long time in part because it took a long time for people to understand deeply where *braidings* fit into the picture.

But what about 4-categories and higher n? Well, the history is complicated and I won't get it right, but let me say a bit anyway. First of all, there are some things called "strict n-categories" that people have known how to define for arbitrarily high n for quite a while. In fact, people know how to go up to infinity and define "strict ω-categories"; see for example:

4) S. E. Crans, *On combinatorial models for higher dimensional homotopies*, Ph.D. thesis, University of Utrecht, Utrecht, 1991.

Strict n-categories are quite interesting and important, but I'm mainly mentioning them here to emphasize that they are *not* what I'm talking about. People sometimes often call strict n-categories simply "n-categories", and call the more general n-categories I'm talking about above "weak n-categories". However, I think the weak n-categories will will eventually be called simply "n-categories", because they are far more interesting and important than the strict ones. Anyway, that's what I'm doing here.

Secondly, when you define n-categories you have to make some choice about the "shapes" of your j-morphisms. In general they should be some j-dimensional things, but they could be simplices, or cubes, or other shapes. In some ways the simplest shapes are "globes", a j-dimensional globe being a j-dimensional ball with its boundary divided into two hemispheres, the "inface" and "outface", which are themselves $(j-1)$-dimensional globes. This corresponds to a picture where each "process" has one input and one output, which are themselves processes having the same input and output. The definitions of category, bicategory, and tricategory work this way. In fact, Ross Street came up with a very nice definition of n-categories for all n using simplices in 1987:

5) Ross Street, "The algebra of oriented simplexes", *Jour. Pure Appl. Alg.* **49** (1987), 283–335.

Since then, however, he and his students and collaborators seem to have been working to translate this definition into the "globular" formalism... while also making some other important adjustments too technical to discuss here. In particular, Dominic Verity and Todd Trimble have done a lot of work on getting the definition of n-category worked out, and a while ago I learned that Trimble came up with a definition of "tetracategory" (or what I'm calling simply "4-category") in August of 1995. I don't think this has been published, however.

James Dolan came to U. C. Riverside in the fall of 1993, and ever since then, he and have been talking about n-categories and their role in physics. Most of the category theory I know, I learned in this process. It soon became clear that we needed a nice definition of n-category for all n in order to turn our hopes and dreams into theorems. After a while we started working pretty hard on this. His job was to come up with all the bright ideas, and mine was to get him to explain them, to try to poke holes in them, and to figure out rigorous proofs of all the things that were so obvious to him that he couldn't figure out how (or why) to prove them. We sent a summarized version of our definition to Ross Street at the end of 1995:

6) J. Baez and J. Dolan, "n-Categories — sketch of a definition", letter to Ross Street, Nov. 29, 1995, available at `http://math.ucr.edu/home/baez/ncat.def.html`

and then for a year I worked on trying to write up a longer, clearer version, while all the meantime Dolan kept coming up with new ways of looking at everything. I finished in February of this year:

7) J. Baez and J. Dolan, "Higher-dimensional algebra III: n-Categories and the algebra of opetopes", to appear in *Adv. Math.*, preprint available as `q-alg/9702014` and at `http://math.ucr.edu/home/baez/op.ps`, or in compressed form as `http://math.ucr.edu/home/baez/op.ps.Z`

The key feature of this definition is that it uses "j-dimensional opetopes" as the shapes for j-morphisms. These shapes are very handy because the $(j+1)$-dimensional opetopes describe all the legal ways of sticking together a bunch of j-dimensional opetopes to form another j-dimensional opetope! They are related to the theory of "operads", which is part of the reason for their name. (By the way, the first two syllables are pronounced exactly as in "operation".)

In the meantime, Michael Makkai and John Power had begun work using our definition. Also, other definitions of "n-category" have appeared on the scene! Zouhair Tamsamani came up with one in terms of "multi-simplicial sets":

8) Z. Tamsamani, *Sur des notions de ∞-categorie et ∞-groupoide non-strictes via des ensembles multi-simpliciaux*, Ph.D. thesis, Universite Paul Sabatier, Toulouse, France, 1995.

Michael Batanin also has a definition of ω-categories, of the "globular" sort:

9) M. A. Batanin, "On the definition of weak ω-category", *Macquarie Mathematics Report* number **96/207**.

Now the fun will begin! These different definitions of (weak) n-category should be equivalent, albeit in a rather subtle sense, so we should check to see if they really are. Also, we need to develop many more tools for working with n-categories. Then we can really start using them as a tool.

When I started writing this I thought I was going to explain the definition that Dolan and I came up with. Now I'm too tired! It takes a while to explain, so I think I'll stop here and save that for some other week or weeks. Perhaps I'll mix it in with my report on the Workshop on Higher Category Theory and Physics, which is taking place next weekend at Northwestern University.

This is the end of the "Tale of n-Categories". If you want more, try 'An Introduction to n-Categories' (in Postscript form), or else read the above papers.
