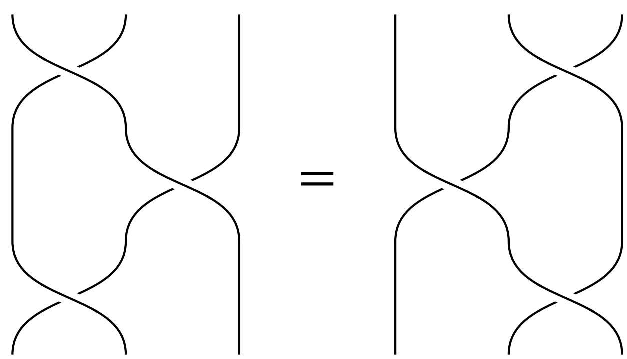
This Week's Finds in Mathematical Physics

Weeks 1 to 50

January 19, 1993 to *March 12, 1995*

by John Baez

typeset by Tim Hosgood



Preface

These are the first 50 issues of *This Week's Finds of Mathematical Physics*. This series has sometimes been called the world's first blog, though it was originally posted on a "usenet newsgroup" called sci.physics.research — a form of communication that predated the world-wide web. I began writing this series as a way to talk about papers I was reading and writing, and in the first 50 issues I stuck closely to this format. These issues focus rather tightly on quantum gravity, topological quantum field theory, knot theory, and applications of n-categories to these subjects. However, there are also digressions into Lie algebras, elliptic curves, linear logic and other subjects.

Tim Hosgood kindly typeset all 300 issues of *This Week's Finds* in 2020. They will be released in six installments of 50 issues each. I have edited the issues here to make the style a bit more uniform and also to change some references to preprints, technical reports, etc. into more useful arXiv links. This accounts for some anachronisms where I discuss a paper that only appeared on the arXiv later.

I thank Blake Stacey for helping me fix many mistakes. There are undoubtedly many still remaining. If you find some, please contact me and I will try to fix them.

Contents

Week 1

January 19, 1993

I thought I might try something that may become a regular feature on sci.physics.research, if that group comes to be. The idea is that I'll briefly describe the papers I have enjoyed this week in mathematical physics. I am making no pretense at being exhaustive or objective... what I review is utterly a function of my own biases and what I happen to have run into. I am not trying to "rate" papers in any way, just to entertain some people and perhaps inform them of some papers they hadn't yet run into. "This week" refers to when I read the papers, not when they appeared (which may be much earlier or also perhaps later, since some of these I am getting as preprints).

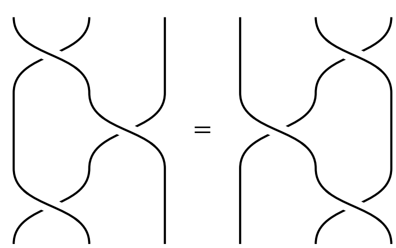
1) J. Scott Carter and Masahico Saito, "Syzygies among elementary string interactions in 2+1 dimensions", *Lett. Math. Phys.* **23** (1991), 287–300.

   J. Scott Carter and Masahico Saito, "On formulations and solutions of simplex equations", preprint.

   J. Scott Carter and Masahico Saito, "A diagrammatic theory of knotted surfaces", preprint.

   J. Scott Carter and Masahico Saito, "Reidemeister moves for surface isotopies and their interpretations as moves to movies", preprint.

The idea here is to take what has been done for knots in 3-dimensional space and generalize it to "knotted surfaces," that is, embedded 2-manifolds in 4-dimensional space. For knots it is convenient to work with 2-dimensional pictures that indicate over- and under-crossings; there is a well-known small set of "Reidemeister moves" that enable you to get between any two pictures of the same knot. One way to visualize knotted surfaces is to project them down to $\mathbb{R}^3$; there are "Roseman moves" analogous to the Reidemeister moves that enable to get you between any two projections of the same knotted surface. Carter and Saito prefer to work with "movies" that display a knotted surface as the evolution of knots (actually links) over time. Each step in such a movie consists of one of the "elementary string interactions." They have developed a set of "movie moves" that connect any two movies of the same knotted surface. These papers contain a lot of fascinating pictures! And there does seem to be more than a minor relation to string theory. For example, one of the movie moves is very analogous to the 3rd Reidemeister move — which goes



I won't try to draw the corresponding movie move, but just as the 3rd Reidemeister move is the basis for the Yang–Baxter equation $R_{23}R_{13}R_{12} = R_{12}R_{13}R_{23}$ (the subscripts indicate which strand is crossing which), the corresponding movie move is the basis for a variant of the "Frenkel–Moore" form of the "Zamolodchikov tetrahedron equation" which first arose in string theory. This variant goes like $S_{124}S_{135}S_{236}S_{456} = S_{456}S_{236}S_{135}S_{124}$, and Carter and Saito draw pictures that make this equation almost as obvious as the Yang–Baxter equations.

In any event, this is becoming a very hot subject, since topologists are interested in generalizing the new results on knot theory to higher dimensions, while some physicists (especially Louis Crane) are convinced that this is the right way to tackle the "problem of time" in quantum gravity (which, in the loop variables approach, amounts to studying the relationship of knot theory to the 4th dimension, time). In particular, Carter and Saito are investigating how to construct solutions of the Zamolodchikov equations from solutions of the Yang–Baxter equation — the goal presumably being to find invariants of knotted surfaces that are closely related to the link invariants coming from quantum groups. This looks promising, since Crane and Yetter have just constructed a 4-dimensional topological quantum field theory from the quantum $\mathrm{SU}(2)$. But apparently nobody has yet done it.

Lovers of category theory will be pleased to learn that the correct framework for this problem appears to be the theory of 2-categories. These are categories with objects, morphisms between objects, and also "2-morphisms" between objects. The idea is simply that tangles are morphisms between sets of points (i.e., each of the tangles in the picture above are morphisms from 3 points to 3 points), while surfaces in $\mathbb{R}^4$ are 2-morphisms between tangles. The instigators of the 2-categorical approach here seem to be Kapranov and Voevodsky, whose paper "2-categories and Zamolodhikov tetrahedra equations", to appear in *Proc. Symp. Pure Math.*, is something I will have to get ahold of soon by any means possible (I can probably nab it from Oleg Viro down the hall; he is currently hosting Kharmalov, who is giving a series of talks on knotted surfaces at 2-categories here at UCR). But it seems to be Louis Crane who is most strongly proclaiming the importance of 2-categories in *physics*.

2) Jorge Pullin, "Knot theory and quantum gravity in loop space: a primer", to appear in *Proceedings of the Vth Mexican School of Particles and Fields*, ed. J. L. Lucio, World Scientific, Singapore, now available as hep-th/9301028.

This is a review of the new work on knot theory and the loop representation of quantum gravity. Pullin is among a group who has been carefully studying the "Chern–Simons state" of quantum gravity, so his presentation, which starts with a nice treatment of the basics, leads towards the study of the Chern–Simons state. This is by far the best-understood state of quantum gravity, and is defined by $\mathrm{SU}(2)$ Chern–Simons theory in terms of the connection representation, or by the Kauffman bracket invariant of knots in the loop representation. It is a state of Euclideanized quantum gravity with nonzero cosmological constant, and is not invariant under CP. Ashtekar has recently speculated that it is a kind of "ground state" for gravity with cosmological constant (evidence for this has been given by Kodama), and that its CP violation may be a "reason" for why the cosmological constant is actually zero (this part is extremely speculative). Louis Crane, on the other hand, seems convinced that the Chern–Simons state (or more generally states arising from modular tensor categories) is **the wavefunction of the universe**. In any event, it's much nicer to have one state of quantum gravity to play with than none, as was the case until recently.

3) Lee Smolin, "Time, measurement and information loss in quantum cosmology", preprint available as gr-qc/9301016.

This is, as usual for Smolin, a very ambitious paper. It attempts to sketch a solution of some aspects of the problem of time in quantum gravity (in terms of the loop representation). I might as well quote from the introduction:

> *Thus, to return to the opening question, if we are, within a nonperturbative framework, to ask what happens after a black hole evaporates, we must be able to construct spacetime diffeomorphism invariant operators that can give physical meaning to the notion of "after the evaporation." Perhaps I can put it in the following way: the questions about loss of information or breakdown of unitary evolution rely, implicitly, on a notion of time. Without reference to time it is impossible to say that something is being lost. In a quantum theory of gravity, time is a problematic concept which makes it difficult to even ask such questions at the nonperturbative level, without reference to a fixed spacetime manifold. [I would prefer to say "fixed background metric" — JB] The main idea, which it is the purpose of this paper to develop, is that the problem of time in the nonperturbative framework is more than an obstacle that blocks any easy approach to the problem of loss of information in black hole evaporation. It may be the key to its solution.*
>
> *As many people have argued, the problem of time is indeed the conceptual core of the problem of quantum gravity. Time, as it is conceived in quantum mechanics is a rather different thing than it is from the point of view of general relativity. The problem of quantum gravity, especially when put in the cosmological context, requires for its solution that some single concept of time be invented that is compatible with both diffeomorphism invariance and the principle of superposition. However, looking beyond this, what is at stake in quantum gravity is indeed no less and no more than the entire and ancient mystery: What is time? For the theory that will emerge from the search for quantum gravity is likely to be the background for future discussions about the nature of time, as Newtonian physics has loomed over any discussion about time from the seventeenth century to the present.*
>
> *I certainly do not know the solution to the problem of time. Elsewhere I have speculated about the direction in which we might search for its ultimate resolution. In this paper I will take a rather different point of view, which is based on a retreat to what both Einstein and Bohr taught us to do when the meaning of a physical concept becomes confused: reach for an operational definition. Thus, in this paper I will adopt the point of view that time is precisely no more and no less than that which is measured by physical clocks. From this point of view, if we want to understand what time is in quantum gravity then we must construct a description of a physical clock living inside a relativistic quantum mechanical universe.*

Technically speaking, what Smolin does is roughly as follows. He considers quantum gravity coupled to matter, modelled in such a way that the Hilbert space is spanned by states labelled by isotopy classes of: any number N loops in a compact 3-manifold

M ("space") and N surfaces with boundary in M. (This trick is something I hadn't seen before, though Smolin gives references to it.) He then introduces a "clock field," which is just a free scalar field coupled to the gravity, and does gauge-fixing to see what evolution with respect to this clock field looks like. I will have to read this a number of times!

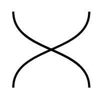
Week 2

January 24, 1993

Well, this week I have had guests and have not been keeping up with the literature. So "this week's finds" are mostly papers that have been sitting around in my office and that I am now filing away.

1) Daniel Armand Ugon, Rodolfo Gambini, and Pablo Mora, "Link invariants for intersecting loops", October 1992 preprint, available from Gambini, Instituto de Física, Facultad de Ciencias, Tristán Narvaja 1674, Montevideo, Uruguay.

The authors generalize the standard trick for getting link invariants from solutions of the Yang–Baxter equations, and show how to get link invariants applicable to generalized links with 4-valent or 6-valent vertices, that is, transverse double points, like

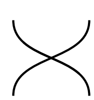


and transverse triple points. This involves working with a generalization of the braid group that includes generators for these vertices as well as the usual generators for crossings. In this case of 4-valent vertices, rigorously working out the generators and relations was done by Joan Birman in the paper below, but various people had used the answer already. The case of triple points is very important in physics due to the connection with the loop representation of quantum gravity (which is what Gambini is working on these days). In this representation, states are invariants of (possibly generalized) links, and only by considering links with triple points can one define operators such as the "total volume of the universe".

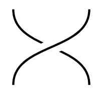
It is thus quite interesting that the authors make progress on determining the "right" extension of the HOMFLY polynomial invariant of links to links with transverse triple points — that is, the extension that one gets by doing calculations in $\mathrm{SU}(n)$ Chern–Simons theory. In a special case, namely when the HOMFLY polynomial reduces to the Kauffman bracket (which corresponds to the Lie group $\mathrm{SU}(2)$), one gets a state of quantum gravity that has been under extensive investigation these days. The authors compute the Kaufmann bracket of links with triple points using first order perturbation theory in Chern–Simons theory. A nonperturbative calculation would be very good to have!

2) Joan Birman, "New points of view in knot theory", *Bull. Amer. Math. Soc.* **28** (1993), 253–287. Also available as math/9304209.

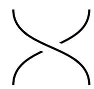
This is a nice review of the recent work on Vassiliev invariants of links. Given an invariant of oriented links, one can extend it to links with arbitrarily many double points by setting the value of the invariant on a link with a double point



to be the invariant of the link with the double point changed to



minus the invariant of the link with the double point changed to



Note that the link has to be oriented for this rule to make sense, and the strands shown in the pictures above should be pointing *downwards*. Now, having made this extension, we say a link invariant is a Vassiliev invariant of degree n if it vanishes on all links with $n+1$ or more double points.

It is interesting that this rule for extending link invariants to links with double points, when applied to the Kauffman bracket, does *not* give the extension computed by Gambini et al in the paper above. (This is not surprising, actually, but it shows that some interesting things are going on in the subject of invariants for links with self-intersections and Chern–Simons theory.)

3) Louis Kauffman and P. Vogel, "Link polynomials and a graphical calculus", *Jour. of Knot Theory and its Ramifications*, **1** (1992), 59–104.

This is another nice treatment of link invariants for generalized links with self-intersections. It concentrates on the famous link invariants coming from Chern–Simons theory — the HOMFLY polynomial (from $\mathrm{SU}(n)$) and the Kauffman polynomial (from $\mathrm{SO}(n)$). Lots of good pictures.

And, switching back to the category theory, 2-categories, and the like, let me list these before filing them away:

4) Louis Crane, "Categorical physics", available as `hep-th/9301061`.

   Louis Crane and David Yetter, A categorical construction of 4d topological quantum field theories, available as `hep-th/9301062`.

   Louis Crane and Igor Frenkel, Hopf Categories and their representations, draft version.

   Louis Crane and Igor Frenkel, Categorification and the construction of topological quantum field theory, draft version.

These outline Louis Crane's vision of an approach to generally covariant 4-dimensional quantum field theories (e.g. quantum gravity or a "theory of everything") based on 2-categories. "Categorical physics" sketches the big picture, while the paper with Yetter provides a juicy mathematical spinoff — the first known four-dimensional TQFT, based on the representations of quantum $\mathrm{SU}(2)$ and very similar in spirit to the Turaev–Viro construction of a 3d TQFT from quantum $\mathrm{SU}(2)$. The papers with Frenkel (apparently still not in their final form) describe the game plan and hint at marvelous things still to come. The conjecture is stated: "a 4d TQFT can be reconstructed from a tensor 2-category". This follows up on Crane's earlier work on getting 3d TQFTs from modular tensor categories (big example: the categories of representations of quantum groups at roots of unity). And the authors define the notion of a Hopf category, show how the category of module categories of a Hopf category is a tensor 2-category, and use "categorification" to turn the universal enveloping algebra of a quantum group into a Hopf category. Sound abstract? Indeed it is. But the aim is clear: to cook up 4d TQFTs from quantum groups. Such quantum field theories might be physically important; indeed, the one associated to $\mathrm{SU}(2)$ is likely to have a lot to do with quantum gravity.

I am currently perusing Kapranov and Voevodsky's massive paper on 2-categories, which seems to be the starting point for Crane's above papers and also those of Carter/Saito that I mentioned last week. Next week I should post an outline of what this paper does.

5) S. W. Hawking, R. Laflamme and G. W. Lyons, "The origin of time asymmetry", preprint available as gr-qc/9301017.

I haven't had a chance to read this one yet but it looks very ambitious and is likely to be interesting. Let me just quote from the introduction to get across the goal:

> *The laws of physics do not distinguish the future from the past direction of time. More precisely, the famous CPT theorem says that the laws are invariant under the combination of charge conjugation, space inversion and time reversal. In fact effects that are not invariant under the combination CP are very weak, so to a good approximation, the laws are invariant under the time reversal operation T alone. Despite this, there is a very obvious difference between the future and past directions of time in the universe we live in. One only has to see a film run backward to be aware of this.*
>
> *There are are several expressions of this difference. One is the so-called psychological arrow, our subjective sense of time, the fact that we remember events in one direction of time but not the other. Another is the electromagnetic arrow, the fact that the universe is described by retarded solutions of Maxwell's equations and not advanced ones. Both of these arrows can be shown to be consequences of the thermodynamic arrow, which says that entropy is increasing in one direction of time. It is a non trivial feature of our universe that it should have a well defined thermodynamic arrow which seems to point in the same direction everywhere we can observe. Whether the direction of the thermodynamic arrow is also constant in time is something we shall discuss shortly.*
>
> *There have been a number of attempts to explain why the universe should have a thermodynamic arrow of time at all. Why shouldn't the universe be in a state of maximum entropy at all times? And why should the direction of the thermodynamic arrow agree with that of the cosmological arrow, the direction in which the universe is expanding? Would the thermodynamic arrow reverse, if the universe reached a maximum radius and began to contract?*
>
> *Some authors have tried to account for the arrow of time on the basis of dynamic laws. The discovery that CP invariance is violated in the decay of the K meson, inspired a number of such attempts but it is now generally recognized that CP violation can explain why the universe contains baryons rather than anti baryons, but it can not explain the arrow of time. Other authors have questioned whether quantum gravity might not violate CPT, but no mechanism*

has been suggested. One would not be satisfied with an ad hoc CPT violation that was put in by hand.

The lack of a dynamical explanation for the arrow of time suggests that it arises from boundary conditions. The view has been expressed that the boundary conditions for the universe are not a question for Science, but for Metaphysics or Religion. However that objection does not apply if there is a sense in which the universe has no boundary. We shall therefore investigate the origin of the arrow of time in the context of the no boundary proposal of Hartle & Hawking. This was formulated in terms of Einsteinian gravity which may be only a low energy effective theory arising from some more fundamental theory such as superstrings. Presumably it should be possible to express a no boundary condition in purely string theory terms but we do not yet know how to do this. However the recent COBE observations indicate that the perturbations that lead to the arrow of time arise at a time during inflation when the energy density is about 10^{-12} *of the Planck density. In this regime, Einstein gravity should be a good approximation.*

I'll skip some more technical stuff on the validity of perturbative calculations in quantum gravity...

One can estimate the wave functions for the perturbation modes by considering complex metrics and scalar fields that are solutions of the Einstein equations whose only boundary is the surface S. *When* S *is a small three sphere, the complex metric can be close to that of part of a Euclidean four sphere. In this case the wave functions for the tensor and scalar modes correspond to them being in their ground state. As the three sphere* S *becomes larger, these complex metrics change continuously to become almost Lorentzian. They represent universes with an initial period of inflation driven by the potential energy of the scalar field. During the inflationary phase the perturbation modes remain in their ground states until their wave lengths become longer than the horizon size. The wave function of the perturbations then remains frozen until the horizon size increases to be more than the wave length again during the matter dominated era of expansion that follows the inflation. After the wave lengths of the perturbations come back within the horizon, they can be treated classically.*

This behaviour of the perturbations can explain the existence and direction of the thermodynamic arrow of time. The density perturbations when they come within the horizon are not in a general state but in a very special state with a small amplitude that is determined by the parameters of the inflationary model, in this case, the mass of the scalar field. The recent observations by COBE indicate this amplitude is about 10^{-5}. *After the density perturbations come within the horizon, they will grow until they cause some regions to collapse as protogalaxies and clusters. The dynamics will become highly non linear and chaotic and the coarse grained entropy will increase. There will be a well defined thermodynamic arrow of time that points in the same direction everywhere in the universe and agrees with the direction of time in which the universe is expanding, at least during this phase.*

> *The question then arises: If and when the universe reaches and maximum size, will the thermodynamic arrow reverse? Will entropy decrease and the universe become smoother and more homogeneous during the contracting phase?*

I'll skip some stuff on why Hawking originally thought entropy had to decrease during the Big Crunch if the no-boundary proposal were correct. . . and why he no longer thinks so.

> *The thermodynamic arrow will agree with the cosmological arrow for half the history of the universe, but not for the other half. So why is it that we observe them to agree? Why is it that entropy increases in the direction that the universe is expanding? This is really a situation in which one can legitimately invoke the weak anthropic principle because it is a question of where in the history of the universe conditions are suitable for intelligent life. The inflation in the early universe implies that the universe will expand for a very long time before it contracts again. In fact, it is so long that the stars will have all burnt out and the baryons will have all decayed. All that will be left in the contracting phase will be a mixture of electrons, positrons, neutrinos and gravitons. This is not a suitable basis for intelligent life.*
>
> *The conclusion of this paper is that the no boundary proposal can explain the existence of a well defined thermodynamic arrow of time. This arrow always points in the same direction. The reason we observe it to point in the same direction as the cosmological arrow is that conditions are suitable for intelligent life only at the low entropy end of the universe's history.*

Week 3

January 30, 1993

Here's this week's reading material. The first test will be in two weeks. :-)

1) Dror Bar-Natan, "On the Vassiliev knot invariants", Harvard University "pre-preprint".

I went to U.C. San Diego this week to give a talk, and the timing was nice, because Dror Bar-Natan was there. He is a student of Witten who has started from Witten's ideas relating knot theory and quantum field theory and developed them into a beautiful picture that shows how knot theory, the theory of classical Lie algebras, and abstract Feynman diagrams are three faces of the same thing. To put it boldly, in a deliberately exaggerated form, Bar-Natan has proposed a conjecture saying that knot theory and the theory of classical Lie algebras are one and the same!

This won't seem very exciting if you don't know what a classical Lie algebra is. Let me give a brief and very sketchy introduction, apologizing in advance to all the experts for the terrible sins I will commit, such as failing to distinguish between complex and real Lie algebras.

Well, remember that a Lie algebra is just a vector space equipped with a "bracket" such that the bracket $[x, y]$ of any two vectors x and y is again a vector, and such that the following hold:

a) skew-symmetry: $[x, y] = -[y, x]$.

b) bilinearity: $[x, ay] = a[x, y]$, $[x, y + z] = [x, y] + [x, z]$. ($a$ is a number.)

c) Jacobi identity: $[x, [y, z]] + [y, [z, x]] + [z, [x, y]] = 0$.

The best known example is good old $\mathbb{R}^3$ with the cross product as the bracket. But the real importance of Lie algebras is that one can get one from any Lie group — roughly speaking, a group that's also a manifold, and such that the group operations are smooth maps. And the importance of Lie groups is that they are what crop up as the groups of symmetries in physics. The Lie algebra is essentially the "infinitesimal version" of the corresponding Lie group, as anyone has seen who has taken physics and seen the relation between the group of rotations in $\mathbb{R}^3$ and the cross product. Here the group is called $\mathrm{SO}(3)$ and the Lie algebra is called $\mathfrak{so}(3)$. (So $\mathbb{R}^3$ with its cross product is called $\mathfrak{so}(3)$.) One can generalize this to any number of dimensions, letting $\mathrm{SO}(n)$ denote the group of rotations in $\mathbb{R}^n$ and $\mathfrak{so}(n)$ the corresponding Lie algebra. (However, $\mathfrak{so}(n)$ is not isomorphic to $\mathbb{R}^n$ except for $n = 3$, so there is something very special about three dimensions.)

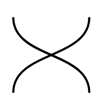
Similarly, if one uses complex numbers instead of real numbers, one gets a group $\mathrm{SU}(n)$ and Lie algebra $\mathfrak{su}(n)$. And if one looks at the symmetries of a $2n$-dimensional classical phase space — so-called canonical transformations, or symplectic transformations — one gets the group $\mathrm{Sp}(n)$ and Lie algebra $\mathfrak{sp}(n)$. To be precise, $\mathrm{SO}(n)$ consists of all $n \times n$ orthogonal real matrices with determinant 1, $\mathrm{SU}(n)$ consists of all $n \times n$ unitary complex matrices with determinant 1, and $\mathrm{Sp}(n)$ consists of all $(2n) \times (2n)$ real matrices preserving a nondegenerate skew-symmetric form.

These are all very important in physics. Indeed, all the "gauge groups" of physics are Lie groups of a certain sort, so-called compact Lie groups, and in the standard model all the forces are symmetrical under some gauge group or other. Electromagnetism a la Maxwell is symmetric under the group $\mathrm{U}(1)$ of complex numbers of unit magnitude, or "phases". The electroweak force (unified electromagnetism and weak force) is symmetric under $\mathrm{U}(1) \times \mathrm{SU}(2)$, where one uses the fact that one can build up bigger semisimple Lie groups as direct sums (also called products) of smaller ones. The gauge group for the strong force is $\mathrm{SU}(3)$. And, finally, the gauge group of the whole standard model is simply $\mathrm{U}(1) \times \mathrm{SU}(2) \times \mathrm{SU}(3)$, which results from lumping the electroweak and strong gauge groups together. This direct sum business also works for the Lie algebras, so the Lie algebra relevant to the standard model is written $\mathfrak{u}(1) \times \mathfrak{su}(2) \times \mathfrak{su}(3)$.

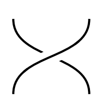
There are certain very special Lie algebras called simple Lie algebras which play the role of "elementary building blocks" in the world of Lie algebras. They cannot be written as the direct sum of other Lie algebras (and in fact there is an even stronger sense in which they cannot be decomposed). On the other hand, the Lie algebra of any compact Lie group is a direct sum of simple Lie algebras and copies of $\mathfrak{u}(1)$ — the one-dimensional Lie algebra with zero Lie bracket which, for technical reasons, people don't call "simple".

These simple Lie algebras were classified by the monumental work of Killing, Cartan and others, and the classification is strikingly simple: there are infinite series of "classical" Lie algebras of type $\mathfrak{su}(n)$, $\mathfrak{so}(n)$, and $\mathfrak{sp}(n)$, and five "exceptional" Lie algebras called G_2, F_4, E_6, E_7, and E_8. Believe it or not, there is a deep connection between the exceptional Lie algebras and the Platonic solids. But that is another story, one I barely know. . . .

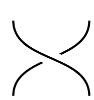
Now, Witten showed how one could use quantum field theory to constuct an invariant of knots, or even links, corresponding to any representation of a compact Lie group. (You won't even need to know what a representation is to understand what follows.) This had been done in a different way, in terms of "quantum groups," by Reshetikhin and Turaev (following up on work by many other people). These invariants are polynomials in a variable q (for "quantum"), and if one writes q as $e^{\hbar}$ and expands a power series in $\hbar$, the coefficient of $\hbar^n$ is a "Vassiliev invariant of degree n". Recall from last week that given an invariant of oriented knots, one can extend it to knot with arbitrarily many nice crossings by setting the value of the invariant on a knot with a crossing like



to be the invariant of the knot with the crossing changed to



minus the invariant of the knot with the crossing changed to



(Again, the knot has to be oriented for this rule to make sense, and the strands shown in the pictures above should be pointing downwards.) Having made this extension, one says a knot invariant is a Vassiliev invariant of degree n if it vanishes on all knot with $n+1$ or more double points.

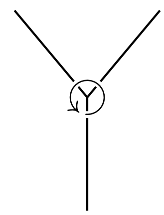
This is where Dror stepped in, roughly. First of all, he showed that the Vassiliev invariant of degree n is just what you get when you do Witten's quantum-field-theoretic calculations perturbatively using Feynman diagrams and look at the terms of order n in Planck's constant, $\hbar$! Secondly, and more surprisingly, he developed a bunch of relationships between Feynman diagrams and pictures of knots! The third and most amazing thing he did takes a bit longer to explain...

Roughly, he showed that any Vassiliev invariant of degree n is determined by some combinatorial data called a "weight system." He showed that any representation of a Lie algebra determines a weight system and hence a Vassiliev invariant. But the really interesting thing he showed is that many of the things one can do for Lie algebras can be done for arbitrary weight systems. This makes it plausible that **every** weight system, hence every Vassiliev invariant, comes from a representation of a simple Lie algebra. In fact, Dror conjectures that every Vassiliev invariant comes from a representation of a classical simple Lie algebra. Now there is another conjecture floating around these days, namely that Vassiliev invariants almost form a complete set — that is, that if two knots cannot be distinguished by any Vassiliev invariants, they must either be the same or differ simply by reversing the orientation of all the strands. If BOTH these conjectures are true, one has in some sense practically reduced the theory of knots to the theory of the classical Lie algebras! This wouldn't mean that all of sudden we know the answer to every question about knots, but it would certainly help a lot, and more importantly, in my opinion, it would show that the connection between topology and the theory of Lie algebras is far more profound than we really understand. The ramifications for physics, as I hope all my chatting about knots, gauge theories and quantum gravity makes clear, might also be profound.

Well, we *certainly* don't understand all this stuff yet, since we don't know how to prove these conjectures! But Dror's conjecture — that all weight systems come from representations of simple Lie algebras — is tantalizingly close to being within grasp, since he has reduced it to a fairly elementary combinatorial problem, which I will now state. Note that "elementary" does not mean easy to solve! Just easy to state.

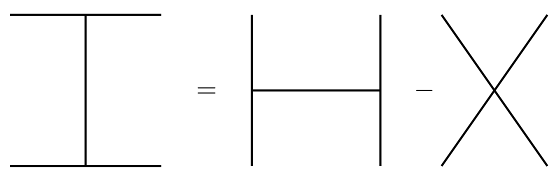
Before I state the combinatorial problem, let me say something about the evidence for the conjecture that all Vassiliev invariants come from representations of classical Lie algebras. In addition to all sorts of "technical" evidence, Dror has shown the conjecture is true for Vassiliev invariants of degree $\leqslant 9$ by means of many hours of computation using his Sparcstation. In fact, he said in his talk that he felt guilty about having a Sparcstation unless it was always computing something, and that even as he spoke his computer was busily verifying the conjecture for higher degrees. (I suggested that it was the Sparcstation that should feel guilty when it was not working, not him.) He also advertised that his programs, a mixture of C and Mathematica code, are available by anonymous ftp from `math.harvard`. Use user name "ftp", go to the directory "dror". You folks with Crays should feel VERY guilty if they are just sitting there and not helping Dror verify this important conjecture. (I suggest that you first read his papers and the file README in his directory, then check out his programs, and then ask him where he's at and what would be worth doing. Please don't pester him unless you are a good enough mathematician to discuss this stuff intelligently and have megaflops to burn. If you want to make a fool of yourself, *don't* say I sent you.)

Okay, with no further ado, here's the conjecture in its elementary combinatorial form. Let B be the vector space spanned by finite graphs with univalent and "oriented" trivalent vertices, modulo some relations... first of all, a trivalent vertex is "oriented" if there is a cyclic ordering of the three incident edges. That is, we "orient" the vertex



by drawing a little clockwise or counterclockwise-pointing circle at the vertex. (Or, for those of an algebraic bent, label the edges by 1,2,3 but then mod out by cyclic permutations.) The relations are:

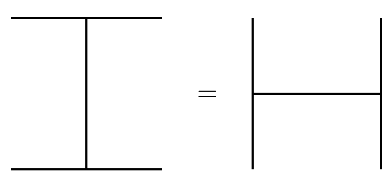
1) if we reverse the orientation of a trivalent vertex, that's equivalent to multiplying the graph by -1. (Remember we're in a vector space spanned by graphs.)

2) 

   (That is, we can make this substitution anywhere we want; these pictures might be part of a bigger graph. Note that the "X" is not a vertex, since there aren't quadrivalent vertices; it's just one edge going over or under another. It doesn't matter whether it goes over or under since these are abstract graphs, not graphs embedded in space.)

Now, let B_m be the vector space spanned by "labelled" finite graphs with univalent and oriented trivalent vertices, modulo some relations... but first I have to say what "labelled" means. It means that each edge is labelled with a 1 or -1. The relations are:

1) if we reverse the orientation of a trivalent vertex, it's the same as multiplying the labellings of all three incident edges by -1.

2) 

   if the internal edge is labelled with a 1. (Here the 4 external edges can have any labellings and we don't mess with that.)

Now, define a linear map from B to B_m by mapping any graph to the signed sum of the $2^{\text{number of edges}}$ ways of labelling the edges with 1 or -1. Symbolically,

$$\underline{\qquad\qquad} \;\mapsto\; \overset{1}{\underline{\qquad\qquad}} \;-\; \overset{-1}{\underline{\qquad\qquad}}$$

Of course, one must work a bit to show this map is well-defined. (This just takes a paragraph — see Proposition 6.5 of Dror's paper.)

Okay, the conjecture is:

THIS MAP IS ONE-TO-ONE.

If you can solve it, you've made great progress in showing that knots and classical Lie groups are just two aspects of the same branch of mathematics. Don't work on it, though, until you get Dror's paper and make sure I stated it exactly right!!!!!

2) Abhay Ashtekar, "Mathematical problems of non-perturbative quantum general relativity", lectures delivered at the 1992 Les Houches summer school on Gravitation and Quantization, December 2, 1992, available as gr-qc/9302024.

This is a good overview of the loop variables approach to quantizing general relativity as it currently stands. It begins with a review of the basic difficulties with quantizing gravity, as viewed from three perspectives: the particle physicist, the mathematical physicist, and the general relativist. Technically, a main problem is that general relativity consists of both evolution equations and constraint equations on the initial data (which are roughly the metric of space at a given time and its first time derivative, or really "extrinsic curvature"). So Ashtekar reviews Dirac's ideas on quantizing constrained systems before sketching how this program is carried out for general relativity.

Then he considers a "toy model" — quantum gravity in $2+1$ dimensions. This is a funny theory because *classically* Einstein's equations in $2+1$ dimensions simply say that spacetime is flat (in a vacuum)! No gravitational waves exist as in $3+1$ dimensions, and one can say that the information in the gravitational field is "purely global" — locally, everywhere looks the same as everywhere else (like Iowa), but there may be global "twists" that you notice when going around a noncontractible loop. There has been a lot of work on $2+1$ gravity recently — in a sense this problem has been solved, by a number of methods — and this allows one to understand *some* of the conceptual difficulties of honest $3+1$-dimensional quantum gravity without getting caught in an endless net of technical complications.

Then Ashtekar jumps back to $3+1$ dimensions and gives a more thorough introduction to the loop variables approach. He ends by going through some of the many open problems and possible ways to attack them.

I have worn myself out trying to do justice to Bar-Natan's work, so I will postpone until next week a review of Kapranov and Voevodsky's paper on 2-categories.

Week 4

February 8, 1993

I will begin with a couple of small things and then talk about the work of Kapranov and Voevodsky.

1) R. Sole, D. Lopez, M. Ginovart and J. Valls, "Self-organized criticality in Monte Carlo simulated ecosystems", *Phys. Lett.* **A172** (1992), p. 56.

This is mainly of interest to me thanks to a reference to some earlier work on Conway's game of Life. At MIT, Tom Toffoli, Norm Margolus, and grad students in the Physics of Computation group build special-purpose computers for simulating cellular automata, the so-called CAM machines. I have spent many enjoyable hours watching beautiful patterns do their thing on a big-screen color TV while CAM 6 busily simulates them on a 256×256 lattice at the rate of many generations a second. (The CAM 8 chip was still being debugged when I last checked.) More recently, Jim Gilliam, a grad student here at UCR, found a very nice program for the game of Life on Xwindows, called xlife. On my Sparcstation it is even bigger and faster than CAM6. One can zoom in and out, and, zooming all the way out, one sees something vaguely reminiscent of nebulae of distant stars twinkling in the night sky... My computer science pal, Nate Osgood, muttered something about the author, Chuck Silvers, using cleverly optimized loops. It apparently can be found using the program archie. Please don't ask *me* for a copy, since it involves many files.

The game of Life is actually one of the less fun cellular automata to watch, since, contrary to its name, if one starts with a random configuration it almost always eventually lapses into an essentially static configuration (perhaps with some blinkers executing simple periodic motions). I am pleased to find that this seemingly dull final state might be fairly interesting in the study of self-organized criticality! Recall that this is the phenomenon whereby a physical system naturally works its way into a state such that the slightest disturbance can have an arbitrarily large effect. The classic example is a sand dune, which apparently works its way towards slopes close to the critical one at which an avalanche occurs. Drop one extra grain of sand on it and you can get a surprisingly wide distribution of possible sizes for the resulting sandslide! Similar but more formidable effects may be at work in earthquakes. The above paper cites

2) Per Bak, Kan Chen, and Michael Creutz, "Self-organized criticality in the 'Game of Life'", *Nature* **342** (1989), 780–782.

which claims that in the final state of the game of Life, the density of clusters $D(s)$ of size s scales as about $s^{-1.4}$, and that the probability that a small perturbation will cause a flurry of activity lasting a time t scales as about $t^{-1.6}$. I'm no expert, but I guess that the fact that the latter is a power law rather than an exponential would be a signal of self-organized criticality. But the paper also cites

3) Charles Bennett and Marc S. Bourzutschy,"'Life' not critical?", *Nature* **350** (1991), 468.

who claim that the work of Bac, Chen and Creutz is wrong. I haven't gotten to read these papers; if anyone wants to report on them I'd be interested.

The paper I read considers fancier variations on this theme, investigating the possibility that ecosystems are also examples of self-organized criticality. It's hard to know how to make solid science out of this kind of thing, but I think there would be important consequences if it turned out that the "balance of nature," far from being a stable equilibrium, was typically teetering on the brink of drastic change.

4) H. D. Zeh, "There are no quantum jumps, nor are there particles!", *Phys. Lett.* **A173** (1993), 189–192.

Having greatly enjoyed Zeh's book *The Physical Basis for the Direction of Time* — perhaps the clearest account of a famously murky subject — I naturally took a look at this particle despite its overheated title. (Certainly exclamation marks in titles should add to one's crackpot index.) It is a nice little discussion of "quantum jumps" and the "collapse of the wavefunction" that takes roughly the viewpoint I espouse, namely, that all one needs is Schrödinger's equation (and lots of hard work) to understand what's going on in quantum theory — no extra dynamical mechanisms. It's not likely to convince anyone who thinks otherwise, but it has references that might be useful no matter which side of the debate one is on.

Also, this just in — what you've all been waiting for — another interpretation of quantum mechanics! It's a book by David Bohm and Basil Hiley, entitled *The Undivided Universe — An Ontological Interpretation of Quantum Theory*. I have only seen an advertisement so far; it's published by Routledge. The contents include such curious things as "the ontological interpretation of boson fields." Read it at your own risk.

5) M. M. Kapranov and V. A. Voevodsky, "2-categories and Zamolodchikov tetrahedra equations" in *Algebraic Groups and their Generalization: Quantum and Infinite-Dimensional Methods, University Park, PA (1991)*, eds. W. J. Haboush and B. J. Parshall, Proc. Sympos. Pure Math. **56**, American Mathematical Society, Providence, Rhode Island, 1994, pp. 177–259.

This serious and rather dry paper is the basis for a lot of physicists are just beginning to try to do: burst from the confines of 3 dimensions, where knots and topological quantum field theories like Chern–Simons theory live, to $3+1$ dimensions, where we live. The "incomplete version" I have now is 220 pages long, mostly commutative diagrams, and doesn't have much to say about physics. But I have a hunch that it will become required reading for many people fairly soon, so I'd like to describe the main ideas in fairly simple terms.

I will start from scratch and then gradually accelerate. First, what's a category? A category consists of a set of 'objects' and a set of 'morphisms'. Every morphism has a 'source' object and a 'target' object. (The easiest example is the category in which the objects are sets and the morphisms are functions. If $f\colon X \to Y$, we call X the source and Y the target.) Given objects X and Y, we write $\mathrm{Hom}(X, Y)$ for the set of morphisms 'from' X 'to' Y (i.e., having X as source and Y as target).

The axioms for a category are that it consist of a set of objects and for any 2 objects X and Y a set $\mathrm{Hom}(X, Y)$ of morphisms from X to Y, and

a) Given a morphism g in $\mathrm{Hom}(X,Y)$ and a morphism f in $\mathrm{Hom}(Y,Z)$, there is morphism which we call $f \circ g$ in $\mathrm{Hom}(X,Z)$. (This binary operation $\circ$ is called 'composition'.)

b) Composition is associative: $(f \circ g) \circ h = f \circ (g \circ h)$.

c) For each object X there is a morphism id_X from X to X, called the 'identity' on X.

d) Given any f in $\mathrm{Hom}(X,Y)$, $f \circ \mathrm{id}_X = f$ and $\mathrm{id}_Y \circ f = f$.

Again, the classic example is Set, the category with sets as objects and functions as morphisms, and the usual composition as composition! But lots of the time in mathematics one is some category or other, e.g.:

- Vect — vector spaces as objects, linear maps as morphisms
- Group — groups as objects, homomorphisms as morphisms
- Top — topological spaces as objects, continuous functions as morphisms
- Diff — smooth manifolds as objects, smooth maps as morphisms
- Ring — rings as objects, ring homomorphisms as morphisms

or in physics:

- Symp — symplectic manifolds as objects, symplectomorphisms as morphisms
- Poiss — Poisson manifolds as objects, Poisson maps as morphisms
- Hilb — Hilbert spaces as objects, unitary operators as morphisms

(The first two are categories in which one can do classical physics. The third is a category in which one can do quantum physics.)

Now, what's a 2-category? This has all the structure of a category but now there are also "2-morphisms," that is, morphisms between morphisms! This is rather dizzying at first. Indeed, much of category theory is rather dizzying until one has some good examples to lean on (at least for down-to-earth people such as myself), so let us get some examples right away, and leave the definition to Kapranov and Voevodsky! My favorite example comes from homotopy theory. Take a topological space X and let the objects of our category be points of X. Given x and y in X, let $\mathrm{Hom}(x,y)$ be the set of all unparametrized paths from x to y. We compose such paths simply by sticking a path from x to y and a path from y to z to get a path from x to z, and we need unparametrized paths to make composition associative. Now given two paths from x to y, say f and g, let $\mathrm{Hom}(f,g)$, the set of 2-morphisms from f to g, be the set of unparametrized homotopies from f to g — that is, ways of deforming the path f continuously to get the path g, while leaving the endpoints fixed.

This is a very enlightening example since homotopies of paths are really just "paths of paths," making the name 2-morphism quite appropriate. (Some of you will already be pondering 3-morphisms, 4-morphisms, but it's too late, they've already been invented!

I won't discuss them here.) The notation for 2-morphisms is quite cute: given f, g in $\mathrm{Hom}(x, y)$, we write F in $\mathrm{Hom}(f, g)$ as the following diagram:

$$\begin{tikzcd} x \arrow[r, bend left=50, "f"] \arrow[r, bend right=50, "g"'] \arrow[r, phantom, "\Downarrow F"] & y \end{tikzcd}$$

In other words, while ordinary morphisms are 1-dimensional objects (arrows), 2-morphisms are 2-dimensional "cells" filling in the space between two ordinary morphisms. We thus see that going up to "morphisms between morphisms" is closely related to going up to higher dimensions. And this is really why "braided monoidal 2-categories" may play as big a role in four-dimensional field theory as "braided monoidal categories" do in three-dimensional field theory!

Rather than write down the axioms for a 2-category, which are in Kapranov and Voevodsky, let me note the key new thing about 2-morphisms: there are two ways to compose them, "horizontally" and "vertically". First of all, given the following situation:

$$\begin{tikzcd} x \arrow[r, bend left=50, "f"] \arrow[r, bend right=50, "g"'] \arrow[r, phantom, "\Downarrow F"] & y \arrow[r, bend left=50, "f'"] \arrow[r, bend right=50, "g'"'] \arrow[r, phantom, "\Downarrow F'"] & z \end{tikzcd}$$

we can compose F and F' horizontally to get a 2-morphism from $f' \circ f$ to $g \circ g'$. (Check this out in the example of homotopies!) But also, given the following situation:

$$\begin{tikzcd} x \arrow[r, bend left=70, "f"] \arrow[r, "g"] \arrow[r, bend right=70, "h"'] \arrow[r, phantom, shift left=3.5ex, "\Downarrow F"] \arrow[r, phantom, shift right=3.5ex, "\Downarrow G"] & y \end{tikzcd}$$

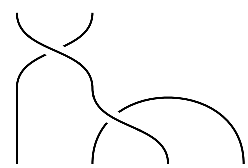
(f, g, h in $\mathrm{Hom}(x, y)$, F in $\mathrm{Hom}(f, g)$, and G in $\mathrm{Hom}(g, h)$), we can compose F and G vertically to get a 2-morphism from f to g.

As Kapranov and Voevodsky note: "Thus 2-categories can be seen as belonging to the realm of a new mathematical discipline which may be called **2-dimensional algebra** and contrasted with usual 1-dimensional algebra dealing with formulas which are written in lines." This is actually very important because already in the theory of braided monoidal categories we began witnessing the rise of mathematics that incorporated aspects of geometry into the notation itself.

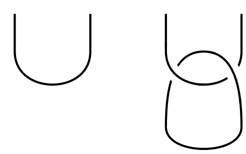
The theory of 2-categories is not new; it was apparently invented by Ehresmann, Benabou and Grothendieck in an effort to formalize the structure possessed by the category of all categories. (If this notion seems dangerously close to Russell's paradox, you are right — but I will not worry about such issues in what follows.) This category has as its objects categories and as its morphisms "functors" between categories. It is, in fact, a 2-category, taking as the 2-morphisms "natural transformations" between functors. (For a brief intro to functors and natural transformations, try my webpage "categories".) What is new to Kapranov and Voevodsky is the notion of a monoidal 2-category — where one can take tensor products of objects, morphisms, and 2-morphisms — and "braided" monoidal 2-category — where one has "braidings" that switch around the two factors in a tensor product.

Let me turn to the possible relevance of all this to mathematical physics. Here there is a nice 2-category, namely the category of "2-tangles." First recall the category of tangles:

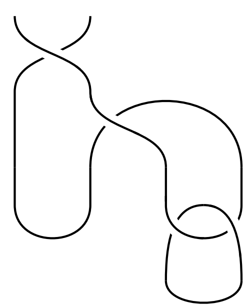
The objects are simply the natural numbers $\{0, 1, 2, 3, \ldots\}$. We think of the object n as a horizontal row of n points. The morphisms in $\mathrm{Hom}(n, m)$ are tangles connecting a row of n points above to a row of m points below. Rather than define "tangles" I will simply draw pictures of some examples. Here is an element of $\mathrm{Hom}(2, 4)$:



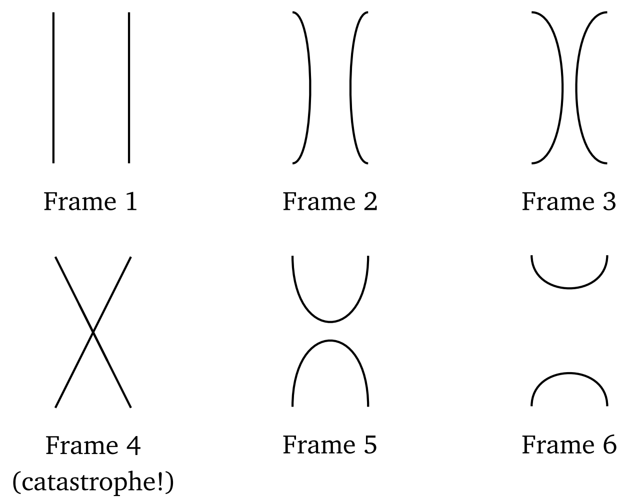
and here is an element of $\mathrm{Hom}(4, 0)$:



Note that we can "compose" these tangles to get one in $\mathrm{Hom}(2, 0)$:



Now, given tangles f, g in $\mathrm{Hom}(m, n)$, a 2-morphism from f to g is a "2-tangle." I won't define these either, but we may think of a 2-tangle from f to g roughly as a "movie" whose first frame is the tangle f and last frame is the tangle g, and each of whose intermediate frames is a tangle except at certain times when a catastrophe occurs. For example, here's a 2-tangle shown as a movie...



Well, it'll never win an Academy Award, but this movie is pretty important. It's a picture of the 3-dimensional slices of a 2-dimensional surface in (3+1)-dimensional spacetime, and this surface is perfectly smooth but has a saddle point which we are seeing in frame 4. It is one of what Carter and Saito (see "Week 2") call the "elementary string interactions." The relevance to string theory is pretty obvious: we are seeing a movie of part of a string worldsheet, which is a surface in (3+1)-dimensional spacetime. My interest in 2-tangles and 2-categories is precisely because they offer a bridge between string theory and the loop variables approach to quantum gravity, which may actually be the **same thing** in two different disguises. You heard it here first, folks!

The reader may have fun figuring out what the two ways of composing 2-morphisms amount to in the category of 2-tangles.

There are, in fact, many clues as to the relation between string theory and 2-categories, one being the Zamolodchikov equation. This is the analog of the Yang–Baxter equation — an equation important in the theory of braids — one dimension up. It was discovered by Zamolodchikov in 1980; a 1981 paper that might be a bit easier to get is

6) A. B. Zamolodchikov, "Tetrahedron equations and relativistic S-matrix for straight strings in 2+1 dimensions," *Commun. Math. Phys.* **79** (1981), 489–505.

(It plays a different role in the (3+1)-dimensional context, though.) Just as braided monoidal categories are a good way to systematically find solutions of the Yang–Baxter equation, braided monoidal 2-categories, as defined by Kapranov and Voevodsky, seem to be a good way for finding solutions for the Zamolodchikov equation. (I will post in a while about a new paper by Soibelman and Kazhdan that does this. Also see the paper by Crane and Frenkel in "Week 2".)

There are also lots of tantalizing ties between the loop variables approach to quantum gravity and 2-categories; one can see some of these if one reads the work of Carter and Saito in conjunction with my paper "Quantum gravity and the algebra of tangles" (`hep-th/9205007`). I hope to make these a lot clearer as time goes by.

Week 5

February 13, 1993

I think I'll start out this week's list of finds with an elementary introduction to Lie algebras, so that people who aren't "experts" can get the drift of what these are about. Then I'll gradually pick up speed...

1) Vyjayanathi Chari and Alexander Premet, "Indecomposable restricted representations of quantum $\mathfrak{sl}_2$", University of California at Riverside, preprint.

Vyjayanathi is our resident expert on quantum groups, and Sasha, who's visiting, is an expert on Lie algebras in characteristic p. They have been talking endlessly across the hall from me and now I see that it has born fruit. This is a pretty technical paper and I am afraid I'll never really understand it, but I can see why it's important, so I'll try to explain that!

Let me start with the prehistory, which is the sort of thing everyone should learn. Recall what a Lie algebra is... a vector space with a "bracket" operation such that the bracket $[x, y]$ of any two vectors x and y is again a vector, and such that the following hold:

a) skew-symmetry: $[x, y] = -[y, x]$.

b) bilinearity: $[x, ay] = a[x, y]$, $[x, y + z] = [x, y] + [x, z]$. ($a$ is any number)

c) Jacobi identity: $[x, [y, z]] + [y, [z, x]] + [z, [x, y]] = 0$.

These conditions, especially the third, may look sort of weird if you are not used to them, but the examples make it all clear. The easiest example of a Lie algebra is $\mathfrak{gl}(n, \mathbb{C})$, which just means all $n \times n$ complex matrices with the bracket defined as the "commutator":

$$[x, y] = xy - yx.$$

Then straightforward calculations show a)–c) hold... so these conditions are really encoding the essence of the commutator.

Now recall that the trace of a matrix, the sum of its diagonal entries, satisfies $\mathrm{tr}(xy) = \mathrm{tr}(yx)$. So the trace of any commutator is zero, and if we let $\mathfrak{sl}(n, \mathbb{C})$ denote the matrices with zero trace, we see that it's a sub-Lie algebra of $\mathfrak{gl}(n, \mathbb{C})$ — that is, if x and y are in $\mathfrak{sl}(n)$ so is $[x, y]$, so we can think of $\mathfrak{sl}(n, \mathbb{C})$ as a Lie algebra in its own right. Going from $\mathfrak{sl}(n, \mathbb{C})$ to $\mathfrak{gl}(n, \mathbb{C})$ is essentially a trick for booting out the identity matrix, which commutes with everything else (hence has vanishing commutators). The identity matrix is the only one with this property, so it's sort of weird, and it simplifies things to get rid of it here.

The simplest of the $\mathfrak{sl}(n, \mathbb{C})$'s is the Lie algebra $\mathfrak{sl}(2, \mathbb{C})$, affectionately known simply as $\mathfrak{sl}(2)$, which is a 3-dimensional Lie algebra with a basis given by matrices people call E, F, and H for mysterious reasons:

$$E = \begin{pmatrix} 0 & 1 \\ 0 & 0 \end{pmatrix} \quad F = \begin{pmatrix} 0 & 0 \\ 1 & 0 \end{pmatrix} \quad H = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}$$

You will never be an expert on Lie algebras until you know by heart that

$$[H, E] = 2E, \quad [H, F] = -2F, \quad [E, F] = H.$$

Typically that's the sort of remark I make before screwing up by a factor of two or something, so you'd better check! This is a cute little multiplication table... but very important, since $\mathfrak{sl}(2)$ is the primordial Lie algebra from which the whole theory of "simple" Lie algebras unfolds.

Physicists are probably more familiar with a different basis of $\mathfrak{sl}(2)$, the Pauli matrices:

$$\sigma_1 = \begin{pmatrix} 0 & 1 \\ 1 & 0 \end{pmatrix} \quad \sigma_2 = \begin{pmatrix} 0 & -i \\ i & 0 \end{pmatrix} \quad \sigma_3 = \begin{pmatrix} 1 & 0 \\ 0 & -1 \end{pmatrix}$$

For purposes of Lie algebra theory it's actually better to divide each of these matrices by i and call the resulting matrices I, J, and K, respectively. We then have

$$IJ = -JI = K, \quad JK = -KJ = I, \quad KI = -IK = J, \quad I^2 = J^2 = K^2 = -1$$

which is just the multiplication table of the quaternions! From the point of view of Lie algebras, though, all that matters is

$$[I, J] = 2K, \quad [J, K] = 2I, \quad [K, I] = 2J.$$

Given the relation of these things and cross products, it should be no surprise that the Pauli matrices have a lot to with angular momentum around the x, y, and z axes in quantum mechanics.

If we take all *real* linear combinations of E,F,H we get a Lie algebra over the *real* numbers called $\mathfrak{sl}(2, \mathbb{R})$, and if we take all real linear combinations of I,J,K we get a Lie algebra over the reals called $\mathfrak{su}(2)$. These two Lie algebras are two different "real forms" of $\mathfrak{sl}(2)$.

Now, people know just about everything about $\mathfrak{sl}(2)$ that they might want to. Well, there's always something more, but I'm certainly personally satisfied! I recall when as an impressionable student I saw a book by Serge Lang titled simply "$\mathrm{SL}(2, \mathbb{R})$", big and fat and scary inside. I knew what $\mathrm{SL}(2, \mathbb{R})$ was, but not how one could think of a whole book's worth of things to write about it! A whole book on 2×2 matrices??

Part of how one gets so much to say about a puny little Lie algebra like $\mathfrak{sl}(2)$ is by talking about its representations. What's a representation? Well, first you have to temporarily shelve the idea that $\mathfrak{sl}(2)$ consists of 2×2 matrices, and think of it more abstractly simply as a 3-dimensional vector space with basis E,F,H, equipped with a Lie algebra structure given by the multiplication table $[H, E] = 2E$, $[H, F] = -2F$, $[E, F] = H$. If this is how I'd originally defined it, it would then be a little *theorem* that this Lie algebra has a "representation" as 2×2 matrices. And it would turn out to have other representations too. For example, there's a representation as 3×3 matrices given by sending

$$E \mapsto \begin{pmatrix} 0 & 1 & 0 \\ 0 & 0 & 2 \\ 0 & 0 & 0 \end{pmatrix} \quad F \mapsto \begin{pmatrix} 0 & 0 & 0 \\ 2 & 0 & 0 \\ 0 & 1 & 0 \end{pmatrix} \quad H \mapsto \begin{pmatrix} 2 & 0 & 0 \\ 0 & 0 & 0 \\ 0 & 0 & -2 \end{pmatrix}$$

In other words, these matrices satisfy the same commutation relations as E,F, and H do.

More generally, and more precisely, we say an n-dimensional representation of a Lie algebra L (over the complex numbers) is a linear function R from L to $n \times n$ matrices such that

$$R([x, y]) = [R(x), R(y)]$$

for all x, y in L. Note that on the left the brackets are the brackets in L, while on the right they denote the commutator of $n \times n$ matrices.

One good way to understand the essence of a Lie algebra is to figure out what representations it has. And in quantum physics, Lie algebra representations are where it's at: the symmetries of the world are typically Lie groups, each Lie group has a corresponding Lie algebra, the states of a quantum system are unit vectors in a Hilbert space, and if the system has a certain Lie group of symmetries there will be a representation of the Lie algebra on the Hilbert space. As any particle physicist can tell you, you can learn a lot just by knowing which representation of your symmetry group a given particle has.

So the name of the game is classifying Lie algebra representations... and many tomes have been written on this by now. To keep things from becoming too much of a mess it's crucial to make two observations. First, there's an easy way to get new representations by taking the "direct sum" of old ones: the sum of an n-dimensional representation and an m-dimensional one is an $(n + m)$-dimensional one, for example. Another way, not so easy, to get new representations from an old one is to look for "subrepresentations" of the given representation. In particular, a direct sum of two representations has them as subrepresentations. (I won't define "direct sum" and "subrepresentation" here... hopefully those who don't know will be tempted to look it up.)

So rather than classifying *all* representations, it's good to start by classifying "irreducible" representations — those that have no suprepresentations (other than themselves and the trivial 0-dimensional representation). This is sort of like finding prime numbers... they are "building blocks" in representation theory. But things are a little bit messier, alas. We say a representation is "completely reducible" if it is a direct sum of irreducible representations. Unfortunately, not all representations need be completely reducible!

Let's consider the representations of $\mathfrak{sl}(2, \mathbb{C})$. (The more sophisticated reader should note that I am implicitly only considering finite- dimensional complex representations!) Here life is as nice as could be: all representations are completely reducible, and there is just one irreducible n-dimensional representation for each n, with the 2-dimensional and 3-dimensional representations as above. (By the way, I really mean that there is only one irreducible n-dimensional representation up to a certain equivalence relation!) Physicists — who more often work with the real form $\mathfrak{su}(2)$ — call these the spin-0, spin-$\frac{1}{2}$, spin-1, etc. representations. The "spin" of a particle is, in mathematical terms, just the thing that tells you which representation of $\mathfrak{su}(2)$ it corresponds to!

Now let me jump up several levels of sophistication. In the last few years people have realized that Lie groups are just a special case of something called "quantum groups"... nobody talks about "quantum Lie algebras" but that's essentially a historical accident: quantum groups are *not* groups, they're a generalization of them, and they *don't* have Lie algebras, but they have a generalization of them — so-called quantized enveloping algebras.

Quantum groups can be formed from simple Lie algebras, and they depend on a parameter q, a nonzero complex parameter. This parameter — q is for quantum, naturally — can be thought of as

$$e^{\hbar}$$

The exponential of Planck's constant! When we set $\hbar = 0$ we get $q = 1$, and we get back to the "classical case" of plain old-fashioned Lie algebras and groups. Every representation of a quantum group gives an invariant of links (actually even tangles), and these link invariants are functions of q. If we take the nth derivative of one of these invariants with respect to $\hbar$ and evaluate it at $\hbar = 0$ we get a "Vassiliev invariant of degree n" (see "Week 3" for the definition). Better than that, when q is a root of unity each quantum group gives us a 3-dimensional "topological quantum field theory," or TQFT known as Chern–Simons theory. In particular, we get an invariant of compact oriented 3-manifolds. So there is a hefty bunch of mathematical payoffs from quantum groups. And there are good reasons to think of them as the right generalization of groups for dealing with symmetries in the physics of 2 and 3 dimensions. If string theory *or* the loop variables approach to quantum gravity have any truth to them, quantum groups play a sneaky role in honest 4-dimensional physics too.

In particular, there is a quantum version of $\mathfrak{sl}(2)$ called $\mathfrak{sl}_q(2)$. When $q = 1$ we essentially have the good old $\mathfrak{sl}(2)$. Chari and Premet have just worked out a lot of the representation theory of $\mathfrak{sl}_q(2)$. First of all, it's been known for some time that as long as q is not a root of unity — that is, as long as we don't have

$$q^n = 1$$

for some integer n — the story is almost like that for ordinary $\mathfrak{sl}(2)$. Namely, there is one irreducible representation of each dimension, and all representations are completely reducible. This fails at roots of unity — which turns out to be the reason why one can cook up TQFTs in this case. It turns out that if q is an nth root of unity one can still define representations of dimension 0,1,2,3, etc., more or less just like the classical case, but only those of dimension $\leqslant n$ are irreducible. There are, in fact, exactly n irreducible representations, and the fact that there are only finitely many is what makes all sorts of neat things happen. The k-dimensional representations with $k \geqslant n$ are not completely reducible. And, besides the representations that are analogous to the classical case, there are a bunch more. They have not been completely classified — they are, according to Chari, a mess! But she and Premet have classified a large batch of the "indecomposable" ones, that is, the ones that aren't direct sums of other ones. I guess I'll leave it at that.

2) David Kazhdan and Iakov Soibelman, "Representations of the quantized function algebras, 2-categories and Zamolodchikov tetrahedra equations", *The Gelfand Mathematical Seminars 1990–1992*, Springer, Berlin, 1993, pp. 163–71.

In this terse paper, Kazhdan and Soibelman construct a braided monoidal 2-category using quantum groups at roots of unity. As I've said a few times, people expect braided monoidal 2-categories to play a role in generally covariant 4d physics analogous to what braided monoidal categories do in 3d physics. In particular, one might hope to get invariants of 4-dimensional manifolds, or of surfaces embedded in 4-manifolds, this way. (See last week's post for a little bit about the details.) I don't feel I understand this construction well enough yet to want to say much about it, but it is clearly related to the construction of a braided monoidal 2-category from the category of quantum group representations given by Crane and Frenkel (see "Week 2").

3) Adrian Ocneanu, "A note on simplicial dimension shifting", available as hep-th/9302028.

Ouch! This paper claims to show that the very charming 4d TQFT constructed by Crane and Yetter in "A categorical construction of 4d topological quantum field theories" (hep-th/9301062) is trivial! In particular, he says the resulting invariant of compact oriented 4-manifolds is identically equal to 1. If so, it's back to the drawing board. Crane and Yetter took the 3d TQFT coming from $\mathfrak{sl}_q(2)$ at roots of unity and then used a clever trick to get 3-manifolds from a simplicial decomposition of a 4-manifold to get a 4d TQFT. Ocneanu claims this trick, which he calls "simplicial dimension shifting," only gives trivial 4-manifold invariants.

I am not yet in a position to pass judgement on this, since both Crane/Yetter and Ocneanu are rather sketchy in key places. If indeed Ocneanu is right, I think people are going to have to get serious about facing up to the need for 2-categorical thinking in 4-dimensional generally covariant physics. I had asked Crane, a big proponent of 2-categories, why they played no role in his 4d TQFT, and he said that indeed he felt like the kid who took apart a watch, put it back together, and found it still worked even though there was a piece left over. So maybe the watch didn't really work without that extra piece after all. In late March I will go to the Conference on Quantum Topology thrown by Crane and Yetter (at Kansas State U. at Manhattan), and I'm sure everyone will try to thrash this stuff out.

4) Abhay Ashtekar and Jerzy Lewandowski, "Representation theory of analytic holonomy C^*-algebras", available as gr-qc/9311010.

This paper is a follow-up of the paper

5) Abhay Ashtekar and Chris Isham, "Representations of the holonomy algebras of gravity and non-Abelian gauge theories", *Journal of Classical and Quantum Gravity* **9** (1992), 1069–1100. Also available as hep-th/9202053.

and sort of complements another,

6) John Baez, "Link invariants, holonomy algebras and functional integration", available as hep-th/9301063.

The idea here is to provide a firm mathematical foundation for the loop variables representation of gauge theories, particularly quantum gravity. Ashtekar and Lewandowski consider an algebra of gauge-invariant observables on the space of $\mathfrak{su}(2)$ connections on any real-analytic manifold, namely that generated by piecewise analytic Wilson loops. This is the sort of thing meant by a "holonomy algebra". They manage to construct an explicit diffeomorphism-invariant state on this algebra. They also relate this algebra to a similar algebra for $\mathfrak{sl}(2)$ connections — the latter being what really comes up in quantum gravity. And they do a number of other interesting things, all quite rigorously. My paper dealt instead with an algebra generated by "regularized" or "smeared" Wilson loops, and showed that there was a 1-1 map from diffeomorphism-invariant states on this algebra to invariants of framed links — thus showing that the loop variables picture, in which states are given by link invariants, doesn't really lose any of the physics present in traditional approaches to gauge theories. I am busy at work trying to combine Ashtekar and Lewandowski's ideas with my own and push this program further — my own personal goal being to make the Chern–Simons path integral rigorous — it being one of those mysterious "measures on the space of all connections mod gauge transformations" that physicists like, which unfortunately aren't really measures, but some kind of generalization thereof. What it *should* be is a state (or continuous linear functional) on some kind of holonomy algebra.

Week 6

February 20, 1993

1) Alexander Vilenkin, "Quantum cosmology", talk given at Texas/Pascos 1992 at Berkeley, available as gr-qc/9302016.

This is, as Vilenkin notes, an elementary review of quantum cosmology. It won't be news to anyone who has kept up on that subject (except perhaps for a few speculations at the end), but for those who haven't been following this stuff, like myself, it might be a good way to get started.

Let's get warmed up....

Quantizing gravity is mighty hard. For one thing, there's the "problem of time" — the lack of a distinguished time parameter in *classical* general relativity means that the usual recipe for quantizing a dynamical system — "represent time evolution by the unitary operators $\exp(-iHt)$ on the Hilbert space of states, where t is the time and H, the Hamiltonian, is a self-adjoint operator" — breaks down! As Wheeler so picturesquely put it, in general relativity we have "many-fingered time"; there are lots of ways of pushing a spacelike surface forwards in time.

But if we simplify the heck out of the problem, we might make a little progress. (This is a standard method in physics, and whether or not it's really justified, it's often the only thing one can do!) For one thing, note that in the Big Bang cosmology there is a distinguished "rest frame" (or more precisely, field of timelike vectors) given by the galaxies, if we discount their small random motions. In reality these are maybe not so small, and maybe not so random — such things as the "Virgo flow" show this — but we're talking strictly theory here, okay? — so don't bother us with facts! So, if we imagine that things go the way the simplest Big Bang models predict, the galaxies just sit there like dots on a balloon that is being inflated, defining a notion of "rest" at each point in spacetime. This gives a corresponding notion of time, since one can measure time using clocks that are at rest relative to the galaxies. Then, since we are pretending the universe is completely homogeneous and isotropic — and let's say it's a closed universe in the shape of a 3-sphere, to be specific — the metric is given by

$$dt^2 - r(t)^2[(d\psi)^2 + (\sin\psi)^2(d\theta)^2 + (\sin\theta)^2(d\varphi)^2]$$

What does all this mean? Here $r(t)$ is the radius of the universe as a function of time, the following stuff is just the usual metric on the unit 3-sphere with hyperspherical coordinates ψ, θ, φ generalizing the standard coordinates on the 2-sphere we all learn in college:

$$(d\psi)^2 + (\sin\psi)^2(d\theta)^2 + (\sin\theta)^2(d\varphi)^2$$

and the fact that the metric on spacetime is dt^2 minus a bunch of stuff reflects the fact that spacetime geometry is "Lorentzian," just as in flat Minkowski space the metric is

$$dt^2 - dx^2 - dy^2 - dz^2.$$

The name of the game in this simple sort of Big Bang cosmology is thus finding the function $r(t)$! To do this, of course, we need to see what Einstein's equations reduce to in this special case, and since Einstein's equations tell us how spacetime curves in response to the stress-energy tensor, this will depend on what sort of matter we have around. We are assuming that it's homogeneous and isotropic, whatever it is, so it turns out that all we need to know is its density ρ and pressure P (which are functions of time). We get the equations

$$r''/r = -\frac{4\pi}{3}(\rho + 3P)$$
$$(r')^2 = \frac{8\pi}{3}\rho r^2 - 1$$

Here primes denote differentiation with respect to t, and I'm using units in which the gravitational constant and speed of light are equal to 1.

Let's simplify this even more. Let's assume our matter is "dust," which is the technical term for zero pressure. We get two equations:

$$\begin{aligned} r''/r &= -\frac{4\pi}{3}\rho \\ (r')^2 &= \frac{8\pi}{3}\rho r^2 - 1 \end{aligned} \tag{1}$$

Now let's take the second one, differentiate with respect to t,

$$2r''r' = \frac{8\pi}{3}(\rho' r^2 + 2\rho r r')$$

plug in what the first equation said about r'',

$$-\frac{8\pi}{3}\rho r r' = \frac{8\pi}{3}(\rho' r^2 + 2\rho r r')$$

clear out the crud, and lo:

$$3\rho r' = -\rho' r$$

or, more enlighteningly,

$$\frac{d(\rho r^3)}{dt} = 0.$$

This is just "conservation of dust" — the dust density times the volume of the universe is staying constant. This, by the way, is a special case of the fact that Einstein's equations *automatically imply* local conservation of energy (i.e., that the stress-energy tensor is divergence-free).

Okay, so let's say $\rho r^3 = D$, with D being the total amount of dust. Then we can eliminate ρ from equations (1) and get:

$$\begin{aligned} r'' &= -\frac{4\pi D}{3r^2} \\ (r')^2 - \frac{8\pi}{3}\frac{D}{r} &= -1 \end{aligned} \tag{2}$$

What does this mean? Well, the first one looks like it's saying there's a force trying to make the universe collapse, and that the strength of this force is proportional to $1/r^2$. Sound vaguely familiar? It's actually misleadingly simple — if we had put in something besides dust it wouldn't work quite this way — but as long as we don't take it too seriously, we can just think of this as gravity trying to get the universe to collapse. And the second one looks like it's saying that the "kinetic" energy proportional to $(r')^2$, plus the "potential" energy proportional to $-1/r$, is constant! In other words, we have a nice analogy between the Big Bang cosmology and a very old-fashioned system, a classical particle in one dimension attracted to the origin by a $1/r^2$ force!

It's easy enough to solve this equation, and easier still to figure it out qualitatively. The key thing is that since the total "energy" in the second equation of (2) is negative, there won't be enough "energy" for r to go to infinity, that is, there'll be a Big Bang and then a big crunch. Here's r as a function of t, roughly:

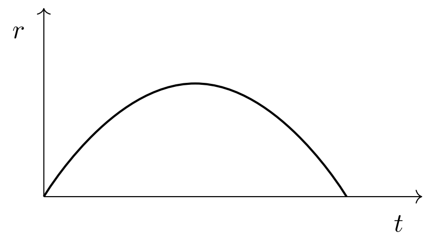


Figure 1

What goes up, must come down! This curve, which I haven't drawn too well, is just a cycloid, which is the curve traced out by a point on the rim of rolling wheel. So, succumbing to romanticism momentarily we could call this picture *one turn of the great wheel of time*.... But there is *no* reason to expect further turns, because the differential equation simply becomes singular when $r = 0$. We may either say it doesn't make sense to speak of "before the Big Bang" or "after the big crunch" — or we can look for improved laws that avoid these singularities. (I should repeat that we are dealing with unrealistic models here, since for example there is no evidence that there is enough matter around to "close the universe" and make this solution qualitatively valid — it may well be that there's a Big Bang but no big crunch. In this case, there's only one singularity to worry about, not two.)

People have certainly not been too ashamed to study the *quantum* theory of this system (and souped-up variants) in an effort to get a little insight into quantum gravity. We would expect that quantum effects wouldn't matter much until the radius of the universe is very small, but when it *is* very small they would matter a lot, and maybe — one might hope — they would save the day, preventing the nasty singularities. I'm not saying they *do* — this is hotly debated — but certainly some people hope they do. Of course, serious quantum gravity should take into account the fact that geometry of spacetime has all sorts of wiggles in it — it isn't just a symmetrical sphere. This may make a vast difference in how things work out. (For example, the big crunch would be a lot more exciting if there were lots of black holes around by then.) The technical term for the space of all metrics on space is "superspace" (sigh), and the toy models one gets by ignoring all but finitely many degrees of freedom are called "minisuperspace" models.

Let's look at a simple minisuperspace model. The simplest thing to try is to take the classical equations of motion (2) and try to quantize them just like one would a particle in a potential. This is a delicate business, by the way, because one can't just take some classical equations of motion and quantize them in any routine way. There are lots of methods of quantization, but all of them require a certain amount of case-by-case finesse.

The idea of "canonical quantization" of a classical system with one degree of freedom — like our Big Bang model above, where the one degree of freedom is r — is to turn the "position" (that's r) into a multiplication operator and the "momentum" (often that's something like r', but watch out!) into a differentation operator, say $-i\hbar\frac{d}{dr}$, so that we get the "canonical commutation relations"

$$[-i\hbar\frac{d}{dr}, r] = -i\hbar.$$

We then take the formula for the energy, or Hamiltonian, in terms of position and momentum, and plug in these operators, so that the Hamiltonian becomes an operator. (Here various "operator-ordering" problems can arise, because the position and momentum commuted in the original classical system but not anymore!) To explain what I mean, why don't I just do it!

So: I said that the formula

$$(r')^2 - \frac{8\pi}{3}\frac{D}{r} = -1 \tag{3}$$

looks a lot like a formula of the form "kinetic energy plus potential energy is constant". Of course, we could multiply the whole equation by anything and get a valid equation, so it's not obvious that the "right" Hamiltonian is

$$(r')^2 - \frac{8\pi}{3}\frac{D}{r}$$

or (adding 1 doesn't hurt)

$$(r')^2 - \frac{8\pi}{3}\frac{D}{r} + 1$$

In fact, note that multiplying the Hamiltonian by some function of r just amounts to reparametrizing time, which is perfectly fine in general relativity. In fact, Vilenkin and other before him have decided it's better to multiply the Hamiltonian above by r^2. Why? Well, it has to do with figuring out what the right notion of "momentum" is corresponding to the "position" r. Let's do that. We use the old formula

$$p = \frac{dL}{dq'}$$

relating momentum to the Lagrangian, where for us the position, usually called q, is really r.

The Lagrangian of general relativity is the "Ricci scalar" R — a measure of curvature of the metric — and in the present problem it turns out to be

$$R = 6\left(\frac{r''}{r} + \frac{(r')^2}{r^2}\right)$$

But we are reducing the full field theory problem down to a problem with one degree of freedom, so our Lagrangian should be the above integrated over the 3-sphere, which has volume $16\pi r^3/3$, giving us

$$32\pi(r''r^2 + (r')^2 r)$$

However, the r'' is a nuisance, and we only use the integral of the Lagrangian with respect to time (that's the action, which classically is extremized to get the equations of motion), so let's do an integration by parts, or in other words add a total divergence, to get the Lagrangian

$$L = -32\pi(r')^2 r.$$

Differentiating with respect to r' we get the momentum "conjugate to r",

$$p = -64\pi r' r.$$

Now I notice that Vilenkin uses as the momentum simply $-r'r$, somehow sweeping the monstrous 64π under the rug. I have the feeling that this amounts to pushing this factor into the definition of $\hbar$ in the canonical commutation relations. Since I was going to set $\hbar$ to 1 in a minute anyway, this is okay (honest). So let's keep life simple and use

$$p = -r'r.$$

Okay! Now here's the point, we want to exploit the analogy with good old quantum mechanics, which typically has Hamiltonians containing something like p^2. So let's take our preliminary Hamiltonian

$$(r')^2 - \frac{8\pi}{3}\frac{D}{r} + 1$$

and multiply it by r^2, getting

$$H = p^2 - \frac{8\pi D}{3} r + r^2.$$

Hey, what's this? A harmonic oscillator! (Slightly shifted by the term proportional to r.) So the universe is just a harmonic oscillator... I guess that's why they stressed that so much in all my classes!

Actually, despite the fact that we are working with a very simple model of quantum cosmology, it's not quite *that* simple. First of all, recall our original classical equation, (3). This constrained the energy to have a certain value. I.e., we are dealing not with a Hamiltonian in the ordinary sense, but a "Hamiltonian constraint" — typical of systems with time reparametrization invariance. So our quantized equation says that the "wavefunction of the universe," $\psi(r)$, must satisfy

$$H\psi = 0.$$

Also, unlike the ordinary harmonic oscillator we have the requirement that $r \geqslant 0$. In other word, we're working with a problem that's like a harmonic oscillator and a "wall" that keeps $r \geqslant 0$. Think of a particle in a potential like this:

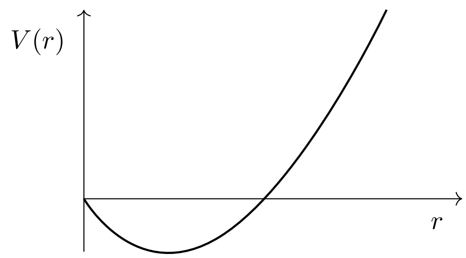


Figure 2

Here $V(r) = -(8\pi D/3)r + r^2$. The minimum of V is at $r = 4\pi D/3$ and the zeroes are at $r = 0$ and $8\pi D/3$. Classically, a particle with zero energy starting at $r = 0$ will roll to the right and make it out to $r = 4\pi D/3$ before rolling back to $r = 0$. This is basically the picture we had in Figure 1, except that we've reparametrized time so we have simple harmonic motion instead of cycloid.

Quantum mechanically, however one must pick boundary conditions at $r = 0$ to make the problem well-defined!

This is where the fur begins to fly!! Hawking and Vilenkin have very different ideas about what the right boundary conditions are. And note that this is not a mere technical issue, since they determine the wavefunction of the universe in this approach! I will not discuss this since Vilenkin does so quite clearly, and if you understand what I have written above you'll be in a decent position to understand him. I will just note that Vilenkin, rather than working with a universe full of "dust," considers a universe in which the dominant contribution to the stress-energy tensor is the cosmological constant, that is, the negative energy density of a "false vacuum", which believers in inflation (such as Vilenkin) think powered the exponential growth of the universe at an early stage. So his equations are slightly different from those above (and are only meant to apply to the early history of the universe).

[Let me just interject a question to the experts if I may — since I've written this long article primarily to educate myself. It would seem to me that the equation $H\psi = 0$ above would only have a normalizable solution if the boundary conditions were fine-tuned! I.e., maybe the equation $H\psi = 0$ itself determines the boundary conditions! This would be very nice; has anyone thought of this? It seems reasonable because, with typical boundary conditions, the operator H above will have pure point spectrum (only eigenvalues) and it would be rather special for one of them to be 0, allowing a normalizable solution of $H\psi = 0$. Also, corrections and education of any sort are welcomed. I would love to discuss this with some experts.]

Anyway, suppose we find some boundary conditions and calculate ψ, the "wavefunction of the universe." (I like repeating that phrase because it sounds so momentous, despite the fact that we are working with a laughably oversimplified toy model.) What then? What are the implications for the man in the street?

Let me get quite vague at this point. Think of the radius of the universe as analogous to a particle moving in the potential of Figure 2. In the current state of affairs classical mechanics is an excellent approximation, so it seems to trace out a classical trajectory. Of course it is really obeying the laws of quantum mechanics, so the trajectory is really a "wave packet" — technically, we use the WKB approximation to see how the wave packet can seem like a classical trajectory. But near the Big Bang or big crunch, quantum mechanics matters a lot: there the potential is rapidly varying (in our simple model it just becomes a "wall") and the wave packet may smear out noticeably. (Think of how when you shoot an electron at a nucleus it bounces off in an unpredictable direction — it's wavefunction just tells you the *probability* that it'll go this way or that!) So some quantum cosmologists have suggested that if there is a big crunch, the universe will pop back out in a highly unpredictable, random kind of way!

I should note that Vilenkin has a very different picture. Since this stuff makes large numbers of assumptions with very little supporting evidence, it is science that's just on the brink of being mythology. Still, it's very interesting.

2) Lee Smolin, "Finite, diffeomorphism invariant observables in quantum gravity", available as gr-qc/9302011.

The big problem in canonical quantization of gravity, once one gets beyond "mini-" and "midisuperspace" models, is to find enough diffeomorphism-invariant observables. There is a certain amount of argument about this stuff, and various approaches, but one common viewpoint is that the "physical" observables, that is, the really observable observables, in general relativity are those that are invariant under all diffeomorphisms of spacetime. I.e., those that are independent of any choice of coordinates. For example, saying "My position is $(242, 2361, 12, -17)$" is not diffeomorphism-invariant, but saying "I'm having the time of my life" is. It's hard to find lots of (tractable) diffeomorphism invariant observables — or even any! Try figuring out how you would precisely describe the shape of a rock without introducing any coordinates, and you'll begin to see the problem. (The quantum mechanical aspects make it harder.)

Rovelli came up a while back with a very clever angle on this problem. It's rather artificial but still a big start. Using a "field of clocks" he was able to come up with interesting diffeomorphism invariant observables. The idea is simply that if you had clocks all around you could say "when the bells rang 2 a.m. I was having the time of my life" — and this would be a diffeomorphism-invariant statement, since rather than referring to an abstract coordinate system it expresses the coincidence of two physical occurences, just like "the baseball broke through the window". Then he pushed this idea to define "evolving constants of motion" — a deliberate oxymoron — to deal with the famous "problem of time" in general relativity: how to treat time evolution in a coordinate-free manner on a spacetime that's not flat and, worse, whose geometry is "uncertain" a la Heisenberg? This is treated, by the way, in

3) Carlo Rovelli, "Time in quantum gravity: an hypothesis", *Phys. Rev.* **D43** (1991), 442–456.

Also, an excellent and very thorough review of the problem of time and various proposed solutions, including Rovelli's, is given in

4) Chris J. Isham, "Canonical quantum gravity and the problem of time", 125 pages, available as gr-qc/9210011.

Anyway, in a paper I very briefly described in "Week 1":

5) Lee Smolin, "Time, measurement and information loss in quantum cosmology", available as gr-qc/9301016.

Smolin showed, how, using a clever trick sketched in the present paper to get "observables" invariant under spatial but not temporal observables, together with Rovelli's idea, one could define lots of *real* observables, invariant under spacetime diffeomorphisms that is, thus making a serious bite into this problem.

I warn the reader that there is a fair amount that is not too realistic about these methods. First there's the "clock field" — this can actually be taken as a free massless scalar field, but in so doing there is the likelihood of serious technical problems. Some of these are discussed in

6) P. Hajicek, "Comment on 'Time in quantum gravity — an hypothesis'", *Phys. Rev.* **D44** (1991), 1337–1338.

(But I haven't actually read this, just Isham's description.) Also, the clever trick of the present paper is to couple gravity to an antisymmetric tensor gauge field so that in addition to having loops as part of one's "loop representation," one has surfaces — a "surface representation". But this antisymmetric tensor gauge field is not the sort of thing that actually seems to arise in physics (unless I'm missing something). Still, it's a start. I think I'll finish by quoting Smolin's abstract:

> *Two sets of spatially diffeomorphism invariant operators are constructed in the loop representation formulation of quantum gravity. This is done by coupling general relativity to an anti- symmetric tensor gauge field and using that field to pick out sets of surfaces, with boundaries, in the spatial three manifold. The two sets of observables then measure the areas of these surfaces and the Wilson loops for the self-dual connection around their boundaries. The operators that represent these observables are finite and background independent when constructed through a proper regularization procedure. Furthermore, the spectra of the area operators are discrete so that the possible values that one can obtain by a measurement of the area of a physical surface in quantum gravity are valued in a discrete set that includes integral multiples of half the Planck area. These results make possible the construction of a correspondence between any three geometry whose curvature is small in Planck units and a diffeomorphism invariant state of the gravitational and matter fields. This correspondence relies on the approximation of the classical geometry by a piecewise flat Regge manifold, which is then put in correspondence with a diffeomorphism invariant state of the gravity-matter system in which the matter fields specify the faces of the triangulation and the gravitational field is in an eigenstate of the operators that measure their areas.*

> In the Space and Time marriage we have the greatest Boy meets Girl story of the age. To our great-grandchildren this will be as poetical a union as the ancient Greek marriage of Cupid and Psyche seems to us.
>
> — *Lawrence Durrell*

Week 7

March 1, 1993

1) Abhay Ashtekar, "Mathematical problems of non-perturbative quantum general relativity" (lectures delivered at the 1992 Les Houches summer school on Gravitation and Quantization), 87 pages, Plain TeX, available as `gr-qc/9302024`.

I described this paper in "Week 3", but now it's available from gr-qc. It's a good quick introduction to the loop representation of quantum gravity.

2) Abhay Ashtekar, *Lectures on Non-perturbative Canonical Gravity*, World Scientific Press, 1991.

This book, which I finally obtained, is *the* introduction to the loop representation of quantum gravity. What's the loop representation? Well, this is a long story, so you really should read the book. But just to get you going, let me describe Ashtekar's "new variables," which form the basis for Rovelli and Smolin's construction of the loop representation.

First, recall that general relativity is usually thought of as a theory about a metric on spacetime — more precisely, a Lorentzian metric. Here spacetime is a 4-dimensional manifold, and a Lorentzian metric allows you to calculate the "dot product" of any two tangent vectors at a point. This is in quotes because, while a normal dot product might look like

$$(v_0, v_1, v_2, v_3) \cdot (w_0, w_1, w_2, w_3) = v_0w_0 + v_1w_1 + v_2w_2 + v_3w_3$$

relative to some basis, for a Lorentzian metric we can always find a basis of the tangent space such that

$$(v_0, v_1, v_2, v_3) \cdot (w_0, w_1, w_2, w_3) = v_0w_0 - v_1w_1 - v_2w_2 - v_3w_3$$

Now the metric in general relativity defines a "connection," which tells you a tangent vector might "twist around" as you parallel translate it, that is, move it along while trying to keep it from rotating unnecessarily. Here "twist around" is in quotes because, since you are parallel translating the vector, it's not really "twisting around" in the usual sense, but it might seem that way relative to some coordinate system. For example, if you used polar coordinates to describe parallel translation on the plane, it might seem that the unit vector in the r direction "twisted around" towards the θ direction as you dragged it along. But in another coordinate system — say the usual x-y system — it would not appear to be "twisting around". This fact is expressed by saying "the connection is not a tensor".

But from the connection we can cook up a big fat tensor, the "Riemann tensor" R^i_{jkl}, which says how much the vector in the lth direction (here the indices range from 0 to 3) twists towards the ith direction when you move it around a teeny little square in the j-k plane. The Lagrangian in ordinary GR is just the integral of the "Ricci scalar curvature," R, which is gotten from the Riemann tensor by "contraction", i.e. summing over the indices in a certain way:

$$R = R^{i\ \,j}_{ji}$$

where we are raising indices using the metric in a manner beloved by physicists and feared by many mathematicians. If you integrate the Lagrangian over a region of spacetime you get the "action", and in classical general relativity (in a vacuum, for simplicity) one can formulate the laws of motion simply by saying: any teeny change in the metric that vanishes on the boundary of the region should leave the action constant to first order. In other words, the solutions of the equations of general relativity are the *stationary points* of the action. If you know how to do variational calculus you can derive Einstein's equations from this variational principle, as it's called. Mathematicians will be pleased to know that Hilbert beat Einstein to the punch here, so the integral of R is called the "Einstein–Hilbert" action for general relativity.

But there's another formulation of general relativity in terms of an action principle. This is called the "Palatini" action — and actually I'm going to describe a slight variation on it, that is conceptually simpler, and apparently appears for the first time in Ashtekar's book. The Palatini approach turns out to be more elegant and is a nice stepping-stone to the Ashtekar approach. In the Palatini approach one thinks of general relativity not as being a theory of a metric, but of a "tetrad" and an "$\mathfrak{so}(3,1)$ connection". To explain what these are, I will cut corners and assume all the fiber bundles lurking around are trivial; the experts will easily be able to figure out the general case. So: an (orthonormal) tetrad, or "vierbein," is a just a kind of field on spacetime which at each point consists of an (ordered) orthonormal basis of the tangent space. If we express the metric in terms of a tetrad, it looks just like the formula for the standard "inner product"

$$(v_0, v_1, v_2, v_3) \cdot (w_0, w_1, w_2, w_3) = v_0 w_0 - v_1 w_1 - v_2 w_2 - v_3 w_3$$

This allows us to identify the group of linear transformations of the tangent space that preserve the metric with the group of linear transfomations preserving the standard "inner product," which is called $\mathrm{SO}(1,3)$ since there's one plus sign and three minuses. And from the connection mentioned above one gets an $\mathrm{SO}(1,3)$ connection, or, what's more or less the same thing, an $\mathfrak{so}(1,3)$-valued 1-form, that is, a kind of field that can eat a tangent vector at any point and spits out element of the Lie algebra $\mathfrak{so}(1,3)$.

What's $\mathfrak{so}(1,3)$? Well, elements of $\mathfrak{so}(1,3)$ include "infinitesimal" rotations and Lorentz transformations, since $\mathrm{SO}(1,3)$ is generated by rotations and Lorentz transformations. More precisely, $\mathfrak{so}(1,3)$ is a 6-dimensional Lie algebra having as a basis the three infinitesimal rotations J_1, J_2, and J_3 around the three axes, and the three infinitesimal Lorentz transformations or "boosts" K_1, K_2, K_3. The bracket in this most important Lie algebra is given by

$$[J_i, J_j] = J_k$$
$$[K_i, K_j] = -J_k$$
$$[J_i, K_j] = K_k$$

where (i,j,k) is a cyclic permutation of $(1,2,3)$. (I hope I haven't screwed up the signs.) Note that the J's by themselves form a Lie subalgebra called $\mathfrak{so}(3)$, the Lie algebra of the rotation group $\mathrm{SO}(3)$. Note that $\mathfrak{so}(3)$ is isomorphic to the the cute little Lie algebra $\mathfrak{su}(2)$ I described in my post "Week 5"; J_1, J_2, and J_3 correspond to the guys I, J, and K divided by two.

The $\mathfrak{so}(1,3)$ connection has a curvature, and using the tetrads again we can identify this with the Riemann curvature tensor. So the Palatini trick is to rewrite the Einstein–Hilbert action in terms of the curvature of the $\mathfrak{so}(1,3)$ connection and the tetrad field.

This is called the Palatini action. Charmingly, even though the tetrad field is utterly unphysical, we can treat it and the $\mathfrak{so}(1,3)$ connection as independent fields and, doing calculus of variations to find stationary points of the action, we get equations equivalent to Einstein's equations.

Ashtekar's "new variables" — from this point of view — rely on a curious and profound fact about $\mathfrak{so}(1,3)$. Note that $\mathfrak{so}(1,3)$ is a Lie algebra over the real numbers. But if we allow ourselves to form *complex* linear combinations of the J's and K's, thus:

$$M_i = (J_i + iK_i)/2$$
$$N_i = (J_i - iK_i)/2$$

(please don't mix up the subscript $i = 1, 2, 3$ with the other i, the square root of minus one) we get the following brackets:

$$[M_i, M_j] = M_k$$
$$[N_i, N_j] = N_k$$
$$[M_i, N_j] = 0$$

I think the signs all work but I wouldn't trust me if I were you. The wonderful thing here is that the M's and N's commute with each other, and each set has commutation relations just like the J's! The J's, recall, are infinitesimal rotations, and the Lie algebra they span is $\mathfrak{so}(3)$. So in a sense the Lie algebra of the Lorentz group can be "split" into "left-handed" and "right-handed" copies of $\mathfrak{so}(3)$, also known as "self-dual" and "anti-self-dual" copies. This is, in fact, what lies behind the handedness of neutrinos, and many other wonderful things.

But let me phrase this result more precisely. Since we allowed ourselves complex linear combinations of the J's and K's, we are now working in the "complexification" of the Lie algebra $\mathfrak{so}(3,1)$, and we have shown that this Lie algebra over the complex numbers splits into two copies of $\mathfrak{so}(3,\mathbb{C})$, the complexification of $\mathfrak{so}(3)$.

Ashtekar came up with some "new variables" for general relativity in the context of the Hamiltonian approach. Here we are working in the Lagrangian approach, where things are simpler because they are "generally covariant," not requiring a split of spacetime into space and time. The Lagrangian approach to the new variables is due to Samuel, Jacobson and Smolin, and in this approach all they amount to is this: $\mathfrak{so}(1,3)$ connection of the Palatini approach, think of the $\mathfrak{so}(1,3)$ as sitting inside the complexification thereof, and consider only the "right-handed" part! Thus, from an $\mathfrak{so}(1,3)$ connection, we get a $\mathfrak{so}(3,\mathbb{C})$ connection. The "new variables" are just the tetrad field and this $\mathfrak{so}(3,\mathbb{C})$ connection.

I have tried to keep down the indices but I think I will write down the Palatini Lagrangian and then the "new variables" Lagrangian, without explaining exactly what they mean, just to show how amazingly similar-looking they are. In the Palatini approach we have a tetrad field, which now we write in its full glory as e^i_I, and the curvature of the $\mathfrak{so}(1,3)$ connection, which now we write as Ω^{IJ}_{ij}. The Lagrangian is then

$$e^i_I e^j_J \Omega^{IJ}_{ij}$$

(which we integrate against the usual volume form to get the action). In the new variables approach we have a tetrad field again, and we write the curvature of the $\mathfrak{so}(3,\mathbb{C})$

connection as F_{ij}^{IJ}. (This turns out to be just the "right-handed" part of Ω_{ij}^{IJ}.) The Lagrangian is

$$e_I^i e_J^j F_{ij}^{IJ}!$$

Miraculously, this also gives Einstein's equations.

What's utterly unclear from what I've said so far is why this helps so much in trying to quantize gravity. I may eventually get around to writing about that, but for now, read the book!

3) David Yetter and Louis Crane, "We are not stuck with gluing", available as hep-th/9302118.

Well, in "Week 2" I mentioned Crane and Yetter's marvelous construction of a 4d topological quantum field theory using the representations of the quantum group $SU_q(2)$ — and in "Week 5" I mentioned Ocneanu's "proof" that the resulting 4-manifold invariants were utterly trivial (equal to 1 for all 4-manifolds). Now Crane and Yetter have replied, saying that their 4-manifold invariants are not trivial and that Ocneanu interpreted their paper incorrectly. I look forward to the conference on quantum topology in Kansas at the end of May, where the full story will doubtless come out.

4) R. Capovilla, J. Dell and T. Jacobson, "The initial value problem in light of Ashtekar's variables", available as gr-qc/9302020.

The advantage of Ashtekar's new variables is that they simplify the form of the constraint equations one gets in the initial-value problem for general relativity. This is true both of the classical and quantum theories. Rovelli and Smolin used this to find, for the first time, lots of states of quantum gravity defined by link invariants. Here the above authors are trying to apply the new variables to the *classical* theory.

5) Sergey Piunikhin, "Combinatorial expression for universal Vassiliev link invariant", available as hep-th/9302084.

Somebody ought to teach those Russians how to use the word "the" now that the cold war is over. Anyway, this paper defines a kind of universal object for Vassiliev invariants, which is sort of similar to what I was trying to do in

6) John Baez, "Link invariants of finite type and perturbation theory", *Lett. Math. Phys.* **26** (1992) 43–51. Also available as hep-th/9207041.

but more concrete, and (supposedly) simpler than Kontsevich's approach. My parenthesis simply indicates that I haven't had time to figure out what's going on here.

Week 8

March 5, 1993

I was delighted to find that Louis Kauffman wants to speak at the workshop at UCR on knots and quantum gravity; he'll be talking on "Temperley–Lieb recoupling theory and quantum invariants of links and manifolds". His books

1) Louis Kauffman, *On Knots*, Annals of Mathematics Studies **115**, Princeton U. Press, Princeton, 1987.

and more recently

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

are a lot of fun to read, and convinced me to turn my energies towards the intersection of knot theory and mathematical physics. As you can see by the title of the series he's editing, he is a true believer the deep significance of knot theory. This was true even before the Jones polynomial hit the mathematical physics scene, so he was well-placed to discover the relationship between the Jones polynomial (and other new knot invariants) and statistical mechanics, which seems to be what won him his fame. He is now the editor of a journal, *Journal of Knot Theory and its Ramifications*.

He sent me a packet of articles and preprints which I will briefly discuss. If you read *any* of the stuff below, *please* read the delightful reformulation of the 4-color theorem in terms of cross products that he discovered! I am strongly tempted to assign it to my linear algebra class for homework. . . .

3) Louis Kauffman, "Map coloring and the vector cross product", *J. Comb. Theory* **48** (1990), 145–154.

   Louis Kauffman, "Map coloring, q-deformed spin networks, and Turaev–Viro invariants for 3-manifolds", *Int. Jour. Mod. Phys.* **6** (1992), 1765–1794.

   Louis Kauffman, "An algebraic approach to the planar colouring problem", *Commun. Math. Phys.* **152** (1993), 565–590.

As we all know, the usual cross product of vectors in $\mathbb{R}^3$ is not associative, so the following theorem is slightly interesting:

> **Theorem.** Consider any two bracketings of a product of any finite number of vectors, e.g.:
>
> $$L = a \times (b \times ((c \times d) \times e) \quad \text{and} \quad R = ((a \times b) \times c) \times (d \times e).$$
>
> Let i, j, and k be the usual canonical basis for $\mathbb{R}^3$:
>
> $$i = (1, 0, 0), \quad j = (0, 1, 0), \quad k = (0, 0, 1).$$
>
> Then we may assign $a, b, c, \ldots$ values taken from $\{i, j, k\}$ in such a way that $L = R$ and both are nonzero.

But what's really interesting is:

> **Meta-Theorem.** The above proposition is equivalent to the 4-color theorem. Recall that this theorem says that any map on the plane may be colored with 4 colors in such a way that no two regions with the same color share a border (an edge).

What I mean here is that the only way known to prove this Theorem is to deduce it from the 4-color theorem, and conversely, any proof of this Theorem would easily give a proof of the 4-color theorem! As you all probably know, the 4-color theorem was a difficult conjecture that resisted proof for about a century before succumbing to a computer-based proof that required the consideration of many, many special cases:

4) Kenneth Appel and Wolfgang Haken, *Every Planar Map is Four Colorable* Contemporary Mathematics **98** American Mathematical Society, Providence Rhode Island, 1989.

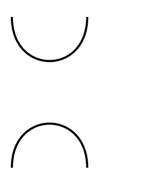
So the Theorem above may be regarded as a *profoundly* subtle result about the "associativity" of the cross product!

Of course, I hope you all rush out now and find out how this Theorem is equivalent to the 4-color theorem. For starters, let me note that it uses a result of Tait: first, to prove the 4-color theorem it's enough to prove it for maps where only 3 countries meet at each vertex (since one can stick in a little new country at each vertex), and second, 4-coloring such a map is equivalent to coloring the *edges* with 3 colors in such a way that each vertex has edges of all 3 colors adjoining it. The 3 colors correspond to i, j, and k!

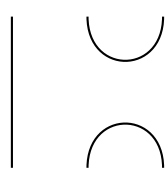
Kauffman and Saleur (the latter a physicist) come up with another algebraic formulation of the 4-color theorem in terms of the Temperley–Lieb algebra. The Temperley–Lieb algebra TL_n is a cute algebra with generators $e_1, \ldots, e_{n-1}$ and relations that depend on a constant d called the "loop value":

$$\begin{aligned} e_i^2 &= de_i \\ e_i e_{i+1} e_i &= e_i \\ e_i e_{i-1} e_i &= e_i \\ e_i e_j &= e_j e_i \quad \text{for } |i-j| > 1. \end{aligned}$$

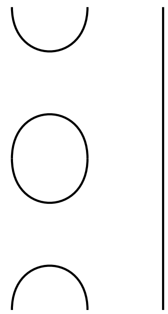
The point of it becomes clear if we draw the e_i as tangles on n strands. Let's take $n = 3$ to keep life simple. Then e_1 is



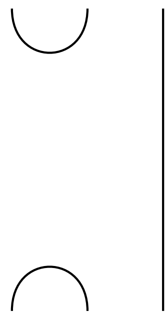
while e_2 is



In general, e_i "folds over" the ith and $(i+1)$st strands. Note that if we square e_i we get a loop — e.g., e_1 squared is

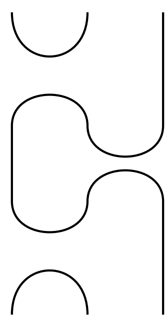


Here we are using the usual product of tangles (see my webpage "tangles"). Now the rule in Temperley–Lieb land is that we can get rid of a loop if we multiply by the loop value d; that is, the loop "equals" d. So e_1 squared is just d times



which — since we are doing topology — is the same as e_1. That's why $e_i^2 = de_i$.

The other relations are even more obvious. For example, $e_1e_2e_1$ is just



which, since we are doing topology, is just e_1! Similarly, $e_2e_1e_2 = e_1$, and e_i and e_j commute if they are far enough away to keep from running into each other.

As an exercise for combinatorists: figure out the dimension of TL_n.

Okay, very cute, one might say, but so what? Well, this algebra was actually first discovered in statistical mechanics, when Temperley and Lieb were solving a 2-dimensional problem:

5) H. N. V. Temperley and E. H. Lieb, "Relations between the 'percolation' and 'coloring' problem and other graph-theoretical problems associated with regular planar lattices: some exact results on the 'percolation' problem", *Proc. Roy. Soc. Lond.* **322** (1971), 251–280.

It gained a lot more fame when it appeared as the explanation for the Jones polynomial invariant of knots — although Jones had been using it not for knot theory, but in the study of von Neumann algebras, and the Jones polynomial was just an unexpected spinoff. Its importance in knot theory comes from the fact that it is a quotient of the group algebra of the braid group (as explained in *Knots and Physics*). It has also found a lot of other applications; for example, I've used it in my paper on quantum gravity and the algebra of tangles. So it is nice to see that there is also a formulation of the 4-color theorem in terms of the Temperley–Lieb algebra (which I won't present here).

6) Louis Kauffman, "Knots and physics", *Proc. Symp. Appl. Math.* **45** (1992), 131–246.

Louis Kauffman, "Spin networks, topology and discrete physics", University of Illinois at Chicago preprint.

Louis Kauffman,"Vassiliev invariants and the Jones polynomial", University of Illinois at Chicago preprint.

Louis Kauffman, "Gauss codes and quantum groups", University of Illinois at Chicago preprint.

Louis Kauffman and H. Saleur,"Fermions and link invariants", Yale University preprint YCTP-P21-91, July 5, 1991.

Louis Kauffman, "State models for link polynomials", *L'Enseignement Mathematique*, **36** (1990), 1–37.

F. Jaeger, Louis Kauffman and H. Saleur, "The Conway polynomial in $\mathbb{R}^3$ and in thickened surfaces: a new determinant formulation", preprint.

These are a variety of papers on knots, physics and everything.... The more free-wheeling among you might enjoy the comments at the end of the first paper on "knot epistemology."

I am going to a conference on gravity at U.C. Santa Barbara on Friday and Saturday, which I why I am posting this early, and why I have no time to describe the above papers. I'll talk about my usual obsessions, and hear what other people are up to, perhaps bringing back some words of wisdom for next week's "This Week's Finds".

Week 9

March 12, 1993

1. Boguslaw Broda,"Surgical invariants of four-manifolds", preprint available as hep-th/9302092.

There a number of attempts underway to get invariants of four-dimensional manifolds (and 4d topological quantum field theories) by techniques analogous to those that worked in three dimensions. The 3-manifold invariants and 3d topological quantum field theories got going with the work of Witten on Chern–Simons theory, but since this was not rigorous a number of ways were devised to make it so. These seem different at first glance but all give the same answer. Two approaches that use a lot of category theory are the Heegard splitting approach (due to Crane, Kohno and Kontsevich, in which one writes a 3-manifold as two solid n-holed tori glued together by a diffeomorphism of their boundaries), and the surgery on links approach (due to Reshetikhin and Turaev, in which one builds up 3-manifolds by starting with the 3-sphere, cutting out thickened links and gluing them back in a different way, allowing one to define invariants of 3-manifolds from link invariants). In the case of 3 dimensions a nice paper relating the Heegard splitting and the surgery on links approaches is

2) Sergey Piunikhin, "Reshetikhin-Turaev and Crane-Kohno-Kontsevich 3-manifold invariants coincide", *Journal of Knot Theory and its Ramifications* **2** (1993), 65–95.

People are now trying to generalize all these ideas to 4-manifolds. There is already an interesting bunch of 4-manifold invariants out there, the Donaldson invariants, which are hard to compute, but were shown (heuristically) by Witten to be related to a quantum field theory. Lately people have been trying to define invariants using category theory; these may or may not turn out to be the same.

I've already been trying to keep you all updated on the story about Crane and Yetter's 4d TQFT. This week I'll discuss another approach, with a vast amount of help from Daniel Ruberman, a topologist at Brandeis. Any errors in what I write on this are likely to be due to my misunderstandings of what he said — caveat emptor! Broda's paper is quite terse — probably due to the race that is going on — and is based on:

3) E. Cesar de Sa, "A link calculus for 4-manifolds", in *Topology of Low-Dimensional Manifolds, Proc. Second Sussex Conf.*, Springer Lecture Notes in Mathematics **722**, Springer, Berlin, 1979, pp. 16–30.

so I should start by describing what little I understand of de Sa's work.

One can describe (compact, smooth) 4-manifolds in terms of handlebody decomposition. This allows one to actually draw pictures representing 4-manifolds. A lot of times when people first hear about topology they get they impression that it's all about rubber doughnuts, Möbius strips, and other Dali-esque wiggly objects in hyperspace. Then, when they take courses in it, they are confronted with nasty separation axioms and cohomology theories! This is just to scare away outsiders! Handlebody theory really *is* about wiggly objects in hyperspace, and it's lots of fun — though to be good in it you need to know your point set topology and your algebraic topology, I'm afraid — and much better than I do!

Recall:

$$D^n = \text{unit ball in } \mathbb{R}^n$$
$$S^n = \text{unit sphere in } \mathbb{R}^{n+1}$$

In particular note that S^0 is just two points. Note that:

- the boundary of D^4 is S^3
- the boundary of $D^3 \times D^1$ is $D^3 \times S^0 \cup S^2 \times D^1$
- the boundary of $D^2 \times D^2$ is $D^2 \times S^1 \cup S^1 \times D^2$
- the boundary of $D^1 \times D^3$ is $D^1 \times S^2 \cup S^0 \times D^3$
- the boundary of D^4 is S^3

I have written this rather redundant chart in a way that makes the pattern very clear and will come in handy below for those who aren't used to this stuff.

To build up a 4-manifold we can start with a "0-handle," D^4, which has as boundary S^3.

Then we glue on "1-handles," that is, copies of $D^3 \times D^1$. Note that part of the boundary of $D^1 \times D^3$ is $D^3 \times S^0$, which is two D^3's; when we glue on a 1-handle we simply attach these two D^3's to the S^3 by a diffeomorphism. The resulting space is not really a smooth manifold, but it can be smoothed. It then becomes a smooth 4-manifold with boundary.

Then we glue on "2-handles" by attaching copies of $D^2 \times D^2$ along the part of their boundary that is $D^2 \times S^1$. Then we smooth things out.

Then we glue on "3-handles" by attaching copies of $D^1 \times D^3$ along the part of their boundary that is $D^1 \times S^2$. Then we smooth things out.

Then we glue on "4-handles" by attaching copies of D^4 along their boundary, i.e. S^3.

We can get any compact oriented 4-manifold this way using attaching maps that are compatible with the orientations. The reader who is new to this may enjoy constructing 2-manifolds in an analogous way. Compact oriented 2-manifolds with boundary are just n-holed tori.

What's cool is that with some tricks one can still *draw* what's going in the case of 3-manifolds and 4-manifolds. Here I'll just describe how it goes for 4-manifolds, since that's what Cesar de Sa and Broda are thinking about. By the way, a good introduction to this stuff is

4) *The Topology of 4-manifolds*, by Robion C. Kirby, Springer Lecture Notes in Mathematics **1374**, 1989.

So — here is how we *draw* what's going on. I apologize for being somewhat sketchy here (sorry for the pun, too). I am a bit rushed since I'm heading off somewhere else next weekend… and I am not as familiar with this stuff as I should be.

So, when we start with our 0-handle, or D^4, we "draw" its boundary, S^3. Think of S^3 as $\mathbb{R}^3$ and a point at infinity. Since we use perspective when drawing pictures of 3-d objects, this boils down to pretending that our blackboard is a picture of S^3!

As we add handles we continue to "draw" what's happening at the boundary of the 4-manifold we have at each stage of the game. 1-handles are attached by gluing a $D^3 \times D^1$ onto the boundary along two D^3's — or balls — so we can just draw the two balls.

2-handles are attached by gluing a $D^2 \times D^2$ onto the boundary of the 4-manifold we have so far along a $D^2 \times S^1$ — or solid torus, so we just need to figure out how to draw an embedded solid torus. Well, for this we just need to draw a knot (that is, an embedded circle), and write an integer next to it saying how many times the embedded solid torus "twists" — plus or minus depending on clockwise or counterclockwise — as we go around the circle. In other words, an embedded solid torus is (up to diffeomorphism) essentially the same as a framed knot. If we are attaching a bunch of 2-handles we need to draw a framed link.

Things get a bit hairy in the case when one of the framed links goes through one of the 1-handles that we've already added. It's easier to draw this situation if we resort to another method of drawing the 1-handles. It's a bit more subtle, and took me quite a while to be able to visualize (unfortunately I seem to have to visualize this stuff to believe it). So let's go back to the situation where we have D^4, with S^3 as its boundary, and we are adding 1-handles. Instead of drawing two balls, we draw an unknotted circle with a dot on it! The dot is just to distinguish this kind of circle from the framed links we already have. But what the circle *means* is this. The circle is the boundary of an obvious D^2, and we can push the interior of this D^2 (which is sitting in the S^3) into the interior of D^4. If we then remove a neighborhood of the D^2, what we have left is $S^1 \times D^3$, which is just the result of adding a 1-handle to D^4.

This is probably easier to visualize one dimension down: if we have a good old unit ball, D^3, and slap an interval, or D^1, onto its boundary, and then push the interior of the interval into the interior of the ball, and remove a neighborhood of the interval, what we have left is just an $S^1 \times D^2$.

So in short, we can draw all the 1-handles by drawing unlinked, unknotted circles with dots on them, and then draw all the 2-handles by drawing framed links that don't intersect these circles.

At this point, if you have never seen this before, you are probably dreading the 3-handles and 4-handles. Luckily a theorem comes to our rescue! If we start at the other end of our handlebody decomposition, as it were, we start with 4-handles and glue on 3-handles. If you ponder the chart and see what the pattern of what we're doing is, you'll see that a single 4-handle with some 3-handles stuck on is just the same as a 0-handle with some 1-handles stuck on. So when we now glue this thing (or things) onto the stuff we've built out of 0-, 1-, and 2-handles, we are doing so using a diffeomorphism of its boundary. But a theorem of Laudenbach and Poenaru,

5) F. Laudenbach and V. Poenaru, "A note on 4-dimensional handlebodies", *Bull. Math. Soc. France* **100** (1972), 337–344.

says that any such diffeomorphism extends to one of the interior. This means that it doesn't make a darn bit of difference which diffeomorphism we use to glue it on. In short, all the information is contained in the 1- and 2-handles, so we can *draw* 4-manifolds by first drawing a batch of unknotted unlinked circles with dots on them and then drawing a framed link in the complement.

(A question for the experts, since I'm just learning this stuff: in the above we seem to be assuming that there's only one 0-handle. Is this an okay assumption or do we need something fancier if there's more?)

Now a given 4-manifold may have lots of different handlebody decompositions. So, as usual, we would like to have a finite set of "moves" that allow us to get between any pair of handlebody decompositions of the same 4-manifold. Then we can construct a 4-manifold invariant by cooking up a number from a handlebody decomposition — presented as a picture as above, if we want — and showing that it doesn't change under these "moves".

So, what de Sa did was precisely to find such a set of moves. (There, that's what I understand of his work!)

And what Broda did was precisely to use the Kauffman bracket invariant of framed links to cook up an invariant of 4-manifolds from the handlebody decomposition — which, note, involves lots of links. Recall that the Kauffman bracket assigns to each link a polynomial in one variable, q. Here "q" is just the same q that appears in the quantum group $SU_q(2)$. As I mentioned in "Week 5", this acts quite differently when q is a root of unity, and the 3d topological quantum field theories coming from quantum groups, as well as Crane and Yetter's 4d topological quantum field theory, come from considering this root-of-unity case. So it's no surprise that Broda requires q to be a root of unity.

Ruberman had some other remarks about Broda's invariant, but I think I would prefer to wait until I understand them. . . .

6) Abhay Ashtekar, Ranjeet S. Tate and Claes Uggla, "Minisuperspaces: symmetries and quantization", available as gr-qc/9302026.

   Abhay Ashtekar, Ranjeet S. Tate and Claes Uggla, "Minisuperspaces: observables and quantization", available as gr-qc/9302027.

I was just at the Pacific Coast Gravity Meeting last weekend and heard Ranjeet Tate talk on this work. Recall first of all that minisuperspaces are finite-dimensional approximations to the phase space of general relativity, and are used to get some insight into quantum gravity. I went through an example in "Week 6". In these papers, the authors quantize various "Bianchi type" minisuperspace models. The "Bianchi type" business comes from a standard classification of homogeneous (but not necessarily isotropic) cosmologies and having a lot of symmetry. It is based in part on Bianchi's classification of 3-dimensional Lie algebras into nine types. The second paper gives a pretty good review of this stuff before diving into the quantization, and I should learn it!

The most exciting aspect of these papers, at least to the dilettante such as myself, is that one can quantize these models and show that quantization does NOT typically remove the singularities ("Big Bang" and/or "big crunch"). Of course, these models have only finitely many degrees of freedom, and are only a caricature of full-fledged quantum gravity, so one can still argue that *real* quantum gravity will get rid of the singularities. But a number of general relativists are arguing that this is not the case, and we simply have to learn to live with singularities. So it's good to look at models, however simple, where one can work things out in detail, and not just argue about generalities.

7) Alan D. Rendall, "Unique determination of an inner product by adjointness relations in the algebra of quantum observables", Max-Planck-Institut für Astrophysik preprint.

I had known Rendall from his work on the perturbative expansion of the time evolution operators in classical general relativity. He became interested in quantum gravity a while ago and visited Ashtekar and Smolin at Syracuse University, since (as he said) the best way to learn is by doing. There he wrote this paper on Ashtekar's approach to finding the right inner product for the space of states of quantum gravity. I had heard about this paper, but hadn't seen it until I met Rendall at the gravity meeting last weekend. He gave me a copy and explained it. It is a simple and beautiful paper — such nice mathematical results that I am afraid someone else may have found them earlier somewhere.

Ashtekar's idea is to fix the inner product by requiring that the physical observables, which are operators on the space of states, be self-adjoint. Rendall shows the following. Let A be a *-algebra acting on a vector space V. Let us say that an inner product on V is "strongly admissable" if 1) the representation is a *-representation with respect to this inner product, 2) for each element of A, the corresponding linear transformation on V is bounded relative to the norm given by this inner product, and 3) the completion of V in the inner product is a topologically irreducible representation of A. Rendall shows the uniqueness of a strongly admissable inner product on any representation V of A (up to a constant multiple). Of course, such an inner product need not exist, but when it does, it is unique. This is as nice a result along these lines as one could hope for. He also has a more complicated result that applies to unbounded operators. A good piece of work on the foundations of quantum theory!

8) Arlen Anderson, "Thawing the frozen formalism: the difference between observables and what we observe", preprint available as gr-qc/9211028.

There were a number of youngish folks giving talks at the gravity meeting who have clearly been keeping up with the recent work on the problem of time and other conceptual problems in quantum gravity. In very brief terms, the problem of time is that in general relativity, we have not a Hamiltonian in the traditional sense, but a "Hamiltonian constraint" $H = 0$, so when we quantize it superficially appears that there are no dynamics whatsoever (as it seems like we have a zero Hamiltonian!). That's the reason for the term "frozen formalism" — and the desire to "thaw" it, or find the dynamics lurking in it. In fact, the Hamiltonian constraint is just a reflection of the fact that general relativity has no preferred time coordinate, and we are just learning how to deal with the quantum theory of such systems. For a good survey of the problem and some new proposed solutions, I again refer everyone to Isham's paper:

9) Chris J. Isham, "Canonical quantum gravity and the problem of time", 125 pages, available as gr-qc/9210011.

In particular, one interesting approach is due to Rovelli, and is called "evolving constants of motion" (a deliberate and very accurate oxymoron). While there are serious technical problems with this approach, it's very natural from a physical point of view — at least once you get used to it. I have the feeling that the younger physicists are, as usual, getting used to it a lot more quickly than the older folks who have been pondering the problem of time for many years. Anderson is one of these younger folks, and his paper develops Rovelli's approach in terms of in a toy model, namely the case of two free particles satisfying the Schrödinger equation.

10) Cayetano Di Bartolo, Rodolfo Gambini and Jorge Griego, "The extended loop group: an infinite dimensional manifold associated with the loop space", 42 pages, available as gr-qc/9303010.

Unfortunately I don't have the time now to give this paper the discussion it deserves. Gambini is one of the original inventors of the loop representation of gauge theories, so his work is especially worth paying attention to. He explained the idea of this paper to me a while back. Its aim is to provide a workable "calculus" for the loop representation by enlarging the ordinary loop group to a larger group which is actually an infinite-dimensional Lie group — the point being that the usual loop group doesn't have a Lie algebra, but this one does. As one might expect, the Lie algebra of this group is closely related to the theory of Vassiliev invariants. The paper considers some applications to quantum gravity and knot theory.

Week 10

March 20, 1993

The most substantial part of this issue is some remarks by Daniel Ruberman on the paper I was talking about last time by Boguslaw Broda. They apparently show that Broda's invariant is not as new as it might appear. But they're rather technical, so I'll put them near the end, and start off with something on the light side, and then note some interesting progress on the Vassiliev invariant scene.

1) Marcia Bartusiak, "Beyond Einstein — is space loopy?", *Discover*, April 1993.

In the airport in Montreal I ran into this article, which was the cover story, with an upside-down picture of Einstein worked into a bunch of linked key-rings. I bought it — how could I resist? — since it is perhaps the most "pop" exposition of the loop representation of quantum gravity so far. Those interested in the popularization of modern physics might want to compare

2) John Horgan, "Gravity quantized? A radical theory of gravity weaves space from tiny loops", *Scientific American*, September 1992.

Given the incredible hype concerning superstring theory, which seems to have faded out by now, I sort of dread the same thing happening to the loop representation of quantum gravity. It is intrinsically less hype-able, since it does not purport to be a theory of everything, and it comes right after superstrings were supposed to have solved all the mysteries of the universe. Also, its proponents are (so far) a more cautious breed than the string theorists — note the question marks in both titles! But we will see. . . .

Marcia Bartusiak is a contributing editor of Discover and the author of a book on current topics in astronomy and astrophysics, *Thursday's Universe*, which I haven't read. She'll be coming out with a book in June, *Through a Universe Darkly*, that's supposed to be about how theories of cosmology have changed down through the ages. She does a decent job of sketching vaguely the outlines of the loop representation to an audience who must be presumed ignorant of quantum theory and general relativity. Of course, there is also a certain amount of human-interest stuff, with Ashtekar, Rovelli and Smolin (quite rightly) coming off as the heroes of the story. There are, as usual, little boxes with gee-whiz remarks like

```
WITH REAMS OF PAPER
             SPREAD OUT
   OVER THE KITCHEN TABLE
     THEY FOUND
   SOLUTION AFTER SOLUTION
   FOR EQUATIONS
         THOUGHT IMPOSSIBLE TO SOLVE
```

(which is, after all, true — nobody had previously found solutions to the constraint equations in canonical quantum gravity, and all of a sudden here were lots of 'em!). And there are some amusing discussions of personality: "Affable, creative, and easy-going, Rovelli quickly settled into the role of go-between, helping mesh the analytic powers of the quiet, contemplative Ashtekar with the creativity of the brash, impetuous Smolin." And discussions of how much messier Smolin's office is than Ashtekar's.

In any event, it's a fun read, and I recommend it. Of course, I'm biased, so don't trust me.

3) Sergey Piunikhin,"Vassiliev invariants contain more information than all knot polynomials", preprint.

   Sergey Piunikhin, "Turaev–Viro and Kauffman-Lins invariants for 3-manifolds coincide", *Journal of Knot Theory and its Ramifications* **1** (1992), 105–135.

   Sergey Piunikhin, "Different presentations of 3-manifold invariants arising in rational conformal field theory", preprint.

   Sergey Piunikhin, "Weights of Feynman diagrams, link polynomials and Vassiliev knot invariants", preprint.

   Sergey Piunikhin "Reshetikhin–Turaev and Crane–Kohno–Kontsevich 3-manifold invariants coincide", preprint.

I received a packet of papers by Piunikhin a while ago. The most new and interesting thing is the first paper listed above. In "Week 3" I noted a conjecture of Bar-Natan that all Vassiliev invariants come from quantum group knot invariants (or in other words, from Lie algebra representations.) Piunikhin claims to refute this by showing that there is a Vassiliev invariant of degree 6 that does not. (However, other people have told me his claim is misleading!) I have been too busy to read this paper yet.

4) Bernd Bruegmann, Bibliography of publications related to classical and quantum gravity in terms of the Ashtekar variables, available as gr-qc/9303015.

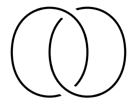
Let me just quote the abstract; this should be a handy thing:

> *This bibliography attempts to give a comprehensive overview of all the literature related to the Ashtekar variables. The original version was compiled by Peter Huebner in 1989, and it has been subsequently updated by Gabriela Gonzalez and Bernd Bruegmann. Information about additional literature, new preprints, and especially corrections are always welcome.*

5) Boguslaw Broda,"Surgical invariants of four-manifolds", preprint available as hep-th/9302092. (Revisited — see "Week 9")

Let me briefly recall the setup: we describe a compact 4-manifold by a handlebody decomposition, and represent this decomposition using a link in S^3. The 2-handles are represented by framed knots, while the 1-handles are represented by copies of the unknot (which we may think of as having the zero framing). The 1-handles and 2-handles play quite a different role in constructing the 4-manifold — which is why one normally draws the former as copies of the unknot with a *dot* on them — but Broda's construction does NOT care about this. Broda simply takes the link, forgetting the dots, and cooks up a number from it, using cabling and the Kauffman bracket at an root of unity. Let's call Broda's invariant by $b(M)$ — actually for each primitive rth root of unity, we have $b_r(M)$.

Broda shows that this is a 4-manifold invariant by showing it doesn't change under the de Sa moves. One of these consists of adding or deleting a Hopf link



in which both components have the zero framing and one represents a 1-handle and the other a 2-handle. This move depends on the fact that we can "cancel" a 1-handle and 2-handle pair, a special case of a general result in Morse theory.

But since Broda's invariant doesn't care which circles represent 1-handles and which represent 2-handles, Broda's invariant is also invariant under adding two 2-handles that go this way. This amounts to taking a connected sum with $S^2 \times S^2$. I.e., $b(M) = b(M\#S^2 \times S^2)$.

Now, Ruberman told me a while back that we must also have $b(M) = b(M\#\mathbb{CP}^2\# - \mathbb{CP}^2)$, that is, the invariant doesn't change under taking a connected sum with a copy of $\mathbb{CP}^2$ (complex projective 2-space) and an orientation-reversed copy of $\mathbb{CP}^2$. This amounts to adding or deleting a Hopf link in which one component has the zero framing and the other has framing 1. I didn't understand this, so I pestered Ruberman some more, and this is what he says (modulo minor edits). I have not had time to digest it yet:

> *The first question you asked was about the different framings on a 2-handle which goes geometrically once over a 1-handle, i.e. makes a Hopf link in which one of the circles is special (i.e is really a 1-handle, i.e. in Akbulut–Kirby's notation is drawn with a dot.) The answer is that the framing doesn't matter, since the handles cancel. This is explained well (in the PL case) in Rourke-Sanderson's book. (Milnor's book on the h-cobordism theorem explains it in terms of Morse functions, in the smooth case.)*
>
> *From this, it follows that $b(M) = b(M\#S^2 \times S^2) = b(M\#\mathbb{CP}^2\# - \mathbb{CP}^2)$. For M is unchanged if you add a cancelling 1,2 pair, independent of the framing on the 2-handle. If you change the special circle to an ordinary one, $b(M)$ doesn't change. On the other hand, M has been replaced by its sum with either $S^2 \times S^2$ or $\mathbb{CP}^2\# - \mathbb{CP}^2$, depending on whether the framing on the 2-handle is even or odd. (Exercise: why is only the parity relevant?)*
>
> *Now as I pointed out before, if one replaces all of the 1-handles (special circles) of a 4-manifold with 2-handles, the invariant doesn't change. This operation corresponds to doing surgery on the 4-manifold, along the cores of the 1-handles. In particular, the manifold has changed by a cobordism. (This is a basic construction; when you do surgery you produce a cobordism, in this case it's $M \times I$ with 2-handles attached to it along the circles which you surgered.)*
>
> *From this, I will now show that Broda's invariant is determined by the signature. (This is in the orientable case. Actually it seems that his invariant is really an invariant of an oriented manifold.) The argument above says that for any M, there is an M', with $b(M) = b(M')$, where M' has no 1-handles, and where M*

and M' are cobordant. In particular, M' is simply connected. So it suffices to show that $b(N) = b(N')$ if N and N' are simply connected.

So now you can assume you have two simply connected manifolds N, N' which are cobordant via a 5-dimensional cobordism W, which you can also assume simply connnected. By high-dimensional handlebody theory, you can get rid of the 1-handles and 4-handles of W, and assume that all the 2-handles are added, then all of the 3-handles. If you add all the 2-handles to N, you get $N\#k(S^2 \times S^2)\#l(\mathbb{CP}^2\#(-\mathbb{CP}^2))$ for some k and l. (Here is where simple connectivity is relevant; the attaching circle of a 2-handle is null-homotopic, and therefore isotopic to an unknotted circle. It's a simple exercise to see what happens when you do surgery on a trivial circle, ie you add on $S^2 \times S^2$ or $\mathbb{CP}^2\# - \mathbb{CP}^2$. On the other hand you get the same manifold as the result of adding 2-handles to N'. So

$$N\#k(S^2xS^2)\#l(\mathbb{CP}^2\#(-\mathbb{CP}^2)) = N'\#k'(S^2xS^2)\#l'(\mathbb{CP}^2\#(-\mathbb{CP}^2)),$$

so by previous remarks $b(N) = b(N')$, i.e b is a cobordism invariant.

Now: b is also multiplicative under connected sum, because connected sum just takes the union of the link diagrams. The cobordism group is $\mathbb{Z}$, detected by the signature, so b must be a multiple of the signature, modulo some number. (Maybe at this point I realize b should be b_r or some such). If you compute (as a grad student Tian-jin Li did for me) $b_r(\mathbb{CP}^2)$, you find that b_r lives in the group of rth (or maybe $4r$th; I'm at home and don't have my note) roots of unity.

My conclusion: this invariant is a rather complicated way to compute the signature of a 4-manifold (modulo r or $4r$) from a link diagram of the manifold.

There is an important moral of the story, which is perhaps not obvious to someone outside of 4-manifolds. Any invariant which purports to go beyond classical ones (i.e. invariants of the intersection form) must treat $\mathbb{CP}^2$ and $-\mathbb{CP}^2$ very differently. It seems to be the case that many manifolds which are different (i.e. nondiffeomorphic) become diffeomorphic after you add on $\mathbb{CP}^2$. Thus any useful invariant should get rather obliterated by adding $\mathbb{CP}^2$. On the other hand, non-diffeomorphic manifolds seem to stay non-diffeomorphic, no matter how many $-\mathbb{CP}^2$'s you add on. This phenomenon doesn't seem to be exhibited by any of the quantum-group type constructions for 3-manifolds; as it shouldn't, since (from the 3-manifold point of view) an unknot with framing + or -1 doesn't change the 3-manifold. So if you're looking for a combinatorial invariant, it seems critical that you try to build in the asymmetry wrt orientation which 4-manifolds seem to possess.

Exercise: do the nonorientable case. The answer should be that b is determined by the Euler characteristic, $\mod 2$.

Week 11

March 23, 1993

I'm hitting the road again tomorrow and will be going to the Quantum Topology conference in Kansas until Sunday, so I thought I'd post this week's finds early. As a result they'll be pretty brief. Let me start with one that I mentioned in "Week 9" but is now easier to get:

1) Alan D. Rendall, "Unique determination of an inner product by adjointness relations in the algebra of quantum observables", available as gr-qc/9303026.

and then mention another thing I've gotten as a spinoff from the gravity conference at UCSB:

2) Ranjeet S. Tate, *An Algebraic Approach to the Quantization of Constrained Systems: Finite Dimensional Examples*, Ph.D. thesis, Physics Department, Syracuse University, August 1992, SU-GP-92/8-1.

Both the technical problems of "canonical" quantum gravity and one of the main conceptual problems — the problem of time — stem from the fact that general relativity is a system in which the initial data have constraints. So improving our understanding of quantizing constrained classical systems is important in understanding quantum gravity.

Let me say a few words about these constraints and what I mean by "canonical" quantum gravity.

First consider the wave equation in 2 dimensions. This is an equation for a function from $\mathbb{R}^2$ to $\mathbb{R}$, say $\varphi(t, x)$, where t is a timelike and x is a spacelike coordinate. The equation is simply

$$\frac{d^2\varphi}{dt^2} - \frac{d^2\varphi}{dx^2} = 0.$$

Now this equation can be rewritten as an evolutionary equation for initial data as follows. We consider pairs of functions (Q, P) on $\mathbb{R}$ — which we think of φ and $d\varphi/dt$ on "space", that is, on a surface $t =$ constant. And we rewrite the second-order equation above as a first-order equation:

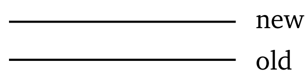
$$\frac{d}{dt}(Q, P) = \left(P, \frac{d^2Q}{dx^2}\right). \tag{1}$$

This is a standard trick. We call the space of pairs (Q, P) the "phase space" of the theory. In canonical quantization, we treat this a lot like the space $\mathbb{R}^2$ of pairs (q, p) describing the initial position and momentum of a particle. Note that for a harmonic oscillator we have an equation a whole lot like (1):

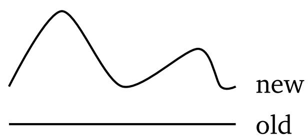
$$\frac{d}{dt}(q, p) = (p, -q).$$

This is why when we quantize the wave equation it's a whole lot like the harmonic oscillator.

Now in general relativity things are similar but more complicated. The analog of the pairs $(\varphi, d\varphi/dt)$ are pairs (Q, P) where Q is the metric on spacetime restricted to a spacelike hypersurface — that is, the "metric on space at a given time" — and P is concocted from the extrinsic curvature of that hypersurface as it sits in spacetime. Now the name of the game is to turn Einstein's equation for the metric into a first-order equation sort of like (1). The problem is, in general relativity there is no god-given notion of time. So we need to *pick* a "lapse function" on our hypersurface, and a "shift vector field" on our hypersurface, which say how we want to push our hypersurface forwards in time. The lapse function says at each point how much we push it in the normal direction, while the shift vector field says at each point how much we push it in some tangential direction. These are utterly arbitrary and give us complete flexibility in how we want to push the hypersurface forwards. Even if spacetime was flat, we could push the hypersurface forwards in a dull way like:



or in a screwy way like



Of course, in general relativity spacetime is usually not flat, which makes it ultimately impossible to decide what counts as a "dull way" and what counts as a "screwy way," which is why we simply allow all possible ways.

Anyway, having *chosen* a lapse function and shift vector field, we can rewrite Einstein's equations as an evolutionary equation. This is a bit of a mess, and it's called the ADM (Arnowitt–Deser–Misner) formalism. Schematically, it goes like

$$\frac{d}{dt}(Q,P) = (\text{stuff}_1, \text{stuff}_2). \tag{2}$$

where both "stuff_1" and "stuff_2" depend on both Q and P in a pretty complex way.

But there is a catch. While the evolutionary equations are equivalent to 6 of Einstein's equations (Einstein's equation for general relativity is really 10 scalar equations packed into one tensor equation), there are 4 more of Einstein's equations which turn into *constraints* on Q and P. 1 of these constraints is called the Hamiltonian constraint and is closely related to the lapse function; the other 3 are called the momentum or diffeomorphism constraints and are closely related to the shift vector field.

For those of you who know Hamiltonian mechanics, the reason why the Hamiltonian constraint is called what it is is that we can write it as

$$H(Q,P) = 0$$

for some combination of Q and P, and this $H(Q,P)$ acts a lot like a Hamiltonian for general relativity in that we can rewrite (2) using the Poisson brackets on the "phase space" of all (Q,P) pairs as

$$\begin{aligned}\frac{d}{dt}Q &= \{P, H(Q,P)\}\\ \frac{d}{dt}P &= \{Q, H(Q,P)\}.\end{aligned}$$

The funny thing is that H is not zero on the space of all (Q, P) pairs, so the equations above are nontrivial, but it does vanish on the submanifold of pairs satisfying the constraints, so that, in a sense, "the Hamiltonian of general relativity is zero". But one must be careful in saying this because it can be confusing! It has confused lots of people worrying about the problem of time in quantum gravity, where they naively think "What — the Hamiltonian is zero? That means there's no dynamics at all!"

The problem in quantizing general relativity in the "canonical" approach is largely figuring out what to do with the constraints. It was Dirac who first seriously tackled such problems, but the constraints in general relativity always seemed intractible (when quantizing) until Ashtekar invented his "new variables" for quantum gravity, that all of a sudden make the constraints look a lot simpler. Ashtekar also has certain generalizations of Dirac's general approach to quantizing systems with constraints, and part of what Tate (who was a student of Ashtekar) is doing is to study a number of toy models to see how Ashtekar's ideas work.

I should note that there are lots of other ways to handle problems with constraints, like BRST quantization, that aren't mentioned here at all.

Well, I'm off to Kansas and I hope to return with a bunch of goodies and some gossip about 4-manifold invariants, topological quantum field theories and the like. Lee Smolin will be talking there too so I will try to extract some information about quantum gravity from him.

Week 12

April 10, 1993

I had a lot of fun at the "Quantum Topology" conference at Kansas State University, in Manhattan (yes, that's right, Manhattan, Kansas, the so-called "Little Apple"), and then spent a week recovering. Now I'm back, ready for the next quarter...

The most novel idea I ran into at the conference was due to Oleg Viro, who, ironically, is right here at U. C. Riverside. He spoke on work with Turaev on generalizing the Alexander module (a classical knot invariant) to get a similar sort of module from any 3-dimensional topological quantum field theory. A "topological quantum field theory," or TQFT for short, is (in the language of physics) basically just a generally covariant quantum field theory, one that thinks all coordinate systems are equally good, just as general relativity is a generally covariant classical field theory. For a more precise definition of TQFTs (which even mathematicians who know nothing of physics can probably follow), see my article "symmetries". In any event, I don't think Viro's work exists in printed form yet; I'll let you all know when something appears.

The most lively talk was one by Louis Crane and David Yetter, the organizers of the conference. As I noted a while back, they claimed to have constructed a FOUR-dimensional TQFT based on some ideas of Ooguri, who was working on 4-dimensional quantum gravity. This would be very exciting as long as it isn't "trivial" in some sense, because all the TQFTs developed so far only work in 3-dimensional spacetime. A rigorous 4-dimensional TQFT might bring us within striking distance of a theory of quantum gravity — this is certainly Crane's goal. Ocneanu, however, had fired off a note claiming to prove that the Crane–Yetter TQFT was trivial, in the sense that the partition function of the field theory for any compact oriented 4-manifold equalled 1! In a TQFT, the partition function of the field theory on a compact manifold is a invariant of the manifold, and if it equalled 1 for all manifolds, it would be an extremely dull invariant, hence a rather trivial TQFT.

So, on popular demand, Crane and Yetter had a special talk at 8 pm in which they described their TQFT and presented results of calculations that showed the invariant did NOT equal 1 for all compact oriented 4-manifolds. So far they have only calculated it in some special cases: S^4, $S^3 \times S^1$, and $S^2 \times S^2$. Amusingly, Yetter ran through the calculation in the simplest case, S^4, in which the invariant *does* happen to equal 1. But he persuaded most of us (me at least) that the invariant really is an invariant and that he can calculate it. I say "persuade" rather than prove because he didn't present a proof that it's an invariant; the current proof is grungy and computational, but Viro and Kauffman (who were there) pointed out some ways that it could be made more slick, so we should see a comprehensible proof one of these days. However, it's still up in the air whether this invariant might be "trivial" in some more sophisticated sense, e.g., maybe it's a function of well-known invariants like the signature and Euler number. Unfortunately, Ocneanu decided at the last minute not to attend. Nor did Broda (inventor of another 4-manifold invariant that Ruberman seems to have shown "trivial" in previous This Week's Finds) show up, though he had been going to.

On a slightly more technical note, Crane and Yetter's TQFT depends on chopping up the 4-manifold into simplices (roughly speaking, 4-dimensional versions of tetrahedra).

Their calculation involves drawing projections of these beasts into the plane and applying various rules; it was quite fun to watch Yetter do it on the blackboard. Turaev and Viro had constructed such a "simplicial" TQFT in 3 dimensions, and Ooguri had been working on simplicial quantum gravity. As I note below, Lee Smolin has a new scheme for doing 4-dimensional quantum gravity using simplices. During the conference he was busy trying to figure out the relation of his ideas to Crane and Yetter's.

Also while at the conference, I found a terrible error in "Week 10" in my description of

> Sergey Piunikhin,"Vassiliev invariants contain more information than all knot polynomials", preprint.

I had said that Piunikhin had discovered a Vassiliev invariant that could distinguish knots from their orientation-reversed versions. No! The problem was a very hasty reading on my part, together with the following typo in the paper, that tricked my eyes:

> *Above constructed Vassiliev knot invariant w of order six does knot detect orientation of knots.*

Ugh! Also, people at the conference said that Piunikhin's claim in this paper to have found a Vassiliev invariant not coming from quantum group knot polynomials is misleading. I don't understand that yet.

Here are some papers that have recently shown up...

1) Karel Kuchar, "Canonical quantum gravity", available as gr-qc/9304012.

Kuchar (pronounced Koo-kahsh, by the way) is one of the grand old men of quantum gravity, one of the people who stuck with the subject for the many years when it seemed absolutely hopeless, who now deserves some of the credit for the field's current resurgence. He has always been very interested in the problem of time, and for anyone who knows a little general relativity and quantum field theory, this is a very readable introduction to some of the key problems in canonical quantum gravity. I should warn the naive reader, however, that Kuchar's views about the problem of time expressed in this paper go strongly against those of many other experts! It is a controversial problem.

Briefly, many people believe that physical observables in quantum gravity should commute with the Hamiltonian constraint (cf. "Week 11"); this means that they are time-independent, or constants of motion, and this makes the dynamics of quantum gravity hard to ferrett out. Kuchar calls such quantities "perennials." But Rovelli has made a proposal for how to recover dynamics from perennials, basically by considering 1-parameter families A_t of perennials, ironically called "evolving constants of motion." Kuchar argues against this proposal on two grounds: first, he does not think physical observables need to commute with the Hamiltonian constraint, and second, he argues that there may be very few if any perennials. The latter point is much more convincing to me than the former, at least at the *classical* level, where the presence of enough perennials would be close to the complete integrability of general relativity, which is most unlikely. But on the quantum level things are likely to be quite different, and Smolin has recently been at work attempting to construct perennials in quantum gravity (cf. "Week 1"). As for Kuchar's former point, that observables in quantum gravity need not be perennials, his arguments seem rather weak. In any event, read and enjoy, but realize that the subject is a tricky one!

2) John Fischer, "2-categories and 2-knots", preprint, last revised Feb. 6 1993.

This is the easiest way to learn about the 2-category of 2-knots. Recall (from "Week 1" and "Week 4") that a 2-knot is a surface embedded in $\mathbb{R}^4$, which may visualized as a "movie" of knots evolving in time. Fischer shows that the algebraic structure of 2-knots is captured by a braided monoidal 2-category, and he describes this 2-category.

3) Mark Miller and Lee Smolin,"A new discretization of classical and quantum general relativity", available as gr-qc/9304005.

Here Smolin proposes a new simplicial approach to general relativity (there is an older one known as the Regge calculus) which uses Ashtekar's "new variables," and works in terms of the Capovilla–Dell–Jacobson version of the Lagrangian. Let me just quote the abstract, I'm getting tired:

> *We propose a new discrete approximation to the Einstein equations, based on the Capovilla–Dell–Jacobson form of the action for the Ashtekar variables. This formulation is analogous to the Regge calculus in that it results from the application of the exact equations to a restricted class of geometries. Both a Lagrangian and Hamiltonian formulation are proposed and we report partial results about the constraint algebra of the Hamiltonian formulation. We find that in the limit that the* $\mathrm{SO}(3)$ *gauge symmetry of frame rotations is reduced to the abelian* $\mathrm{U}(1)^3$*, the discrete versions of the diffeomorphism constraints commute with each other and with the Hamiltonian constraint.*

4) "Higher algebraic structures and quantization", by Dan Freed, available as hep-th/9212115.

This is about TQFTs and the high-powered algebra needed to do justice to the "ladder of field theories" that one can obtain starting with a d-dimensional TQFT — gerbs, torsors, n-categories and other such scary things. I am too beat to do this justice.

Week 13

April 20, 1993

Well, folks, this'll be the last "This Week's Finds" for a while, since I'm getting rather busy preparing for my conference on knots and quantum gravity, and I have a paper that seems to be taking forever to finish.

1) Anthony W. Knapp, *Elliptic Curves*, Mathematical Notes, Princeton University Press, 1992.

This is a shockingly user-friendly introduction to a subject that can all too easily seem intimidating. I'm certainly no expert but maybe just for that reason I should sketch a brief "introduction to the introduction" that may lure some of you into studying this beautiful subject.

What I will say will perhaps appeal to people who like complex analysis or mathematical physics, but Knapp concentrates on the aspects related to number theory. For other approaches one might try

2) Serge Lang, *Elliptic Functions*, 2nd edition, Springer, Berlin 1987.

3) Dale Husemoeller, *Elliptic Curves*, Springer, Berlin, 1987.

Okay, where to start? Well, how about this: the sine function is an analytic function on the complex plane with the property that

$$\sin(z+2\pi) = \sin z.$$

It also satisfies a nice differential equation

$$(\sin' z)^2 = 1 - (\sin z)^2$$

and for this reason, we could, if we hadn't noticed the sine function otherwise, have run into it when we tried to integrate

$$\frac{1}{\sqrt{1-u^2}}$$

The differential equation above implies that the integral is nice to do by the substitution $u = \sin z$, and we get the answer $\arcsin u$. If the sine function — or more generally, trig functions — didn't exist yet, we would have invented them when we tried to do integrals involving square roots of quadratic polynomials.

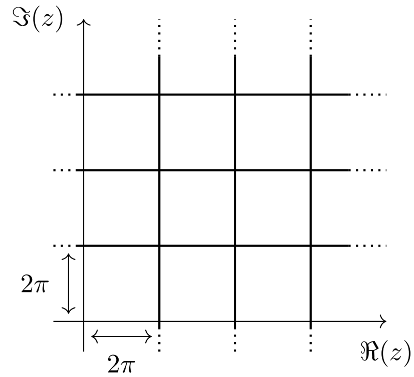
Elliptic functions are a beautiful generalization of all of this stuff. Say we wanted, just for the heck of it, an analytic function that was periodic not just in one direction on the complex plane, like the sine function, but in *two* directions. For example, we might want some function $P(z)$ with

$$P(z+2\pi) = P(z)$$

and also

$$P(z+2\pi i) = P(z).$$

This function would look just the same on each 2π-by-2π square:

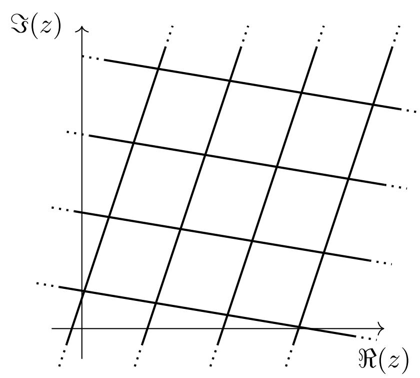


so if we wanted, we could think of it as being a function on the torus formed by taking one of these squares and identifying its top side with its bottom side, and its left side with its right side.

More generally — while we're fantasizing about this wonderful doubly-periodic function — we could ask for one that was periodic in any old two directions. That is, fixing two numbers ω_1 and ω_2 that aren't just real-valued multiples of each other, we could hope to find an analytic function on the complex plane with ω_1 and ω_2 as periods:

$$P(z+\omega_1) = P(z)$$
$$P(z+\omega_2) = P(z).$$

Then $P(z)$ would be the same at all points on the "lattice" of points $n\omega_1 + m\omega_2$, which might look like the squares above or might be like



or some such thing.

Let's think about this nice function $P(z)$ we are fantasizing about. Alas, if it were analytic on the whole plane (no poles), it would be bounded on each little parallelogram, and since it's doubly periodic, it would be a bounded analytic function on the complex plane, hence **constant** by Liouville's theorem. Well, so a constant function has all the wonderful properties we want — but that's too boring!

So let's allow it to have poles! But let's keep it as nice as possible, so let's have the only poles occur at the lattice points

$$L = \{n\omega_1 + m\omega_2\}.$$

And let's make the poles as nice as possible. Can we have each pole be of order one? That is, can we make $P(z)$ blow up like $1/(z-\omega)$ at each lattice point ω in L? No, because if it did, the integral of P around a nicely chosen parallelogram around the pole would be zero, because the contributions from opposite sides of the parallelogram would cancel by symmetry. (A fun exercise.) But by the Cauchy residue formula this means that the residue of the pole vanishes, so it can't be of order one.

Okay, try again. Let's try to make the pole at each lattice point be of order two. How can we cook up such a function? We might try something obvious: just sum up, for all ω in the lattice L, the functions

$$\frac{1}{(z-\omega)^2}$$

We get something periodic with poles like $1/(z-\omega)^2$ at each lattice point ω. But there's a big problem — the sum doesn't converge! (Another fun exercise.)

Oh well, try again. Let's act like physicists and **renormalize** the sum by subtracting off an infinite constant! Just subtract the sum over all ω in L of $1/\omega^2$. Well, all ω except zero, anyway. This turns out to work, but we really should be careful about the order of summation here: really, we should let $P(z)$ be $1/z^2$ plus the sum for all nonzero ω in the lattice L of $1/(z-\omega)^2 - 1/\omega^2$. This sum does converge and the limit is a function $P(z)$ that's analytic except for poles of order two at the lattice points. This is none other than the Weierstrass elliptic function, usually written with a fancy Gothic $\mathfrak{P}$ to intimidate people. Note that it really depends on the two periods ω_1 and ω_2, not just z.

Now, it turns out that $P(z)$ really *is* a cool generalization of the sine function. Namely, it satisfies a differential equation like the one the sine does, but fancier:

$$P'(z)^2 = 4P(z)^3 - g_2P(z) - g_3$$

where g_2 and g_3 are some constants that depend on the periods ω_1 and ω_2. Just as with the sine function we can use the *inverse* of Weierstrass $\mathfrak{P}$ function to do some integrals, but this time we can do integrals involving square roots of cubic polynomials! If you look in big nasty books of special functions or tables of integrals, you will see that there's a big theory of this kind of thing that was developed in the 1800's — back when heavy-duty calculus was hip.

There are, however, some other cool ways of thinking about what's going on here. First of all, remember that we can think of $P(z)$ as a function on the torus. We can think of this torus as being "coordinatized" — I use the word loosely — by $P(z)$ and its first derivative $P'(z)$. I.e., if we know $x = P(z)$ and $y = P'(z)$ we can figure out where the point z is on the torus. But of course x and y can't be any old thing; the differential equation above says they have to satisfy

$$y^2 = 4x^3 - g_2x - g_3.$$

Here x and y are complex numbers of course. But look what this means: it means that if we look at the pairs of complex numbers (x, y) satisfying the above cubic equation, we get something that looks just like a torus! This is called an elliptic curve, since for algebraic geometers a "curve" is the set of solutions (x, y) of some polynomial in two *complex* variables — not two real variables.

So — an "elliptic curve" is basically just the solutions of a cubic equation in two variables. Actually, we want to rule out curves that have singularities, that is, places where there's no unique tangent line to the curve, as in $y^2 = x^3$ or $y^2 = x^2(x + 1)$ — draw these in the real plane and you'll see what I mean. Anyway, all elliptic curves can, by change of variables, be made to look like our favorite one,

$$y^2 = 4x^3 - g_2 x - g_3.$$

There are lots of more fancy ways of thinking about elliptic curves, and one is to think of the fact that they look like a torus as the key part. In a book on algebraic geometry you might see an elliptic curve as a curve with genus one (i.e., with one "handle," like a torus has). One nice thing about a torus is that is a group. That is, we know how to add complex numbers, and we can add modulo elements of the lattice L, so the torus becomes a group with addition $\mod L$ as the group operation. This is simple enough, but it means that when we look at the solutions of

$$y^2 = 4x^3 - g_2 x - g_3$$

they must form a group somehow, and viewed this way it's not at all obvious! Nonetheless, there is a beautiful geometric description of the group operation in these terms — I'll leave this for Knapp to explain..

Let me wrap this up — the story goes on and on, but I'm getting tired — with a bit about what it has to do with number theory. It has a lot to do with Diophantine equations, where one wants integer, or rational solutions to a polynomial equation. Suppose that g_2 and g_3 are rational, and one has some solutions to the equation

$$y^2 = 4x^3 - g_2 x - g_3.$$

Then it turns out that one can use the group operation on the elliptic curve to get new solutions! Actually, it seems as if Diophantus knew this way back when in some special cases. For example, for the problem

$$y(6 - y) = x^3 - x$$

Diophantus could start with the trivial solution $(x, y) = (-1, 0)$, do some mysterious stuff, and get the solution $(17/9, 26/27)$. Knapp explains how this works in the Overview section, but then more deeply later. Basically, it uses the fact that this curve is an elliptic curve, and uses the group structure.

In fact, one can solve mighty hard-seeming Diophantine problems using these ideas. Knapp talks a bit about a problem Fermat gave to Mersenne in 1643 — this increased my respect for Fermat a bit. He asked, find a Pythagorean triple (X, Y, Z), that is:

$$X^2 + Y^2 = Z^2,$$

such that Z is a square number and $X + Y$ is too! One can solve this using elliptic curves. I don't know if Mersenne got it — the answer is at the end of this post, but heavy-duty number theorists out there might enjoy trying this one if they don't know it already.

Some more stuff:

4) Jim Stasheff, "Closed string field theory, strong homotopy Lie algebras and the operad actions of moduli spaces", available as hep-th/9304061.

One conceptually pleasing approach to string theory is closed string field theory, where one takes as the basic object unparametrized maps from circle into a manifold M representing "space", i.e., elements of

$$\mathsf{Maps}(S^1, M)/\mathsf{Diff}^+(S^1).$$

A state of closed string field theory would be roughly a function on the above set. Then one tries to define all sorts of operations on these states, in order to define write down ways the strings can interact. For example, there is a "convolution product" on these functions which almost defines a Lie algebra structure. However, the Jacobi identity only holds "up to homotopy," so we have an algebraic structure called a homotopy Lie algebra. Physicists would say that the Jacobi identity holds modulo a BRST exact term. This is just the beginning of quite a big bunch of mathematics being developed by Stasheff, Zwiebach, Getzler, Kapranov and many others. My main complaint with the physics is that all these structures seem to depend on choosing a Riemannian metric on M — a so-called "background metric." Since string theory is supposed to include a theory of quantum gravity it is annoying to have this God-given background metric stuck in at the very start. Perhaps I just don't understand this stuff. I am looking around for stuff on background-independent closed string field theory, since I have lots of reason to believe that it's related to the loop representation of quantum gravity. Unfortunately, I scarcely know the subject — I had hoped Stasheff's work would help me, but it seems that this metric always enters.

5) Sergey Matveev and Michael Polyak, "A geometrical presentation of the surface mapping class group and surgery", preprint.

This paper shows how to express the mapping class group of a surface in terms of tangles. This gives a nice relationship between two approaches to 3d TQFTs (topological quantum field theories): the Heegard decomposition approach, and the surgery on links approach.

6) Michael Polyak, "Invariants of 3-manifolds and conformal field theories", preprint.

The main good thing about this paper in my opinion is that it simplifies the definition of a modular tensor category. Recall that Moore and Seiberg showed how any string theory (more precisely, any rational conformal field theory) gave rise to a modular tensor category, and then Crane showed that any modular tensor category gave rise to a 3d TQFT. Unfortunately a modular tensor category seems initially to be a rather baroque mathematical object. In this paper Polyak shows how to get lots of the structure of a modular tensor category from just the "fusion" and "braiding" operators, subject to some mild conditions. I have a conjecture that all nonnegative link invariants (in the sense of my paper on tangles and quantum gravity) give rise to modular tensor categories, and this simplifies things to the point where maybe I might eventually be able to prove it. There are lots of nice pictures here, too, by the way.

Answer to puzzle:

$$X = 1061652293520$$

$$Y = 4565486027761$$

$$Z = 4687298610289$$

Week 14

May 8, 1993

Things are moving very fast in the quantum gravity/4d topology game, so I feel I should break my vow not to continue this series until after next weekend's conference on Knots and Quantum Gravity.

Maybe I should recall where things were when I left off. The physics problem motivating a lot of work in theoretical physics today is reconciling general relativity and quantum theory. The key feature of general relativity is that time and space do not appear as a "background structure," but rather are dynamical variables. In mathematical terms, this just means that there is not a fixed metric; instead gravity *is* the metric, and the metric evolves with time like any other physical field, satisfying some field equations called the Einstein equations.

But it is worth stepping back from the mathematics and trying to put into simple words why this makes general relativity so special. Of course, it's very hard to put this sort of thing into words. But roughly, we can say this: in Newtonian mechanics, there is a universal notion of time, the "t" coordinate that appears in all the equations of physics, and we assume that anyone with a decent watch will be able to keep in synch with everyone else, so there is no confusion about what this "t" is (apart from choosing when to call $t = 0$, which is a small sort of arbitrariness one has to live with). In special relativity this is no longer true; watches moving relative to each other will no longer stay in synch, so we need to pick an "inertial frame," a notion of rest, in order to have a "t" coordinate to play with. Once we pick this inertial frame, we can write the laws of physics as equations involving "t". This is not too bad, because there is only a finite-parameter family of inertial frames, and simple recipes to translate between them, and also because nothing going on will screw up the functioning of our (idealized) clocks: that is, the "t" coordinate doesn't give a damn about the *state* of the universe. That's what is meant by saying a "background structure" — it's some aspect of the universe that is unaffected by everything else that's going on.

In general relativity, things get much more interesting: there is no such thing as an inertial frame that defines coordinates on spacetime, because there is no way you can get a lot of things at different places to remain at rest with each other — this is what is meant by saying that spacetime is curved. You can measure time with your watch, so-called "proper time," but this applies only near you. More interestingly still, to compare what your watch is doing to what someone else's is doing, you actually need to know a lot about the state of the universe, e.g., whether there are any heavy masses around that are curving spacetime. The "metric," whereby one measures distances and proper time, depends on the state of the universe — or more properly, it is part of the state of the universe.

Trying to do *quantum* theory in this context has always been too hard for people. Part of the reason why is that built into the heart of traditional quantum theory is the "Hamiltonian," which describes the evolution of the state of the system relative to a God-given "background" notion of "t". Anyone who has taken quantum mechanics will know that the star of the show is the Schrödinger equation:

$$i\frac{d\psi}{dt} = H\psi$$

saying how the wavefunction ψ changes with time in a way depending on the Hamiltonian H. No "t," no "H" — this is one basic problem with trying to reconcile quantum theory with general relativity.

Actually, it turns out that the analog to Schrödinger's equation for quantum gravity is the Wheeler–DeWitt equation. The Hamiltonian is replaced by an operator called the "Hamiltonian constraint" and we have

$$H\psi = 0.$$

Note how this cleverly avoids mentioning "t"! The problem is, people still aren't quite sure what to do with the solutions to this equation — we're so used to working with Schrödinger's equation.

Now in 1988 Witten wrote a paper in which he coined the term "topological quantum field theory," or TQFT, for short. This was meant to capture in a rigorous way what field theories like quantum gravity should be like. Actually, Witten was working on a different theory called Donaldson theory, which also has the property of having no background structures. Shortly thereafter the mathematician Atiyah came up with a formal definition of a TQFT. To get an idea of this definition, try my notes on "symmetries" and (if you don't know what categories are) "categories". For a serious tour of TQFTs and the like, try his book:

1) Michael Atiyah, *The Geometry and Physics of Knots*, Cambridge U. Press, Cambridge, 1990.

One can think of a TQFT as a framework in which a Wheeler–DeWitt-like equation governs the dynamics of a quantum field theory. Experts may snicker here, but it is true, if not as enlightening as other things one can say.

I won't bother to define TQFTs here, but I think Smolin put it very well when he said the idea of TQFTs really helped us break out of our traditional idea of fields as being something defined at every point of spacetime, wiggling around, and allowed us to see field theory from many new angles. For example, TQFTs let us wiggle out of the old conundrum of whether spacetime is continuous or discrete, because many TQFTs can be *equivalently* described in either of two ways: via a continuum model of spacetime, or via a discrete one in which spacetime is given a "simplicial structure," like a big tetrahedral tinkertoy lattice kind of thing. The latter idea appears to be due to Turaev and Viro, although certainly physicists have had similar ideas for years, going back to Ponzano and Regge, who worked on simplicial quantum gravity.

Now the odd thing is that while interesting 3d TQFTs have been found, the most notable being Chern–Simons theory, nobody has quite been able to make 4d TQFTs rigorous. Witten's original work on Donaldson theory has led to many interesting things, but not yet a full-fledged TQFT in the rigorous sense of Atiyah. And quantum gravity still resists being formulated as a TQFT.

A while back I noted that Crane and Yetter had invented a 4d TQFT using the simplicial approach. There has been a lot of argument over whether this TQFT is interesting or "trivial." Of course, trivial is not a precise concept. For a while Ocneanu claimed that the partition function of every compact 4-manifold equalled 1 in this TQFT, which counts as very trivial. But this appears not to be the case. Broda invented another 4d TQFT and here on "This Week's Finds" Ruberman showed it was trivial in the sense that the partition function of any compact 4-manifold was a function of the "signature" of the 4-manifold. This is trivial because the signature is a well-understood invariant and if we are trying to do something new and interesting that just isn't good enough.

In the following paper:

2) Justin Roberts, "Skein theory and Turaev–Viro invariants", *Topology*, **34**, 771–787. Available as https://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.138.8587.

Roberts *almost* claims to show that the Crane–Yetter invariant is trivial in the same sense, namely that the partition function of any compact 4-manifold is an exponential of the signature. Now if Crane and Yetter's own computations are correct, this cannot be the case, but it *could* be an exponential of a linear combination of the signature and the Euler characteristic, as far as I know. The catch is that Roberts does not normalize his version of the Crane–Yetter invariant in the same way that Crane and Yetter do, so it is hard to compare results. But Roberts says: "The normalisations here do not agree with those in Crane and Yetter, and I have not checked the relationship. However, when dealing with the [3d TQFT] invariants, different normalisations of the initial data change the invariants by factors depending on standard topological invariants (for example Betti numbers), so there is every reason to belive that these [4d TQFT] invariants are trivial (that is, they differ from 1 only by standard invariant factors) in all normalisations."

This is a bit of a disappointment, because Crane at least had hoped that their TQFT might actually turn out to *be* quantum gravity. This was not idle dreaming; it was because the Crane–Yetter construction was a rigorous analog of some work by Ooguri on simplicial quantum gravity.

Then, about a week ago, Rovelli put a paper onto the net:

3) Carlo Rovelli, "The basis of the Ponzano–Regge–Turaev–Viro–Ooguri model is the loop representation basis", available as `hep-th/9304164`.

This is a remarkable paper that I have not been able to absorb yet. First it goes over 3d quantum gravity — which *has* been made into a rigorous TQFT. It works with the simplicial formulation of the theory. That is, we consider our (3-dimensional) spacetime as being chopped up into tetrahedra, and assign to each edge a length, which is required to be $0, \frac{1}{2}, 1, \frac{3}{2}, \ldots$. This idea of quantized edge-lengths goes back to 4d work of Ponzano and Regge, but recently Ooguri showed that in 3d this assumption gives the same answers as Witten's continuum approach to 3d quantum gravity. The "half-integers" $0, \frac{1}{2}, 1, \frac{3}{2}, \ldots$ should remind physicists of spin, which is quantized in the same way, and mathematically this is exactly what is going on: we are really labelling edges with representations of the group $\mathrm{SU}(2)$, that is, spins. What Rovelli shows is that if one starts with the loop representation of 3d quantum gravity (yet another approach), one can prove it equivalent to Ooguri's approach, and what's more, using the loop representation one can *calculate* the lengths of edges of triangles in a given state of space (space here is a 2-dimensional triangulated surface) and *show* that lengths are quantized in units of the Planck length over 2. (Here the Planck length L is the fundamental length scale in quantum gravity, about 1.6×10^{-33} meters.)

And, most tantalizing of all, he sketches a generalization of the above to 4d. In 4d it is known that in the loop representation of quantum gravity it is areas of surfaces that are quantized in units of $L^2/2$, rather than lengths. Rovelli considers an approach where one chops 4-dimensional spacetime up into simplices and assigns to each 2-dimensional face a half-integer area. He uses this to write down a formula for the inner product in the Hilbert space of quantum gravity — thus, at least formally, answering the long-standing "inner product problem" in quantum gravity. The problem is that, unlike in 3d quantum gravity, here one must sum over ways of dividing spacetime into simplices, so the formula for the inner product involves a sum that does not obviously converge. This is however sort of what one might expect, since in 4d quantum gravity, unlike 3d, there are "local excitations" — local wigglings of the metric, if you will — and this makes the Hilbert space be infinite-dimensional, whereas the 3d TQFTs have finite-dimensional Hilbert spaces.

I think I'll quote him here. It's a bit technical in patches, but worth it...

> *We conclude with a consideration on the formal structure of 4-d quantum gravity, which is important to understand the above construction. Standard quantum field theories, as QED and QCD, as well as their generalizations like quantum field theories on curved spaces and perturbative string theory, are defined on metric spaces. Witten's introduction of the topological quantum field theories has shown that one can construct quantum field theories defined on a manifold which has only its differential structure, and no fixed metric structure. The theories introduced by Witten and axiomatized by Atiyah have the following peculiar feature: they have a finite number of degrees of freedom, or, equivalently, their quantum mechanical Hilbert spaces are finite dimensional; classically this follows from the fact that the number of fields is equal to the number of gauge transformations. However, not any diff-invariant field theory on a manifold has a finite number of degrees of freedom. Witten's gravity in 3-d is given by the action*
>
> $$S[A, E] = \int F \wedge E$$
>
> *which has finite number of degrees of freedom. Consider the action*
>
> $$S[A, E] = \int F \wedge e \wedge e$$
>
> *in 3+1 dimensions, for a (self dual)* $\mathrm{SO}(3,1)$ *connection* A *and a (real) one form* e *with values in the vector representation of* $\mathrm{SO}(3,1)$. *This theory has a strong resemblance with its 2+1 dimensional analog: it is still defined on a differential manifold without any fixed metric structure, and is diffeomorphism invariant. We expect that a consistent quantization of such a theory should be found along lines which are more similar to the quantization of the* $\int(F \wedge E)$, *theory than to the quantization of theories on flat space, based on the Wightman axioms namely on* n*-points functions and related objects. Still, the theory* $\int(F \wedge e \wedge e)$ *has genuine field degrees of freedom: its physical phase space is infinite dimensional, and we expect that its Hilbert state space will*

also be infinite dimensional. There is a popular belief that a theory defined on a differential manifold without metric and diffeomorphism invariant has necessarily a finite number of degrees of freedom ("because thanks to general covariance we can gauge away any local excitation"). This belief is of course wrong. A theory as the one defined by the action $\int(F \wedge e \wedge e)$ *is a theory that shares many features with the topological theories, in particular, no quantity defined "in a specific point" is gauge invariant; but at the same time it has genuinely infinite degrees of freedom. Indeed, this theory is of course nothing but (Ashtekar's form of) standard general relativity.*

The fact that "local" quantities like the n*-point functions are not appropriate to describe quantum gravity non-perturbatively has been repeatedly noted in the literature. As a consequence, the issue of what are the quantities in terms of which a quantum theory of gravity can be constructed is a much debated issue. The above discussion indicates a way to face the problem: The topological quantum field theories studied by Witten and Atiyah provide a framework in terms of which quantum gravity itself may be framed, in spite of the infinite degrees of freedom. In particular, Atiyah's axiomatization of the topological field theories provides us with a clean way of formulating the problem. Of course, we have to relax the requirement that the theory has a finite number of degrees of freedom. These considerations leads us to propose that the correct general axiomatic scheme for a physical quantum theory of gravity is simply Atiyah's set of axioms up to finite dimensionality of the Hilbert state space. We denote a structure that satisfies all Atiyah's axioms, except the finite dimensionality of the state space, as a* ***generalized topological theory****.*

The theory we have sketched is an example of such a generalized topological theory. We associate to the connected components of the boundary of M *the infinite dimensional state space of the Loop Representation of quantum gravity. Eq. 5 [the magic formula I alluded to — jb], then, provides a map, in Atiyah's sense, between the state spaces constructed on two of these boundary components. Equivalently, it provides the definition of the Hilbert product in the state space.*

One could argue that the framework we have described cannot be consistent, because it cannot allow us to recover the "broken phase of gravity" in which we have a nondegenerate background metric: in the proposed framework one has only non-local observables on the boundaries, while in the broken phase a local *field in* M *has non-vanishing vacuum expectation value. We think that this argument is weak because it disregards the diffeomorphism invariance of the theory: in classical general relativity no experiment can distinguish a Minkowskian spacetime metric from a non-Minkowkian flat metric. The two are physically equivalent, as two gauge-related Maxwell potentials. For the same reason, no experiment could detect the absolute* position *of, say, a gravitational wave, (while of course the position of an e.m. wave is observable in Maxwell theory). Physical locality in general relativity is only defined as coincidence of some physical variable with some other physical variable, while in non general relativistic physics locality is defined with respect to a fixed metric structure. In classical general relativity, there is no gauge-invariant obervable which is local*

> *in the coordinates. Thus, any observation can be described by means of the value of the fields on arbitrary boundaries of spacetime. This is the correct consequence of the fact that "thanks to general covariance we can gauge away any local excitation", and this is the reason for which one can have the ADM "frozen time" formalism. The spacetime manifold of general relativity is, in a sense, a much weaker physical object than the spacetime metric manifold of ordinary theories. All the general relativistic physics can be read from the boundaries of this manifold. At the same time, however, these boundaries still carry an infinite dimensional number of degrees of freedom.*

Next, let me take the liberty of describing some work of my own:

4) John Baez, "Diffeomorphism-invariant generalized measures on the space of connections modulo gauge transformations", *Proceedings of the Conference on Quantum Topology*, ed. David N. Yetter, World Scientific Press, Singapore, 1994, pp. 21–43. Also available as hep-th/9305045.

This is an extremely interesting paper by a very good mathematician. Whoops! Let's be objective here. In the loop representation of quantum gravity, states of quantum gravity are given naively by certain "measures" on a space A/G of connections modulo gauge transformations. The Chern–Simons path integral is also such a "measure". In both cases, the "measure" in question is invariant under diffeomorphisms of space. And in both cases, the loop transform allows one to think of these measures as instead being functions of multiloops (collections of loops in space). This is the origin of the relationship to knot theory.

The problem, as always in quantum field theory, is that the notion of "measure" must be taken with a big grain of salt — it's not the sort of measure they taught you about in real analysis. Instead, these measures are a kind of "generalized measure" that allows you to integrate not all continuous functions on A/G but only those lying in an algebra called the "holonomy algebra," defined by Ashtekar, Isham and Lewandowski. To be precise and technical, this is the closure in the L^∞ norm of the algebra of functions on A/G generated by "Wilson loops," or traced holonomies around loops. So what we are really interested in is not diffeomorphism-invariant measures on A/G, but diffeomorphism invariant elements of the dual of the holonomy algebra. I begin with a review of generalized measures, introduce the holonomy algebra, and then do a bunch of new work in which I show how to rigorously construct lots of diffeomorphism-invariant elements of the dual of the holonomy algebra by doing lattice gauge theory on graphs embedded in space. Again, as with the work discussed above, we see that the discrete and continuum approaches to space go hand-in-hand! And we see that there are some interesting connections between singularity theory and group representation theory showing up when we try to understand "measures" on the space A/G.

The following is a part of a paper discussed in "Week 5", now available from gr-qc:

5) Abhay Ashtekar and Jerzy Lewandowski,"Completeness of Wilson loop functionals on the moduli space of $\mathrm{SL}(2,\mathbb{C})$ and $\mathrm{SU}(1,1)$-connections", available as gr-qc/9304044.

I didn't discuss this aspect of the paper, so let me quote the abstract:

> *The structure of the moduli spaces $M := A/G$ of (all, not just flat) $\mathrm{SL}(2,\mathbb{C})$ and $\mathrm{SU}(1,1)$ connections on a n-manifold is analysed. For any topology on the corresponding spaces A of all connections which satisfies the weak requirement of compatibility with the affine structure of A, the moduli space M is shown to be non-Hausdorff. It is then shown that the Wilson loop functionals — i.e., the traces of holonomies of connections around closed loops — are complete in the sense that they suffice to separate all separable points of M. The methods are general enough to allow the underlying n-manifold to be topologically non-trivial and for connections to be defined on non-trivial bundles. The results have implications for canonical quantum general relativity in 4 and 3 dimensions.*

By the way, someone should extend this result to more general noncompact semisimple Lie groups, and also show that for all compact semisimple Lie groups the Wilson loop functionals in any faithful representation *do* separate points (this is known for the fundamental representation of $\mathrm{SU}(n)$). If I had a bunch of grad students I would get one to do so.

The following was discussed in an earlier edition of this series, "Week 11", but is now available from gr-qc:

6) Ranjeet S. Tate, *An Algebraic Approach to the Quantization of Constrained Systems: Finite Dimensional Examples*, Ph.D. Thesis, Department of Physics, Syracuse University, 124 pages, available as `gr-qc/9304043`.

I haven't read the following one but it seems like an interesting application of loop variables to more down-to-earth physics; Gambini was one of the originators of the loop representation, and intended it for use in QCD:

6) Rodolfo Gambini and Leonardo Setaro, "$\mathrm{SU}(2)$ QCD in the path representation", available as `hep-lat/9305001`. ("`hep-lat`" is the computational and lattice physics preprint list.)

Let me quote the abstract:

> *We introduce a path-dependent hamiltonian representation (the path representation) for $\mathrm{SU}(2)$ with fermions in 3 + 1 dimensions. The gauge-invariant operators and hamiltonian are realized in a Hilbert space of open path and loop functionals. We obtain a new type of relation, analogous to the Mandelstam identity of second kind, that connects open path operators with loop operators. Also, we describe the cluster approximation that permits to accomplish explicit calculations of the vacuum energy density and the mass gap.*

Week 15

May 23, 1993

Last weekend we had a conference on Knots and Quantum Gravity here at Riverside. I will briefly describe the talks, many of which will eventually appear in a conference proceedings volume. I think that to be nice I will list these talks in order of how technical my descriptions will be, rather than chronologically.

Oleg Viro spoke on "simplicial topological quantum field theories". There has been a lot of interest recently in constructing topological quantum field theories using triangulations of manifolds. The most famous of these is due to Turaev and Viro. Witten showed that this one is the same as quantum gravity in $2+1$ dimensions. The nice thing is that this gives an alternate description of the Turaev–Viro theory in terms of more traditional ideas from field theory, so the same theory has a "discrete" and a "continuum" formulation — some evidence for my notion that quantum gravity will resolve the old "is space continuous or discrete" argument by saying "both, and neither," just as quantum mechanics resolved the old "is light a wave or a particle" dispute! (Hegel would've loved it.)

When constructing simplicial topological quantum field theories, one has to prove that the answer you get is independent of the triangulation. Viro reviewed a couple sets of "moves" whereby one can get between any two triangulations of the same manifold — the Alexander moves, and the Pachner moves. He also discussed an alternate, and more convenient, way of describing manifolds by "special spines". Here the idea is as follows. Pick a bunch of points in the manifold. From each one, start blowing a little bubble, which grows bigger and bigger until it bumps into the other bubbles. The result is something very much like a foam of soap bubbles, which generically have polyhedral faces, with edges and vertices of a special sort. Look at a mess of foam sometime if you don't know what I mean: in 3 dimensions, three bubbles meet at an edge, and four at a vertex. One can describe this situation purely combinatorially, and it contains all the information about the manifold. There are a certain set of moves, the Matveev moves, relating any two such "special spines" for the same manifold. One can figure out what these moves might be by staring some foam and watching how the bubbles move.

Louis Kaufmann changed his talk from the announced subject and instead talked about Vassiliev invariants of knots and their relation to perturbative Chern–Simons theory. Let me just recall what all this is about. Chern–Simons theory is a TQFT (topological quantum field theory) in 3 dimensions in which the field is a connection. In physical terms, a connection is just a generalization of the vector potential in electromagnetism. Recall that in 3 dimensional space, the vector potential is a vector field A whose curl is the magnetic field. In quantum theory, the significance of the vector potential is as follows. If we take a particle and carry it around a loop, its wavefunction gets multiplied by a phase, that is, a complex number of absolute value 1. These "phases" form a group, since the product of two phases is a phase. This group is called $\mathrm{U}(1)$, since we can think of phases as 1×1 unitary matrices. A key idea in modern physics is to generalize the heck out of electromagnetism by allowing other groups to play the role of phases. The group we choose is called the "gauge group." The second simplest choice after $\mathrm{U}(1)$ is $\mathrm{SU}(2)$, the 2×2 unitary matrices with determinant $= 1$.

You can do Chern–Simons theory with any gauge group but it's especially simple with gauge group $\mathrm{SU}(2)$. An $\mathrm{SU}(2)$ connection is just a kind of field that lets one do "parallel translation" around a loop in space and get an element of $\mathrm{SU}(2)$. Mathematicians call this the holonomy of the connection around the loop. Physicists typically take the trace of the group element (in this case, just the sum of the diagonal entries of the 2×2 matrix), and call that the "Wilson loop observable," a function of the connection that depends on the loop.

Now the great thing about Chern–Simons theory is that the theory is independent of any choice of coordinates or background metric on spacetime. This is part of what we mean by saying the theory is a TQFT. Another aspect of a TQFT is that there is a "vacuum state" and we can calculate the expectation value of a Wilson loop in the vacuum state. The idea one should have is that the connection is undergoing all sorts of "quantum fluctuations" in the vacuum state, but that we can ask for the *average* value for the trace of the connection of the holonomy around a loop in the vacuum state. Given a knot K, we write this expectation value as $\langle K\rangle$. Now the great thing about Chern–Simons theory is that the vacuum state does not care what coordinates you use to describe it. Thus $\langle K\rangle$ does not depend on the *geometry* of the knot K (which would take coordinates or a metric to describe), but only on its topology. In other words, $\langle K\rangle$ is a knot invariant. In fact we can define $\langle K\rangle$ not just for knots, but also for links (bunches of knots, possibly intertangled), by taking the expectation value of the product of the Wilson loops, one for each knot. So Chern–Simons theory really gives a link invariant. Witten showed that this link invariant is just the Kauffman bracket, which is an invariant easily calculated using the rules:

Rule 1: If K is the "empty link," the link with NO components whatsoever — i.e., just the empty set — we have

$$\langle K\rangle = 1.$$

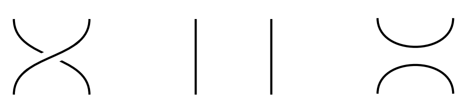
This is sort of a normalization rule.

Rule 2: If K' is obtained from K by adding an unlinked copy of the unknot (an unknotted circle) to K,

$$\langle K'\rangle = -(a^2 + a^{-2})\langle K\rangle.$$

Here a is an adjustable parameter that appears in Chern–Simons theory — a function of the coupling constant.

Rule 3: Suppose K, L, and L' are 3 knots or links differing at just one crossing (we're supposing them to be drawn as pictures in 2 dimensions). And suppose at this crossing they look as follows:



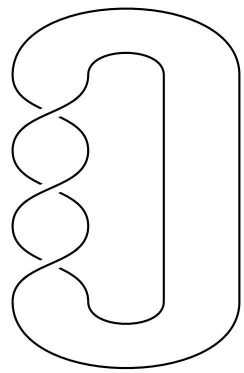
K L L'

Any rotated version of this picture is fine too.

Then

$$\langle K\rangle = a\langle L\rangle + a^{-1}\langle L'\rangle.$$

That's it! I leave as an exercise for the reader to calculate $\langle K \rangle$ for the trefoil knot,



and get $-A^5 - A^{-3} - A^{-7}$. Then try the mirror-image trefoil, or "left-handed trefoil," and see what you get.

Now in quantum field theory people like doing perturbative calculations, and that's interesting here even though we know the exact answer. Namely, there is a coupling constant c in Chern–Simons theory such that $A = \exp(c)$, and if one uses Feynman diagrams and the rest of the usual machinery for quantum field theory and does a perturbative calculation of the vacuum expectation value of Wilson loops, one gets the same answer, but as a power series in c. The coefficient of the c^n is a special sort of link invariant called a "Vassiliev invariant" of degree n. I discussed these a lot in "Week 3" (see below for how to get that article), so I won't repeat myself here. In any event, Kauffman gave a nice discussion of this sort of thing.

Viktor Ginzburg talked about his work with Milgram on Vassiliev invariants. He had hoped to show that these were an almost complete set of knot invariants, able to distinguish between any two knots that weren't just orientation-reversed versions of each other (here we equip the knots with an orientation, or field of arrows running along the knot). He came to the conference, as he said, sadder and wiser. He presented a nice result on Vassiliev invariants that might be a step towards proving completeness.

Dana Fine spoke on "Chern–Simons theory and the Wess–Zumino–Witten model". There is a very interesting "ladder of field theories" that contains "topological quantum gravity" in 4 dimensions, Chern–Simons theory in 3 dimensions, and the Wess-Zumino-witten model in 2 dimensions. Dana Fine spoke on the bottom 2 rungs of this ladder. he described a very explicit, although still formal, reduction of the Chern–Simons path integral (the integral one does to compute the expectation values I mentioned above) to the path integral in the Wess–Zumino–Witten model. The relation between CS theory and the WZW model is what witten used in his original argument that CS theory gives interesting link invariants, so this is of interest in knot theory as well as physics.

On Saturday morning we had a nice trio of talks from the Syracuse gang, Syracuse University being a hotbed of new work on quantum gravity. Ashtekar and Smolin are there, as are a bunch of good grad students (Bernd Bruegmann was there until very recently) and postdocs, including Jerzy Lewandowski and Renate Loll. The whole gang is moving down to Penn State this summer, and they will be hiring Jorge Pullin, now at Utah State. There are not many people working on quantum gravity — and not many jobs in the field — so Penn State will become arguably *the* center, at least in the US (let us not forget Penrose, Hawking, Isham, et al!), for work on this subject.

Renate Loll spoke on the "Loop representation of gauge theory and gravity." This was an introduction to the ideas of Gambini, Trias, Rovelli, Smolin, et al on doing quantum field theory solely in terms of Wilson loops. It is this approach that makes the connection between quantum gravity and knot theory.

Abhay Ashtekar spoke on "Loop transforms." The process of encoding a connection in terms of all its Wilson loops is called the loop transform, and it can be regarded as a nonlinear generalization of the Fourier transform. Ashtekar has led an effort to make this transform thoroughly rigorous and mathematically respectable, and he discussed this work.

Jorge Pullin spoke on "The quantum Einstein equations and the Jones polynomial". He outlined his work with Gambini and Bruegmann in which they show perturbatively that the Kaufmann bracket (or, alternatively, Chern–Simons theory) gives a state of 4-d quantum gravity. This is perhaps the most exciting aspect of the "ladder of field theories" mentioned above, since it touches upon the real world — or at least comes darn close.

On Sunday, Gerald Johnson started things off with an introduction to his work on making the Feynman path integral rigorous. This is relevant because a main problem with Witten's otherwise marvelous work is that the path integral in Chern–Simons theory has not been made rigorous. Dana Fine's talk offered one approach, and my own talk offered another (based on my recent paper).

Perhaps the most novel talk was by Paolo Cotta-Ramusino ("4d quantum gravity and knot theory") describing his work with Maurizio Martellini on 4-dimensional TQFTs and invariants of 2-knots, that is, embedded surfaces in $\mathbb{R}^4$ (or more general 4-manifolds). This is an attempt to push the Wilson loop story up one dimension, in an effort to make it applicable to theories similar to quantum gravity. These theories are the so-called "BF theories," whose Lagrangian is of the form $\mathrm{tr}(B \wedge F)$, where B is a Lie algebra valued 2-form and F is the curvature of a connection. Martellini and Cotta-Ramusino's work on this is still in a preliminary stage but it seems rather promising.

Perhaps the most controversial talk was by Louis Crane, entitle "Quantum gravity, spin geometry and categorical physics." This was about his ideas on using category theory to construct 4-dimensional TQFTs. He also emphasized the importance of TQFTs that use triangulations but wind up being independent of the triangulation, thus slipping through the discrete/continuous distinction. Many of his assertions provoked violent reactions from the physicists present.

Finally, I spoke on "Strings, tangles and gauge fields," beginning by pointing out some relationships between closed string field theory and the loop representation of quantum gravity, and then retreating to safer ground and describing my work on trying to make the Chern–Simons path integral and the loop representation more mathematically rigorous. I will write a paper on this subject this summer and try to further build up my case for the conjecture that string theory and gauge field theory are in a sense dual descriptions of certain TQFTs.

A rather technical introduction to currently interesting topics in closed string field theory has just appeared:

1) Barton Zwiebach,"Closed string field theory — an introduction", available as hep-th/9305026.

Unfortunately for me, he mainly treats theories of strings moving around on a manifold with a background metric, while it seems that the loop representation of quantum gravity is very like a "background-free" string field theory. A paper that recently came out and appears to support my notions is the following:

2) S. G. Naculich, H. A. Riggs, and H. J. Schnitzer, "Two-dimensional Yang–Mills theories are string theories", available as hep-th/9305097.

Apparently this builds on work by Gross, Taylor, and Minahan which treated $\mathrm{SU}(n)$ Yang–Mills theories in 2 dimensions as string theories, and does something similar for the gauge groups $\mathrm{SO}(n)$ and $\mathrm{Sp}(n)$.

I have a pack of interesting papers to describe but I am already worn out, so I will put that off until next week, except for the following paper by Smolin that I seem to have neglected:

3) Lee Smolin, "What can we learn from the study of non-perturbative quantum general relativity?',' available as gr-qc/9211019.

This is a nice introduction to current issues associated to the loop representation of quantum gravity and nonperturbative quantum gravity in general. As should be evident from my weekly reviews, the subject seems to be moving faster and faster, and it is best to read some of the review papers like this one by Smolin if one wants to figure out where things are now and where they might be heading.

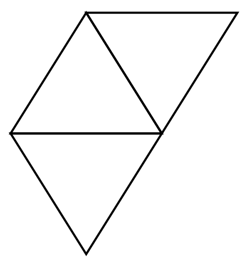
Week 16

May 30, 1993

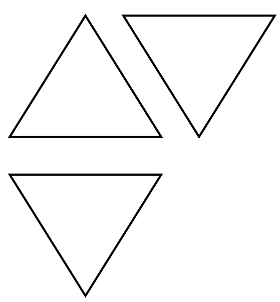
A nice crop of papers has built up while I've been taking a break... In "Week 15" I talked a bit about constructing topological quantum field theories starting with a triangulation of spacetime, and how this seems to sneak around the old "is spacetime continuous or discrete" argument. Let me describe a bit about one of the more mathematically elegant physics papers I've run across in a while, which treats exactly this issue. Then I'll describe two review articles, one on gravity in 2+1 dimensions (which is closely related to the lattice business), and one on Lagrangians for quantum gravity.

1) Stephen-wei Chung, Masafumi Fukuma and Alfred Shapere, "Structure of topological lattice field theories in three dimensions", available as `hep-th/9305080`.

What's a 2-dimensional "topological lattice field theory"? According to the definition used in this paper, it goes like this. First take a compact oriented 2-manifold without boundary M, that is, an n-holed torus. (One could also discuss the case when there is a boundary, but to keep life simple we won't here.) We want to calculate a number $Z(M)$, the partition function of M, since the partition function is a basic ingredient in Feynman's approach to quantum field theory. We first triangulate M... so a patch might look like:



Then "disassemble" M into separate triangles, like this:

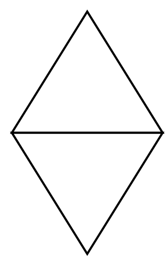


Now assign to each edge of the disassembled version of M a "color" taken from a fixed finite set S. Note that there are twice as many edges in the disassembled version of M as in the original triangulation of M. Any way of assigning a color to each edge of the disassembled M will be called a "coloring". We think of a coloring as a "history of the world" and we will compute $Z(M)$ by summing a certain quantity over all colorings.

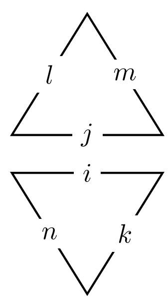
To compute this quantity, we need two pieces of data that determine our theory. First, for each i, j in S, we fix a complex number g^{ij}. We require that the matrix g^{ij} be invertible. We define g_{ij} to be the matrix inverse of g^{ij}. We can raise and lower indices with g as if it were a metric. The matrix g will be used when we glue two edges of the disassembled M together in the process of rebuilding M. Second, for each i, j, k in S, we fix a number c_{ijk}. This number comes in because each triangle has three edges.

Here's how we calculate $Z(M)$. Write down one index next to each edge of the disassembled M — by "index" I mean something like i, j, k running over S. Then write down the obvious factor of g for each pair of edges that get glued together when we form M, and write down the obvious factor of c for each triangle in M. Finally, sum over all colorings to get $Z(M)$.

For example, if M were a torus that we triangulated with two triangles like this



— with opposite edges of the parallelogram identified — we would dissasemble M and label the edges like this, say:

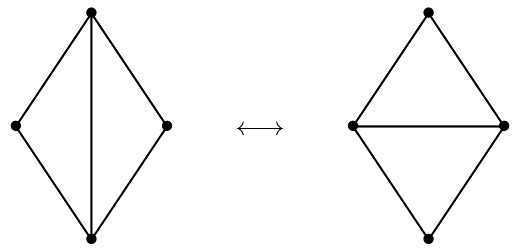


To form M we glue i to j, k to l, and m to n. So we write down

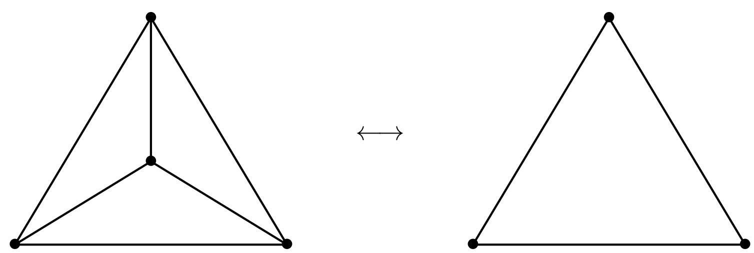
$$g^{ij} g^{kl} g^{mn} c_{jml} c_{ink}$$

and then sum over i, j, k, l, m, n to get $Z(M)$. Notice that for this procedure to be well defined it had better not matter whether we write g^{ij} or g^{ji}, since we have no way of knowing which to use. So g had better be symmetric. Similarly, we had better have $c_{ijk} = c_{jki}$ — invariance under cyclic permutations. Note that since M is oriented we can (and will) require that we go around each triangle counterclockwise when writing down things like c_{ink}, as we have done above.

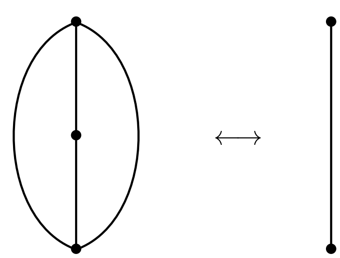
Okay, this is a pretty scheme, but the real point is that it should be independent of the triangulation of M we chose, for us to have something that deserves to be called "topological." This imposes extra conditions on g and c. Here it is handy to know that we can get between any two triangulations of M using a sequence of two moves and their inverses. The first move is called the "(2,2) move." It looks like this:



It is called the (2,2) move since it really amounts to taking 2 faces of a tetrahedron and replacing them with the other 2 faces! There is a similar (3,1) move that takes 3 faces of a tetrahedron and replaces them with the other 1, as follows:



(This drawing done by my friend Bruce Smith in a fit of insomnia!) These are examples of the "Pachner moves," and the same idea works in any dimension. But in 2 dimensions we can use a move called the "bubble move" instead of the (3,1) move. Here is where drawing vertices as •'s is crucial:

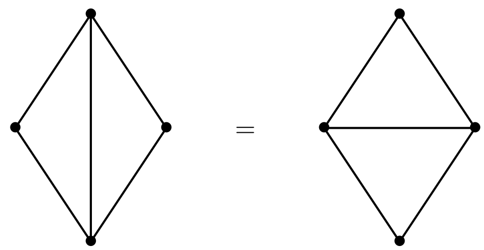


On the left, we have two hideously deformed triangles (remember, this is topology!) that are attached along TWO edges, leaving two edges exposed, and in the right we have collapsed them down to a single edge. We leave it as a fun exercise to show that you can do anything with the (2,2) move and the bubble move that you can do with the (2,2) move and the (3,1) move.

Requiring that $Z(M)$ be invariant under the (2,2) moves amounts to the following equation — if you check it, you will make sure you understand what's going on:

$$c^u_{xy} c^w_{uz} = c^w_{xu} c^u_{yz}.$$

Here I have raised indices using the "metric" g. This equation looks sort of hairy, but it's actually something very nice in disguise. We need to tease out its inner essence! Suppose we take a vector space A having the colors in S as a basis, and use the tensor c_{ij}^k to define a bilinear map from $A \times A$ to A. Then the equation above says this map is an associative product! If you ponder the picture of the (2,2) move for a while, this should become obvious to you. Think of each triangle as being a gadget that you can feed vectors into from two sides and have the "product" pop out on the third side. Then the equation



really is just associativity! To understand this in a deeper way, read Kapranov and Voevodsky's paper (reviewed in "Week 4"), especially the section on the "associahedron".

Requiring that $Z(M)$ be invariant under the bubble move amounts to the following:

$$c_{xu}^v c_{yv}^u = g_{xy}$$

Here g_{xy} is the matrix inverse of g^{xy}. Again, I leave it as an exercise to show this is the right equation. It is a formula expressing the "metric" g on A in terms of the product on A! In fact, it has a beautiful algebraic interpretation: it says that the algebra A is "semisimple." A semisimple algebra is just a direct sum of matrix algebras, and in such algebras the inner product $g(a,b)$ of any two elements is just equal to $\mathrm{tr}(a^T b)$, where a^T is the transpose of a.

So we discover a charming fact: there is a one-to-one correspondence between topological lattice field theories in 2 dimensions and finite-dimensional semsimple algebras over the complex numbers!

Actually this was apparently already shown by

2) C. Bachas and P. M. S. Petropoulos, "Topological models on the lattice and a remark on string theory cloning", *Commun. Math. Phys.* **152** (1993) 191.

and

3) M. Fukuma, S. Hosono and H. Kawai, "Lattice topological field theory in two-dimensions", available as `hep-th/921254`.

The big result of the present paper is to generalize this to 3 dimensions. The authors consider a specific definition of 3d topological lattice field theories in which one chops a 3d manifold up into tetrahedra and assigns colors to edges. They claim to get a one-to-one correspondence between these and finite-dimensional Hopf algebras for which the antipode squared is the identity! If you don't know what a Hopf algebra is, let me simply say it is a very beautiful sort of thing that has both a product and a "coproduct," and they come up all the time in group theory, knot theory, and the study of quantum groups. So we are seeing that there is a profound correspondence between topology and algebra, with higher-dimensional topology giving more subtle algebra.

(In fact, I am a little worried that the authors haven't stated the theorem quite precisely enough to have it be quite true, but it's basically right — I am afraid only only gets a particular class of Hopf algebras, those which are semisimple and cosemisimple. I may be missing something.)

Let me conclude with a few very exciting open problems.

A. One could instead consider theories in which colors are only assigned to faces. This turns out not to broaden the class of examples: any Hopf algebra has a dual Hopf algebra, and one just gets the theory associated to the dual Hopf algebra this way! But if one considers theories in which colors are assigned BOTH to edges AND faces one apparently gets a larger class of 3d examples. What algebraic structure do these correspond to? B. The Turaev–Viro theory of quantum gravity — described below — is a 3d topological lattice field theory of some sort. Where does it fit into this picture? The authors ask this question but don't answer it. Also, a more difficult problem — where does Chern–Simons gauge theory fit into this picture? C. The 64,000 dollar question: how does all this generalize to 4 dimensions? What sort of algebraic structure corresponds to a 4d topological lattice field theory? It is becoming increasingly clear that 4d field theories will involve some kind of "higher algebra" that we are only beginning to understand.

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4) Steven Carlip, "Six ways to quantize (2+1)-dimensional gravity", available as gr-qc/9305020.

While we have no real way to quantize gravity in 3+1 dimensions — although lots of good ideas — we have six, count 'em, six, ways to do it in 2+1 dimensions! Sometimes this sort of thing makes one yearn to be a physicist in some other, lower-dimensional universe. However, lest one make such wish prematurely to a genie passing by, one should note that life in 2+1 dimensions is boring compared to our 3+1-dimensional world. The reason can be seen from the following count of the number of independent components of the Riemann tensor R_{ijkl} which vanishes when spacetime is flat, and the Einstein tensor, which vanishes when the vacuum Einstein equations hold:

dimension	Riemann	Einstein
1	0	0
2	1	1
3	6	6
4	20	10

What this means is that, until one gets up to dimension 4, the vacuum Einstein equations imply that spacetime is flat. That means that there are no gravitational waves in empty space; there are only global, topological effects. Typically this means that if space is compact there are only finitely many degrees of freedom. This means that 2+1 quantum gravity is really quantum mechanics, not full-fledged quantum field theory (which deals with *local* excitations — wiggles in the metric and such — and infinitely many degrees of freedom). The good news is, this means that 2+1 gravity is somewhat understandable — no nasty infinities or ill-defined integrals needed, etc.. The bad news is, it means 2+1 gravity is not too much like 3+1 gravity. But still, many of the most puzzling qualitative features of quantum gravity are present in the 2+1 case. In particular, one has a testing ground in which to look at the interlocking triad of problems that stump us in the 3+1 case: the problem of time, the problem of observables, and the inner product problem. In brief these are: what is time evolution in quantum gravity, what are the observables in quantum gravity, and what is the inner product on the space of states of quantum gravity? As you can see, we are overwhelmingly ignorant about quantum gravity! I think that work on 2+1 gravity has given us some interesting clues about these problems.

Carlip describes 6 approaches to 2+1 gravity. I'll list them and comment on them briefly below. But one point to make is that these approaches have *not* all been shown to be equivalent; on the contrary, they seem to give different answers. Part of the problem in my opinion is that we do not have enough criteria for a "good" theory of 2+1 quantum gravity. Certainly one would like to see that in the $\hbar \to 0$ limit the theory reduces to classical gravity in some sense or other (but this is a bit vague). Perhaps another thing one could hope for is that the theory be a 2+1-dimensional TQFT. I am not sure which of the approaches below give a TQFT (although #6 definitely does and probably so does #2):

#1 Reduced ADM phase space quantization The "ADM" or Arnowitt–Deser–Misner formalism amounts to what people would typically call canonical quantization: one writes down a description of the phase space of quantum gravity in terms of initial data, figures out the Poisson brackets of functions on this phase space, and then tries to quantize by turning them into commutators. In gravity the roles of "position" and "momentum" variables are played by the metric on space at a given time, and the extrinsic curvature (or more precisely, something cooked up from it).

#2 Chern–Simons theory/Connection representation This is essentially the 2+1 analog of Ashtekar's approach in 3+1 dimensions, in that a connection and triad field play the main role, rather than the metric. However, in 2+1 dimensions we can lump the triad field and the connection together to get an "$I\mathrm{SO}(2,1)$ connection" — where $I\mathrm{SO}(2,1)$ is mildly terrifying notation for the Poinaré group in 2+1 dimensions (or "inhomogeneous Lorentz group," hence the "I"). The action for the theory then becomes the Chern–Simons action, as noted by Witten.

#3 Covariant canonical quantization This might sound oxymoronic to some, but what it means is that the phase space of solutions is describe in a manifestly covariant way, rather than in terms of initial data, and then one tries to turn Poisson brackets into commutators.

#4 Loop representation The loop representation of quantum gravity starts with the connection representation and then takes traces of holonomies around loops — so-called Wilson loops — as the basic variables to quantize. This suffers irritating technical problems in 2+1 dimensions, as noted in the following recent paper:

5) Donald Marolf, An illustration of 2+1 gravity loop transform troubles, available as gr-qc/9305015.

I know that Ashtekar and Loll are attacking these problems this right now; Loll discussed this a bit in a lecture she gave in my seminar.

#5 The Wheeler–DeWitt equation Here we proceed as in approach #1 but attempt to impose the Hamiltonian and diffeomorphism constraints after quantizing. That is, we start with an overly large phase space of initial data for general relativity — overly large because a given solution of Einstein's equations will have many different initial data on different spacelike slices — quantize by turning Poisson brackets into commutators, and *then* try to take care of the mistake we made by defining the "physical" states to be those annihilated by certain operators, the Hamiltonian and diffeomorphism constraints. I gave a brief intro to this in "Week 11". This is the most traditional approach in 3+1 gravity.

#6 Lattice approaches These are closely related to the topological lattice field theories described above. Here we treat spacetime as discrete, that is, as a kind of lattice. One approach here is due to Regge and Ponzano, and recently worked out rigorously by Turaev and Viro. To get going in this theory, you "triangulate" your 3-dimensional spacetime, that is, chop it into tetrahedra. All we need to work with is this "simplicial complex" consisting of tetrahedra, their triangular faces, their line-segment edges, and the vertex points. We assume for simplicity that spacetime is compact, so we can use finitely many tetrahedra. Thus everything in sight is finite and discrete. A "history of the world" in this theory amounts to labelling each edge with a length, or "spin", that must be $0, \frac{1}{2}, 1, \frac{3}{2}, \ldots$ or $j/2$. There are thus finitely many possible histories. To do calculations in this theory, we follow Feynman's procedure and "sum over histories" — write down a formula for the quantity we are interested in, and add up its value for all histories, weighted by a quantity depending on the history, the exponential of the action of that history, to obtain the vacuum expectation value of the quantity. The formula for the action is very familiar to folks knowledgeable about quantum theory. Each tetrahedron has 6 edges labelled by spins, and we calculate a quantity called the "$6j$ symbol" from these spins and then add it up for all tetrahedra. In the Turaev–Viro version, we have replaced the gauge group $\mathrm{SU}(2)$ by the corresponding quantum group, with the quantum parameter q a root of unity, so there are only finitely many irreducible representations, or spins, to sum over. (See "Week 5" for the vaguest of introductions to quantum groups and their representations!) The beauty of this theory is that the answer one gets is independent of the triangulation one has chosen.

While I'm at it, let me list some key references to the subject of lattice 2+1 gravity, a subject I'm fascinated by these days.

The grandaddy of them all, the Ponzano–Regge paper, is:

6) G. Ponzano and T. Regge, "Semiclassical limits of Racach coefficients", in F. Bloch (ed.), *Spectroscopic and Group Theoretical Methods in Physics*, Amsterdam: North-Holland 1968.

Then there are:

7) Edward Witten, "(2+1)-dimensional gravity as an exactly soluble system", *Nucl. Phys.* **B311** (1988), 46–78.

8) V. G. Turaev and O. Y. Viro, "State sum invariants of 3-manifolds and quantum 6j-symbols", *Topology* **31** (1992), 865–902.

Also Ooguri wrote a paper on 3+1 lattice gravity that has been quite influential:

9) Hirosi Ooguri, "Topological lattice models in four dimensions", *Mod. Phys. Lett.* **A7** (1992), 2799–2810. Available as hep-th/9205090.

And there is also the recent paper by Rovelli, which I discussed in "Week 14". This is very readable (once you know what's going on!) and conceptual.

10) Peter Peldan, "Actions for gravity, with generalizations: a review", 61 pages, available as gr-qc/9305011.

The classic action principle for general relativity is the Einstein–Hilbert action: the Ricci scalar times the volume form associated to the metric. An important modification, often called the Palatini action, takes a connection and tetrad (aka vierbein or frame field) as basic. More recently, Plebanski invented an action using the self-dual part of the connection and a tetrad field; this turns out to be closely related to an action naturally associated with the Ashtekar "new variables" (a self-dual connection and tetrad field), although this was realized only subsequently by Capovilla, Dell, and Jacobson. More recently still, there is the Capovilla-Dell-Jacobson action. These new action principles shed a very interesting new light on gravity, particularly when it comes to quantizing it. Of course it must be remembered that actions that give the *same* classical dynamics can (and typically DO) give *different* quantum theories. So a traditionalist might question whether these new actions give the "right" quantum theory of gravity. Of course, the correct response to such a traditionalist is "well, you come up with the 'right' quantum theory of gravity and then we can compare!" The point is that the good old Einstein–Hilbert action is extremely intractable when it comes to quantization — so perhaps it is not the "right" one, and *any* quantization is more enlightening than none at this stage.

Peldan presents a grand tour of the various Lagrangian formulations of gravity, and on page 3 of this large manuscript there is a large diagram of the main Lagrangian and Hamiltonian approaches to gravity in 3+1 dimensions, while on page 35 there is a somewhat smaller chart for 2+1 gravity. (A very brief preliminary warmup on some of these formulations appears in my earlier article, "Week 7".) I plan on going through this carefully in order to be able to make up for years of neglect on my part of this sort of thing.

Week 17

June 13, 1993

This'll be the last "This Week's Finds" for a few weeks, as I am going up to disappear until July. I've gotten some requests for introductory material on gauge theory, knot theory, general relativity, TQFTs and such recently, so I just made a list of some of my favorite books on this kind of thing — with an emphasis on the readable ones.

Also, just as a little plug here, a graduate student here at UCR (Javier Muniain) and I are turning my course notes from this year into a book called "Gauge Fields, Knots and Gravity," meant to be an elementary introduction to these subjects. This will eventually be published by World Scientific if all goes well. It will gently remind the reader about manifolds, differential forms, Lagrangians, etc., develop a little gauge theory, knot theory, and general relativity, and at the very end it'll get to the relationship between knot theory and quantum gravity — at which point one could read more serious stuff on the subject.

A while back Lee Rudolph asked my opinion of the following article:

1) Arthur Jaffe and Frank Quinn, "Theoretical mathematics: toward a cultural synthesis of mathematics and theoretical physics", *Bull. Amer. Math. Soc.* **29** (1993), 1–13. Available as `arXiv:math/9307227`.

People who are seriously into mathematical physics will know that with string theory the interaction between mathematicians and physicists, especially mathematicians who haven't traditionally been close to physics (e.g. algebraic geometers), has strengthened steadily for the last 10 years or so. Physicists are coming up with lots of exciting mathematical "results" — often *not* rigorously proved! — and mathematicians are getting very interested. Let me quote the abstract:

> *Is speculative mathematics dangerous? Recent interactions between physics and mathematics pose the question with some force: traditional mathematical norms discourage speculation; but it is the fabric of theoretical physics. In practice there can be benefits, but there can also be unpleasant and destructive consequences. Serious caution is required, and the issue should be considered before, rather than after obvious damage occurs. With the hazards carefully in mind, we propose a framework that should allow a healthy and a positive role for speculation.*

Replies have been solicited, so there may be a debate on this timely subject in the AMS Bulletin. This subject has a great potential for flame wars — or, as Greeks and academics refer to them, "polemics." Luckily Jaffe and Quinn take a rather careful and balanced tone. I think anyone interested in the culture of mathematics and physics should take a look at this.

Now for two books:

2) DeWitt L. Sumner, *New Scientific Applications of Geometry and Topology*, *Proc. Symp. Appl. Math.* **45**, AMS.

This volume has a variety of introductory papers on applications of knot theory; the titles are roughly "Evolution of DNA topology," "Geometry and topology of DNA and DNA-protein interactions," "Knot theory and DNA," "Topology of polymers," "Knots and Chemistry," and "Knots and physics."

3) Louis Kauffman and Sostenes Lins, *Temperley–Lieb Recoupling Theory and Invariants of 3-Manifolds*, Princeton: Princeton U. Press, 1994.

This is an elegant exposition of the 3-manifold invariants obtained from the quantum group $SU_q(2)$ — or in other words, from Chern–Simons theory. In part this is a polishing of existing work, but it also contains some interesting new ideas.

And now for some papers:

4) M. Carfora, M. Martellini and A. Marzuoli, "$12j$-symbols and four-dimensional quantum gravity", Dipartimento di Fisica, Universita di Roma "La Sapienza" preprint.

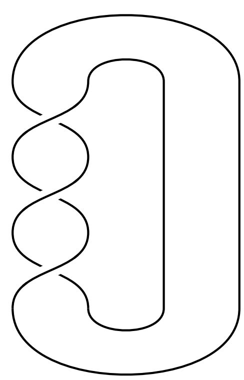
This is an attempt to do for 4d quantum gravity what Regge, Ponzano and company so nicely did for 3d quantum gravity (see "Week 16") — describe it using triangulated manifolds and angular momentum theory.

5) Y. S. Soibelman, "Selected topics in quantum groups", Lectures for the European School of Group Theory, Harvard University preprint.

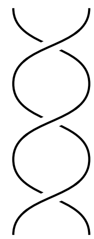
This is a nice review of Soibelman's work on quantum groups, quantum spheres, and other aspects of "quantum geometry."

6) J. Scott Carter and Masahico Saito , "Braids and movies", *Journal of Knot Theory and Its Ramifications* **5** (1996), 589–608.

Just as every knot or link is given as the closure of a braid — for example, the trefoil knot



is the closure of



— every "2-knot" or "2-link" — that is, a surface embedded in $\mathbb{R}^4$, is the closure of a "2-braid". Just as there are "Markov moves" that say when two links come from the same braid, there are moves for 2-braids — as discussed here.

7) Danny Birmingham and Mark Rakowski, "Combinatorial invariants from four dimensional lattice models: II", available as hep-th/9305022.

The previous paper obtains some invariants of 4-manifolds by triangulating them and doing a kind of "state sum" much like those I described in "Week 16". This paper shows those invariants are trivial — at least for compact manifolds, where one just gets the answer "1". This seems to be happening a lot lately.

8) Boguslaw Broda, "A note on the four-dimensional Kirby calculus", available as hep-th/9305101.

An earlier attempt by Broda to construct 4-manifold invariants along similar lines was discussed here in "Week 9" and "Week 10" — the upshot being that the invariant was trivial. This is a new attempt and Broda has told me that the arguments for the earlier invariant being trivial do not apply. Here's hoping!

9) H.-J. Matschull, "Solutions to the Wheeler DeWitt constraint of canonical gravity coupled to scalar matter fields", available as gr-qc/9305025.

One very important technical issue in the loop representation of quantum gravity is how to introduce matter fields into the picture. Let me quote:

> *It is shown that the Wheeler DeWitt constraint of canonical gravity coupled to Klein Gordon scalar fields and expressed in terms of Ashtekar's variables admits formal solutions which are parametrized by loops in the three dimensional hypersurface and which are extensions of the well known Wilson loop solutions found by Jacobson, Rovelli and Smolin.*

10) Luis J. Garay, "Hilbert space of wormholes", available as gr-qc/9306002.

I think I'll just quote the abstract on this one:

> *Wormhole boundary conditions for the Wheeler–DeWitt equation can be derived from the path integral formulation. It is proposed that the wormhole wave function must be square integrable in the maximal analytic extension of minisuperspace. Quantum wormholes can be invested with a Hilbert space structure, the inner product being naturally induced by the minisuperspace metric,*

> *in which the Wheeler–DeWitt operator is essentially self–adjoint. This provides us with a kind of probabilistic interpretation. In particular, giant wormholes will give extremely small contributions to any wormhole state. We also study the whole spectrum of the Wheeler–DeWitt operator and its role in the calculation of Green's functions and effective low energy interactions.*

11) Vipul Periwal, "Chern–Simons theory as topological closed string", available as hep-th/9305115.

Lately people have been getting interested in gauge theories that can be interpreted as closed string field theories. I mentioned one recent paper along these lines in "Week 15", which considers Yang–Mills in 2 dimensions. (This was not the first paper to do so, I should emphasize.) A while back Witten wrote a paper on Chern–Simons gauge theory in 3 dimensions as a background-free open string field theory, but I was unable to understand it. This paper seems conceptually simpler, although it uses some serious mathematics. I think I might be able to understand it. It starts:

> *The perturbative expansion of any quantum field theory (qft) with fields transforming in the adjoint representation of* $\mathrm{SU}(N)$ *is a topological expansion in surfaces, with* N^{-2} *playing the role of a handle-counting parameter. For* N *large, one hopes that the dynamics of the qft is approximated by the sum (albeit largely intractable) of all planar diagrams. The topological classification of diagrams has nothing a priori to do with approximating the dynamics with a theory of strings evolving in spacetime.*
>
> *Gross (see also refs. . .) has shown recently that the large* N *expansion does actually provide a way of associating a theory of strings in QCD. Maps of two-dimensional string worldsheets into two-dimensional spacetimes are necessarily somewhat constricted. What one would like is a qft with fields transforming in the adjoint representation in* $d > 2$*, which is at the same time exactly solvable. One could then, in principle, attempt to associate a theory of strings with such a qft by exhibiting a 'sum over connected surfaces' interpretation for the free energy of the qft. There is no guaranty that such an association will exist.*

The author argues that Chern–Simons theory is a "rara avis among QFTs" for which such an association exists. He takes the free energy for $\mathrm{SU}(N)$ Chern–Simons theory on S^3, does a large-N expansion on it, and shows that the coefficient of the N^{2-2g} term is the (virtual) Euler characteristics of the moduli space of surfaces with g handles. I wish I understood this better at a very pedestrian level! E.g., is there some string-theoretic reason why one might expect the free energy to be of this form? Anyway, then he considers T^3, and gets something related to surfaces with a single puncture in them.

Week 18

September 11, 1993

I will be resuming this series of articles this fall, though perhaps not at a rate of one "Week" per week, as I'll be pretty busy. For those of you who haven't seen this series before, let me explain. It's meant to be a guide to some papers, mostly in preprint form, that I have found interesting. I should emphasize that it's an utterly personal and biased selection — if more people did this sort of thing, we might get a fairer sample, but I'll be unashamed in focussing on my own obsessions, which these days lean towards quantum gravity, topological quantum field theories, knot theory, and the like.

Quite a pile of papers has built up over the summer, but I will start by describing what I did over my summer vacation:

1) John Baez, "Strings, loops, knots, and gauge fields", available as hep-th/9309067.

When I tell layfolk that I'm working on the loop representation of quantum gravity, and try to describe its relation to knot theory, I usually say that in this approach one thinks of space, not as a smoothly curved manifold (well, I try not to say "manifold"), but as a bunch of knots linked up with each other. If thy have been exposed to physics popularizations they will usually ask me at this point if I'm talking about superstring theory. To which I used to respond, somewhat annoyed, that no, it was quite different. Superstring theory, I explained, is a grandiose "theory of everything" that tries to describe all known forces and particles, and lots more, too, as being vibrating loops of string hurling around in 349-dimensional space. (Well, maybe just 10, or 26.) It is a complicated mishmash of all previous failed approaches to unifying gravity with the other forces: Yang–Mills theory, Kaluza-Klein models, strings, and supersymmetry. (The last is a symmetry principle that postulates for every particle another one, a mysterious "superpartner," despite the fact that no such superpartners have been seen.) And it has made no testable predictions as of yet. The loop representation of quantum gravity, on the other hand, is a much more conservative project. It simply attempts to use some new mathematics to reconcile two theories which both seem true, but up to now have been as immiscible as oil and water: quantum field theory, and general relativity. If it works, it will still be only the first step towards unifying gravity with the other forces. If the questioner has the gall to ask if *it* has made any testable predictions, I say that so far it is essentially a mathematics project. On the one hand, here are Einstein's equations; on the other hand, here are the rules of thumb for "quantizing" some equations. Is there a consistent and elegant way of applying those rules to those equations? People have tried for 40 years or so without real success, but quite possibly they just weren't being clever enough, since the rules of thumb leave a lot of scope for creativity. Then a physicist named Ashtekar came along and reformulated Einstein's equations using some new variables (usually known by experts as the "new variables"). This made the equations look much more like those that describe the other forces in physics. This led to renewed hope that Einstein's equations might be consistently quantized after all. Then physicists named Rovelli and Smolin , working with Ashtekar, made yet *another* change of variables, based on the new variables. Rovelli and Smolin's variables were labelled by loops in space, so they are called the loop variables. These loops are quite unlike strings, since they are merely mathematical artifacts for playing with Einstein's equations, not actual little *objects* whizzing about. But using them, Rovelli and Smolin were able to quantize Einstein's equations and actually find a lot of solutions! However, they were making up a lot of new mathematics as they went along, and, as usual in theoretical physics, it wasn't 100% rigorous (which, as we know, is like the the woman who could trace her descent from William the Conqueror with only two gaps). So I, as a mathematician, got interested in this and am trying to help out and see how much of this apparently wonderful development is for real. . . .

The odd thing is that there are a lot of mathematical connections between string theory and the loop representation. Gradually, as time went on, I became more and more convinced that maybe the layfolk were right — maybe the loop representation of quantum gravity really WAS string theory in disguise, or vice versa. This made a little embarrassed by how much I had been making fun of string theory. Still, it could be a very good thing. On the one hand, the loop representation of quantum gravity is much more well-motivated from basic physical principles than string theory — it's not as baroque — a point I still adhere to. So maybe one could use it to understand string theory a lot more clearly. On the other hand, string theory really attempts to explain, not just gravity, but a whole lot more — so maybe it might help people see what the loop representation of quantum gravity has to do with the other forces and particles (if in fact it actually works).

I decided to write a paper about this, and as I did some research I was intrigued to find more and more connections between the two approaches, to the point where it is clear that while they are presently very distinct, they come from the same root, historically speaking.

Here's what I wound up saying:

> *The notion of a deep relationship between string theories and gauge theories is far from new. String theory first arose as a model of hadron interactions. Unfortunately this theory had a number of undesirable features; in particular, it predicted massless spin-2 particles. It was soon supplanted by quantum chromodynamics (QCD), which models the strong force by an* $\mathrm{SU}(3)$ *Yang–Mills field. However, string models continued to be popular as an approximation of the confining phase of QCD. Two quarks in a meson, for example, can be thought of as connected by a string-like flux tube in which the gauge field is concentrated, while an excitation of the gauge field alone can be thought of as a looped flux tube. This is essentially a modern reincarnation of Faraday's notion of "field lines," but it can be formalized using the notion of Wilson loops. If A denotes a classical gauge field, or connection, a Wilson loop is simply the trace of the holonomy of A around a loop in space. If instead A denotes a quantized gauge field, the Wilson loop may be reinterpreted as an operator on the Hilbert space of states, and applying this operator to the vacuum state one obtains a state in which the Yang–Mills analog of the electric field flows around the loop.*
>
> *In the late 1970's, Makeenko and Migdal, Nambu, Polyakov, and others attempted to derive equations of string dynamics as an approximation to the Yang–Mills equation, using Wilson loops. More recently, D. Gross and others have been able to* exactly *reformulate Yang–Mills theory in* 2*-dimensional spacetime as a string theory by writing an asymptotic series for the vacuum expectation values of Wilson loops as a sum over maps from surfaces (the string*

> *worldsheet) to spacetime. This development raises the hope that other gauge theories might also be isomorphic to string theories. For example, recent work by Witten and Periwal suggests that Chern–Simons theory in 3 dimensions is also equivalent to a string theory.*
>
> *String theory eventually became popular as a theory of everything because the massless spin-2 particles it predicted could be interpreted as the gravitons one obtains by quantizing the spacetime metric perturbatively about a fixed "background" metric. Since string theory appears to avoid the renormalization problems in perturbative quantum gravity, it is a strong candidate for a theory unifying gravity with the other forces. However, while classical general relativity is an elegant geometrical theory relying on no background structure for its formulation, it has proved difficult to describe string theory along these lines. Typically one begins with a fixed background structure and writes down a string field theory in terms of this; only afterwards can one investigate its background independence. The clarity of a manifestly background-free approach to string theory would be highly desirable.*
>
> *On the other hand, attempts to formulate Yang–Mills theory in terms of Wilson loops eventually led to a full-fledged "loop representation" of gauge theories, thanks to the work of Gambini, Trias, and others. After Ashtekar formulated quantum gravity as a sort of gauge theory using the "new variables," Rovelli and Smolin were able to use the loop representation to study quantum gravity nonperturbatively in a manifestly background-free formalism. While superficially quite different from modern string theory, this approach to quantum gravity has many points of similarity, thanks to its common origin. In particular, it uses the device of Wilson loops to construct a space of states consisting of "multiloop invariants," which assign an amplitude to any collection of loops in space. The resemblance of these states to wavefunctions of a string field theory is striking. It is natural, therefore, to ask whether the loop representation of quantum gravity might be a string theory in disguise — or vice versa.*
>
> *The present paper does not attempt a definitive answer to this question. Rather, we begin by describing a general framework relating gauge theories and string theories, and then consider a variety of examples. Our treatment of examples is also meant to serve as a review of Yang–Mills theory in 2 dimensions and quantum gravity in 3 and 4 dimensions.*

I should add that the sort of string theory I talk about in this paper is fairly crude compared to that which afficionados of the subject usually concern themselves with. It treats strings only as maps from a surface (the string worldsheet) into spacetime, and only cares about such maps up to diffeomorphism, i.e., smooth change of coordinates. In most modern string theory the string worldsheet is equipped with more geometrical structure (a conformal structure) — it looks locally like the complex plane, so one can talk about holomorphic functions on it and the like. This is why string theorists are always muttering about conformal field theory. But the sort of string theory that Gross and others (Taylor, Minahan, and Polychronakos, particularly) have been using to describe 2d Yang–Mills theory does not require a conformal structure on the string worldsheet, so it's at least *possible* that more interesting theories like 4d quantum gravity can be formulated as string theories without reference to conformal structures. (Of course, if one integrates over all conformal structures, that's a way of referring to conformal structures without actually picking one.) I guess I'm rambling on here a bit, but this is really the most mysterious point as far as I'm concerned.

One hint of what might be going on is as follows. And here, I'm afraid, I will be quite technical. As noted by Witten and formalized by Moore, Seiberg, and Crane, a rational conformal field theory gives rise to a particularly beautiful sort of category called a modular tensor category. This contains, as it were, the barest essence of the theory. Any modular tensor category gives rise in turn to a 3d topological quantum field theory — examples of which are Chern–Simons theory and quantum gravity in 3 dimensions. And Crane and Frenkel have shown (or perhaps it's fairer to say that if they ever finish their paper they *will* have shown) that the nicest modular tensor categories give rise to braided tensor 2-categories, which should, if there be justice, give 4d topological quantum field theories. (For more information on all these wonderful things — which no doubt seem utterly intimidating to the uninitiated — check out previous "This Week's Finds.") Quantum gravity in 4 dimensions is presumably something roughly of this sort, if it exists. So what I'm hinting at, in brief, is that a bunch of category theory may provide the links between *modern* string theory with its conformal fields and the loop representation of quantum gravity. This is not as outre as it may appear. The categories being discussed here are really just ways of talking about *symmetries* (see my stuff on categories and symmetries for more on this). As usual in physics, the clearest way to grasp the connection between two seemingly disparate problems is often by recognizing that they have the same symmetries.

Week 19

September 27, 1993

I will now start catching up on some of the papers that have accumulated over the summer. This time I'll say a bit about recent developments in quantum field theory and 4-dimensional topology.

The quantum field theories that describe three of the forces of nature (electromagnetic, strong and weak) depend for their formulation on a fixed metric on spacetime — that is, a way of measuring distance and time. Indeed, it seems pretty close to being true that spacetime is $\mathbb{R}^4$, and that the "interval" between any two points in 4-dimensional space is given by the Minkowski metric

$$dt^2 - dx^2 - dy^2 - dz^2$$

where dt is the change in the time, or t, coordinate, dx is the change in the spatial x coordinate, and so on. However, it's apparently not quite true. In fact, the presence of matter or energy distorts this metric a little, and the effect of the resulting "curvature of spacetime" is perceived as gravity. This is the basic idea of general relativity, which is nicely illustrated by the way in which the presence of the sun bends starlight that passes nearby.

Gravity is thus quite different from the other forces, at least to our limited understanding. The other forces we have quantum theories of, and these theories depend on a *fixed* (that is, pre-given) metric. We have no quantum theory of gravity yet, only a classical theory, and this theory is precisely a set of equations describing a *variable* metric, that is, one dependent upon the state of the universe. These are, of course, Einstein's equations.

In fact it is no coincidence that we have no quantum theory of gravity. For most of the last 50 years or so physicists have been working very hard at inventing and understanding quantum field theories that rely for their formulation on a fixed metric. Indeed, physicists spent huge amounts of effort trying to make a theory of quantum gravity along essentially these lines! This is what one calls "perturbative" quantum gravity. Here one says, "Well, we know the metric isn't quite the Minkowski metric, but it's awfully close, so we'll write it as the Minksowski metric plus a small perturbation, derive equations for this perturbation from Einstein's equations, and make a quantum field theory based on *those* equations." That way we could use the good old Minkowski metric as a "background metric" and thus use all the methods that work for other quantum field theories. This was awfully fishy from the standpoint of *elegance*, but if it had worked it might have been a very good thing, and indeed we learned a lot from its failure to work. Mainly, though, we learned that we need to bite the bullet and figure out how to do quantum field theory without any background metric.

A recent big step was made when people (in particular Witten and Atiyah) formulated the notion of a "topological quantum field theory." This is a precise list of properties one would like a quantum field theory independent of any background metric to satisfy. A wish list, as it were. One of the best-understood examples of such a "TQFT" is Chern–Simons theory. This is a quantum field theory that makes sense in 3-dimensional spacetime, not 4d spacetime, so in a sense it has no shot at being "true." However, it connects up to honest 4d physics in some very interesting ways, it serves as warmup for more serious physics yet to come, AND it has done wonders for the study of topology.

It is also worth noting that one particular case of Chern–Simons theory is equivalent to quantum gravity in 3d spacetime. Here I am being a bit sloppy; there are various ways of doing quantum gravity in 3 dimensions and they are not all equivalent, but the approach that relates to Chern–Simons theory is, in my opinion, the nicest. This approach to 3d quantum gravity was the advantage that it can also be described using a "triangulation" of spacetime. In other words, if we prefer the discrete to the continuum, we can "triangulate" it, or cut it up into tetrahedra, and formulate the theory solely in terms of this triangulation. Of course, it's pretty common in numerical simulations to approximate spacetime by a lattice or grid like this. What's amazing here is that one gets *exact* answers that are *independent* of the triangulation one picks. The idea for doing this goes back to Ponzano and Regge, but it was all done quite rigorously for 3d quantum gravity by Turaev and Viro just a few years ago. In particular, they were able to show the 3d quantum gravity is a TQFT using only triangulations, no "continuum" stuff.

It is tempting to try to do something like this for 4 dimensions. But it is unlikely to be so simple. A number of people have recently tried to construct 4d TQFTs copying tricks that worked in 3d. Some papers along these lines that I have mentioned before are:

> Louis Crane and David Yetter, "A categorical construction of 4d topological quantum field theories", available as hep-th/9301062. (Week 2)

> Boguslaw Broda, "Surgical invariants of four-manifolds", available as hep-th/9302092. (Week 9 and Week 10)

(I have listed which "Week" I discussed these in case anyone wants to go back and check out some of the details.)

These papers ran into stiff opposition as soon as they came out! First Ocneanu claimed that the Crane–Yetter construction was trivial, in the sense that the number it associated to any compact 4-dimensional spacetime manifold was 1. (This number is called the partition function of the quantum field theory, and having it be 1 for all spacetimes means the theory is deadly dull.)

> Adrian Ocneanu, "A note on simplicial dimension shifting", available as hep-th/9302028. ("Week 5")

Crane and Yetter wrote a rebuttal noting that Ocneanu was not dealing with quite the same theory:

> David Yetter and Louis Crane, "We are not stuck with gluing", available as hep-th/9302118. (Week 7)

They also presented, at their conference this spring, calculations showing that their partition function was not equal to 1 for certain examples.

In my discussions of Broda's work I extensively quoted some correspondence with Dan Ruberman, who showed that in Broda's original construction, the partition function of a 4-dimensional manifold was just a function of its signature and possibly some Betti numbers — these being well-known invariants, it's not especially exciting from the point of view of topology. This was also shown by Justin Roberts:

Justin Roberts, "Skein theory and Turaev–Viro invariants", *Topology*, **34**, 771–787. Available as https://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.138.8587. (Week 14)

He suggested that the Crane–Yetter partition function was also a function of the signature and Betti numbers, but did not check their precise normalization conventions, and so did not quite *prove* this. However, more recently Crane and Yetter, together with Kauffman, have shown this themselves:

1) Louis Crane, Louis H. Kauffman and David N. Yetter, "Evaluating the Crane–Yetter Invariant", available as `hep-th/9309063`.

Abstract: We provide an explicit formula for the invariant of 4-manifolds introduced by Crane and Yetter (in `hep-th/9301062`). A consequence of our result is the existence of a combinatorial formula for the signature of a 4-manifold in terms of local data from a triangulation. Potential physical applications of our result exist in light of the fact that the Crane–Yetter invariant is a rigorous version of ideas of Ooguri on $B \wedge F$ theory.

They also have shown that Broda's original construction, and also a souped-up construction of his, give a partition function that depends only on the signature:

2) Louis Crane, Louis H. Kauffman and David N. Yetter, "On the Classicality of Broda's $\mathrm{SU}(2)$ Invariants of 4-Manifolds", available as `hep-th/9309102`.

Abstract: Recent work of Roberts has shown that the first surgical 4-manifold invariant of Broda and (up to an unspecified normalization factor) the state-sum invariant arising from the TQFT of Crane–Yetter are equivalent to the signature of the 4-manifold. Subsequently Broda defined another surgical invariant in which the 1- and 2- handles are treated differently. We use a refinement of Roberts' techniques developed by the authors in `hep-th/9309063` to show that the "improved" surgical invariant of Broda also depends only on the signature and Euler character.

Now let me say just a little bit about what this episode might mean for physics as well as mathematics. The key is the "$B \wedge F$" theory alluded to above. This is a quantum field theory that makes sense in 4 dimensions. I have found that the nicest place to read about it is:

3) Gary Horowitz, "Exactly soluble diffeomorphism-invariant theories", *Commun. Math. Phys.* **125** (1989), 417–437.

This theory is a kind of simplified version of 4d quantum gravity that is a lot closer in *character* to Chern–Simons theory. Like Chern–Simons theory, there are no "local degrees of freedom" — every solution looks pretty much like every other one as long as we don't take a big tour of space and notice that funny things happen when we go around a noncontractible loop, which is the sort of thing that can only exist if space has a nontrivial topology. 4d quantum gravity, on the other hand, should have loads of local degrees of freedom — the local curving of spacetime!

What Crane and Yetter were dreaming of doing was constructing 4d quantum gravity as a TQFT using triangulations of spacetime. What they really did, it turns out, was to construct $B \wedge F$ theory as a TQFT using triangulations. (Broda constructed it another way.) On the one hand, the simplicity of $B \wedge F$ theory compared to honest-to-goodness 4d quantum gravity makes it possible to understand it a lot better, and calculate it out explicitly. On the other hand, $B \wedge F$ theory is so simple that it doesn't tell us much new about topology, at least not the topology of 4-dimensional manifolds per se. Via Donaldson theory and the work of Kronheimer and Mrowka it's probably telling us a lot about the topology of 2-dimensional surfaces embedded in 4-dimensional manifolds — but alas, I don't understand this stuff very well yet!

Getting our hands on 4d quantum gravity as a TQFT along these lines is still, therefore, an unfinished business. But we are, at last, able to study some examples of 4d TQFTs and ponder more deeply what it means to do quantum field theory without any background metric. The real thing missing is local degrees of freedom. Without them, any model is really just a "toy model" not much like physics as we know it. The loop representation of quantum gravity has these local degrees of freedom (to the extent that we understand the loop representation!), and so the challenge (well, one challenge!) is to better relate it to what we know about TQFTs.

Week 20

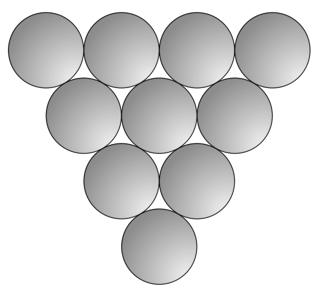
October 2, 1993

I think I'll depart from my usual concerns this week and talk about a book I'd been meaning to get my hands on for ages:

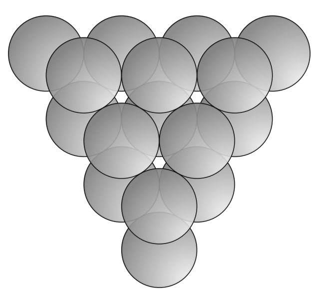
1) John H. Conway and Neil J. A. Sloane, *Sphere Packings, Lattices and Groups*, second edition, Grundlehren der mathematischen Wissenschaften **290**, Springer, Berlin, 1993.

This is a mind-boggling book. I have always regarded research in combinatorics as a pleasure I must deny myself, for the study of the *discrete* presents endless beautiful tapestries in which one could easily lose oneself for life, and I regard this as a kind of self-indulgence when there is, after all, the physical universe out there waiting to be understood. Of course, it's good that *someone* does combinatorics, since even the most obscure corners have a strong tendency to become useful eventually — and if they write books about it, I can have my cake and eat it too, by *reading* about it. Conway and Sloane are two masters of combinatorics, and this book is like a dessert tray piled so high with delicacies that it's hard to know where to begin. Rather than attempt to describe it, let me simply show you a few things I found in it. The book is 679 pages long, so what I'll say is only the most minute sample!

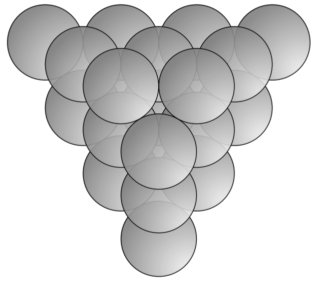
Let's begin with sphere packings. Say one is stacking cannonballs on one's lawn, which is a quaint custom now that cannons have been replaced by far more horrible weapons. A nice way to do it is to lay out a triangle of cannonballs thus:



and then set down another triangle of cannonballs on top of the first layer, one in every other hole, thus:



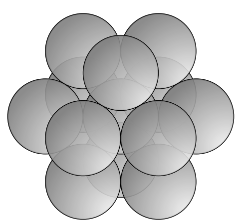
and another:



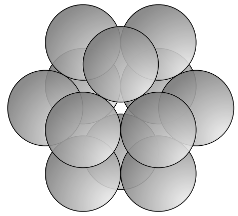
and finally one on top — which I won't attempt to draw. (Actually all the figures here were made by Simon Burton.) Very nice stack! There are many mathematical reflections it could lead one to, one of which is: is this the *densest* possible way one can pack spheres of equal radius? The density is $\pi/\sqrt{18} = .7405\ldots$; can one do better? Apparently it was Kepler who first conjectured that the answer is "no." This is a famous hard problem. In 1991 W.-Y. Hsiang announced that he had a proof, but I am not sure how many experts have read it and been convinced.

This packing is a very regular sort of packing, what people call a lattice packing. A lattice is a discrete subset of n-dimensional Euclidean space closed under addition, and the packing above corresponds to a lattice called the "face-centered cubic" or fcc lattice, which is the set of all points (x, y, z) where x, y, and z are integers adding up to an *even* number. (One might have a bit of fun drawing some of those points and seeing why they do the job.) Naturally, because of their regularity lattice packings are easier to study than non-lattice ones. In fact, Gauss showed in 1831 that the fcc lattice is the densest of all *lattice* packings of spheres in 3 dimensions. This justifies the practice of stacking grapefruit this way in the supermarket.

Let's take a bit closer look at what's going on here. Imagine an fcc lattice going off infinitely in all directions. . . each sphere is touching 12 others: 6 in its own layer, as it were, 3 in the layer above and 3 in the layer below. If we only pay attention to a given sphere and those touching it we see something like:



If we remove the central sphere to clean up the picture a bit, the centers of the remaining are the vertices of a shape called the cuboctahedron:



It has 12 vertices, 8 triangular faces, and 6 square faces. One can get it either by cutting off the corners of a cube just right, or by cutting off the corners of an octahedron!

Let's return to thinking about the 12 spheres touching the central one. This raises another question: is 12 the largest number of equal-radius spheres able to touch a given one? This is the "kissing spheres" problem — not quite the same as the packing problem! In 1694, Newton conjectured that 12 was the largest number one could achieve. His correspondent, David Gregory, thought 13 might be possible. It turns out that Newton was right (some proofs appeared in the late 1800's), but it is important to realize that Gregory's guess was not as dumb as it might sound!

Why? Well, there's another way to get 12 spheres to touch a central one. Namely, locate them at the vertices of a (regular) icosahedron. If one wants to get to know the icosahedron a bit better one might read my article Some Thoughts on the Number Six. But I hope you have an icosahedron available, or at least a good mental image of one. It's easy to describe the vertices of the icosahedron mathematically in terms of the golden ratio,

$$\frac{\sqrt{5}+1}{2} = 1.6180339887498948482045868343 7\ldots$$

This usually goes by the name of "Φ", or sometimes "τ". The magic properties of Φ are too numerous to list here, but what counts is that one gets the 12 vertices of a (regular) icosahedron by taking the points

$$\begin{gathered}(\pm\Phi, \pm 1, 0),\\ (\pm 1, 0, \pm\Phi),\\ (0, \pm\Phi, \pm 1).\end{gathered}$$

This is easy to check.

Now the *interesting* thing is that when one gets 12 spheres to touch a central one using the icosahedron, the 12 sphere don't touch each other! There's room to move 'em around a bit, and perhaps (thought Gregory) even enough room to stick in another one! Well, Gregory was wrong, but one can do something pretty cool with this wiggle room. First, though, let's check that there really *is* a little space between those outer spheres. First, compute the distance between neighboring vertices of the icosahedron by taking two and working it out:

$$\|(\Phi, 1, 0) - (\Phi, -1, 0)\| = 2$$

Then, compute the distance from any of the vertices to the origin, which is the center of the central sphere:

$$\|(\Phi, 1, 0)\| = \sqrt{\Phi^2 + 1}$$

Using the charming fact that $\Phi^2 = \Phi + 1$ this simplifies to $\sqrt{\Phi + 2}$, but then I guess we just need to grind it out:

$$\sqrt{\Phi + 2} = 1.902\ldots$$

The point is that this is less than 2, so two neighboring spheres surrounding the central one are farther from each other than from the central one, i.e., they don't touch.

Now here's an interesting question: say we labelled the 12 spheres touching the central one with numbers 1-12. Is there enough room to roll these spheres around, always touching the central one, and permute the spheres 1-12 in an interesting way?

Notice that it's trivially easy to roll them around in a way that amounts to rotating the icosahedron, but are there more interesting permutations one can get?

Recall that the group of permutations of 12 things is called S_{12}, and it has

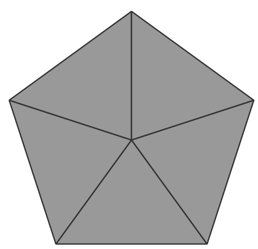
$$12! = 479001600$$

elements. The rotational symmetry group of the icosahedron is much smaller. One can count its elements as follows: pick a vertex and say which new vertex it gets rotated to — there are 12 possibilities — and then note that there are 5 ways that could have happened, for a total of 60. In fact (as I show in the article "Six") the rotational symmetry group of the icosahedron is A_5, the group of *even* permutations of 5 things. So we have a nice embedding of the group A_5 into S_{12}, but we are hoping that by some clever wiggling we can get a bigger subgroup of S_{12} as the group of permutations we can achieve by rolling spheres around.

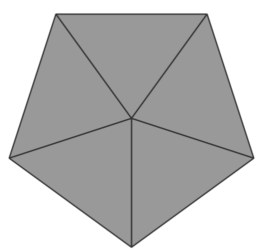
In fact, one can achieve all EVEN permutations of the 12 spheres this way! In other words, we get the group A_{12}, with

$$\frac{12!}{2} = 239500800$$

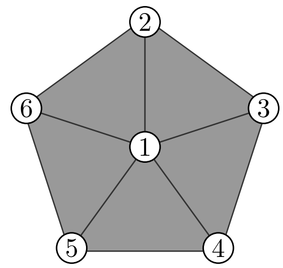
elements. How? Well, I wish I could draw an icosahedron for you, but I can't really do so well on this medium. The best way is to draw a top view:



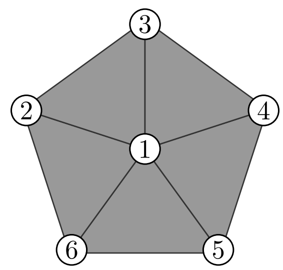
and a bottom view:



Now, if we take the top 6 spheres and bunch them up so they are all touching, and the bottom 6 spheres and bunch them up so *they* all touch, we can, in fact, twist the top 6 counterclockwise around $1/5$ of a turn. That is, we can map



to



I haven't actually *done* this, or even proved I can, but Conway and Sloane say so. And then the point is that all of A_{12} is generated by "twists" of this form. Conway and Sloane give a sophisticated and quick proof of this fact, which I can't resist mentioning. Readers who don't know much group theory can skip the next paragraph!

First, let $t(i)$ be the element of S_{12} corresponding to the clockwise twist about the ith vertex of the icosahedron (so that what's drawn above is $t(1)$). This is a 5-cycle, and we need to show these 12 5-cycles generate A_{12}. Consider the subgroup generated by elements of the form $t(i)t(j)^{-1}$ — a clockwise twist followed by a counterclockwise twist. This is the Mathieu group M_{12}, a most remarkable group! In particular, its action on the vertices of the icosahedron is able to map any 5 vertices to any 5 others (we say it's quintuply transitive), so by conjugating $t(i)$ with elements of the Mathieu group we can get *any* 5-cycle in S_{12}. Then we use the fact that A_{12} is generated by the 5-cycles.

Anyway, what this indicates is that there is an interesting relation between the icosahedron and a certain finite group, the Mathieu group M_{12}. This group has

$$12!/7! = 95040$$

elements and it is a "simple" group, in the technical sense. The simple groups are to finite groups roughly as the prime numbers are to the counting numbers; that is, they are the elementary building blocks from which other finite groups are made (although one has to specify *how* one gloms them together to get other groups). One of the remarkable achievements of this century is the classification of these simple groups. In addition to various infinite families of simple groups, like the alternating groups A_n (consisting of even permutations) there are a finite number of "sporadic" simple groups such as the Mathieu groups, the Fischer groups, the Suzuki groups, and, biggest of all, the Monster group, which has

$$246 \cdot 320 \cdot 59 \cdot 76 \cdot 112 \cdot 133 \cdot 17 \cdot 19 \cdot 23 \cdot 29 \cdot 31 \cdot 41 \cdot 47 \cdot 59 \cdot 71$$
$$= 808017424794512875886459904961710757005754368000000000$$

elements!

Here's a fun story about this number.

In the 1970s, the mathematicians Fricke, Ogg and Thompson were studying the quotient of the hyperbolic plane by various subgroups of $\mathrm{SL}(2, \mathbb{R})$ — the group of 2×2 real matrices with determinant one — which acts as isometries of the hyperbolic plane. Sitting inside $\mathrm{SL}(2, \mathbb{R})$ is the group of 2×2 integer matrices with determinant one, called $\mathrm{SL}(2, \mathbb{Z})$. Sitting inside that is the group $\Gamma_0(p)$ consisting of matrices whose lower left corner is congruent to zero $\mod p$ for the prime p. But Fricke, Ogg and Thompson were actually considering a somewhat larger group $\Gamma_0(p)+$, which is the normalizer of $\Gamma_0(p)$ inside $\mathrm{SL}(2, \mathbb{R})$.

If you don't know what this stuff means, don't worry! The point is that they asked this question: if we take the quotient of the hyperbolic plane by this group $\Gamma_0(p)+$, when does the resulting Riemann surface have genus zero? And the *real* point is that they found the answer was: precisely when p is 2, 3, 5, 7, 11, 13, 17, 19, 23, 29, 31, 41, 47, 59 or 71.

Later, Ogg went to a talk on the Monster and noticed that these primes were precisely the prime factors of the size of the Monster! He wrote a paper offering a bottle of Jack Daniels whiskey to anyone who could explain this fact. This was the beginning of a subject which Conway dubbed "Monstrous Moonshine": the mysterious relation between the Monster group, the group $\mathrm{SL}(2, \mathbb{R})$, and Riemann surfaces.

It turns out that lying behind Monstrous Moonshine is a certain string theory having the Monster as symmetries, and this was the key to understanding many strange "coincidences".

So, the sporadic groups are telling us something very deep and mysterious about the universe, since they are very complicated and yet somehow a basic, intrinsic part of the weave of mathematics. Conway and Sloane have a lot to say about them and their relations to lattices and error-correcting codes. For more about Monstrous Moonshine and string theory, the reader should try this:

2) Igor Frenkel, James Lepowsky, and Arne Meurman, *Vertex Operator Algebras and the Monster*, Academic Press, New York, 1988.

Rather than attempt to describe this work, which I am not really qualified to do (not that that usually stops me!), I think I will finish up by describing a charming connection between the beloved icosahedron and a lattice in 8 dimensions that goes by the name of E_8. I'll be a bit more technical here.

The group of rotational symmetries of the icosahedron is, as we have said, A_5. This is a subgroup of the 3d rotation group $\mathrm{SO}(3)$. As all physicists know, whether they know it or not, the group $\mathrm{SO}(3)$ has the group $\mathrm{SU}(2)$ of 2×2 unitary matrices with determinant 1 as its double cover. So we can find a corresponding double cover of A_5 as a subgroup of $\mathrm{SU}(2)$; this has twice as many elements as A_5, for a total of 120.

Now the group $\mathrm{SU}(2)$ has a nice description as the group of *unit quaternions*, that is, things of the form

$$a + bI + cJ + dK$$

where a, b, c, d are real numbers with $a^2 + b^2 + c^2 + d^2 = 1$, and I,J, and K satisfy

$$IJ = -JI = K, \quad JK = -KJ = I, \quad KI = -IK = J, \quad I^2 = J^2 = K^2 = -1$$

(Physicists in the 20th century usually use the Pauli matrices instead, which are basically the same thing; for the relationship, read "Week 5".)

It's natural to ask what the double cover of A_5 looks like explicitly in terms of the unit quaternions. Conway and Sloane give a nice description. Let's write (a, b, c, d) for $a + bI + cJ + dK$, write Φ for the golden ratio as before, and ϕ for the inverse of the golden ratio:

$$\phi = \Phi^{-1} = \Phi - 1 = 0.6180339887498948482045868343 7\ldots$$

Then the elements of the double cover of A_5 are of the form

$$(\pm 1, 0, 0, 0),$$
$$(\pm\frac{1}{2}, \pm\frac{1}{2}, \pm\frac{1}{2}, \pm\frac{1}{2}),$$
$$(0, \pm\frac{1}{2}, \pm\phi/2, \pm\Phi/2).$$

and everything else that can be gotten by *even* permutations of the coordinates. (Check that there are 120 and that they are closed under multiplication!)

Charming, but what does it have to do with E_8? Well, note that if we take all finite sums of elements of the double cover of A_5 we get a subring of the quaternions that Conway and Sloane calls the "icosians." Any icosian is of the form

$$a + bI + cJ + dK$$

where a,b,c, and d live in the "golden field" $\mathbb{Q}(\Phi)$ — this is the field of numbers of the form

$$x + \sqrt{5}y$$

where x and y are rational. Thus we can think of an icosian as an 8-tuple of rational numbers. We don't get all 8-tuples, however, but only those lying in a given lattice.

In fact, we can put a norm on the icosians as follows. First of all, there is usual quaternionic norm

$$\|a + bI + cJ + dK\|^2 = a^2 + b^2 + c^2 + d^2$$

But for an icosian this is always of the form $x + \sqrt{5}y$ for some rational x and y. It turns out we can define a new norm on the icosians by setting

$$|a + bI + cJ + dK|^2 = x + y.$$

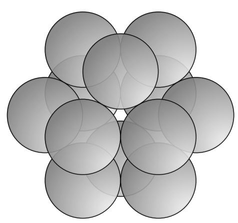
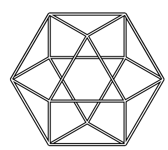
With respect to this norm, the icosians form a lattice that fits isometrically in 8-dimensional Euclidean space and is the famous one called E_8! E_8 is known to yield the densest lattice packing of spheres in 8 dimensions, a fact that is not only useful for 8-dimensional greengrocers, but also is apparently used in error-correcting codes in a number of commercially available modems! (If anyone knows *which* modems use E_8, let me know — I might just buy one!) The density with which one can pack spheres in 8 dimensions using E_8, by the way, is $\pi^4/384$, or about $.2537$.

Group theorists and some physicists, of course, will know that E_8 is also the root lattice of the largest exceptional Lie group, also known as E_8. This appears as the gauge group in some string theories. While I find those string theories a bit baroque for my taste, there is clearly a lot of marvelous mathematics floating around here, and anyone who wants more might start with Conway and Sloane.

Addendum: I had written:

> *Now here's an interesting question: say we labelled the 12 spheres touching the central one with numbers 1-12. Is there enough room to roll these spheres around, always touching the central one, and permute the spheres 1-12 in an interesting way? Notice that it's trivially easy to roll them around in a way that amounts to rotating the icosahedron, but are there more interesting permutations one can get?*
>
> *In fact, one can achieve all EVEN permutations of the 12 spheres this way!*

I got some email from Conway saying that in fact one get ALL permutations of the 12 spheres. The point is this. One can start with the 12 spheres touching the central one arranged so their centers are at the vertics of an icosahedron. Then one can roll them around so their centers lie at the vertices of a cuboctahedron:

At this point they touch each other, but one can indeed get to this position. There is a nice picture of *how* to do this in Conway and Sloane's book, but you might enjoy figuring it out.

Now if we rotate the cuboctahedron around the axis pointing towards the reader, we get an odd permutation of the 12 spheres, with cycle structure $(123)(456)(789101112)$. So one can in fact get all of S_{12} if one lets the 12 spheres "just touch." If one thinks about it a bit, the method described in the previous post gets all of A_{12} without the 12 spheres touching at all. Conway says he doesn't know if one can get all of S_{12} without the 12 spheres touching each other at all. So this might be a fun problem to work on. I bet the answer is "no".

Conway says the permutation problem came up before a lecture he gave at U. of Penn. a while ago, that he solved it during the lecture, and told Jim Propp about it afterwards.

Someone with much better intuition about these things than I have might want to consider similar "rolling spheres permutation problems" in higher dimensions. E.g., what permutations can one can achieve in 4 dimensions, where it is possible to get 24 spheres to touch a central one? (In 4d it is not known whether one can get 25 to touch, but

25 is an upper bound.) Greg Kuperberg informs me that the known method for having 24 spheres touch the central sphere is rigid, so that only the "obvious" permutations are possible starting from this arrangement.

The only other cases where the rolling spheres permutation problem appears to be solved is in dimensions 1 and 2 (boring), 8, and 24. I described the E_8 lattice in 8 dimensions in "This Week's Finds." In the corresponding lattice packing, each sphere touches 240 others. This is, up to rotation, the *only* way to get 240 spheres to touch a central one in 8 dimensions. (Also, one cannot get *more* than 240 to touch the central one.) So the subgroup of S_{240} that one can get by rolling 240 spheres around a central one is precisely the "obvious" subgroup, that is, the subgroup of $\mathrm{SO}(8)$ that preserves the E_8 lattice. This turns out to be just the Weyl group of E_8, which has

$$2^{14} \cdot 3^5 \cdot 5^2 \cdot 7 = 696729600$$

elements.

Dimension 24 is probably the most interesting for lattice theory, and here the densest lattice packing is the Leech lattice. I am somewhat sorry not to have even mentioned the Leech lattice in my article, since this is the real star of Conway and Sloan's book. The Leech lattice is probably related to the appearance of 26-dimensional spacetime in string theory; to get it, start with the unique even unimodular lattice in 26-dimensional Minkowski space, and then look at M, the set of vectors in the lattice perpendicular to the vector $w = (70, 1, 2, 3, ..., 24)$. This is a null vector since

$$1^2 + 2^2 + ... + 24^2 = 70^2,$$

i.e., it is perpendicular to itself. Taking the quotient of M by w itself, we get the Leech lattice. In this lattice each sphere touches 196560 others, which is the most one can attain, and again this is the *only* way to get 196560 spheres to touch a central one in 24 dimensions. This should be obvious by visualizing it. :-) So again the answer to the rolling spheres permutation problem is the subgroup of $\mathrm{SO}(24)$ that preserves the Leech lattice. The isomorphism group of the Leech lattice is an interesting group called Co_0 or $.0$ (pronounced "dotto"). It has

$$2^{22} \cdot 3^9 \cdot 5^4 \cdot 7^2 \cdot 11 \cdot 13 \cdot 23 = 8315553613086720000$$

elements. The Monster group can also be produced using the Leech lattice.

Please don't be fooled into thinking I understand this stuff!

Jim Buddenhagen raised another interesting point:

> *Since 13 unit spheres can't quite all touch a unit sphere, one may ask how much bigger the central sphere must be to allow all 13 to touch.*
>
> *In 1951 K. Schutte and B. L. van der Waerden found an arrangement of the 13 unit spheres that allows all of them to touch a central sphere of radius* $r = 1.04557\ldots$ *This is thought to be optimal but has not been proved optimal.*
>
> *This* r *is an algebraic number and is a root of the polynomial*
>
> $$4096x^{16} - 18432x^{12} + 24576x^{10} - 13952x^8 + 4096x^6 - 608x^4 + 32x^2 + 1$$
>
> *They computed* r *but did not publish the polynomial.*

Week 21

October 10, 1993

Louis Kauffman is editing a series of volumes called "Series on Knots and Everything," published by World Scientific. The first volume was his own book, *Knots and Physics*. Right now I'd like to talk about the second volume, by Carter. I got to know Carter and Saito when it started seeming that a deeper understanding of string theory and the loop representation of quantum gravity might require understanding how 2-dimensional surfaces can be embedded in 4-dimensional spacetime. The study of this subject quickly leads into some very fascinating algebra, such as the "Zamolodchikov tetrahedron equations" (which first appeared in string theory). A nice review of this subject and their work on it will appear in a while:

1) J. Scott Carter and Masahico Saito, "Knotted surfaces, braid movies, and beyond", in *Knots and Quantum Gravity*, ed. John Baez, Oxford U. Press, Oxford, 1994.

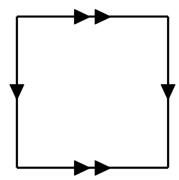
but for the non-expert, a great way to get started is:

2) J. Scott Carter, *How Surfaces Intersect in Space: An Introduction to Topology*, World Scientific Press, Singapore, 1993.

You can tell this isn't a run-of-the-mill introductory topology book as soon as you read the little blurb about the author on the back dustjacket. Occaisionally there will be tantalizing personal details in these blurbs that indicate that the author is not just a mathematical automaton; for example, on the back of Hartshorne's famous text on algebraic topology it says "He has travelled widely, speakes several foreign languages, and is an experienced mountain climber. He is also an accomplished amateur musician; has played the flute for many years, and during his last visit to Kyoto, he began studying the shakuhachi." This somehow fits with the austere and slightly intimidating quality of the text itself. The tone of the blurb on the back of Scott Carter's book could not be more different: "When he is not drawing pictures, cooking, or playing with Legos, he is writing songs and playing guitar for his band The Anteaters who have recorded an eight-song cassette published by Lobe Current Music." This is a book that invites the reader into topology without taking itself too seriously.

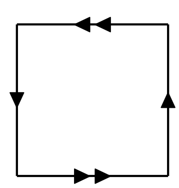
I remember first reading about topology as the study of doughnuts, Möbius strips and the like, and then being in a way disappointed as an undergrad – although in another way quite excited – when it seemed that what topologists *really* did was a lot of "diagram-chasing," the algebraic technique widely used in homology and homotopy theory. Once, however, as a grad student, I took a course in "geometric topology" by Tim Cochran, and was immensely pleased to find that *some* topologists really did draw wild pictures of many-handled doughnuts and the like in 4 dimensions, and prove things by sliding handles around. The nice thing about this book is that it is readable by any undergraduate — it doesn't assume or even mention the definition of a topological space! — but covers some very nontrivial geometric topology. It is not a substitute for the usual introductory course; instead, it concentrates on the study of surfaces embedded or immersed in 3 and 4 dimensional space, and shows how much there is to ponder about them. It is *packed* with pictures and is lots of fun to read.

The intrinsic topology of surfaces is very simple. The simplest one is the sphere (by which, of course, mathematicians mean the *surface* of a ball, not the ball itself). The next is the torus, that is, the surface of the doughnut. One can also think of the torus as what you get by taking a square and gluing together the edges as below:

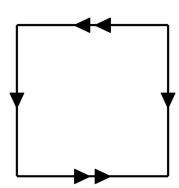


gluing the two horizontal edges together so the double arrows match up, and gluing the two vertical edges together so the single arrows match up. There is also a two-handled torus, and so on. The number of handles is called the "genus." All these surfaces are orientable, that is, one can define a consistent notion of "right" and "left" on them, so that if one writes a little word on them and slides the word around it'll never come back mirror-imaged. And in fact, all orientable surfaces are just n-handled tori, so they are classified by their genus.

A nice example of a nonorientable surface is the projective plane. One way to visualize this is to take the surface of the sphere and "identify" opposite points, that is, decree them "the same" by fiat. Imagine, for example, a globe in which antipodal points have been identified. If one writes a word on the north pole and then slides it down through the Americas to Ecuador, since the southern hemisphere has been identified with the northern one, we can think of it popping out over around India somewhere (sorry, my geography is a little rusty when it comes to antipodes!), but we will see when we slide it back to the north pole that it has been reversed, and is now written backwards! We see from this not only that the projective plane is nonorientable, but that it has another description: simply take a disc and identify opposite points along the boundary. Since we're doing topology, a square is just as good as a disc, so we can think of the projective plane as the result of identifying the points on the boundary of a square as follows:



Another famous example of a nonorientable surface is the Klein bottle, which is given by

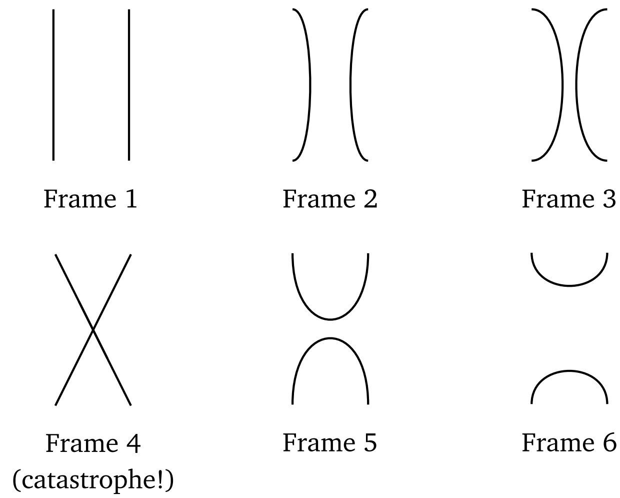


We can take either the Klein bottle or the projective plane and get more nonorientable surfaces by adding handles. Every nonorientable surface is of one of these forms. I've included a few more basic facts about the classification of surfaces as puzzles at the end of this article.

Now, the intrinsic topology of surfaces considers them as abstract spaces in their own right, but the "extrinsic topology" of them considers the ways they may be mapped into other spaces — for example, 3- or 4-dimensional Euclidean space. And here things get much more interesting and subtle. For example, while one can embed any orientable surface in 3d space, one cannot embed any of the nonorientable ones. Here an embedding is a 1-1 continuous map. However, one can immerse the non-orientable ones. An immersion is a map that is locally an embedding, but not necessarily globally; e.g., a figure 8 is an immersion of the circle in the plane. There's a standard way of immersing the Klein bottle in 3d space with a circle of "double points," that is, places where the immersion is 2-1. One can easily turn this immersion into an embedding of the Klein bottle 4d space by representing the 4th coordinate by how *red* the surface is and having the Klein bottle blush as it passes through itself. In fact, one can embed any surface into 4d space.

While one can't embed the nonorientable surfaces in 3d space, it is interesting to see how close one can come. The simplest way an immersion can fail to be an embedding is by having double points. Another simple way is to have triple points. Carter discusses a charming immersion of the projective plane in 3d space that only has curves of double points and a single triple point. This is known as "Boy's surface." A somewhat sneakier way immersions fail to be embeddings is by having "branch points." Think, for example, of the function $\sqrt{z}$ on the complex plane. This is a two-valued function, so its graph consists of two "sheets" which glom together in a funny way at $z = 0$, the branch point. Carter also talks about another neat immersion of the projective plane in $\mathbb{R}^3$ that just has double points and a branch point — the "cross cap." Another immersion, the "Roman surface," has both triple points and a branch point.

The general question, then, is what sort of embeddings and immersions different surfaces admit in 3 and 4 dimensions, and how to classify these. If we are studying embeddings into 4 dimensions, a nice technique is that of movies. Calling the 4th coordinate "time," we can draw slices at different times and get frames of a movie. Most of the frames of a movie of an embedded surface will show simply a bunch of knots. At a few times, however, a "catastrophe" will occur, e.g.:



However, there are always many different movies of essentially the same embedding. We can, however, always relate these by a sequence of transformations called "movie moves." I wish I could draw these, but it would take too long, so look at Carter's book!

And while you're at it, check out the index. You will enjoy finding the excuses he has for such entries as "hipster jive," "math jail," "basket shaped thingy," and "chocolate." Heck, I can't resist one... on page 81: "Mathematicians use the term "word" to mean any finite sequence of letters or numbers. This practice can freak out (disturb) people who are not hip to the lingo (aware of the terminology)."

I should add that the following book also has a lot of interesting pictures of surfaces in it:

3) George Francis, *A Topological Picturebook,* Springer, Berlin, 1987.

Problems:

A. Take a projective plane and cut out a little disc. Show that what's left is a Möbius strip.

B. Take two projective planes, cut out a little disc from each one and attach them along the resulting circles. This is called taking the "connected sum" of two projective planes. Show that the result is a Klein bottle. In symbols, $P + P = K$, or $2P = K$.

C. Now take the connected sum of a projective plane and a Klein bottle. Show that this is the same as a projective plane with a handle attached. A projective plane with a handle attached is just the connected sum of a projective plane and a torus, so we have: $3P = P + K = P + T$.

D. Show: $4P = K + K = K + T$.

E. Show: $(2n + 1)P = P + nT$.

F. Show: $(2n + 2)P = K + nT$.

Week 22

October 16, 1993

Lately I've been having fun in this series discussing some things that I don't really know much about, like lattice packings of spheres. Next week I'll get back to subjects that I actually know something about, but today I want to talk about the 4-color theorem, the golden mean, the silver root, knots and quantum field theory. I know a bit about *some* of these subjects, but I've only become interested in the 4-color theorem recently, thanks to my friend Bruce Smith, who has a hobby of trying to prove it, and Louis Kauffman's recent work connecting it to knot theory. The sources for what follows are:

1) Thomas L. Saaty and Paul C. Kainen, *The Four-Color Problem: Assault and Conquest*, McGraw-Hill, 1977.

and

2) Louis Kauffman, "Map coloring and the vector cross product", *J. Comb. Theory* B **48** (1990), 45.

   Louis Kauffman, "Map coloring, 1-deformed spin networks, and Turaev–Viro invariants for 3-manifolds", *Int. Jour. of Mod. Phys.* B, **6** (1992), 1765–1794.

   Louis Kauffman and H. Saleur, "An algebraic approach to the planar colouring problem", Yale University preprint `YCTP-P27-91`, November 8, 1991.

(I discussed this work of Kauffman already in "Week 8", where I described a way to reformulate the 4-color theorem as a property of the vector cross product.)

Where to start? Well, probably back in October, 1852. When Francis Guthrie was coloring a map of England, he wondered whether it was always possible to color maps with only 4 colors in such a way that no two countries (or counties!) touching with a common stretch of boundary were given the same color. Guthrie's brother passed the question on to De Morgan, who passed it on to students and other mathematicians, and in 1878 Cayley publicized it in the *Proceedings of the London Mathematical Society*.

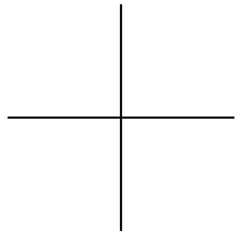
In just one year, Kempe was able to prove it. Whoops! In 1890 Heawood found an error in Kempe's proof. And then the real fun starts....

But I don't want to tell the whole story leading up to how Appel and Haken proved it in 1976 (with the help of a computer calculation involving 10^{10} operations and taking 1200 hours). I don't even understand the structure of the Appel–Haken proof — for that, one should probably try:

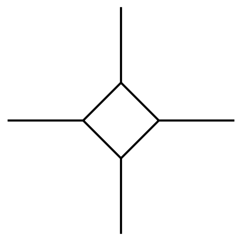
3) Kenneth Appel and Wolfgang Haken, *Every Planar Map is Four Colorable*, Contemporary Mathematics **98**, American Mathematical Society, Providence, Rhode Island, 1989.

Instead, I'd like to talk about some tantalizing hints of relationships between the 4-color theorem and physics!

First, note that to prove the 4-color theorem, it suffices to consider the case where only three countries meet at any "corner," since if more meet, say four:



we can stick in a little country at each corner:



so that now only three meet at each corner. If we can color the resulting map, it's easy to check that the same coloring with the little countries deleted gives a coloring of the original map.

Let us talk in the language of graph theory, calling the map a "graph," the countries "faces," their borders "edges," and the corners "vertices." What we've basically shown is it suffices to consider trivalent planar graphs without loops — that is, graphs on the plane that have three edges meeting at any vertex, and never have both ends of the same edge incident to the same vertex.

Now, it's easy to see that 4-coloring the faces of such a graph is equivalent to 3-coloring the *edges* in such a way that no two edges incident to the same vertex have the same color. For suppose we have a 4-coloring of faces with colors 1, i, j, and k. Wait — you say — those don't look like colors, they look like the quaternions. True! Now color each edge either i, j, or k according to product of the the colors of the two faces it is incident to, where we define products by:

$$\begin{aligned} &1i = i1 = i, \quad 1j = j1 = j, \quad 1k = k1 = k \\ &ij = ji = k, \quad jk = kj = i, \quad ki = ik = j. \end{aligned}$$

These are *almost* the rules for multiplying quaternions, but with some minus signs missing. Since today (October 16th, 1993) is the 150th birthday of the quaternions, I suppose I should remind the reader what the right signs are:

$$ij = -ji = k, \quad jk = -kj = i, \quad ki = -ik = j, \quad i^2 = j^2 = k^2 = -1.$$

Anyway, I leave it to the reader to check that this trick really gives us a 3-coloring of the edges, and conversely that a 3-coloring of the edges gives a 4-coloring of the faces.

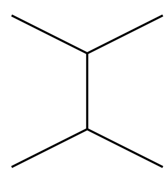
So, we see that the edge-coloring formulation of the 4-color problem points to some relation with the quaternions, or, pretty much the same thing, the group $\mathrm{SU}(2)$! (For what $\mathrm{SU}(2)$ has to do with quaternions, see "Week 5".) Those wrong signs look distressing, but in the following paper Penrose showed they weren't really so bad:

4) Roger Penrose, "Applications of negative dimensional tensors", in *Combinatorial Mathematics and its Applications*, ed. D. J. A. Welsh, Academic Press, 1971.

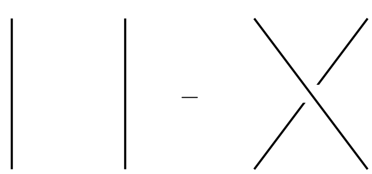
Namely, he showed one could count the number of ways to 3-color the edges of a planar graph as follows. Consider all ways of labelling the edges with the quaternions i, j, and k. For each vertex, take the product of the quaternions at the three incident edges in counterclockwise order and then multiply by i, getting either i or $-i$. Take the product of these plus-or-minus-i's over all vertices of the graph. And THEN sum over all labellings!

This recipe may sound complicated, but only if you haven't ever studied statistical mechanics of lattice systems. It's exactly the same as how one computes the "partition function" of such a system — the partition function being the philosopher's stone of statistical mechanics, since one can squeeze out so much information from it. (If we could compute the partition function of water we could derive its melting point.) To compute a partition one sums over states (labellings of edges) the product of the exponentials of interaction energies (corresponding to vertices). The statistical mechanics of 2-dimensional systems is closely connected to all sorts of nice subjects like knot theory and quantum groups, so we should suspect already that something interesting is going on here. It's especially nice that Penrose's formula makes sense for arbitrary trivalent graphs (although it does not count their 3-colorings unless they're planar), and satisfies some juicy "skein relations" reminiscent of those satisfied by the quantum group knot invariants. Namely, we can recursively calculate Penrose's number for any trivalent graph using the following three rules:

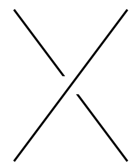
1) Wherever you see



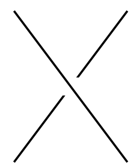
you can replace it with



In other words, replace the problem of computing Penrose's number for the original graph by the problem computing the difference of the Penrose numbers for the two graphs with the above changes made. For knot theory fans I should emphasize that we are talking about abstract graphs here, not graphs in 3d space, so there's no real difference between an "overcrossing" and an "undercrossing" — i.e., we could have said



instead of



above, and it wouldn't matter.

2) If you do this you will start getting weird loops that have NO vertices on them. You are allowed to dispose of such a loop if you correct for that by multiplying by 3. (This is not magic, this is just because there were 3 ways to color that loop!)

3) Finally, when you are down to the empty graph, use the rule that the empty graph equals 1.

Greg Kuperberg pointed out to me that this is a case of the quantum group knot invariant called the Yamada polynomal. This is associated to the spin-1 representation of the quantum group $\mathrm{SU}(2)$, and it is a polynomial in a variable q that represents $e^{\hbar}$, where $\hbar$ is Planck's constant. But the "Penrose number" is just the value at $q = 1$ of the Yamada polynomial — the "classical case" when $\hbar = 0$. This makes perfect sense if one knows about quantum group knot invariants: the factor of 3 in rule B above comes from the fact that the spin-1 representation of $\mathrm{SU}(2)$ is 3-dimensional; this representation is really just another way of talking about the vector space spanned by the quaternions i, j, and k. Also, quantum group knot invariants fail to distinguish between overcrossings and undercrossings when $\hbar = 0$.

Now let me turn to a different but related issue. Consider the problem of trying to color the *vertices* of a graph with n colors in such a way that no two vertices at opposite ends of any given edge have the same color. Let $P(n)$ denote the number of such n-colorings. This turns out to be a polynomial in n — it's not hard to see using recursion relations similar to the skein relations above. It also turns out that the 4-color theorem is equivalent to saying that the vertices of any planar graph can be 4-colored. (To see this, just use the idea of the "dual graph" of a graph — the vertices of the one being in 1-1 correspondence with the edges of the other.) So another way to state the 4-color theorem is that for no planar graph does the polynomial $P(n)$ have a root at $n = 4$.

$P(n)$ is called the "chromatic polynomial" and has been intensively investigated. One very curious thing is this. Remember the golden mean

$$\Phi = \frac{\sqrt{5}+1}{2} = 1.6180339887498948482045868343 7\ldots?$$

Well, $\Phi{+}1$ is never a root of the chromatic polynomial of a graph! (Unless the polynomial vanishes identically, which happens just when the graph has loops.) The proof is not all that hard, and it's in Saaty and Kainen's book. However — and here's where things get *really* interesting — in 1965, Hall, Siry and Vanderslice figured out the chromatic polynomial of a truncated icosahedron. (This looks like a soccer ball or buckyball.) They found that of the four real roots that weren't integers, one agreed with $\Phi + 1$ up to 8 decimal places! Of course, here one might think the 5-fold symmetry of the situation was secretly playing a role. But in 1966 Barri tabulated a bunch of chromatic polynomials in her thesis, and in 1969 Berman and Tutte noticed that most of them had a root that agreed with $\Phi + 1$ up to at least 5 decimal places.

This curious situation was at least partially explained by Tutte in 1970. He showed that for a triangular planar graph (that is, one all of whose faces are triangles) with n vertices one has

$$|P(\Phi + 1)| \leqslant \Phi^{5-n}.$$

This is apparently not a *complete* explanation, though, because the truncated icosahedron is not triangular.

This is not an isolated freak curiosity, either! In 1974 Beraha suggested checking out the behavior of chromatic polynomials at what are now called the "Beraha numbers"

$$B(n) = 4\cos^2(\pi/n).$$

These are

$$\begin{aligned} B(1) &= 4 \\ B(2) &= 0 \\ B(3) &= 1 \\ B(4) &= 2 \\ B(5) &= \Phi + 1 \\ B(6) &= 3 \\ B(7) &= S \end{aligned}$$

etc.. Note by the way that $B(n)$ approaches 4 as n approaches ∞. (What's S, you ask? Well, folks call $B(7)$ the "silver root," a term I find most poetic and eagerly want to spread!

$$S = 3.24697960371746706105000976800847962126\ldots$$

If anyone knows charming properties of the silver root, I'd be interested.) Anyway, it turns out that the roots of chromatic polynomials seem to cluster near Beraha numbers. For example, the four nonintegral real roots of the chromatic polynomial of the truncated icosahedron are awfully close to $B(5)$, $B(7)$, $B(8)$ and $B(9)$. Beraha made the following conjecture: let P_i be a sequence of chromatic polynomials of graphs such whose number of vertices approaches ∞ as $i \to \infty$. Suppose r_i is a real root of P_i and suppose the r_i approach some number x. Then x is a Beraha number.

In work in the late 60's and early 70's, Tutte proved some results showing that there really was a deep connection between chromatic polynomials and the Beraha numbers.

Well, to make a long story short (I'm getting tired), the Beraha numbers *also* have a lot to do with the quantum group $\mathrm{SU}(2)$. This actually goes back to some important work of Jones right before he discovered the first of the quantum group knot polynomials, the Jones polynomial. He found that – pardon the jargon burst – the Markov trace on the Temperley–Lieb algebra is only nonnegative when the Markov parameter is the reciprocal of a Beraha number or less than $1/4$. When the relationship of all this stuff to quantum groups became clear, people realized that this was due to the special natural of quantum groups when q is an nth root of unity (this winds up corresponding to the Beraha number $B(n)$.)

This all leads up to a paper that, unfortunately, I have not yet read, in part because our library doesn't get this journal!

5) H. Saleur, "Zeroes of chromatic polynomials: a new approach to the Beraha conjecture using quantum groups", *Commun. Math. Phys.* **132** (1990) 657.

This apparently gives a "physicist's proof" of the Beraha conjecture, and makes use of conformal field theory, that is, quantum field theory in 2 dimensions that is invariant under conformal transformations.

I should say more: about what quantum groups have to do with conformal field theory and knot polynomials, about the Kauffman/Saleur translation of the 4-color theorem into a statement about the Temperley–Lieb algebra, etc.. But I won't! It's time for dinner. Next week, if all goes according to plan, I'll move on to another puzzle in 2-dimensional topology — the Andrews–Curtis conjecture — and Frank Quinn's ideas on tackling *that* using quantum field theory.

Week 23

October 24, 1993

I will soon revert to my older style, in which I list piles of new papers as they accumulate on my desk. This time, though, I want to describe Frank Quinn's work on the Andrews–Curtis conjecture using topological quantum field theories (TQFTs), as promised. Then, if you'll pardon me, I'll list the contents of a book I've just finished editing. It is such a relief to be done that I cannot resist.

So —

1) Frank Quinn, "Topological quantum invariants and the Andrews–Curtis conjecture (Progress report)", preprint, Sept. 1993.

2) Frank Quinn, "Lectures on axiomatic topological quantum field theory", to appear in the proceedings of the Park City Geometry Institute.

3) Wolfgang Metzler, "On the Andrews–Curtis conjecture and related problems", in *Combinatorial Methods in Topology and Algebraic Geometry*, Contemporary Mathematics **44**, AMS, 1985.

Last week I described — in a pretty sketchy way — how the 4-color theorem and the Beraha conjecture are related to TQFTs. These can be regarded as two very hard problems in 2-dimensional topology — one solved by a mixture of cleverness and extreme brute force, the other still open. There is another hard problem in 2-dimensional topology called the Andrews–Curtis conjecture, which Quinn is working on using TQFT methods, which I'll talk about this time. I don't know too much about this stuff, so I hope any experts out there will correct my inevitable mistakes.

Actually, this conjecture is easiest to describe in a purely algebraic way, so I'll start there. Hopefully most of you know the concept of a "presentation" of a group in terms of generators and relations. For example, the group $\mathbb{Z}_n$ (integers $\bmod n$) has the presentation $\langle x \mid x^n \rangle$. This means, roughly, that we form all products of the "generator" x and its inverse, and then mod out by the "relation" $x^n = 1$. A bit more interesting is the dihedral group D_n of symmetries of a regular n-gon, counting rotations and reflections, with presentation $\langle x, y \mid x^n, y^2, (xy)^2 \rangle$. Here x corresponds to a clockwise rotation by $(1/n)$-th of a turn, and y corresponds to a reflection.

A group always has lots of different presentations, so a natural problem is to decide whether two different presentations give the same group (or, strictly speaking, isomorphic groups). It'd be nice to have an algorithm for deciding this question. But it's a famous result of mathematical logic that there is no such algorithm!

If two presentations give the same group, one can get from one to the other by a sequence of the following easy steps, called Tietze moves:

1) Throw in an extra new generator x together with the extra new relation xg^{-1} where g is a product of the previous generators and their inverses.

2) The inverse of 1) — remove a generator x together with the relation xg^{-1}, if possible (the relation xg^{-1} needs to be there!).

3) Throw in a new relation that's a consequence of existing relations.

4) The inverse of 2) — remove a relation that's a consequence of other relations.

So if one has two presentations and wants to see if they give same group, you could always set up a program that blindly tries using these Tietze moves in all possible ways to transform one presentation into the other. If they are the same it'll eventually catch on! But if they're not it'll chug on forever. There's no algorithmic way to tell *when* it should give up and admit the two presentations give different groups! — which is why we say there is no "decision procedure" for this problem.

In one form, the Andrews–Curtis conjecture goes as follows. Remember that the trivial group is the group with just the identity element; it has a presentation $\langle x \mid x\rangle$. Suppose we have some other "balanced" presentation of the trivial group, that is a presentation with just as many generators as relations: $\langle x_1, ..., x_n \mid r_1, ..., r_n\rangle$. Then the conjecture is that it can be reduced to the presentation $\langle x \mid x\rangle$ by a sequence of the following moves that keep the presentation balanced:

1) Throw in an extra new generator x together with the extra new relation x.

2) The inverse of 1).

3) Permute the relations

4) Change r_1 to r_1^{-1}

5) Change r_1 to $r_1 r_2$

6) Change r_1 to $g r_1 g^{-1}$ for any g.

The experts seem to think this conjecture is probably false — but nobody has disproved it. Metzler lists a few presentations of the trivial group that might be counterexamples: nobody has ever found a way to use moves 1)-6) to boil them down to the presentation $\langle x \mid x\rangle$. For example,

$$\langle a, b \mid b^5 a^{-4}, aba(bab)^{-1}\rangle.$$

Try it!

The Andrews–Curtis conjecture is interesting mainly for its implications in topology. When they first stated their conjecture they noted a number of topological consequences, and the referee of the paper noted one more. For example, it would shed some light on the Poinaré conjecture (although not settle it) as follows. Recall that the Poinaré conjecture says every 3-dimensional manifold homotopic to a 3-sphere is homeomorphic to a 3-sphere. The Andrews–Curtis conjecture implies that if the Poinaré conjecture is false, any counterexample can in fact be embedded (topologically) in $\mathbb{R}^4$!

It was the referee (does anyone know who that was?) who noted that the Andrews–Curtis conjecture can be formulated in terms of "CW complexes." This is how Quinn thinks about it, so I suppose I should say what those are.

A 0-complex is simply a set of points given the discrete topology. We call the points "0-cells." To get a 1-complex, we take a set of "1-cells," that is, closed unit intervals, and glue their ends on to the 0-cells in any way we want. In other words, we get a graph, possibly with some edges having both ends at the same vertex. To get a 2-complex, we take a set of "2-cells," that is, 2-dimensional closed disks, and glue their boundaries onto our 1-complex by any continuous map. And so on, with the "n-cells" being just copies of the closed unit ball in $\mathbb{R}^n$.

CW complexes were invented by J. H. C. Whitehead in 1949 and are a key tool in algebraic topology. (The word "CW," by the way, seems to come from "closure-finite" and "weak" – as in "weak topology.") They are a nice class of topological spaces since on the one hand, being built up by gluing simple pieces together, one can really understand them, and on the other hand, they are actually quite general. In fact, if one is interested in the usual invariants studied in algebraic topology (homology and cohomology groups, homotopy groups and the like), CW complexes are pretty much good enough. More precisely, Whitehead proved a "CW approximation theorem" saying that any halfway decent topological space (i.e., any "compactly generated" space) is "weakly homotopy equivalent" to a CW complex. I won't burden you with the definitions here; I learned this stuff once upon a time from

4) George W. Whitehead, *Elements of Homotopy Theory*, Springer, Berlin, 1978.

Anyway, the Andrews–Curtis conjecture can be thought of as being about 2-complexes. In fact, a group presentation can be regarded as instructions for building up a 2-complex — start with a point, glue on 1-cells, one for each generator (obtaining a "bouquet of circles") and then glue on 2-cells, one for each relation, attaching their boundaries to the 1-cells in the manner presecribed by the relation. This 2-complex will have fundamental group equal to the group given by the presentation. The moves 1)-6) above can be thought of as operations on these 2-complexes. So one can translate the Andrews–Curtis conjecture into a statement about 2-complexes. And at this point I guess I'm going to start getting more technical. . .

One topological statement of the Andrews–Curtis conjecture is that "if two 2-complexes are simply equivalent then one can be 2-deformed to the other." I don't understand this as well as I want, so I won't explain it; instead, I'll briefly explain the (weaker?) version corresponding more closely to the algebraic statement above, namely "if X is a contractible 2-complex, it can be 2-deformed to a point." Being "contractible" means that as far as homotopy theory goes X is just like a point. (E.g., the unit disk is contractible, while the circle is not.) And a "2-deformation" roughly means a sequence of moves consisting of adding or deleting 1-cells or 2-cells in a way that doesn't affect things as far as homotopy goes, or doing homotopies of attaching maps of 2-cells. The interesting thing about these formulations of the Andrews–Curtis conjecture is that their analogs for $n > 2$ are true and in fact were shown by J. H. C. Whitehead in 1939!

Quinn's goal is to cook up invariants of 2-complexes that might detect counterexamples to the Andrews–Curtis conjecture, i.e., invariants under 2-deformation. He wants to do it using 1+1-dimensional TQFTs of a sort that assign vector spaces to 1-complexes and linear maps to 2-complexes. Traditionally, TQFTs assign vector spaces to n-manifolds and linear maps to $(n+1)$-manifolds. Quinn calls his TQFTs "modular" because they have a lot of formal similarities to the kind of TQFTs that come up in string theory (where the modular group reigns supreme). He gives a thorough axiomatic description of modular TQFTs in his lecture notes, and this is actually the most fascinating aspect for me, more so than the Andrews–Curtis conjecture per se, since it bears on physics.

The problem with coming up with an TQFT invariant that can catch counterexamples to the Andrews–Curtis conjecture is an interesting "stabilization" property that 2-complexes have. Namely, if two 2-complexes are simply equivalent, one can can wedge them both with some large number k of 2-spheres and get complexes which are 2-deformable to each other. It turns out that this means we want to find a TQFT such that $Z(S^2)^k = 0$. And so Quinn considers TQFTs based, not on the complex numbers, but on integers $\mod p$.

A TQFT of his sort amounts to finding a symmetric tensor category of vector spaces and an object A in this category with some special properties corresponding to the fact that it is the vector space corresponding to the unit interval $[0, 1]$, which is the basic 1-complex from which one can build up more fancy ones. The kind of category he uses has been described by:

5) Sergei Gelfand and David Kazhdan, "Examples of tensor categories", *Invent. Math.* **109** (1992), 595–617.

It is formed by starting with the category of representations of an algebraic group in characteristic p, and then making a semisimple category out of this in a manner strongly reminiscent of what they do in the theory of quantum groups at roots of unity. (See "Week 5" for a bit more about this.) The object A is taken to be the sum of one copy of each irreducible representation. (Again, this is strikingly reminiscent, and no doubt based on, what occurs in the physics of the Wess–Zumino–Witten model, where quantum groups at roots of unity play the role a finite group is playing here.)

So, to round off a long story, Quinn and Ivelini Bobtcheva are currently engaged in some rather massive computer calculations in order to actually explicitly obtain the data necessary to calculate in the TQFTs of this form. They have been looking at the groups $\mathrm{SL}(2)$, $\mathrm{SL}(3)$, $\mathrm{Sp}(4)$ and G_2 over $\mathbb{Z}_p$, where p is small (up to 19 for the $\mathrm{SL}(2)$ case). They are finding some interesting stuff just by calculating the TQFT invariants of the 2-complexes corresponding to the presentations $\langle x \mid x^n \rangle$. (Note that $n = 0$ gives a space that's a wedge of a circle and S^2, while $n = 1$ gives a disk.) Namely, they are finding periodicity in n.

But they haven't found any counterexamples to the Andrews–Curtis conjecture yet!

6) *Knots and Quantum Gravity*, ed. John Baez, Oxford University Press (to appear).

This is the proceedings of a workshop held at U.C. Riverside; a large percentage of the papers contain new results. Let me simply list them:

- Renate Loll, "The loop formulation of gauge theory and gravity".
- Abhay Ashtekar and Jerzy Lewandowski, "Representation theory of Analytic Holonomy C^* Algebras".
- Rodolfo Gambini and Jorge Pullin , "The Gauss linking number in quantum gravity", available as gr-qc/9310025.
- Louis Kauffman,"Vassiliev invariants and the loop states in quantum gravity", available as gr-qc/9310035.

- Steven Carlip, "Geometric structures and loop variables in (2+1)-dimensional gravity", available as gr-qc/9309020.
- Dana S. Fine, "From Chern–Simons to WZW via path integrals".
- Louis Crane, "Topological field theory as the key to quantum gravity", available as hep-th/9308126.
- John Baez, "Strings, loops, knots and gauge fields", available as hep-th/9309067.
- Paolo Cotta-Ramusino and Maurizio Martellini, "BF theories and 2-knots".
- J. Scott Carter and Masahico Saito, "Knotted surfaces, braid movies, and beyond".

Week 24

October 31, 1993

I will now revert to topics more directly connected to physics and start catching up on the papers that have been accumulating. First, two very nice review papers:

1) Chris Isham, "Prima facie questions in quantum gravity", lecture at Bad Honeff, September 1993, available as gr-qc/9310031.

If one wants to know why people make such a fuss about quantum gravity, one could not do better than to start here. There are many approaches to the project of reconciling quantum mechanics with gravity, all of them rather technical, but here Isham focuses on the "prima facie" questions that present themselves no matter *what* approach one uses. He even explains why we should study quantum gravity — a nontrivial question, given how difficult it has been and how little practical payoff there has been so far! Let me quote his answers and urge you to read the rest of this paper:

> ***We must say something.*** *The value of the Planck length suggests that quantum gravity should be quite irrelevant to, for example, atomic physics. However, the non-renormalisability of the perturbative theory means it is impossible to actually compute these corrections, even if physical intuition suggests they will be minute. Furthermore, no consistent theory is known in which the gravitational field is left completely classical. Hence we are obliged to say* something *about quantum gravity, even if the final results will be negligible in all normal physical domains.*
>
> ***Gravitational singularities.*** *The classical theory of general relativity is notorious for the existence of unavoidable spacetime singularities. It has long been suggested that a quantum theory of gravity might cure this disease by some sort of 'quantum smearing'.*
>
> ***Quantum cosmology.*** *A particularly interesting singularity is that at the beginning of a cosmological model described by, say, a Robertson–Walker metric. Classical physics breaks down here, but one of the aims of quantum gravity has always been to describe the 'origin' of the universe as some type of quantum event.*
>
> ***The end state of the Hawking radiation process.*** *One of the most striking results involving general relativity and quantum theory is undoubtedly Hawking's famous discovery of the quantum thermal radiation produced by a black hole. Very little is known of the final fate of such a system, and this is often taken to be another task for a quantum theory of gravity.*
>
> ***The unification of fundamental forces.*** *The weak and electromagnetic forces are neatly unified in the Salam–Weinberg model, and there has also been a partial unification with the strong force. It is an attractive idea that a consistent quantum theory of gravity* must *include a unification of all the fundamental forces.*

> ***The possibility of a radical change in basic physics.*** *The deep incompatibilities between the basic structures of general relativity and of quantum theory have lead many people to feel that the construction of a consistent theory of quantum gravity requires a profound revision of the most fundamental ideas of modern physics. The hope of securing such a paradigm shift has always been a major reason for studying the subject.*

2) Matthias Blau and George Thompson, "Lectures on 2d gauge theories: topological aspects and path integral techniques", available as `hep-th/9310144`.

Most of the basic laws of physics appear to be gauge theories. Gauge theories are tricky to deal with because they are inherently nonlinear. (At least the "nonabelian" ones are — the main example of an abelian gauge theory is Maxwell's equations.) People have been working hard for quite some time trying to develop tools to study gauge theories on their own terms, and *one* reason for the interest in gauge theories in 2-dimensional spacetime is that life is simple enough in this case to exactly solve the theories and see precisely what's going on. Another reason is that in string theory one becomes interested in gauge fields living on the 2-dimesional "string worldsheet."

This paper is a thorough review of two kinds of gauge theories in 2 dimensions: topological Yang–Mills theory (also called BF theory) and the G/G gauged Wess–Zumino–Witten model. Both of these are of great mathematical interest in addition to their physical relevance. Studying the BF theory gives a way to do integrals on the moduli space of flat connections on a bundle over a Riemann surface, while studying the G/G model amounts to a geometric construction of the categories of representations of quantum groups at roots of unity. (Take my word for it, mathematicians find these important!)

I have found this review a bit rough going so far because the authors like to use supersymmetry to study these models. But I will continue digging in, since the authors consider the following topics (and I quote): solution of Yang–Mills theory on arbitrary surfaces; calculation of intersection numbers of moduli spaces of flat connections; coupling of Yang–Mills theory to coadjoint orbits and intersection numbers of moduli spaces of parabolic bundles; derivation of the Verlinde formula from the G/G model; derivation of the shift k to $k + h$ in the G/G model via the index of the twisted Dolbeault complex.

3) J. W. Barrett and T. J. Foxon, "Semi-classical limits of simplicial quantum gravity", available as `gr-qc/9310016`.

This paper looks at quantum gravity in 3 spacetime dimesions formulated along the lines of Ponzano and Regge, that is, with the spacetime manifold replaced by a bunch of tetrahedra (a "simplicial complex"). I describe some work along these lines in "Week 16". Here the Feynman path integral is replaced by a discrete sum over states, in which the edges of the tetrahedra are assigned integer or half-integer lengths, which really correspond to "spins," and the formula for the action is given in terms of $6j$-symbols. The authors look for stationary points of this action and find that some correspond to Riemannian metrics and some correspond to Lorentzian metrics. This is strongly reminiscent of Hartle and Hawking's work on quantum cosmology,

4) J. B. Hartle and S. W. Hawking, "Wave function of the universe", *Phys. Rev.* **D28** (1983), 2960.

in which there is both a Euclidean and a Lorentzian regime (providing a most fascinating answer to the old question, "what came before the Big Bang?"). Here, however, the path integral is oscillatory in the Euclidean regime and exponential in the Lorentzian one — the opposite of what Hartle and Hawking had. This puzzles me.

5) John Baez, "Generalized measures in gauge theory", available as hep-th/9310201.

Path integrals in gauge theory typically invoke the concept of Lebesgue measure on the space of connections. This is roughly an infinite-dimensional vector space, and there *is* no "Lebesgue measure" on an infinite-dimensional vector space. So what is going on? Physicists are able to do calculations using this concept and get useful answers — mixed in with infinities that have to be carefully "renormalized." Some of the infinities here are supposedly due to the fact that one should really be working no on the space of connections, but on a quotient space, the connections modulo gauge transformations. But not all the infinities are removed this way, and mathematically the whole situation is enormously mysterious.

Recently Ashtekar, Isham, Lewandowski and myself have been looking at a way to generalize the concept of measure, suggested by earlier work on the "loop representation" of gauge theories. Ashtekar and Lewandowski managed to rigorously construct a kind of "generalized measure" on the space of connections modulo gauge transformations that acts formally quite a bit like what might hope for. In this paper I show how can define generalized measures directly on the space of connections. All of these project down to generalized measures on the space of connections modulo gauge transformations, but even when one is interested in gauge-invariant quantities, it is sometimes easier to work "upstairs." In particular, when the gauge group is compact, there is a "uniform" generalized measure on the space of connections that projects down to the measure constructed by Ashtekar and Lewandowski. This generalized measure is in some respects a rigorous substitute for the ill-defined "Lebesgue measure," but it is actually built using Haar measure on G. I also define generalized measures on the group of gauge transformations (which is an infinite-dimensional group), and when G is compact I construct a natural example that is a rigorous substitute for Haar measure on the group of gauge transformations . As an application of this "generalized Haar measure" I show that any generalized measure on the space of connections can be averaged against generalized Haar measure to give a gauge-invariant generalized measure on the space of connections.

This doesn't, by the way, mean the problems I mentioned at the beginning are solved!

Week 25

November 14, 1993

Lately, many things give me the feeling that we're on the brink of some deeper understanding of the relations between geometry, topology, and category theory. It is very tantalizing to see the array of clues pointing towards the fact that many seemingly disparate mathematical phenomena are aspects of some underlying patterns that we don't really understand yet. Louis Crane expressed it well when he said that it's as if we are a bunch of archeologists digging away at different sites, and are all starting to find different parts of the skeleton of some gigantic prehistoric creature, the full extent of which is still unclear.

I want to keep studying the following book until I understand it, because I think it makes a lot of important connections... pardon the pun:

1) Jean-Luc Brylinski, *Loop Spaces, Characteristic Classes and Geometric Quantization*, Birkhäuser, Basel, 1993.

The title of this book, while accurate, really does not convey the *novelty* of the ideas it contains. All three subjects listed have been intensively studied by many people for at least several decades, but Brylinski's book is not so much a summary of what is understood about these subjects, as a plan to raise the subjects to a whole new level.

I can't really describe the full contents of the book, since I haven't had time to really absorb some of the most interesting parts, but let me start by listing the contents, and then talk about it a bit.

1. Complexes of Sheaves and Their Hypercohomology
2. Line Bundles and Geometric Quantization
3. Kähler Geometry of the Space of Knots
4. Degree 3 Cohomology — The Dixmier-Douady Theory
5. Degree 3 Cohomology — Sheaves of Groupoids
6. Line Bundles over Loop Spaces
7. The Dirac Monopole

It should be clear that while this is a very mathematical book, it is informed by ideas from physics. As usual, the physical universe is serving to goad mathematics to new heights!

The first two chapters are largely, but not entirely, "standard" material. I put the word in quotes because while Brylinski's treatment of it starts with the basics — the definition of sheaves, sheaf cohomology, Čech cohomology, de Rham theory and the like — even these "basics" are rather demanding, and the slope of the ascent is rather steep. Really, the reader should already be fairly familiar with these ideas, since Brylinski is mainly introducing them in order to describe a remarkable generalization of them in the next chapters.

Let me quickly give a thumbnail sketch of the essential ideas behind this "standard" material. In classical mechanics the main stage is the phase space of a physical system. Points in this space represent physical states; smooth functions on it represent observables. Time evolution acts on this space as a one-parameter group of diffeomorphisms. The remarkable fact is that time evolution is determined by an observable, the Hamiltonian, or energy function, by means of a geometric structure on phase space called a symplectic structure. This is a nondegenerate closed 2-form. The idea is that the differential of the Hamiltonian is a 1-form; since the symplectic structure is nondegenerate it sets up an isomorphism of the tangent and cotangent bundles of phase space, allowing us to turn the differential of the Hamiltonian into a vector field; this vector field generates the 1-parameter group of diffeomorphisms representing time evolution; and by the magic of symplectic geometry, these diffeomorphisms automatically preserve the symplectic structure.

This is the starting-point of the beautiful approach to quantum theory known as geometric quantization, founded by Kostant in the early 1970's. His first paper is still a good place to start:

2) Bertram Kostant, "Quantization and unitary representations", in *Lectures in Modern Analysis and Applications III*, Springer Lecture Notes in Mathematics **170** (1970), 87–208.

Here the idea is to construct a Hilbert space of states of the *quantum* system corresponding to the classical system, and turn time evolution into a one-parameter group of unitary operators on this Hilbert space. Extremely roughly, the idea is to first look at the space of all L^2 complex functions on phase space, and then use a "polarization" to cut down this "prequantum" Hilbert space to "half the size," by which one means something vaguely like how $L^2(\mathbb{R}^n)$ is "half the size" of $L^2(\mathbb{R}^{2n})$ — this being the classic example. But in fact, it turns out one doesn't really want to use *functions* on phase space, but instead sections of a certain complex line bundle. The point is that the classification of line bundles fits in beautifully with symplectic geometry. We can equip any line bundle with a hermitian connection; the curvature of this connection is a closed 2-form; this determines an element of the 2nd cohomology of phase space called the first Chern class. An important theorem says this class is necessarily an *integral* class, that is, it comes from an element of the 2nd cohomology with integer coefficients; moreover, isomorphism classes of line bundles over a manifold are in one-to-one correspondence with elements of its 2nd cohomology with integer coefficients. The trick, then, is to try to cook up a line bundle over phase space with a connection whose curvature is the symplectic structure! This will be possible precisely when the symplectic structure defines an integral cohomology class. In fact, this integrality condition is nothing but the old Bohr-Sommerfeld quantization condition dressed up in spiffy new clothes (and made far more precise).

So: the moral I want to convey here is just that if the symplectic structure on phase space defines an integral class in the 2nd cohomology group, then we get a line bundle over phase space which helps us get going with quantization. It then turns out that the one-parameter group of diffeomorphisms defined by any Hamiltonian on phase space lifts to a one-parameter group of transformations of this line bundle, which allows us to get a unitary operator on the space of L^2 sections of the line bundle. This is not the end of the quantization story; one still needs to chop down this "prequantum" space to half the size, etc.; but let me leave off here.

What Brylinski wants to do is to find analogs of all these phenomena involving the **third** cohomology groups of manifolds.

At first glance, this might seem to be a very artificial desire. Note that importance of the **second** cohomology group in the above story is twofold: 1) symplectic structures give elements of the second cohomology, 2) the curvature of a connection gives an element of the second cohomology, and in fact 2') line bundles are classified by elements of second cohomology. None of these beautiful things seem to have analogs in third cohomology! Of course, one can use the curvature of a connection to get, not just the first Chern class, but higher Chern classes. But the nth Chern class is an element of the $2n$th cohomology group, so the odd cohomology groups don't play a major role here. Of course, experts will immediately reply that there are also Chern–Simons "secondary characteristic classes" that live in odd cohomology, at least when one has a flat bundle around. And the same experts will immediately guess that, because Chern–Simons theory has been near the epicenter of the explosion of new mathematics relating quantum groups, topological quantum field theories, conformal field theory and all that stuff, I must be leading up to something along these lines. . . . Well, there *must be* a relationship here, but actually it is not emphasized in Brylinski's book! He takes a different tack, as follows.

The basic point is that given a manifold M, the space of loops in M, say LM, is a space of great interest in its own right. It is infinite- dimensional, but that should not deter us. When G is a Lie group, LG is also a group (with pointwise operations); these are the famous loop groups, which appear as groups of gauge transformations in conformal field theory. When M is a 3-dimensional manifold, LM contains within it the space of all knots in M; also, we may think of LM as the configuration space for the simplest flavor of string theory in the spacetime $\mathbb{R} \times M$. Loops also serve to define observables called "Wilson loops" in gauge theories, and these are the basis of the loop representation of quantum gravity. So there is a lot of interesting mathematics and physics to be found in the loop space.

What does this have to do with the 3rd cohomology group of M? Well, LM is a bundle over M, so according to algebraic topology there is a natural map from the 3rd cohomology of M to the 2nd cohomology of LM! The ramifications of this are multiple.

First, every compact simple Lie group G has 3rd cohomology equal to $\mathbb{Z}$. (In fact, Brylinski notes that the cohomology group is not merely isomorphic to $\mathbb{Z}$, but canonically so — and this extra nuance turns out to be quite significant!) This gives rise to a special element in the 2nd cohomology of LG. This then gives a line bundle over LG. Alternatively, it gives a circle bundle over LG, in fact a central extension of LG, that is, a bigger group $\widehat{LG}$ and an exact sequence

$$1 \to S^1 \to \widehat{LG} \to LG \to 1$$

This group is called a Kac–Moody group, and these are well-loved by string theorists since it turns out that when one wants to quantize a gauge theory on the string worldsheet (a kind of conformal field theory) one gets, not a representation of the gauge group LG on the Hilbert space of quantum states, but merely a projective representation, or in other words, a representation of the central extension $\widehat{LG}$. Brylinski also notes that in some sense the canonical element in the 3rd cohomology of G is responsible for the existence of quantum groups; this is probably the deep reason for the association between quantum group representations and Kac–Moody group representations, but, alas, this is still quite murky to me.

Second, we can do better if we restrict ourself to knots (possibly with nice self-intersections) rather than loops. Namely, given a 3-manifold M equipped with a 3-form, one gets, not just an element of the 2nd cohomology of LM, but a symplectic structure on the space of knots in M, say KM. It may seem odd to think of the space of knots as a physical *phase* space, but Brylinski shows that this idea is related to the work of Marsden and Weinstein on "vortex filaments," an idealization of fluid dynamics in which all the fluid motion is concentrated along some curves. Brylinski also notes that if M is equipped with a Riemannian structure then KM inherits a Riemannian structure (this is easy), and that if M has a conformal structure KM has an almost complex structure. In fact, in the Riemannian case all these structures on KM fit together to make it a sort of Kähler manifold (although one must be careful, since the almost complex structure is only integrable in a certain formal sense). Brylinski hints that all this geometry may give a nice approach to the study of knot invariants; I will have to look at the following papers sometime:

3) M. Rasetti and T. Regge,"Vortices in He II, current algebras and quantum knots", *Physica* **80A** (1975) 217–233.

4) V. Penna and M. Spera, "A geometric approach to quantum vortices", *J. Math. Phys.* **30** (1989), 2778–2784.

However, Brylinski's real goal is something much more radical! The beauty of 2nd cohomology is that integer classes in the 2nd cohomology of M correspond to line bundles on M; there is, in other words, a very nice geometrical picture of 2nd cohomology classes. What is the natural analog for 3rd cohomology? Instead of just working with LM, it would be nice to have some sort of geometrical objects on M that correspond to integer classes in 3rd cohomology. What should they be?

Brylinski gives two answers, one in Chapter 4 and another in Chapter 5. The first one, due mainly to Dixmier and Douady, is very appealing for a quantum field theorist such as myself. Just as elements of $\mathrm{H}^2(M,\mathbb{Z})$ correspond to line bundles over M, elements of $\mathrm{H}^3(M,\mathbb{Z})$ correspond to projective Hilbert space bundles over M! Recall that in physics two vectors in a Hilbert space correspond to the same physical state if one is a scalar multiple of the other; the space of equivalence classes (starting with a countable-dimensional Hilbert space) is what I'm calling "projective Hilbert space," and it is bundles of such rascals that correspond to elements of $\mathrm{H}^3(M,\mathbb{Z})$. The reason is roughly this: the structure group G for such bundles is the group $\mathrm{Aut}(H)/\mathbb{C}^*$, that is, invertible operators on the Hilbert space H, modulo invertible complex numbers. In other words, we have an exact sequence

$$1 \to \mathbb{C}^* \to \mathrm{Aut}(H) \to G \to 1$$

This gives an exact sequence of sheaves on M, which, combined with the marvelous fact that $\mathrm{Aut}(H)$ is contractible, gives an isomorphism between $\mathrm{H}^1(M,\mathrm{sh}(G))$ (the cohomology of the sheaf of smooth G-valued functions on M) and $\mathrm{H}^2(M,\mathrm{sh}(\mathbb{C}^*))$. But the latter is isomorphic to $\mathrm{H}^3(M,\mathbb{Z})$.

Brylinski pushes the analogy to the line bundle case further by showing how to realize the element of $\mathrm{H}^3(M,\mathbb{Z})$ starting from a connection on a projective Hilbert space bundle.

But in Chapter 5 he takes a more abstract approach that I want to sketch very vaguely, since I don't understand it very well yet. This approach is exciting because it connects to recent work on 2-categories (and higher n-categories), which I am convinced will play a role in unifying the wild profusion of mathematics we are seeing in this tail end of the twentieth century.

Here the best way to see the analogy to the line bundle case is through Čech cohomology. Recall that we can patch a line bundle together by covering our manifold M with charts $O(i)$ and assigning to each intersection $O(i) \cap O(j)$ a $\mathbb{C}^*$-valued function g_{ij}. These "transition functions" must satisfy the compatibility condition

$$g_{ij} g_{jk} g_{ki} = 1$$

We say then that the functions g_{ij} define a 1-cocycle in Čech cohomology — think of this as just jargon, if you like. Note that we will get an isomorphic line bundle if we take some $\mathbb{C}^*$-valued functions f_i, one on each chart $O(i)$, and multiply g_{ij} by $f_i f_j^{-1}$. This simply amounts to changing the trivialization of the bundle on each chart. We say that the new Čech cocycle differs by a coboundary. So line bundles are in 1-1 correspondence with the 1st Čech cohomology with values in $\mathrm{sh}(\mathbb{C}^*)$. This turns out to be the same thing as $\mathrm{H}^2(M, \mathbb{Z})$, as noted above.

Now, there is a marvelous thing called a gerbe, which is like a bundle, but is pieced together using Čech 2-cocyles! These will be classified by the 2nd Čech cohomology with values in $\mathrm{sh}(\mathbb{C}^*)$, which is nothing but $\mathrm{H}^3(M, \mathbb{Z})$.

What are these gerbes? Well, I wish I really understood them. Let me just say what I know. The basic idea is to boost everything up a notch using category-theoretic thinking. When we were getting ready to define bundles, we needed to have the concept of a group at our disposal (to have a structure group.) For gerbes, we need something called a category of torsors. What is a group? Well, it is a **set** equipped with various **maps** satisfying various properties. What is a category of torsors? Well, it is a **category** equipped with various **functors** satisfying utterly analogous properties. Note how we are "categorifying" here. We have more structure, since while a set is just a bunch of naked points, a category is a bunch of points, namely objects, which are connected by arrows, namely morphisms. Given the group $\mathbb{C}^*$ we can get a corresponding category of torsors as follows: the category of all manifolds with a simply transitive $\mathbb{C}^*$-action (which are called torsors). A nice account of why this category looks so much like a group appears in

5) Dan Freed, "Higher algebraic structures and quantization", preprint, available as `hep-th/9212115`.

which I already mentioned in "Week 12".

Just as a group can act on a set, a category of torsors can act on a category. If we "sheafify" this notion, we get the concept of a gerbe. Clear? Well, part of why I am interested in these ideas is the way they make me a bit dizzy, so don't feel bad if you are a bit dizzy too now. I really think that overcoming this dizziness will be necessary for certain advances in mathematics and physics, though.

Instead of actually coming clean and defining the concept of a gerbe, let me finish by saying what Brylinski does next. He defines an analog of connections on bundles, called "connective structures" on gerbes. And he defines an analog of the curvature, the "curving" of a connective structure. This turns out to give an element of $\mathrm{H}^3(M, \mathbb{Z})$ in a natural way. He concludes in a blaze of glory by showing how the Dirac monopole gives a gerbe on S^3 whose curving is the volume form. The integrality condition turns out to be related to Dirac's original argument for quantization of electric charge. Whew!

To wrap up, let me note that the following paper, mentioned in "Week 23", has shown up on `gr-qc`:

6) Abhay Ashtekar and Jerzy Lewandowski, "Representation theory of analytic holonomy C^* Algebras", in *Knots and Quantum Gravity*, ed. J. Baez, available as `gr-qc/9311010`.

Ashtekar and Lewandowski are my friendly competitors in the business of making the loop representation of quantum gravity more rigorous by formalizing the idea of a generalized measure on the space of connections modulo gauge transformations.

Week 26

November 21, 1993

I have been struggling to learn the rudiments of Teichmüller theory, and it's almost time for me to face up to my ignorance of it by posting a "This Week's Finds" attempting to explain the stuff, but I am going to put off the inevitable and instead describe a variety of papers on different subjects. . .

1) Huw Price, "Cosmology, time's arrow, and that old double standard", in S. Savitt, ed., *Time's Arrows Today*, Cambridge University Press, 1994, pp. 66–94. Available as gr-qc/9310022.

Why is the future different from the past? Because it hasn't happened yet? Well, sure, but that's not especially enlightening, in fact, it's downright circular. Unfortunately, a lot of work on the "arrow of time" is just as circular, only so erudite that it is hard to spot it! That's what this article takes some pains to clarify. I think I will be lazy and quote the beginning of the paper:

> *A century or so ago, Ludwig Boltzmann and others attempted to explain the temporal asymmetry of the second law of thermodynamics. The hard-won lesson of that endeavour — a lesson still commonly misunderstood — was that the real puzzle of thermodynamics lies not in the question why entropy increases with time, but in that as to why it was ever so low in the first place. To the extent that Boltzmann himself appreciated that this was the real issue, the best suggestion he had to offer was that the world as we know it is simply a product of a chance fluctuation into a state of very low entropy. (His statistical treatment of thermodynamics implied that although such states are extremely improbable, they are bound to occur occasionally, if the universe lasts a sufficiently long time.) This is a rather desperate solution to the problem of temporal asymmetry, however, and one of the great achievements of modern cosmology has been to offer us an alternative. It now appears that temporal asymmetry is cosmological in origin, a consequence of the fact that entropy is much lower than its theoretical maximum in the region of the Big Bang — i.e., in what we regard as the* early *stages of the universe.*
>
> *The task of explaining temporal asymmetry thus becomes the task of explaining this condition of the early universe. In this paper I want to discuss some philosophical constraints on the search for such an explanation. In particular, I want to show that cosmologists who discuss these issues often make mistakes which are strikingly reminiscent of those which plagued the nineteenth century discussions of the statistical foundations of thermodynamics. The most common mistake is to fail to recognise that certain crucial arguments are blind to temporal direction, so that any conclusion they yield with respect to one temporal direction must apply with equal force with respect to the other. Thus writers on thermodynamics often failed to notice that the statistical arguments concerned are inherently insensitive to temporal direction, and hence unable to account for temporal asymmetry. And writers who did notice this mistake commonly fell for*

> *another: recognising the need to justify the double standard — the application of the arguments in question 'towards the future' but not 'towards the past' — they appealed to additional premisses, without noticing that in order to do the job, these additions must effectively embody the very temporal asymmetry which was problematic in the first place. To assume the uncorrelated nature of initial particle motions (or incoming 'external influences'), for example, is simply to move the problem from one place to another. (It may* look *less mysterious as a result, but this is no real indication of progress. The fundamental lesson of these endeavours is that much of what needs to be explained about temporal asymmetry is so commonplace as to go almost unnoticed. In this area more than most, folk intuition is a very poor guide to explanatory priority.)*
>
> *One of the main tasks of this paper is to show that mistakes of these kinds are widespread in modern cosmology, even in the work of some of the contemporary physicists who have been most concerned with the problem of the cosmological basis of temporal asymmetry — in the course of the paper we shall encounter illicit applications of a temporal double standard by Paul Davies, Stephen Hawking and Roger Penrose, among others. Interdisciplinary point- scoring is not the primary aim, of course: by drawing attention to these mistakes I hope to clarify the issue as to what would count as adequate cosmological explanation of temporal asymmetry.*
>
> *I want to pay particular attention to the question as to whether it is possible to explain why entropy is low near the Big Bang without thereby demonstrating that it must be low near a Big Crunch, in the event that the universe recollapses. The suggestion that entropy might be low at both ends of the universe was made by Thomas Gold in the early 1960s. With a few notable exceptions, cosmologists do not appear to have taken Gold's hypothesis very seriously. Most appear to believe that it leads to absurdities or inconsistencies of some kind. However, I want to show that cosmologists interested in time asymmetry continue to fail to appreciate how little scope there is for an explanation of the low entropy Big Bang which does not commit us to the Gold universe. I also want criticise some of the objections that are raised to the Gold view, for these too often depend on a temporal double standard. And I want to discuss, briefly and rather speculatively, some issues that arise if we take the view seriously. (Could we observe a time-reversing future, for example?)*

And now let me jump forward to a very interesting issue, Hawking's attempt to derive the arrow of time from his "no-boundary boundary conditions" choice of the wavefunction of the universe. (See "Week 3".) I found this rather unsatisfying when I read it, and had a sneaking suspicion that he was falling into the fallacy Gold describes above. Let's hear what Price has to say about it.

> *Our second example is better known, having been described in Stephen Hawking's best seller,* A Brief History of Time*. It is Hawking's proposal to account for temporal asymmetry in terms of what he calls the No Boundary Condition (NBC) — a proposal concerning the quantum wave function of the universe. To see what is puzzling about Hawking's claim, let us keep in mind the basic*

dilemma. It seemed that provided we avoid double standard fallacies, any argument for the smoothness of the universe would apply at both ends or at neither. So our choices seemed to be to accept the globally symmetric Gold universe, or to resign ourselves to the fact that temporal asymmetry is not explicable (without additional assumptions or boundary conditions) by a time-symmetric physics. The dilemma is particularly acute for Hawking, because he has a more reason than most to avoid resorting to additional boundary conditions. They conflict with the spirit of his NBC, namely that one restrict possible histories for the universe to those that 'are finite in extent but have no boundaries, edges, or singularities.'

Hawking tells us how initially he thought that this proposal favoured the former horn of the above dilemma: 'I thought at first that the no boundary condition did indeed imply that disorder would decrease in the contracting phase.' He changed his mind, however, in response to objections from two colleagues: 'I realized that I had made a mistake: the no boundary condition implied that disorder would in fact continue to increase during the contraction. The thermodynamic and psychological arrows of time would not reverse when the universe begins to contract or inside black holes.'

This change of mind enables Hawking to avoid the apparent difficulties associated with reversing the thermodynamic arrow of time. What is not clear is how he avoids the alternative difficulties associated with not *reversing the thermodynamic arrow of time. That is, Hawking does not explain how his proposal can imply that entropy is low near the Big Bang, without equally implying that it is low near the Big Crunch. The problem is to get a temporally asymmetric consequence from a symmetric physical theory. Hawking suggests that he has done it, but doesn't explain how. Readers are entitled to feel a little dissatisfied. As it stands, Hawking's account reads a bit like a suicide verdict on a man who has been stabbed in the back: not an impossible feat, perhaps, but we'd like to know how it was done!*

It seems to me that there are three possible resolutions of this mystery. The first, obviously, is that Hawking has found a way round the difficulty. The easiest way to get an idea of what he would have to have established is to think of three classes of possible universes: those which are smooth and ordered at both temporal extremities, those which are ordered at one extremity but disordered at the other, and those which are disordered at both extremities. If Hawking is right, then he has found a way to exclude the last class, without thereby excluding the second class. In other words, he has found a way to exclude disorder at one temporal extremity of the universe, without excluding disorder at both extremities. Why is this combination the important one? Because if we can't exclude universes with disorder at both extremities, then we haven't explained why our universe doesn't have disorder at both extremities — we know that it has order at least one temporal extremity, namely the extremity we think of as at the beginning of time. And if we do exclude disorder at both extremities, we are back to the answer that Hawking gave up, namely that order will increase when the universe contracts.

Has Hawking shown that the second class of universal histories, the order-

> *disorder universes, are overwhelmingly probable? It is important to appreciate that this would not be incompatible with the underlying temporal symmetry of the physical theories concerned. A symmetric physical theory might be such that all or most of its possible realisations were asymmetric. Thus Hawking might have succeeded in showing that the NBC implies that any (or almost any) possible history for the universe is of this globally asymmetric kind. If so, however, then he hasn't yet explained to his lay readers how he managed it. In a moment I'll describe my attempts to find a solution in Hawking's technical papers. What seems clear is that it can't be done by reflecting on the consequences of the NBC for the state of one temporal extremity of the universe, considered in isolation. For if that worked for the 'initial' state it would also work for the 'final' state; unless of course the argument had illicitly assumed an objective distinction between initial state and final state, and hence applied some constraint to the former that it didn't apply to the latter. What Hawking needs is a more general argument, to the effect that disorder-disorder universes are impossible (or at least overwhelmingly improbable). It needs to be shown that almost all possible universes have at at least one ordered temporal extremity — or equivalently, at most one disordered extremity. (As Hawking points out, it will then be quite legitimate to invoke a weak anthropic argument to explain why we regard the ordered extremity thus guaranteed as an* initial *extremity. In virtue of its consequences for temporal asymmetry elsewhere in the universe, conscious observers are bound to regard this state of order as lying in their past.)*
>
> *That's the first possibility: Hawking has such an argument, but hasn't told us what it is (probably because he doesn't see why it is so important). As I see it, the other possibilities are that Hawking has made one of two mistakes (neither of them the mistake he claims to have made). Either his NBC does exclude disorder at both temporal extremities of the universe, in which case his mistake was to change his mind about contraction leading to decreasing entropy; or the proposal doesn't exclude disorder at either temporal extremity of the universe, in which case his mistake is to think that the NBC accounts for the low entropy Big Bang.*

And, eventually, Price concludes that there is indeed something lacking in Hawking's attempts to derive an arrow of time.

I have said this many times here and there, but I'll say it again. For a good introduction to these issues, read:

2) H. D. Zeh, *The Physical Basis of the Direction of Time*, Second Edition, Springer, Berlin, 1992.

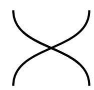
Zeh is one of the most clear-headed writers I know on this vexing problem. Interestingly, Price acknowledges Zeh in his paper.

3) Renate Loll, "Chromodynamics and gravity as theories on loop space", available as hep-th/9309056.

This is an especially thorough review of work on the loop representation of gauge theories, especially the theories of the strong force and gravity. A lot of work has been done on this subject but there are still very many basic mathematical problems when it comes to making any of this work rigorous, and one nice thing about Loll's work is that she is just as eager to point out the problems as the accomplishments. It can be dangerous when people become complacent and simply shrug off various problems just because they are difficult and can be temporarily ignored.

4) Daniel Armand-Ugon, Rodolfo Gambini and Pablo Mora, "Intersecting braids and intersecting knot theory", available as hep-th/9309136.

There are a lot of hints that classical knot theory, which only considers a circle smoothly embedded in space, is only the tip of a very interesting iceberg. Namely, if one looks at the space of all loops, this has the knots as an open dense subset, but then it has loops with a single "transverse double point" like



as a codimension 1 subset (like a hypersurface in the space of all loops), and more fancy singularities appear as still smaller subsets, or as the jargon has it, strata of higher codimension. The recent flurry of work on Vassiliev invariants points out the importance of these other strata — or at least a few — to knot theory. Namely, knot invariants that extend nicely to knots with arbitrarily many transverse double points include the famous quantum group knot invariants like the Jones polynomial, and there is a close relationship between these "Vassiliev invariants" and Lie algebra theory.

Meanwhile, the physicists have been forging ahead into more complicated strata, motivated mainly by the loop representation of quantum gravity. Gambini is one of the originators of the loop representation of gauge theory, so it is not surprising that he is ahead of the game on this business. Together with Pullin and Bruegmann he has been working on extending the Jones polynomial, for example, to loops with various sorts of self-intersections, calculating these extensions directly from the path-integral formula for the Jones polynomial as a Wilson loop expectation value in Chern–Simons theory. The relationship of this extension to the theory of Vassiliev invariants was recently clarified by Kauffman (see "Week 23"), but there is much more to do. Here Gambini and collaborators look at loops with transverse triple points. I guess I'll just quote the abstract:

> *An extension of the Artin Braid Group with new operators that generate double and triple intersections is considered. The extended Alexander theorem, relating intersecting closed braids and intersecting knots is proved for double and triple intersections, and a counter example is given for the case of quadruple intersections. Intersecting knot invariants are constructed via Markov traces defined on intersecting braid algebra representations, and the extended Turaev representation is discussed as an example. Possible applications of the formalism to quantum gravity are discussed.*

Week 27

December 16, 1993

This week I would like to describe some of the essays from the following volume:

1) *Conceptual Problems of Quantum Gravity*, eds. Abhay Ashtekar and John Stachel, based on the proceedings of the 1988 Osgood Hill Conference, 15–19 May 1988, Birkhäuser, Basel, 1991.

As the title indicates, this conference concentrated not on technical, mathematical aspects of quantum gravity but on issues with a more philosophical flavor. The proceedings make it clear how many problems we still have in understanding how to fit quantum theory and gravity together. Indeed, the book might be a bit depressing to those who thought we were close to the "theory of everything" which some optimists once assured us would be ready by the end of the millenium. But to those like myself who enjoy the fact that there is so much left to understand about the universe, this volume should be exciting (if perhaps a bit daunting).

The talks have been divided into a number of groups:

- Quantum mechanics, measurement, and the universe
- The issue of time in quantum gravity
- Strings and gravity
- Approaches to the quantization of gravity
- Role of topology and black holes in quantum gravity

Let me describe a few of the talks, or at least their background, in some detail rather than remaining general and vague.

2) Wojciech H. Zurek, "Quantum measurements and the environment-induced transition from quantum to classical", the volume above.

   W. G. Unruh, "Loss of quantum coherence for a damped oscillator", the volume above.

These talks by Zurek and Unruh fit into what one might call the "post-Everett school" of research on the foundations of quantum theory. To understand what Everett did, and what the post-Everett work is about, you will need to be comfortable with the notions of pure versus mixed states, and superpositions of states versus mixtures of states (which are very different things). So, rather than discussing the talks above, it probably makes more sense for me to talk about these basic notions. A brief mathematical discussion appears below; one really needs the clarity of mathematics to get anywhere with this sort of issue. First, though, let me describe them vaguely in English.

In quantum theory, associated to any physical system there are states and observables. An observable is a real-valued quantity we might conceivably measure about the system. A state represents what we might conceivably know about the system. The previous sentence is quite vague; all it really means is this: given a state and an observable there is a mathematical recipe that lets us calculate a probability distribution on the real number line, which represents the probability of measuring the observable to have a value lying in any subset of the real line. We call this the probability distribution of the observable in the state. Using this we can, if we want, calculate the mean of this probability distribution (let us assume it exists!), which we call the expectation value of the observable in the state.

Given two states ψ and ϕ, and a number c between 0 and 1 there is a recipe for getting a new state, called $c\psi + (1 - c)\phi$. This can be described roughly in words as follows: "with probability c, the system is in state ψ; with probability $1 - c$ it is in state ϕ." This is called a **mixture** of the states ψ and ϕ. If a state is a mixture of two different states, with c not equal to 0 or 1, we call that state a **mixed** state. If a state is not mixed it is **pure**. Roughly speaking, a pure state is a state with as little randomness as possible. (More precisely, it has as little entropy as possible.)

All the remarks so far apply to classical mechanics as well as quantum mechanics. A simple example from classical mechanics is a 6-sided die. If we ignore everything about the die except which side is up, we can say there are six pure states: the state in which the side of the die showing one dot is up, the state in which the side showing two dots is up, etc.. Call these states 1,2,3,4,5, and 6. If it's a fair die, and we roll it and don't look at it, the best state we can use to describe what we know about the die is a mixed state which is a mixture: $1/6$ of state 1 plus $1/6$ of state 2, etc.. Note that if you peek at the die and see that side 4 is actually up, you will be inclined to use a different state to describe your knowledge: a pure state, state 4. Your honest friend, who didn't peek, will still want to use a mixed state. There is no contradiction here; the state simply is a way of keeping track of what you know about the system, or more precisely, a device for calculating expectation values of observables; which state you use reflects your knowledge, and some people may know more than others.

Things get trickier in quantum mechanics. They also get trickier when the system being described includes the person doing the describing. They get even trickier when the system being described is the whole universe – for example, some people rebel at the thought that the universe has "many different states" – after all, it is how it is, isn't it? (Gell-Mann gave a talk at this conference, which unfortunately does not appear in this volume, entitled "Quantum mechanics of this specific universe." I have a hunch it deals with this issue, which falls under the heading of "quantum cosmology.")

The first way things get trickier in quantum mechanics is that something we are used to in classical mechanics fails. In classical mechanics, pure states are always dispersion-free – that is, for *every* observable, the probability measure assigned by the state to that observable is a Dirac delta measure, that is, the observable has a 100% chance of being some specific value and a 0% chance of having any other value. (Consider the example of the dice, with the observable being the number of dots on the face pointing up.) In quantum mechanics, pure states need *not* be dispersion-free. In fact, they usually aren't.

A second, subtler way things get trickier in quantum mechanics concerns systems made of parts, or subsystems. Every observable of a subsystem is automatically an observable for the whole system (but not all observables of the whole system are of that form; some involve, say, adding observables of two different subsystems). So every state of the whole system gives rise to, or as we say, "restricts to," a state of each of its subsystems. In classical mechanics, pure states restrict to pure states. For example, if our system consisted of 2 dice, a pure state of the whole system would be something like "the first die is in state 2 and the second one is in state 5;" this restricts to a pure state for the first die (state 2) and a pure state for the second die (state 5). In quantum mechanics, it is *not* true that a pure state of a system must restrict to a pure state of each subsystem.

It is this latter fact that gave rise to a whole bunch of quantum puzzles such as the Einstein–Podolsky–Rosen puzzle and Bell's inequality. And it is this last fact that makes things a bit tricky when one of the two subsystems happens to be *you*. It is possible, and indeed very common, for the following thing to happen when two subsystems interact as time passes. Say the whole system starts out in a pure state which restricts to a pure state of each subsystem. After a while, this need no longer be the case! Namely, if we solve Schrödinger's equation to calculate the state of the system a while later, it will necessarily still be a pure state (pure states of the whole system evolve to pure states), but it need no longer restrict to pure states of the two subsystems. If this happens, we say that the two subsystems have become "entangled."

In fact, this is the sort of thing that often happens when one of the systems is a measuring apparatus and the other is something measured. Studying this issue, by the way, does *not* require a general definition of what counts as a "measuring apparatus" or a "measurement" – on the contrary, this is exactly what is not needed, and is probably impossible to attain. What is needed is a description in quantum theory of a *particular* kind of measuring apparatus, possibly quite idealized, but hopefully reasonably realistic, so that we can study what goes on using quantum mechanics and see what it actually predicts will occur. For example:, taking a very idealized case for simplicity:

Our system consists of two subsystems, the "detector" and an "electron." The systems starts out, let's suppose, in a pure state which restricts to a pure state of each subsystem: the detector is "ready to measure the electron's spin in the z direction" and the electron is in a state with its spin pointing along the x axis. After a bit of time passes, if we restrict the state of the whole system to the first subsystem, the detector, we get a mixed state like "with 50% probability it has measured the spin to be up, and with 50% probability it has measure the spin to be down." Meanwhile, the if we restrict the state to the second subsystem, the electron it is in the mixed state "with 50% change it has spin up, and with 50% chance it has spin down." In fact these two mixed states are *correlated* in an obvious sense. Namely, the observable of the *whole* system that equals 1 if the reading on the detector agrees with the spin of the electron, and 0 otherwise, will have expectation value 1 (if the detector is accurate). The catchy term "entangled," which is a little silly, really just refers to this correlation. I don't want to delve into the math of correlations, but it is perhaps not surprising that, in classical or quantum mechanics, interesting correlations can only occur between subsystems if both of them are in mixed states. What's sneaky about quantum mechanics is that the whole system can be in a pure state which when restricted to each subsystem gives a mixed state, and that these mixed states are then correlated (necessarily, as it turns out). That's what "entanglement" is all about.

It was through analyses like this, but more detailed, that Everett realized what was going on in a quantum system composed of two subsystems, one of which was a measuring apparatus (or person, for that matter), the other of which was something measured. The post-Everett work amounts to refining Everett's analysis by looking at more realistic examples, and more varied examples. In particular, it is interesting to study situations where nothing very controlled like a scientific "measurement" is going on. For example, one subsystem might be an atom in outer space, and the other subsystem might be its environment (a bunch of other atoms or radiation). If one started out in a state which restricted to a pure state of each subsystem, how fast would the subsystems become entangled? And exactly *how* would they become entangled? – this is very interesting. When we are doing a scientific measurement, it's pretty clear what sort of correlation is involved in the entanglement. In the above example, say, the detector reading is becoming correlated to the electron's spin about the z axis. If all we have is an atom floating about in space, it's not so clear. Can we think of the environment as doing something analogous "measuring" something about the atom, which establishes correlations of a particular kind? This is the kind of thing Zurek and Unruh are studying.

In my description above I have tried to be very matter-of-fact, but probably you all know that this subject is shrouded in mystery, largely because of the misty and dramatic rhetoric people like to use, which presumably makes it seem more profound. At least "entangled" has a precise technical meaning. But anyone studying this subject will soon run into "collapse of the wavefunction," "branches," "the many-worlds interpretation," the "observer," and so on. These things mean many things to many people, and nothing in particular to many more, so one must always be on the alert.

Now for a little math to ground the above discussion. To keep life simple suppose we have a quantum system described by a n-dimensional Hilbert space H which we just think of as $\mathbb{C}^n$, n-dimensional complex space. The main thing to get straight is the difference between superpositions and mixtures of quantum states. An observable in quantum theory is described by a self-adjoint operator A, which for us is just an $n \times n$ self-adjoint matrix. A state is something that assigns to each observable a number called its expectation value, in a manner that is 1) linear, 2) positive, and 3) normalized. To explain this let us call our state ψ. Linearity means $\psi(A + B) = \psi(A) + \psi(B)$ and $\psi(cA) = c\psi(A)$ for all observables A,B and real numbers c. Positivity means $\psi(A) > 0$ when A is a nonzero matrix that has non-negative eigenvalues (a so-called non-negative matrix). And the normalization condition is that $\psi(1) = 1$.

This may seem unfamiliar, and that is because elementary quantum mechanics only considers states of the form

$$\psi(A) = \langle v, Av\rangle$$

where v is a unit vector in H. Not all states are of this form, but they are an extremely important special class of states. It is also important to consider states that are represented as "density matrices," which are non-negative matrices D with trace 1:

$$\mathrm{tr}(D) = \sum_i D_{ii} = 1$$

Such a density matrix defines a state ψ by

$$\psi(A) = \mathrm{tr}(AD).$$

It's worth checking that this really meets the definition of a "state" given above!

The states corresponding to unit vectors in H are in fact a special case of the density matrices. Namely, if v is a unit vector in H we can let D be the self-adjoint matrix corresponding to projection onto v. I.e., the matrix D acts on any other vector, say w, by

$$Dw = \langle v, w\rangle v.$$

It's not to hard to check that the matrix D really is a density matrix (do it!) and that this density matrix defines the same state as does the vector v, that is,

$$\mathrm{tr}(AD) = \langle v, Av \rangle$$

for any observable A.

The entropy of a state ψ corresponding to the density matrix D is defined to be

$$S(\psi) = -\,\mathrm{tr}(D \ln D)$$

where one calculates $D \ln D$ by working in a basis where D is diagonal and replacing each eigenvalue x of D by the number $x \ln x$, which we decree to be 0 if $x = 0$. Check that if D corresponds to a *pure* state as above then $D \ln D = 0$ so the entropy is zero.

Now about superpositions versus mixtures. They teach you how to take superpositions in basic quantum mechanics. They usually don't tell you about density matrices; all they teach you about is the states that correspond to unit vectors in Hilbert space. Given two unit vectors in H, one can take any linear combination of them and, if it's not zero, normalize it to be a unit vector again, which we call a superposition.

Mixtures are an utterly different sort of linear combination. Given two states ψ and ϕ – which recall are things that assign numbers to observables in a linear way – and given any number c between 0 and 1, we can form a new state by taking

$$c\psi + (1 - c)\phi$$

This is called a mixture of ψ and ϕ. Finally, some nontrivial exercises:

Exercise: Recall that a pure state is defined to be a state which is not a mixture of two different states with $0 < c < 1$. Show that the states corresponding to unit vectors in Hilbert space are pure.

Exercise: Conversely, show (in the finite-dimensional case we are considering) that all the pure states correspond to unit vectors in Hilbert space.

Exercise: Show that every density matrix is a mixture of states corresponding to unit vectors in Hilbert space.

Exercise: Show (in the finite-dimensional case we are considering) that all states correspond to density matrices. Show that such a state is pure if and only if its entropy is zero.

Well, this took longer than expected, so let me quickly say a bit more about a few other papers in the conference proceedings. . . .

3) Carlo Rovelli, "Is there incompatibility between the ways time is treated in general relativity and in standard quantum mechanics?", the volume above.

   Karel V. Kuchar, "The problem of time in canonical quantization of relativistic systems", the volume above.

   James B. Hartle, "Time and prediction in quantum cosmology", the volume above.

   Lee Smolin, "Space and time in the quantum universe", the volume above.

In the section on the problem of time in quantum gravity, these papers in particular show a lively contrast between points of view. One nice thing is that discussions after the papers were presented have been transcribed; these make the disagreements even more clear. Let me simply give some quotes that highlight the issues:

Rovelli: *A* ***partial observable*** *is an operation on the system that produces a number. But this number may be totally unpredictable even if the state is perfectly known. Equivalently, this number by itself may give no information on the state of the system [in the Heisenberg picture — jb]. For example, the reading of a clock, or the value of a field, not knowing where and when it has been measured, are partial observables.*

A ***true observable*** *or simply an* ***observable*** *is an operation on the system that produces a number than can be predicted (or whose probability distribution may be predicted) if the (Heisenberg) state is known. Equivalently, it is an observable that gives information about the state of the system.*

. . . .

Time is an experimental fact of nature, a very basic and general experimental fact, but just an experimental fact. The formal development of mechanics, and in particular Heisenberg quantum mechanics and the presymplectic formulation of classical mechanics, suggests that it is possible to give a coherent description of the world that is independent of the presence of time.

. . . .

From the mathematical point of view, ***time*** *is a structure on the set of observables (the foliation that I called a time structure).*

From the physical point of view, time is the experimental fact *that, in the nature as we see it, meaningful observables are always constructed out of two partial observables. That is, it is the experimental fact (not a priori required), that knowing the position of a particle is meaningless unless we also know "at what time" a particle was at that position.*

In the formulation of the theory, this experimental fact is coded in the time structure of the set of observables. If true observables are composed of correspondences of partial observables, one of which is the reading of a clock, then the set of true observables can be foliated into one-parameter families that are given by the same partial observables at different clock readings.

From an operational point of view, mechanics is perfectly well defined in the absence of this time structure. It will describe a world (maybe one slightly unfamiliar to us) in which observables are not arranged along one-parameter lines, in which they have no such time structure (a kind of fixed-time world), or have more complicated structures. We must not confuse the psychological difficulties in visualizing such worlds with their logical impossibility.

. . . Heisenberg states, observables, measurement theory – none of these require time.

The notion of probability does not require time. . . .

What I am proposing is that there may exist a coherent description of a system in the framework of standard quantum mechanics even if it does not have a standard "time evolution."

Why should we be interested in mechanics with no time structure? Because general relativity is *a system (a classical system) with no time structure. At least, it has no clearly defined time structure.*

. . . What we have to do is simple: "forget time."

Kuchar: *For myself, I want to see observables changing along my world line and therefore associated with individual leaves of a foliation. In that sense, the problem of time is shifted to the problem of constucting an appropriate class of quantities one would like to call observables. Now, what I would like to call observables probably differs from what Carlo Rovelli would like to call observables. Carlo may like to restrict that term to constants of motion, while I would like to use variables that depend on a time hypersurface. Of course, both of us know that there is a technical way of translating my observables into his observables. However, it is difficult to subject such a translated observable to an actual observation. In principle, of course, it does not matter at what instant of time one measures a constant of motion. But the constants of motion that are translations of my observables are much too complicated when expressed in terms of the coordinates and the momenta at the time of measurement. You thus have a hard time to design an apparatus that would measure such a constant of motion at a time different from the moment for which it was originally designed.*

Smolin: *Now, as I discussed above, and as Jim Hartle argues at length, there can be no strict implementation of the principle of conservation of probability for a time that this the value of a dynamical variable of a quantum system. Therefore, a sensible measurement theory for quantum cosmology can only be constructed if there is a time variable that is not a dynamical variable of the quantum system that describes the universe.*

Does this mean that quantum cosmology is impossible, since there is no possibility of a clock outside of the system?

There is, as far as I know, exactly one loophole in this argument, which is the one exploited by the program of intrinsic time. This is that one coordinate on the phase space of general relativity might be singled out and called time in such a way that the states, represented by functions on the configuration space, could be read as time-dependent functions over a reduced configuration space from which the privileged time coordinate is excluded.

Jokes:

Kuchar: Because Leibniz didn't believe in the ontological significance of time, he dropped the letter "t" from his name.

Smolin: Is that true?

Kuchar: Yes! He spelled his name with a "z".

DeWitt: It's a good thing that he did believe in space because the "z" would've gone too.

Of course, time is "Zeit" in German, which complicates things.

4) Abhay Ashtekar,"Old problems in the light of new variables", the volume above.

   Carlo Rovelli, "Loop representation in quantum gravity", the volume above.

   Lee Smolin, "Nonperturbative quantum gravity via the loop representation", the volume above.

These are a bit more technical papers that give nice introductions to various aspects of the loop representation.

Week 28

January 4, 1994

I think I'll finally break my New Year's resolution to stop making a fool of myself on the net, and attempt an explanation of some things I am just learning a bit about, namely, Teichmüller space and moduli space. These are concepts that string theorists often throw around, and when I first heard of them in that context I immediately dimissed them as just another example of how physicists were learning far too much mathematics for their own good. I take it all back! They are, in fact, beautifully simple pieces of mathematics suited to physics in 2-dimensional spacetime. Two dimensions is low enough that one can often actually understand exactly what's going on in problems that become infinitely more tricky in higher dimensions. So even if one doesn't "believe" in 2-dimensional physics the way the string theorists do – for them, physics happens on the worldsheet of the string, which is 2-dimensional – it's worth learning as a kind of textbook case.

Everything I know so far is culled from the following sources, which by no means form an exhaustive or even optimal set of references:

1) Y. Imayoshi and M. Taniguchi, *An Introduction to Teichmüller spaces*, Springer, Berlin, 1991.

2) Joe Harris, "An introduction to the moduli space of curves", in *Mathematical Aspects of String Theory* (proceedings of a conference at UC San Diego in 1986), ed. S. T. Yau, World Scientific Press, 1987.

3) R. C. Penner, "The moduli space of punctured surfaces", same volume.

4) John L. Harer, "The cohomology of the moduli space of curves", in *Theory of Moduli (lectures given at the 3rd 1985 session of C.I.M.E. at Mondecatini Terme, Italy)*, ed. E. Sernesi, Springer Lecture Notes in Mathematics **1337**, 1988.

There are a number of ways of describing Teichmüller space and moduli space. Maybe the easiest is this. Start with the surface of a doughnut with g handles, or as the experts say, a "surface of genus g". We can make this into a "Riemann surface" if we cover it with lots of patches, or "charts," each of which looks just like part the complex plane (imagine a little piece of graph paper), and such that the change of coordinates function relating overlapping patches is analytic, in the usual sense of complex variables. The simplest Riemann surface is the Riemann sphere, which is of genus zero; one gets this by taking the complex plane and sticking on one more point, "∞" – think of a sphere with "∞" as the north pole. If we have a Riemann surface, we can tell whether a complex-valued function on it is analytic, simply by working locally in charts, so we can do complex analysis as usual on a doughnut with lots of handles as long as we make it into a Riemann surface! Since the main point of my article will be to provide the reader with lots of buzzwords, I should add that making a surface of genus g into a Riemann surface is called "giving it a complex structure."

Now, how many ways are there to give a surface of genus g a complex structure? First of all we need a good notion of when two Riemann surfaces are "the same". They must, of course, have the same genus, but there must also be a 1-1 and onto function from one to the other that is everywhere analytic, with an analytic inverse. (Again, it makes sense to say such a function is analytic, since we can cover each Riemann surface with charts that we can think of as bits of the complex plane.) Such a mapping is called a "biholomorphic mapping" – holomorphic just being another word for analytic – and if we want to sound fancy, we say that the two Riemann surfaces are "biholomorphically equivalent."

Well, there's a famous old theorem of Riemann that for genus 0, there is only ONE way to do it; any Riemann surface of genus 0 is biholomorphically equivalent with the Riemann sphere. But for higher genus there are infinitely many essentially different ways to give a surface of genus g a complex structure. In fact, we can imagine a big fat "space" of all ways. This is the moduli space of genus g!

The first key problem in the theory is to "get our hands on" moduli space; that is, to describe it quite concretely. String theory provided a lot of motivation for doing this very well, since the worldsheet of a string – that is, the string viewed in spacetime – is just a surface, and Feynman path integrals in string theory involve integrating over all complex structures for this surface. To do integrals over moduli space we need to bring it down to earth!

To do so, it's awfully handy to get involved with Teichmüller space. Note that moduli space can be thought of as the space of equivalence classes of complex structures on a fixed surface of genus g, where two complex structures are deemed "the same" if they are biholomorphically equivalent. Teichmüller space is defined using a more fine-grained notion of "the same". Note that any biholomorphic mapping is a diffeomorphism, that is, a smooth mapping with a smooth inverse. In fact, it must also be orientation-preserving, since an orientation-reversing map like complex conjugation can never be holomorphic! Henceforth I will always mean "orientation-preserving diffeomorophism" when I speak of diffeomorphisms of a surface.

Now, some diffeomorphisms are "connected to the identity" and some aren't. We say a diffeomorphism f is connected to the identity if there is a smooth 1-parameter family of diffeomorphisms starting at f and ending at the identity diffeomorphism. In other words, a diffeomorphism is connected to the identity if you can do it "gradually" without ever having to cut the surface. To really understand this you need to know some diffeomorphisms that *aren't* connected to the identity. Here's how to get one: start with your surface of genus $g > 0$, cut apart one of the handles along a circle, give one handle a 360-degree twist, and glue the handles back together! This is called a Dehn twist.

Anyway, Teichmüller space may be defined as the space of equivalence classes of complex structures on a fixed surface of genus g, where two complex structures are counted as the same if they are biholomorphically equivalent *by a diffeomorphism connected to the identity*.

A good way of understanding the relation between Teichmüller space and moduli space is this. Define the mapping class group (of genus g) to be the group of diffeomorphisms of a surface of genus g modulo the subgroup of those connected to the identity. A beautiful fact is that this group is generated by Dehn twists! In other words, given any diffeomorphism of a surface, you can get it by first doing a bunch of Dehn twists and then doing a diffeomorphism connected to the identity. Since the mapping class group is finitely generated one should think of it as a discrete group. In fact, folks know what the relations between the generators are, too, and these are also very beautiful. I guess good places to read about this stuff are the first paper that gave an explicit presentation of mapping class groups:

5) A. Hatcher and W. Thurston, "A presentation of the mapping class group of a closed, orientable surface", *Topology* **19** (1980), 221–237.

and the simplified treatment in

6) Bronislaw Wajnryb, "A simple presentation for the mapping class group of an orientable surface", *Israel J. Math.* **45** (1983), 157–174.

Actually, though, I must admit my only acquaintance with mapping class groups comes from leafing through

7) Joan S. Birman, *Braids, Links, and Mapping Class Groups*, Annals of Mathematics Studies **82**, Princeton University Press, 1974.

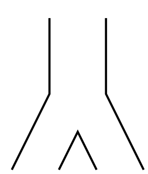
As one can gather from this title there is a close connection between mapping class groups and the braid group and knot theory, which is the main reason why string theory allowed Witten to get new insights into knots. (The more mundane connection, namely that one ties knots out of string, seems largely unexplored, but see "Week 18".) Let me not digress into this fascinating realm, however! The point I want to make here is just that:

> *The mapping class group acts on Teichmüller space, and the quotient by this group action is moduli space.*

Anyone used to how math goes should find this pretty believable, but let me explain: given a diffeomorphism of our surface of genus g, we can use it as a kind of "coordinate transformation" to turn one complex structure into another. So the group of diffeomorphisms acts on Teichmüller space, but, given how Teichmuller space is defined, the subgroup of diffeomorphisms connected to the identity acts trivially. Thus the mapping class group acts on Teichmüller space. By how moduli space is defined, two points in Teichmüller space define the same point in moduli space iff one is obtained from another by an element of the mapping class group.

Now the good thing about Teichmüller space is that it has very nice coordinates on it, called Fenchel-Nielsen coordinates, which reveal it to be diffeomorphic to $\mathbb{R}^{6g-6}$ when $g > 1$. (The case $g = 0$ is utterly dull, since Teichmüller space is a point, and the case $g = 1$ is beautifully treated using the fact that any Riemann surface of genus 1 is biholomorphically equivalent to the quotient of the complex plane by a lattice, relating this case to the subject of elliptic functions, as touched upon in "Week 13". I should also add that "Week 13" indicates, at least in the $g = 1$ case, why moduli space is often called the "moduli space of curves.")

Let me say how these coordinates go, rather sketchily, just so the mysterious number $6g - 6$ becomes not so mysterious! Take your surface of genus g – just think of it as a doughnut with n holes – and cut it up into "pairs of pants," that is, pieces that look like



from above. Topologically, a pair of pants is just a sphere with three discs cut out of it! A more dignified term for a pair of pants is a "trinion," by the way.

The idea now is to describe the complex structure on each pair of pants separately, and then describe how the pairs of pants are glued together. Now, it turns out that the complex structures on each pair of pants are very easily described (up to biholomorphic equivalences connected to the identity). It takes 3 positive real numbers. There's a unique metric on the original surface pants that is compatible with the complex structure (i.e. is a "Kähler metric") and has curvature equal to -1. This is called a hyperbolic metric, as in hyperbolic geometry. Then, we can cut the surface into pairs of pants along circles that are geodesics relative to this metric. To describe the complex structure on each pair of pants we simply need to measure the lengths of the 3 bounding circles; these are called the "geodesic length functions". In other words, if your pair of pants was hyperbolic, a tailor would only need to measure you waistlength and the lengths around the two cuffs, not the inseams!

Now it's a fun exercise to show that we can chop up a surface of genus $g > 1$ into exactly $2g - 2$ pairs of pants. Doing so, moreover, requires that we cut the surface along $3g - 3$ circles. (Draw some pictures!) Thus, we have a total of $3g - 3$ geodesic length functions. However, we also need to describe how the pairs of pants are attached to each other. We can glue them together with any amount of twisting, and this twisting is a real-valued parameter. So there are $3g - 3$ "twisting parameters" required to describe how the pairs of pants are attached. We thus have a total of $6g - 6$ parameters, the Fenchel-Nielsen coordinates, and Teichmüller space is diffeomorphic to $\mathbb{R}^{6g-6}$ (since the positive real numbers are diffeomorphic to $\mathbb{R}$ itself).

I think I want to quit here but not before making a few random remarks.

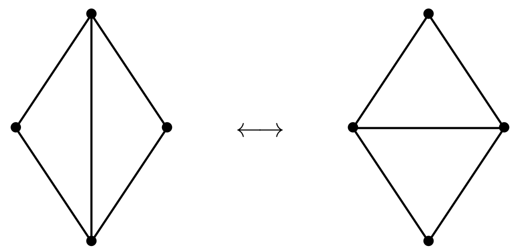
First, there's another description of Teichmüller space which gives it a triangulation, i.e., describes it as a bunch of high-dimensional tetrahedra (simplices) glued together. Harer's paper gives a nice quick sketch of this construction; the buzzword to look for is "quadratic differentials." The nice thing about this is that the mapping class group action respects this triangulation so we get a triangulation of moduli space.

Second, quite recent work by Penner:

8) R. C. Penner, "Universal constructions in Teichmüller theory", *Adv. Math.* **98** (1993), 143–215.

shows how to fit the Teichmüller spaces for different genus g into a single universal object. This was directly motivated by string theory, but the basic idea is (I think) simply that there should be some sense in which one can go "continously" from genus $g + 1$ to genus g by making one handle smaller and smaller (with respect to the hyperbolic metric) until it goes away.

Third, when studying the triangulation of Teichmüller space one repeatedly runs across a certain "Pachner move" which goes from one triangulation of a surface to another:



which reminds me of lattice field theories in 2 dimensions (see "Week 16" for an explanation) and the "pentagon diagram" showing how to get between the 5 simplest ways to triangulate a pentagon using this move (see, for example, Figure 3 in Penner's paper above, or Figure 3.2 of Harer's paper). The pentagon diagram appears both in Moore and Seiberg's famous paper on string theory and chopping up surfaces into pairs of pants:

9) G. Moore and S. Seiberg, "Classical and quantum conformal field theory", *Commun. Math. Phys.* **123** (1989), 177–254

and in category theory, with the relationship *there* now pretty well understood in terms of "modular tensor categories" — see e.g.

10) Louis Crane, "2-d physics and 3-d topology", *Commun. Math. Phys.* **135** (1991) 615-640.

Unfortunately, I haven't been able to get any string guru to sit down and really clarify how the triangulations of Teichmüller space fit in. Lest the reader wonder what the heck I'm going on about, the idea is that category theory provides a marvelous way to unify the profusion of mathematical structures that are coming up these days, and if we ever understood everything in those terms, it would all seem much less confusing.

Week 29

January 14, 1994

I'm awfully busy this week, but feel like attempting to keep up with the pile of literature that is accumulating on my desk, so this will be a rather terse description of papers. All of these papers are related to my current obsession with "higher-dimensional algebra" and its applications to physics.

1) Ruth J. Lawrence, "On algebras and triangle relations", to appear in *Proc. Top. & Geom. Methods in Field Theory* (1992), eds. J. Mickelsson and O. Pekonen, World Scientific, Singapore.

   Ruth J. Lawrence, "A presentation for Manin and Schechtman's higher braid groups", available as MSRI preprint `04129-91`.

   Ruth J. Lawrence, "Triangulations, categories and extended topological field theories", to appear in *Quantum Topology*, eds. L. Kauffman and R. A. Baadhio, World Scientific, Singapore, 1993.

   Ruth J. Lawrence, "Algebras and triangle relations", Harvard U. preprint.

Many people are busily trying to extend the remarkable relationship between knot theory and physics, which is essentially a feature of 3 dimensions, to higher dimensions. Since the 3-dimensional case required the development of new branches of algebra (namely, quantum groups and braided tensor categories), it seems that the higher-dimensional cases will require still further "higher-dimensional algebra." One approach, which is still being born, involves the use of "n-categories," which are generalizations of braided tensor categories suited for higher-dimensional physics. (See for example the papers by Crane in "Week 2", by Kapranov and Voevodsky in "Week 4", by Fischer and Freed (separately) in "Week 12", and the one by Gordon, Power, and Street below.) Lawrence has instead chosen to invent "n-algebras," which are vector spaces equipped with operations corresponding to the ways one can subdivide $(n-1)$-dimensional simplices into more such simplices. (See the paper by Chung, Fukuma and Shapere in "Week 16" for some of the physics motivation here.)

These alternative approaches should someday be seen as different aspects of the same thing, but there as yet I know of no theorems to this effect, so there is a lot of work to be done. Even more importantly, there is a lot of work left to be done about inventing *examples* of these higher-dimensional structures. For example, there may eventually be general results on "boosting" n-algebras to $(n+1)$-algebras, or n-categories to $(n+1)$-categories, which will explain how generally covariant physics in n-dimensional spacetime relates to the same thing in one higher dimension. So far, however, all we have is a few examples, which are not even clearly related to each other. For example, Crane calls this boosting process "categorification" and has done it starting with the braided tensor category of representations of a quantum group. Lawrence, on the other hand, shows how to construct some 3-algebras from quantum groups. And Freed has given a general procedure for "boosting" using path integral methods that are not yet rigorous in the most interesting cases.

2) R. Gordon, A. J. Power, and Ross Street, *Coherence for Tricategories*, Memoirs of the American Mathematical Society **558**, Providence, Rhode Island, 1995.

An "n-category" is a kind of algebraic structure that has "objects," "morphisms" between objects, "2-morphisms" between morphisms, and so on up to "n-morphisms." However, the *correct* definition of an n-category for the purposes of physics is still unclear! I gave a rough explanation of the importance of 2-categories in physics in "Week 4", where I discussed Kapranov and Voevodsky's nice definition of braided tensor 2-categories. However, it seems likely that we will need to understand the situation for larger n as well. This paper makes a big step in this direction, by defining "tricategories" (what I might call "weak 2-categories") and proving a "strictification" or "coherence" result analogous to the result that every braided tensor category is equivalent to a "strict" one. The result is, however, considerably more subtle, as it involves a special way of defining the tensor product of 2-categories due to Gray:

3) John W. Gray, *Formal Category Theory: Adjointness for 2-Categories*, Springer Lecture Notes in Mathematics **391**, Springer, Berlin, 1974.

John W. Gray, "Coherence for the tensor product of 2-categories, and braid groups", in *Algebras, Topology, and Category Theory*, eds. A. Heller and M. Tierney, Academic Press, New York, 1976, pp. 63–76.

Briefly speaking, Gordon–Power–Street use a category they call "Gray," the category of all 2-categories, made into a symmetric monoidal closed category using a modified version of Gray's tensor product. Then they show that every tricategory (as defined by them) is "triequivalent" to a category enriched over Gray.

4) J. M. Maillet, "On pentagon and tetrahedron equations", available as `hep-th/9312037`.

Maillet shows how to obtain solutions of the tetrahedron equations from solutions of pentagon equations; all these geometrical equations are part of the theory of 2-categories, and this is yet another example of a "boosting" construction as alluded to above.

5) David Yetter, "Homologically twisted invariants related to (2+1)- and (3+1)-dimensional state-sum topological quantum field theories", available as `hep-th/9311082`.

Let me simply quote the abstract: "Motivated by suggestions of Paolo Cotta-Ramusino's work at the physical level of rigor relating BF theory to the Donaldson polynomials, we provide a construction applicable to the Turaev–Viro and Crane–Yetter invariants of *a priori* finer invariants dependent on a choice of (co)homology class on the manifold." The dream is that this would give a state-sum formula for the Donaldson polynomials, but Yetter is careful to avoid claiming this! A while back, Crane and Yetter showed how to get 4-dimensional TQFTs from certain 3d TQFTs by another kind of "boosting" procedure related to those mentioned above, but the resulting TQFT in 4-dimensions did not by itself yield interesting new invariants of 4-manifolds. The procedure Yetter describes here generalizes the earlier work by allowing the inclusion of an embedded 2-manifold.

Week 30

January 14, 1994

For the most part, this is a terse description of some papers dealing with quantum gravity. Some look to be quite important, but as I have not had time to read them as thoroughly as I would like, I won't say much.

First, however, let me note some books:

1) Silvan S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger and Tomonaga*, Princeton U. Press, Princeton, 1994.

Back in the 1930s there was a crisis in physics: nobody knew how to reconcile quantum theory with special relativity. This book describes the history of how people struggled with this problem and achieved a marvelous, but flawed, solution: quantum electrodynamics (QED). Marvelous, because it made verified predictions of unparalleled accuracy, involves striking new concepts, and gave birth to beautiful new mathematics. Flawed, only because nobody yet knows for sure whether the theory is mathematically well-defined – for reasons profoundly related to physics at ultra-short distance scales. This story should give some inspiration to those currently attempting to reconcile quantum theory with general relativity! Feynman, Schwinger, and Tomonaga won Nobel prizes for QED, but Dyson was also instrumental in inventing the theory, and the book is mainly a story of these 4 men.

2) Bruce Stephenson, *The Music of the Heavens: Kepler's Harmonic Astronomy*, Princeton U. Press, Princeton, 1994.

   Bruce Stephenson, *Kepler's Physical Astronomy*, Princeton U. Press, Princeton, 1994.

Kepler's work on astronomy was in part based on the notion of the "music of the spheres," and in his Harmonice Mundi (1619) he sought to relate planetary velocities to the notes of a chord. He was also fascinated with geometry, and sought to relate the radii of the planetary orbits to the Platonic solids. While this may seem a bit silly nowadays, it's clear that this faith that mathematical patterns pervade the heavens was a crucial part of how Kepler found his famous laws of planetary motion. Also important, of course, was his use of what we would now call "physical" reasoning to understand the heavens – that is, the use of analogies between the motions of heavenly bodies and that of ordinary terrestial matter. But even this is not as straightforward as one might hope, since (Stephenson argues in the second book) this physical reasoning was what we would now consider incorrect, even though it led to valid laws. More inspiration for those now struggling amid error to understand what the universe is really like!

3) Louis Kauffman and Sostenes Lins, *Temperley–Lieb Recoupling Theory and Invariants of 3-Manifolds*, Annals of Mathematics Studies **133**, Princeton U. Press, Princeton, 1994.

I described this briefly in "Week 17", before I had spent much time on it. Let me recall the main point: in the late 80's Jones invented a new invariant of knots and links in ordinary 3d space, but then Witten recognized that this invariant came from a quantum field theory, and thus could be extended to obtain an invariant of links in arbitrary 3d manifolds. (In particular, taking the link to be empty, one obtains a 3-manifold invariant.) In fact, there is a whole family of such invariants, essentially one for each semisimple Lie algebra, and Jones original example corresponded to the case $\mathfrak{su}(2)$. In this case the combinatorics of the invariants are so simple that one can write a nice exposition in which one forgets the underlying, fairly sophisticated, mathematical physics (quantum groups, conformal field theory and the like) and simply presents the "how-to" using a kind of diagrammatic calculus known as "skein relations," or what Kauffman calls "Temperley–Lieb recoupling theory." That is the approach the authors take here. The curious reader will naturally want to know more! For example, anyone familiar with quantum theory and "$6j$ symbols" will sense that this kind of thing is lurking in the background, and indeed, it is.

Now for the papers:

4) C. Rovelli and L. Smolin, "The physical hamiltonian in quantum gravity", available as gr-qc/9308002.

   H. A. Morales-Tecotl and C. Rovelli, "Fermions in quantum gravity", available as gr-qc/9401011.

The Rovelli–Smolin loop variables program proceeds apace! In the former paper, Rovelli and Smolin consider quantum gravity coupled to a scalar matter field which plays the role of a clock. (Using part of the system described to play the role of a clock is a standard idea for dealing with the "problem of time," which arises in quantum theories on spacetimes having no preferred coordinates, like quantum gravity. However, getting this idea to actually work is not at all easy. For a bit on this issue see "Week 27".) Only after choosing this "clock field" can one work out a Hamiltonian for the theory, write down the analog of Schrödinger's equation, and examine the dynamics. Before, there is only a "Hamiltonian constraint" equation, also known as the Wheeler–DeWitt equation.

In the latter paper, Rovelli and Marales-Tecotl discuss how to include fermions. The beautiful thing here is that fermions are described in the loop representation by the *ends* of arcs, while pure gravity is described by loops. This is completely analogous to the old string theory of mesons, in which mesons were represented as arcs of "string" – the gluon field – connecting two fermionic "ends" – the quarks.

5) C. Di Bartolo, R. Gambini, J. Griego and J. Pullin, "Extended loops: a new arena for nonperturbative quantum gravity", available as gr-qc/9312029.

For a while now, Gambini and collaborators have been developing a modified version of the loop representation that appears to be especially handy for doing perturbative calculations (perturbing in the coupling constant, that is, Newton's gravitational constant – *not* perturbing about a fixed flat "background" spacetime, which is regarded as a "no-no" in this philosophy). The mathematical basis for this "extended" loop representation is something quite charming in itself: it amounts to embedding the loop group into an (infinite-dimensional) Lie group. The "perturbative" calculations described above are thus analogous to how one uses Lie algebras to study Lie groups. In fact, this analogy is a deep one, since the extended loop representation also permits perturbative calculations in Chern–Simons theory, allowing one to calculate "Vassiliev invariants" starting just from Lie-algebraic data. In fact this was done by Bar-Natan (cf "Week 3"), who was using the extended loop representation without particularly knowing about that fact!

This paper puts the extended loop representation to practical use by finding some new solutions to the quantum version of Einstein's equations. These solutions are essentially Vassiliev invariants! (See also the paper by Gambini and Pullin listed in "Week 23").

6) Domenico Giulini, "Ashtekar variables in classical general relativity", available as `gr-qc/9312032`.

This was a lecture given at the 117th WE-Heraeus Seminar: "The Canonical Formalism in Classical and Quantum General Relativity", 13-17 September 1993, Bad Honnef, Germany, the goal of which was to give an introduction to Ashtekar's "new variables" for general relativity.

Week 31

February 18, 1994

Well, I'm really busy these days trying to finish up a big project, hence the low number of "Weeks" per week, but papers are piling up, and there are some pretty interesting ones, so I thought I'd quickly mention a few. This bunch will mainly concern quantum gravity.

1) Louis Crane, "Possible implications of the quantum theory of gravity", available as hep-th/9402104.

This is one paper that everyone can read and enjoy, for although one may find it too close to science fiction for comfort, it is far more interesting than most science fiction. Louis Crane has been doing a lot of excellent work on topological quantum field theory for the last few years, strongly advocating the use of category theory as a unifying principle in physics (essentially as an extension of the concept of symmetry embodied in *group* theory), but this is quite different in flavor.

To begin with, Lee Smolin, one of the originators of the loop representation of quantum gravity, has been spending the last year or so writing a book in a popular style, to be entitled "Life and Light," which tours the cosmos and makes some interesting speculations on "evolutionary cosmology." These speculations are based on 2 hypotheses.

> *A. The formation of a black hole creates "baby universe," the final singularity of the black hole tunnelling right on through to the initial "Big Bang" singularity of the new universe thanks to quantum effects.*

While this must undoubtedly seem outre to anyone unfamiliar with the sort of thing theoretical physicists amuse themselves with these days, in a recent review article by John Preskill on the information loss paradox for black holes, he reluctantly concluded that this was the *most conservative* solution of that famous problem! Recall the problem: if a black hole evaporates its mass away via Hawking radiation, and that radiation is pure blackbody radiation, hence carries none of the information about the matter that originally formed the black hole, one does not have conservation of information, or more technically speaking, the time evolution is not unitary, since a pure state is evolving into a mixed state. Hawking's original solution to this problem was to bite the bullet and accept the nonunitarity, even though it goes against the basic principles of quantum theory. This appears in:

2) S. W. Hawking, "Black holes and thermodynamics", *Phys. Rev. D* **13** (1976), 191–197.

The "baby universe" solution simply says that the matter seeds a baby universe and the information goes *there*. Many other solutions have been proposed; two recent review articles are

3) J. Preskill, "Do black holes destroy information?", available as hep-th/9209058.

4) Don Page, "Black hole information", in *Proceedings of the 5th Canadian Conference on General Relativity and Relativistic Astrophysics, University of Waterloo, 13–15 May, 1993*, eds. R. B. Mann and R. G. McLenaghan, World Scientific, Singapore, 1994. Available as hep-th/9305040.

Personally, I am a complete agnostic about this problem, since it rests upon so many phenomena that are hypothesized but not yet observed, and since any solution would require a theory of quantum gravity. I am merely reporting the ideas of respected physicists! In any event, the second hypothesis is:

> *B. Certain parameters of the baby universe are close to but different than those of the parent universe.*

The notion that certain physical facts that appear as "laws" are actually part of the state of the univese has in fact been rather respectable since the application of spontaneous symmetry breaking to the Weinberg–Salaam model of electroweak interactions, part of the standard model. (Again, being my usual cautious self, I must note that a crucial piece of evidence for this model, the Higgs boson, has not yet been seen.) The notion of spontaneous symmetry breaking has become quite popular in particle physics and is a key component of all current theories, such as GUTs or string theory, that attempt to model the messy heap of observed particles and interactions by some pristinely symmetrical Lagrangian. The spontaneous symmetry breaking would be expected to have occured shortly after the Big Bang, when it got cool enough, much as a hot piece of iron will randomly settle upon some direction of magnetization as its temperature fall below the Curie temperature. One application of this notion to cosmology is already widely popular, namely, inflation. In fact, pursuing the analogy with magnetic domains, i.e. small regions with different directions of magnetization, cosmologists have spend a fair amount of energy thinking about "domain walls," "cosmic strings," monopoles and other defects that might occur as residues of this cooling-down process.

So again, while the idea must seem wild to anyone who has not encountered it before, physicists these days are fairly comfortable with the idea that certain "fundamental constants" could have been other than they were. As for the constants of a baby universe being close to, but different than, those of the parent universe, there is as far as I know no suggested mechanism for this. This is perhaps the weakest link in Smolin's argument (though I haven't seen his book yet). But it is at least conceivable.

Now, given these hypotheses a marvellous consequence ensues: Darwinian evolution! Those universes whose parameters are such that many black holes are formed will have many progeny, so the constants of physics can be expected to be "tuned" for the formation of many black holes. As Smolin emphasizes, while the hypotheses A and B may seem impossible to test directly at present, we do at least have a hope of testing this consequence. He has studied the marvellously intricate process of star formation in the galaxy and attempted to see whether altering the constants of physics appear "tuned" for maximizing black hole production, and he argues in his book that they do appear so tuned. Of course, this is an extremely delicate business, since our understanding of galaxy formation, star formation and black hole formation even in *this* universe is still rather weak — much less for other conceivable universes in which the fundamental constants take different values.

Crane enters the fray at this point, and proposes an additional conjecture:

> *SUCCESSFUL INDUSTRIAL CIVILIZATIONS WILL EVENTUALLY CREATE BLACK HOLES.*

(The capital letters are his.) He breaks it up into two parts for us:

> *SUBCONJECTURE 1: SUCCESSFUL INDUSTRIAL CIVILIZATIONS WILL EVENTUALLY WANT TO MAKE BLACK HOLES*

and

> *SUBCONJECTURE 2: SUCCESSFUL INDUSTRIAL CIVILIZATIONS WILL EVENTUALLY BE ABLE TO PRODUCE BLACK HOLES.*

and argues for each. The result, as any good evolutionist will recognize, is a kind of feedback loop whereby intelligence and baby universe formation both affect each other. Indeed, Crane calls his hypothesis the "meduso-anthropic hypothesis," after certain jellyfish with a two-stage life cycle in which medusids produce polyps and vice versa. This has the charm of completely destroying the usual approach (dare I say "paradigm"?) of physics in which the parameters of the universe are regarded as indifferent to the existence of intelligence. Of course, the anthropic hypothesis is a previous attempt to breach this firewall, but a much less dramatic one, since the only role intelligence plays in that is *noticing* the laws of the universe.

At this point let me leave off with a quote from Crane's paper:

> *"It is not hard to see that if these ideas are true, they will be the victims of abuse to dwarf quantum healing and even quantum golf. That is not sufficient reason to ignore them."*

and let me *gradually* turn towards slightly less speculative realms, eventually finishing with some papers containing rigorous mathematics! To begin with, some more on black hole entropy:

5) Leonard Susskind, "Some speculations about black hole entropy in string theory", available as hep-th/9309145.

   Leonard Susskind and J. Uglum,"Black hole entropy in canonical quantum gravity and superstring theory", available as hep-th/9401070.

The fact that the entropy of a black hole is (at least under certain circumstances) proportional to the area of its event horizon is a curious relationship between general relativity, quantum field theory and statistical mechanics that many people believe to pointing somewhere, but unfortunately nobody is sure where. Part of the reason is that the standard derivations are somewhat indirect, and the event horizon is not a physical object, so the sense in which it is the locus of entropy is difficult to understand. These authors suggest that in string theory it can be explained in terms of open strings having both ends attached to the horizon.

6) C. R. Stephens, G. 't Hooft and B. F. Whiting, "Black hole evaporation without information loss", available as gr-qc/9310006.

This is an attempt to make black holes radiate away and disappear in a manner that preserves unitarity. I've been too busy to read it. And now for some wormholes:

7) Hoi-Kwong Lo, Kai-Ming Lee, and John Preskill, "Complementarity in Wormhole Chromodynamics", available as hep-th/9308044.

Let me just quote the abstract and note that there is probably some quite interesting topology to be obtained by applying this sort of idea to mathematics:

> *The electric charge of a wormhole mouth and the magnetic flux "linked" by the wormhole are non-commuting observables, and so cannot be simultaneously diagonalized. We use this observation to resolve some puzzles in wormhole electrodynamics and chromodynamics. Specifically, we analyze the color electric field that results when a colored object traverses a wormhole, and we discuss the measurement of the wormhole charge and flux using Aharonov-Bohm interference effects. We suggest that wormhole mouths may obey conventional quantum statistics, contrary to a recent proposal by Strominger.*

Finally, lest the mathematicians think I have abandoned ship, some rigorous results:

8) Piotr T. Chrusciel, ""No hair" theorems — folklore, conjectures, results", available as gr-qc/9402032.

The famous "no hair" theorem says that in general relativity static black hole solutions are determined by very few parameters — typically listed as mass, angular momentum and charge in "rest frame" of the black hole. There have been many attempts to extend this result, especially because no *actual* black hole is likely to be utterly static, since it presumably formed at some time. I have not read this but Chrusciel is a very careful person so I expect it will be up to the standards of his nice review of work on the cosmic censorship hypothesis,

9) Piotr T. Chrusciel, "On uniqueness in the large of solutions of Einstein's equations ("Strong cosmic censorship")", in *Mathematical Aspects of Classical Field Theory*, Contemp. Math. **132**, eds. Gotay, Marsden and Moncrief, American Mathematical Society, Providence, Rhode Island, 1992, pp. 235–274.

Addendum: See "Week 33" for a paper by Smolin on his evolutionary cosmology theory. His book came out in 1997 under the title *The Life of the Cosmos'* — see "Week 101" for details.

> The thing that makes things and the thing that makes things fall apart — they're the same thing. Entropy maximization!
>
> — *Chris Lee*

Week 32

March 10, 1994

Well, I visited Georgia Tech last week to spread the gospel of "knots and quantum gravity," and came across a most fascinating development. I'm sure readers of `sci.math` and `sci.math.research` have taken note of the *New York Journal of Mathematics*. This is one of the first refereed electronic journals of mathematics. Neil Calkin at Georgia Tech is helping to start up another one — the *Electronic Journal of Combinatorics*. Though it's unlikely, perhaps some among you are still unaware (or unconvinced) of how essential it is that we develop fully refereed free-of-charge electronic journals of mathematics and physics. The first and most obvious reason is that computer-based media offer all sorts of flexibility that print media lack — more on this later. But the other reason is that the monopoly of print journals *must* be broken.

For example, U. C. Riverside does not subscribe to *Communications in Mathematical Physics*, despite the fact that this is *the* crucial journal in that subject, because this journal costs $3,505 a year! The ridiculous price is, of course, in part precisely because this is the crucial journal in that subject, in part because the journal uses antiquated and expensive production methods involving paper, and in part because, being a big operation, it is basically run by a publishing house rather than mathematical physicists. Luckily, with the advent of the preprint mailing lists `hep-th` and `gr-qc`, I don't *need* to read Communications in Mathematical Physics very often! I simply get my list of abstract each day by email from Los Alamos, and send email to get the papers I want, in LaTeX or TeX form. The middleman has been cut out — at least for the moment.

One problem with preprint mailing lists, though, is that the preprints have not gone through the scrutiny of the referee process. This is, frankly, much less of a problem for the *readers* than is commonly imagined, because this scrutiny is less intense than people who have never refereed papers think! Many refereed papers have errors, and I would personally feel very uncomfortable using a result unless I either understood the proof or knew that most experts believed it. The real need for refereed journals, in my slightly cynical opinion, is that academics need *refereed publications* to advance in their jobs: the people who give tenure, promotions etc. cannot be expected to read and understand ones papers. This is, of course, also the reason for other strange phenomena, such as the idea of *counting* somebody's publications to see how good they are. We need only count Alexander Abian's publications to see the limitations of this approach.

Eventually, a few birds may be killed with one stone by means of "seals of approval" or SOAPs, which are being widely discussed by people interested in the "information superhighway," or — let's call a spade a spade — the Internet. For more on these, check out the `newsgroup comp.interpedia`, or read material about the Xanadu project. The idea here is that eventually we will have a good system whereby people can append comments to documents, such as "there is an error in the proof of Lemma 1.5, which can be fixed as follows..." or simply various seals of approval, functioning similarly to the seal of approval ones paper obtains by being published in a journal. E.g., one could make ones paper available by ftp or some other protocol, and "submitting it to a journal" might amount to asking for a particular SOAP, with various SOAPs carrying various amounts of prestige, and so on.

Of course, journals also function as a kind of information "hub" or central access point. We all know that to find out what's the latest trend in particle physics, it suffices to glance at *Nuc. Phys. B* and certain other journals, and so on. It is not clear that the function of "hub" and the function of SOAP need be combined into a single institution, once the onerous task of transcribing ideas onto dead trees and shipping them all around the world becomes (at least partially) obsolete.

It is also not at all certain whether, in the long run, the monopolistic power of journals to charge large fees for accessing information will be broken by the new revolutions in technology. This is, of course, just one small facet of the political/economic struggle for control over information flow that is heating up these days, at least in the U.S., among telephone companies, cable TV stations, computer networks such as Compuserve, etc. etc.. If mathematicians and physicists don't think about these issues, someone else who has will wind up defining the future for us.

Anyway, for now it seems to make good sense to start refereed journals of mathematics and physics that are accessible electronically, free of charge, over the Internet. Not too long ago one would commonly hear the remark "... but of course nobody would ever want to do that, because..." followed by some reason or other, reminiscent of how **clearly** nobody would want to switch from horses to automobiles because then one would have to build **gas stations all over the place** — obviously too much bother to be worthwhile. Now, however, things are changing and the new electronic journals are getting quite respectable lists of editors, and they seem to have a good chance of doing well. I urge everyone to support free-of-charge electronic journals by submitting good papers!

Let me briefly describe the electronic journals I mentioned above. The New York Journal's chief editor is Mark Steinberger, at SUNY Albany, `mark@sarah.albany.edu`. The journal covers algebra, modern analysis, and geometry/topology. Access is through anonymous ftp, gopher and listserv, the latter being (I believe) a mailing list protocol. One can subscribe by sending email to `listserv@albany.edu` or `listserv@albany.bitnet`; if you want abstracts for all the papers, the body of your email should read

```
subscribe NYJMTH-A <your full name>
```

but you can also subscribe to only certain topics (one of the great things about electronic journals — one can only begin to imagine the possibilities inherent in this concept!), as follows:

Algebra:

```
subscribe NYJM-ALG <your full name>
```

Analysis:

```
subscribe NYJM-AN  <your full name>
```

Geometry/Topology:

```
subscribe NYJM-TOP <your full name>
```

Papers are accepted in amstex and amslatex, and when you get papers you get a `.dvi` file.

The *Electronic Journal of Combinatorics* is taking a somewhat more ambitious approach that has me very excited. Namely, they are using Mosaic, a hypertext interface to the WWW (World-Wide Web). This means, to technical illiterates such as myself, that if you can ever get your system manager to get the software running, you can see a "front page" of the journal, with the names of the articles and other things underlined (or in color if you're lucky). To go to any underlined item, you simply click your mouse on it. In fact, you can use this method to navigate throughout the whole WWW, which is a vast, sprawling network of linked files, including — so I hear — "This Week's Finds"! In the *Electronic Journal of Combinatorics*, when you click on an article you will see it in postscript form, pretty equations and all. You can also get yourself a copy and print it out. Neil showed me all this stuff and my mouth watered! The danger of this ambitious approach is of course that folks who haven't kept up with things like the WWW may find it intimidating... for a while. It's actually not too complicated.

This journal will be widely announced pretty soon. The editor in chief is Herbert S. Wilf, `wilf@central.cis.upenn.edu`, and the managing editor is Neil Calkin, `calkin@math.gatech.edu`. It boasts an impressive slate of editors (even to me, who knows little about combinatorics), including Graham, Knuth, Rota and Sloane. To get browse the journal, which is presently under construction, you just do the following if you can use Mosaic: "Click on the button marked 'Open' and then type in `http://math34.gatech.edu:8080/Journal/journalhome.html`". To *get* Mosaic, do anonymous ftp to `ftp.ncsa.uiuc.edu` and `cd` to `Web/Mosaic_binaries` — and then you're on your own, I just tried it and there were too many people on! — but Neil says it's not too hard to get going. I will try as soon as I have a free day.

"Ahem!" the reader comments. "What does this have to do with mathematical physics?" Well, seeing how little I'm being paid, I see nothing wrong with interpreting my mandate rather broadly, but I should add the following.

1) There are periodic posts on `sci.physics` about physics on the WWW; there's a lot out there, and to get started one always try the following. The information below is taken from Scott Chase's physics FAQ:

```
* How to get to the Web

    If you have no clue what WWW is, you can go over the Internet with
telnet to info.cern.ch (no login required) which brings you to the WWW
Home Page at CERN. You are now using the simple line mode browser. To move
around the Web, enter the number given after an item.

* Browsing the Web

    If you have a WWW browser up and running, you can move around
more easily. The by far nicest way of "browsing" through WWW uses the
X-Terminal based tool "XMosaic". Binaries for many platforms (ready for
use) and sources are available via anonymous FTP from ftp.ncsa.uiuc.edu
in directory Web/xmosaic.  The general FTP repository for browser
software is info.cern.ch (including a hypertext browser/editor for
NeXTStep 3.0)

* For Further Information
```

```
   For questions related to WWW, try consulting the WWW-FAQ: Its most
recent version is available via anonymous FTP on rtfm.mit.edu in
/pub/usenet/news.answers/www-faq , or on WWW at
http://www.vuw.ac.nz:80/overseas/www-faq.html

   The official contact (in fact the midwife of the World Wide Web)
is Tim Berners-Lee, timbl@info.cern.ch. For general matters on WWW, try
www-request@info.cern.ch or Robert Cailliau (responsible for the "physics"
content of the Web, cailliau@cernnext.cern.ch).
```

And: 2) there are rumors, which I had better not elaborate on yet, about an impending electronic journal of mathematical physics! I eagerly await it!

Okay, just a bit about actual mathematical physics per se this time.

1) Carlo Rovelli, "On quantum mechanics", available as hep-th/9403015.

This interesting paper suggests that reason why we are constantly arguing about the meaning of quantum mechanics, despite the fact that it works perfectly well and is obviously correct, is that we are making a crucial conceptual error. Rovelli very nicely compares the problem to special relativity before Einstein did his thing: we had Lorentz transformations, but they seemed very odd and paradoxical, because the key notion that the space/time split was only defined *relative to a frame* (or "observer" if we wish to anthropomorphize) was lacking. Rovelli proposes that in quantum mechanics the problem is that we are lacking the notion that the *state* of a system is only defined relative to an observer. (The "Wigner's friend" puzzle is perhaps the most obvious illustration here.) What, though, is an observer? Any subsystem of a quantum system, says Rovelli; there is no fundamental "observer-observed distinction." This fits in nicely with some recent work by Crane and myself on quantum gravity, so I like it quite a bit, though it is clearly not the last word on this issue (nor does Rovelli claim it to be).

2) Alan D. Rendall, "Adjointness relations as a criterion for choosing an inner product", available as gr-qc/9403001.

The inner product problem in quantum gravity is an instance of a general, very interesting mathematics problem, namely, of determining an inner product on a representation of a star-algebra, by demanding that the representation be a star-representation. Rendall has proved some very nice results on this issue.

3) Maxim Kontsevich and Yuri Manin, "Gromov-Witten classes, quantum cohomology, and enumerative geometry", available as hep-th/9402147.

I will probably never understand this paper so I might as well mention it right away. Kontsevich's work on knot theory, and Manin's work on quantum groups and (earlier) instantons is extremely impressive, so I guess they can be forgiven for their interest in algebraic geometry. (A joke.) Let me simply quote:

> *"The paper is devoted to the mathematical aspects of topological quantum field theory and its applications to enumerative problems of algebraic geometry. In particular, it contains an axiomatic treatment of Gromov-Witten classes, and a*

discussion of their properties for Fano varieties. Cohomological Field Theories are defined, and it is proved that tree level theories are determined by their correlation functions. Applications to counting rational curves on del Pezzo surfaces and projective spaces are given."

Week 33

May 10, 1994

With tremendous relief, I have finished writing a book, and will return to putting out This Week's Finds on a roughly weekly basis. Let me briefly describe my book, which took so much more work than I had expected... and then let me start catching up on listing some of the stuff that's cluttering my desk!

1) John Baez and Javier de Muniain, *Gauge Fields, Knots and Gravity*, World Scientific Press, Singapore, 1994.

This book is based on a seminar I taught in 1992–93. We start out assuming the reader is familiar with basic stuff — Maxwell's equations, special relativity, linear algebra and calculus of several variables — and try to prepare the reader to understand recent work on quantum gravity and its relation to knot theory. It proved difficult to do this well in a mere 460 pages. Lots of tantalizing loose ends are left dangling. However, there are copious references so that the reader can pursue various subjects further.

2) Asher Peres, *Quantum Theory: Concepts and Methods*, Kluwer Academic Publishers, Amsterdam, 1994.

As Peres notes, there are many books that teach students how to solve quantum mechanics problems, but not many that tackle the conceptual puzzles that fascinate those interested in the foundations of the subject. His book aims to fill this gap. Of course, it's impossible not to annoy people when writing about something so controversial; for example, fans of Everett will be distressed that Peres' book contains only a brief section on "Everett's interpretation and other bizarre interpretations". However, the book is clear-headed and discusses a lot of interesting topics, so everyone should take a look at it.

Schrödinger's cat, Bell's inequality and Wigner's friend are old chestnuts that everyone puzzling over quantum theory has seen, but there are plenty of popular new chestnuts in this book too, like "quantum cryptography", "quantum teleportation", and the "quantum Zeno effect", all of which would send shivers up and down Einstein's spine. There are also a lot of gems that I hadn't seen, like the Wigner–Araki–Yanase theorem. Let me discuss this theorem a bit.

Roughly, the WAY theorem states that it is impossible to measure an operator that fails to commute with an additive conserved quantity. Let me give an example to clarify this and then give the proof. Say we have a particle with position q and momentum p, and a measuring apparatus with position Q and momentum P. Let's suppose that the total momentum $p+P$ is conserved — which will typically be the case if we count as part of the "apparatus" everything that exerts a force on the particle. Then as a consequence of the WAY theorem we can see that (in a certain sense) it is impossible to measure the particle's position q; all we can measure is its position *relative* to the apparatus, $q - Q$.

Of course, whenever a "physics theorem" states that something is impossible one must peer into it and determine the exact assumptions and the exact result! Lots of people have gotten in trouble by citing theorems that seem to show something is impossible without reading the fine print. So let's see what the WAY theorem *really* says!

It assumes that the Hilbert space for the system is the tensor product of the Hilbert space for the thing being observed — for short, let's call it the "particle" — and the Hilbert space for the measuring apparatus. Assume also that A and B are two observables belonging to the observed system, while C is an observable belonging to the measuring apparatus; suppose that $B + C$ is conserved, and let's try to show that we can only measure A if it commutes with B. (Our assumptions automatically imply that A commutes with C, by the way.)

So, what do we mean when we speak of "measuring A"? Well, there are various things one might mean. The simplest is that if we start the combined system in some tensor product state $u(i) \otimes v$, where v is the "waiting and ready" state of the apparatus and $u(i)$ is a state of the observed system that's an eigenvector of A:

$$Au(i) = a(i)u(i),$$

then the unitary operator U corresponding to time evolution does the following:

$$\mathrm{U}(u(i) \otimes v) = u(i) \otimes v(i)$$

where the state $v(i)$ of the apparatus is one in which it can be said to have measured the observable A to have value $a(i)$. E.g., the apparatus might have a dial on it, and in the state $v(i)$ the dial reads "$a(i)$". Of course, we are really only justified in saying a measurement has occurred if the states $v(i)$ are *distinct* for different values of i.

Note: here the WAY theorem seems to be restricting itself to nondestructive measurements, since the observed system is remaining in the state $u(i)$. If you go through the proof you can see to what extent this is crucial, and how one might modify the theorem if this is not the case.

Okay, we have to show that we can only "measure A" in this sense if A commutes with B. We are assuming that $B + C$ is conserved, i.e.,

$$U^*(B + C)U = B + C.$$

First note that

$$\langle u(i), [A, B]u(j)\rangle = (a(i) - a(j))\langle u(i), Bu(j)\rangle.$$

On the other hand, since A and B only act on the Hilbert space for the particle, we also have

$$\begin{aligned}\langle u(i), [A, B]u(j)\rangle &= \langle u(i) \otimes v, [A, B]u(j)\rangle \\ &= \langle u(i) \otimes v, [A, B + C]u(j)\rangle \\ &= (a(i) - a(j))\langle u(i) \otimes v, (B + C)u(j) \otimes v\rangle\end{aligned}$$

It follows that if $a(i) - a(j)$ isn't zero,

$$\begin{aligned}\langle u(i), Bu(j)\rangle &= \langle u(i) \otimes v, (B + C)u(j) \otimes v\rangle \\ &= \langle u(i) \otimes v, U^*(B + C)Uu(j) \otimes v\rangle \\ &= \langle u(i) \otimes v(i), (B + C)u(j) \otimes v(j)\rangle \\ &= \langle u(i), Bu(j)\rangle\langle v(i), v(j)\rangle + \langle u(i), u(j)\rangle\langle v(i), Cv(j)\rangle\end{aligned}$$

but the second term vanishes since $u(i)$ are a basis of eigenvectors and $u(i)$ and $u(j)$ correspond to different eigenvalues, so

$$\langle u(i), Bu(j)\rangle = \langle u(i), Bu(j)\rangle\langle v(i), v(j)\rangle$$

which means that either $\langle v(i), v(j)\rangle = 1$, hence $v(i) = v(j)$ (since they are unit vectors), so that no measurement has really been done, OR that $\langle u(i), Bu(j)\rangle = 0$, which means (if true for all i, j) that A commutes with B.

So, we have proved the result, using one extra assumption that I didn't mention at the start, namely that the eigenvalues $a(i)$ are distinct.

I can't say that I really understand the argument, although it's easy enough to follow the math. I will have to ponder it more, but it is rather interesting, because it makes more precise (and general) the familiar notion that one can't measure *absolute* positions, due to the translation-invariance of the laws of physics; this translation invariance is of course what makes momentum be conserved. (What I just wrote makes me wonder if someone has shown a classical analog of the WAY theorem.)

Anyway, here's the table of contents of the book:

Chapter 1: Introduction to Quantum Physics

1-1. The downfall of classical concepts

1-2. The rise of randomness

1-3. Polarized photons

5-2. Incomplete tests and partial traces

5-3. The Schmidt decomposition

5-4. Indistinguishable particles

5-5. Parastatistics

5-6. Fock space

5-7. Second quantization

5-8. Bibliography

Chapter 6: Bell's Theorem

6-1. The dilemma of Einstein, Podolsky, and Rosen

6-2. Cryptodeterminism

6-3. Bell's inequalities

6-4. Some fundamental issues

6-5. Other quantum inequalities

6-6. Higher spins

6-7. Bibliography

Chapter 7: Contextuality

7-1. Nonlocality versus contextuality

7-2. Gleason's theorem

7-3. The Kochen–Specker theorem

7-4. Experimental and logical aspects of inseparability

7-5. Appendix: Computer test for Kochen-Specker contradiction

7-6. Bibliography

Chapter 8: Spacetime Symmetries

8-1. What is a symmetry?

8-2. Wigner's theorem

8-3. Continuous transformations

8-4. The momentum operator

8-5. The Euclidean group

8-6. Quantum dynamics

8-7. Heisenberg and Dirac pictures

8-8. Galilean invariance

8-9. Relativistic invariance

8-10. Forms of relativistic dynamics

8-11. Space reflection and time reversal

8-12. Bibliography

12-7. The measurement of time

12-8. Time and energy complementarity

12-9. Incompatible observables

12-10. Approximate reality

12-11. Bibliography

3) Bernd Bruegmann, "Loop representations", available as gr-qc/9312001.

This is a nice review article on loop representations of gauge theories. Anyone wanting to jump into the loop representation game would be well advised to start here.

4) Lee Smolin, "The fate of black hole singularities and the parameters of the standard models of particle physics and cosmology", available as gr-qc/9404011.

This is about Smolin's "evolutionary cosmology" scenario, which I already discussed in "Week 31". Let me just quote the abstract:

> *A cosmological scenario which explains the values of the parameters of the standard models of elementary particle physics and cosmology is discussed. In this scenario these parameters are set by a process analogous to natural selection which follows naturally from the assumption that the singularities in black holes are removed by quantum effects leading to the creation of new expanding regions of the universe. The suggestion of J. A. Wheeler that the parameters change randomly at such events leads naturally to the conjecture that the parameters have been selected for values that extremize the production of black holes. This leads directly to a prediction, which is that small changes in any of the parameters should lead to a decrease in the number of black holes produced by the universe. On plausible astrophysical assumptions it is found that changes in many of the parameters do lead to a decrease in the number of black holes produced by spiral galaxies. These include the masses of the proton, neutron, electron and neutrino and the weak, strong and electromagnetic coupling constants. Finally,this scenario predicts a natural time scale for cosmology equal to the time over which spiral galaxies maintain appreciable rates of star formation, which is compatible with current observations that* $\Omega = .1 - .2$

Week 34

May 24, 1994

A bit of a miscellany this week. . . .

1) "Algorithms for quantum computation: discrete log and factoring", extended abstract by Peter Shor.

There has been a bit of a stir about this paper; since I know Peter Shor's sister I was able to get a copy and see what it was really all about.

Quantum computers are so far just a theoretical possibility. It's easiest to see why machines that take advantage of quantum theory might be efficient at computation if we think in terms of path integrals. In Feynman's path-integral approach to quantum theory, the probability of getting from state A at time zero to state B some later time is obtained by integrating the exponential of the action

$$\exp(iS/\hbar)$$

over *all* paths from A to B, and then taking the absolute value squared. (Here we are thinking of states A and B that correspond to points in the classical configuration space.) We can think of the quantum system as proceeding along all paths simultaneously; it is the constructive or destructive interference between paths due to the phases $exp(iS/\hbar)$ that makes certain final outcomes B more likely than others. In many situations, there is massive destructive interference except among paths very close to those which are critical points of the action S; the latter are the *classical* paths. So in a sense, a classical device functions as it does by executing all possible motions; motions far from those satisfying Newton's laws simply cancel out by destructive interference! (There are many other ways of thinking about quantum theory; this one can be difficult to make mathematically rigorous, but it's often very handy.)

This raises the idea of building a computer that would take advantage of quantum theory by trying out all sorts of paths, but making sure that paths that give the wrong answer cancel out! It seems that Feynman was the first to seriously consider quantum computation:

2) Richard Feynman, "Simulating physics with computers", *International Journal of Theoretical Physics*, **21**, (1982), 467–488.

but by now quite a bit of work has been done on the subject, e.g.:

3) P. Benioff, "Quantum mechanical Hamiltonian models of Turing machines", *J. Stat. Phys.* **29** (1982), 515–546.

   D. Deutsch, "Quantum theory, the Church–Turing principle and the universal quantum computer", *Proc. R. Soc. Lond. A* **400** (1985), 96–117.

   D. Deutsch, Quantum computational networks, *Proc. R. Soc. Lond. A* **425** (1989), 73–90.

   D. Deutsch and R. Jozsa, Rapid solution of problems by quantum computation, *Proc. R. Soc. Lond. A* **439** (1992), 553–558.

E. Bernstein and U. Vazirani, Quantum complexity theory, *Proc. 25th ACM Symp. on Theory of Computation* (1993), 11–20.

A. Berthiaume and G. Brassard, editors, "The quantum challenge to structural complexity theory", in *Proc. 7th IEEE Conference on Structure in Complexity Theory*, 1992.

A. Yao, "Quantum circuit complexity", in *Proc. 34th IEEE Symp. on Foundations of Computer Science*, 1993.

Thanks to this work, there are now mathematical definitions of quantum Turing machines and the class "BQP" of problems that can be solved in polynomial time with a bounded probability of error. This allows a mathematical investigation of whether quantum computers can, in principle, do things more efficiently than classical ones. Shor shows that factoring integers is in BQP. I won't try to describe how, as it's a bit technical and I haven't really comprehended it. Instead, I'd like say a couple things about the *practicality* of building quantum computers, since people seem quite puzzled about this issue.

There are, as I see it, two basic problems with building quantum computers. First, it seems that the components must be carefully shielded from unwanted interactions with the outside world, since such interactions can cause "decoherence", that is, superpositions of the computer states will evolve into superpositions of the system consisting of the computer together with what it's interacting with, which from the point of view of the computer alone are the same as mixed states. This tends to ruin the interference effects upon which the advantages of quantum computation are based.

Second, it seems difficult to incorporate error-correction mechanisms in a quantum computer. Without such mechanisms, slight deviations of the Hamiltonian of the computer from the design specifications will cause the computation to drift away from what was intended to occur. Luckily, it appears that this drift is only *linear* rather than *exponential* as a function of time. (This impression is based on some simplifications that might be oversimplifications, so anyone who wants to build a quantum computer had better ponder this issue carefully.) Linear increase of error with time sets an upper bound on how complicated a computation one could do before the answer is junk, but if the rate of error increase was made low enough, this might be acceptable.

Certainly as time goes by and computer technology becomes ever more miniaturized, hardware designers will have to pay ever more attention to quantum effects, for good or for ill! (Vaughn Pratt estimates that quantum effects will be a serious concern by 2020.) The question is just whether they are only a nuisance, or whether they can possibly be harnessed. Some designs for quantum computers have already been proposed (sorry, I have no reference for these), and seeing whether they are workable should be a very interesting engineering problem, even if they are not good enough to outdo ordinary computers.

4) Lee Smolin and Chopin Soo, "The Chern–Simons invariant as the natural time variable for classical and quantum cosmology", available as gr-qc/9405015.

Let me just quote the abstract on this one:

> *We propose that the Chern–Simons invariant of the Ashtekar–Sen connection (or its imaginary part in the Lorentzian case) is the natural internal time coordinate*

for classical and quantum cosmology. The reasons for this are: 1) It is a function on the gauge and diffeomorphism invariant configuration space, whose gradient is orthogonal to the two physical degrees of freedom, in the metric defined by the Ashtekar formulation of general relativity. 2) The imaginary part of the Chern–Simons form reduces in the limit of small cosmological constant, Λ, and solutions close to DeSitter spacetime, to the York extrinsic time coordinate. 3) Small matter-field excitations of the Chern–Simons state satisfy, by virtue of the quantum constraints, a functional Schrödinger equation in which the matter fields evolve on a DeSitter background in the Chern–Simons time. We then propose this is the natural vacuum state of the theory for nonzero Λ. 4) This time coordinate is periodic on the Euclidean configuration space, due to the large gauge trans- formations, which means that physical expectation values for all states in non-perturbative quantum gravity will satisfy the KMS condition, and may then be interpreted as thermal states. Finally, forms for the physical hamiltonian and inner product are suggested and a new action principle for general relativity, as a geodesic principle on the connection superspace, is found.

5) "Symplectic geometry", a series of lectures by Mikhail Gromov, compiled by Richard Brown, edited by Robert Miner.

Symplectic geometry is the geometry of classical phase spaces. That is, it's the geometry of spaces on which one can take Poisson brackets of functions in a manner given locally by the usual formulas. Gromov has really revolutionized the subject, and these lectures look like a good place to begin learning what is going on. There is also an appendix on contact geometry (another aspect of classical physics) based on a lecture by Eliashberg.

Week 35

June 5, 1994

1) Alexander Grothendieck, *Pursuing Stacks (A la Poursuite des Champs)*, 1983, 593 pages. Available as https://thescrivener.github.io/PursuingStacks/ps-online.pdf.

I owe Ronnie Brown enormous thanks for sending this to me (before it was available online). Grothendieck is mainly famous for his work on algebraic geometry, in which he introduced the concept of "schemes" to provide a modern framework for the subject. He was also interested in reformulating the foundations of topology, which is reflected in *Pursuing Stacks*. This is a long letter to Quillen, inspired by Quillen's 1967 book *Homotopical Algebra*. It's a fascinating mixture of visionary mathematics, general philosophy and a bit of personal chat. Let me quote a bit:

> *I write you under the assumption that you have not entirely lost interest for those foundational questions you were looking at more than fifteen years ago. One thing which strikes me, is that (as far as I know) there has not been any substantial progress since — it looks to me that understanding of the basic structures underlying homotopy theory, or even homological algebra only, is still lacking — probably because the few people who have a wide enough background and perspective enabling them to feel the main questions, are devoting their energies to things which seem more directly rewarding. Maybe even a wind of disrepute for any foundational matters whatever is blowing nowadays! In this respect, what seems to me even more striking than the lack of proper foundations for homological and homotopical algebra, is the absence I daresay of proper foundations for topology itself! I am thinking here mainly of the development of a context of "tame" topology, which (I am convinced) would have on the everyday technique of geometric topology (I use this expression in contrast to the topology of use for analysts) a comparable impact or even a greater one, than the introduction of the point of view of schemes had on algebraic geometry. The psychological drawback here I believe is not anything like messyness, as for homological and homotopical algebra (or for schemes), but merely the inrooted inertia which prevents us so stubbornly from looking innocently, with fresh eyes, upon things, without being dulled and emprisoned by standing habits of thought, going with a familiar context —* too *familiar a context!*

One reason why I'm interested in this letter is that Grothendieck seems to have understood the importance of "higher algebraic structures" before most people. Recently, interest in these has been heating up, largely because of the recent work on "extended topological quantum field theories." The basic idea is that, just as a traditional quantum field theory is (among other things) a representation of the symmetry group of spacetime, a topological quantum field theory is a representation of a more sophisticated algebraic structure, a "cobordism n-category." An n-category is a wonderfully recursive sort of thing in which there are objects, 1-morphisms between objects, 2-morphisms between morphisms, and so on up to n-morphisms. In a "cobordism n-category" the objects are 0-manifolds, the 1-morphisms are 1-dimensional manifolds that go between 0-manifolds (as the unit interval goes from one endpoint to another), the 2-morphisms are 2-dimensional manifolds that go between 1-manifolds (as a cylinder goes from on circle to another), etc. In practice one must work with manifolds admitting certain types of "corners", and equipped with extra structures that topologists and physicist like, such as orientations, framings, or spin structures. The idea is that all the cutting-and-pasting constructions in n-dimensional topology can be described algebraically in the cobordism n-category. To wax rhapsodic for a moment, we can think of an n-category as exemplifying the notion of "ways to go between ways to go between ways to go between. . . .. ways to go between things," and cobordism n-categories are the particular n-categories that algebraically encode the possibilities along these lines that are implicit in the notion of n-dimensional spacetime.

Now, the problem is that the correct *definition* of an n-category is a highly nontrivial affair! And it gets more complicated as n increases! A 0-category is nothing but a bunch of objects. In other words, it's basically just a *set*, if we allow ourselves to ignore certain problems about classes that are too big to qualify as sets. A 1-category is nothing but a category. Recall the definition of a category:

A category consists of a set of **objects** and a set of **morphisms**. Every morphism has a **source** object and a **target** object. (A good example to think of is the category in which the objects are sets and the morphisms are functions. If $f\colon X \to Y$, we call X the source and Y the target.) Given objects X and Y, we write $\mathrm{Hom}(X, Y)$ for the set of morphisms from X to Y (i.e., having X as source and Y as target).

The axioms for a category are that it consist of a set of objects and for any 2 objects X and Y a set $\mathrm{Hom}(X, Y)$ of morphisms from X to Y, and

1. Given a morphism g in $\mathrm{Hom}(X, Y)$ and a morphism f in $\mathrm{Hom}(Y, Z)$, there is morphism which we call $f \circ g$ in $\mathrm{Hom}(X, Z)$. (This binary operation $\circ$ is called **composition**.)

2. Composition is associative: $(f \circ g) \circ h = f \circ (g \circ h)$.

3. For each object X there is a morphism id_X from X to X, called the **identity on** X.

4. Given any f in $\mathrm{Hom}(X, Y)$, $f \circ \mathrm{id}_X = f$ and $\mathrm{id}_Y \circ f = f$.

Now, a 2-category is more complicated. There are objects, 1-morphisms, and 2-morphisms, and one can compose morphisms and also compose 2-morphisms. There is, however, a choice: one can make ones 2-category "strict" and require that the rules 2) and 4) above hold for the 1-morphisms and 2-morphisms, or one can require them "literally" only for the 2-morphisms, and allow the 1-morphisms some slack. Technically, one can choose between "strict" 2-categories, usually just called 2-categories, or "weak" ones, which are usually called "bicategories."

What do I mean by giving the 1-morphisms some "slack"? This is a very important aspect of the n-categorical philosophy. . . I mean that in a 2-category one has the option of replacing *equations* between 1-morphisms by *isomorphisms* — that is, by 2-morphisms that have inverses! The basic idea here is that in many situations when we like to pretend things are equal, they are really just *isomorphic*, and we should openly admit this when it occurs. So, for example, in a "weak" 2-category one doesn't have associativity of 1-morphisms. Instead, one has "associators", which are 2-morphisms like this:

$$a_{f,g,h}: (f \circ g) \circ h \to f \circ (g \circ h)$$

In other words, the associator is the *process of rebracketing* made concrete. Now, when one replaces equations between 1-morphisms by isomorphisms, one needs these isomorphisms to satisfy "coherence relations" if we're going to expect to be able to manipulate them more or less as if they *were* equations. For example, in the case of the associators above, one can use associators to go from

$$f \circ (g \circ (h \circ k))$$

to

$$((f \circ g) \circ h) \circ k$$

in two different ways: either

$$f \circ (g \circ (h \circ k)) \to (f \circ g) \circ (h \circ k) \to ((f \circ g) \circ h) \circ k$$

or

$$f \circ (g \circ (h \circ k)) \to f \circ ((g \circ h) \circ k) \to (f \circ (g \circ h)) \circ k \to ((f \circ g) \circ h) \circ k$$

Actually there are other ways, but in an important sense these are the basic two. In a "weak" 2-category one requires that these two ways are equal... i.e., this is an identity that the associator must satisfy, known as the pentagon identity. This is one of the first examples of a coherence relation. It turns out that if this holds, *all* ways of rebracketing that get from one expression to another are equal. (Here I'm being rather sloppy, but the precise result is known as Mac Lane's theorem.)

To learn about weak 2-categories, which as I said people usually call bicategories, try:

2) J. Benabou, *Introduction to Bicategories*, Springer Lecture Notes in Mathematics, **47**, Springer, Berlin, 1968.

Now, one can continue this game, but it gets increasingly complex if one goes the "weak" route. In a "weak n-category" the idea is to replace all basic identities that one might expect between j-morphisms, such as the associative law, by $(j+1)$-isomorphisms. These, in turn, satisfy certain "coherence relations" that are really not equations, but $(j+2)$-morphisms, and so on... up to level n. This becomes so complicated that only recently have "weak 3-categories" been properly defined, by Gordon, Power and Street, who call them tricategories (see "Week 29").

A bit earlier, Kapranov and Voevodsky succeeded in defining a certain class of weak 4-categories, which happen to be called "braided monoidal 2-categories" (see "Week 4"). The interesting thing, you see, which justifies getting involved in this business, is that a lot of topology *automatically pops out* of the definition of an n-category. In particular, n-categories have a lot to do with n-dimensional space. A weak 3-category with only one object and one 1-morphism is usually known as a "braided monoidal category," and the theory of these turns out to be roughly the same as the study of knots, links and tangles! The "braided monoidal 2-categories" of Kapranov and Voevodsky are really just weak

4-categories with only one object and one 1-morphism. (The reason for the term "2-category" here is that since all one has is 2-morphisms, 3-morphisms, and 4-morphisms, one can pretend one is in a 2-category in which those are the objects, morphisms, and 2-morphisms.)

In any event, these marvelous algebraic structures have been cropping up more and more in physics (see especially Crane's stuff listed in "Week 2" and Freed's paper listed in "Week 12"), so I got ahold of a copy of Grothendieck's letter and have begun trying to understand it.

Actually, it's worth noting that these n-categorical ideas have been lurking around homotopy theory for quite some time now. As Grothendieck wrote:

> *At first sight it had seemed to me that the Bangor group had indeed come to work out (quite independently) one basic intuition of the program I had envisioned in those letters to Larry Breen — namely, that the study of n-truncated homotopy types (of semisimplicial sets, or of topological spaces) was essentially equivalent to the study of so-called n-groupoids (where n is any natural integer). This is expected to be achieved by associating to any space (say) X its "fundamental n-groupoid" $\Pi_n(X)$, generalizing the familiar Poinaré fundamental groupoid for $n = 1$. The obvious idea is that 0-objects of $\Pi_n(X)$ should be the points of X, 1-objects should be "homotopies" or paths between points, 2-objects should be homotopies between 1-objects, etc. This $\Pi_n(X)$ should embody the n-truncated homotopy type of X, in much the same way as for $n = 1$ the usual fundamental groupoid embodies the 1-truncated homotopy type. For two spaces X, Y, the set of homotopy-classes of maps $X \to Y$ (more correctly, for general X, Y, the maps of X into Y in the homotopy category) should correspond to n-equivalence classes of n-functors from $\Pi_n(X)$ to $\Pi_n(Y)$ — etc. There are some very strong suggestions for a nice formalism including a notion of geometric realization of an n-groupoid, which should imply that any n-groupoid is n-equivalent to a $\Pi_n(X)$. Moreover when the notion of an n-groupoid (or more generally of an n-category) is relativized over an arbitrary topos to the notion of an n-gerbe (or more generally, an n-stack), these become the natural "coefficients" for a formalism of non commutative cohomological algebra, in the spirit of Giraud's thesis.*

The "Bangor group" referred to includes Ronnie Brown, who has done a lot of work on "ω-groupoids". A while back he sent me a nice long list of references on this subject; here are some that seemed particularly relevant to me (though I haven't looked at all of them).

3) G. Abramson, J.-P.Meyer and J. Smith, "A higher dimensional analogue of the fundamental groupoid", in *Recent Developments of Algebraic Topology*, RIMS Kokyuroku **781**, Kyoto, 1992, pp. 38–45,

F. Al-Agl, "Aspects of multiple categories", University of Wales Ph.D. Thesis, 1989.

F.Al-Agl and R.J.Steiner, "Nerves of multiple categories", *Proc. London Math. Soc.* **66** (1992), 92–128.

N. Ashley, "Simplicial T-complexes", University of Wales Ph.D. Thesis, 1976, published as *Simplicial T-complexes and Crossed Complexes: a Non-Abelian Version of a Theorem of Dold and Kan*, Dissertationes Mathematicae **265**, 1988.

H.J. Baues, *Algebraic Homotopy*, Cambridge University Press, Cambridge, 1989.

H.J. Baues, *Combinatorial Homotopy and* 4*-Dimensional Complexes*, De Gruyter, Berlin, 1991.

L. Breen, "Bitorseurs et cohomologie non-Ablienne", in *The Grothendieck Festschrift: a collection of articles written in honour of the 60th birthday of Alexander Grothendieck, Vol. I*, ed. P. Cartier *et al.*, Birkhäuser, Basel, 1990, pp. 401–476.

R. Brown, "Higher dimensional group theory", in *Low-dimensional topology*, ed. R. Brown and T.L.Thickstun, *London Math. Soc. Lect. Notes* **46**, Cambridge University Press, 215–238, 1982.

R. Brown, "From groups to groupoids: a brief survey", *Bull. London Math. Soc.*, **19**, 113–134, 1987.

R. Brown, *Elements of Modern Topology*, McGraw Hill, Maidenhead, 1968.

R. Brown, *Topology: a Geometric Account of General Topology, Homotopy Types and the Fundamental Groupoid*, Ellis Horwood, Chichester, 1988.

R. Brown, "Some problems in non-Abelian homological and homotopical algebra", in *Homotopy Theory and Related Topics: Proceedings Kinosaki*, ed. M. Mimura, Springer Lecture Notes in Mathematics **1418**, 1990, Springer, Berlin pp. 105–129.

R. Brown and P. J. Higgins, "The equivalence of ω-groupoids and cubical T-complexes", *Cah. Top. Geom. Diff.* **22** (1981), 349–370.

R. Brown and P. J. Higgins, "The equivalence of ∞-groupoids and crossed complexes", *Cah. Top. Geom. Diff.* **22** (1981), 371–386.

R. Brown and P. J. Higgins, "The algebra of cubes", *J. Pure Appl. Algebra* **21** (1981), 233–260.

R. Brown and P. J. Higgins, "Tensor products and homotopies for ω-groupoids and crossed complexes", *J. Pure Appl. Algebra*, **47** (1987), 1–33.

R. Brown and J. Huebschmann, "Identities among relations", in *Low-dimensional topology*, eds. R. Brown and T. L. Thickstun, London Math. Soc. Lect. Notes **46**, Cambridge University Press, Cambridge, 1982, pp. 153–202.

R. Brown, "Generalised group presentations", *Trans. Amer. Math. Soc.*, **334** (1992), 519–549.

M. Bullejos, A. M. Cegarra and J. Duskin, "On cat^n-groups and homotopy types", *J. Pure Appl. Algebra* **86** (1993), 135–154.

M. Bullejos, P. Carrasco and A. M. Cegarra, "Cohomology with coefficients in symmetric cat^n-groups. An extension of Eilenberg-Mac Lane's classification theorem." Granada Preprint, 1992.

P. J. Ehlers and T. Porter, "From simplicial groupoids to crossed complexes", UCNW Maths Preprint 92.19, 1992.

D. W. Jones, "Polyhedral T-complexes", University of Wales Ph.D. Thesis, 1984; published as *A General Theory of Polyhedral Sets and Their Corresponding T-complexes*, Dissertationes Mathematicae **266**, 1988.

M. M. Kapranov and V. Voevodsky, “Combinatorial-geometric aspects of polycategory theory: pasting schemes and higher Bruhat orders (list of results)”, *Cah. Top. Geom. Diff. Cat.* **32** (1991), 11–27.

M. M. Kapranov and V. Voevodsky, “∞-groupoids and homotopy types”, *Cah. Top. Geom. Diff. Cat.* **32**, 29–46, 1991.

M. M. Kapranov and V. Voevodsky, “2-categories and Zamolodchikov tetrahedra equations”, preprint, 1992.

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

G. Nan Tie, “Iterated W and T-groupoids”, *J. Pure Appl. Algebra* **56** (1989), 195–209.

T. Porter, “A combinatorial definition of ∞-types”, *Topology* **22** (1993), 5–24.

S. J. Pride, “Identities among relations of group presentations”, in *Proc. Workshop on Group Theory from a Geometrical Viewpoint*, eds. E. Ghys, A. Haefliger, A. Verjodsky, World Scientific (1991), 687–716.

R. Steiner, “The algebra of directed complexes”, University of Glasgow Math Preprint, 1992.

A. Tonks, “Cubical groups which are Kan”, *J. Pure Appl. Algebra* **81** (1992), 83–87.

A. Tonks and R. Brown, “Calculation with simplicial and cubical groups in Axiom”, UCNW Math Preprint `93.04`.

A. R. Wolf, “Inherited asphericity, links and identities among relations”, *J. Pure Appl. Algebra* **71** (1991), 99–107.

Since the month of March last year, so nearly a year ago, the greater part of my energy has been devoted to a work of reflection on the **foundations of non-commutative (co)homological algebra**, or what is the same, after all, of homotopic algebra. These reflections have taken the concrete form of a voluminous stack of typed notes, destined to for then first volume (now being finished) of a work in two volumes to be published by Hermann, under the overall title ***Pursuing Stacks***. I now foresee (after successive extensions of the initial project) that the manuscript of the whole of the two volumes, which I hope to finish definitively in the course of this year, will be about 1500 typed pages in length. These two volumes are moreover for me the first in a vaster series, under the overall title ***Mathematical Reflections***, in which I intend to develop some of the themes sketched in the present report

— *Alexander Grothendieck, Sketch of a Program (1983)*

Week 36

July 15, 1994

I am attempting to keep my nose to the grindstone these days, in part since I'm getting ready for the Knots and Quantum Gravity session of the Marcel Grossman meeting on general relativity, which will take place at Stanford the week after next (I will report any interesting news I hear out there), and in part to make up for earlier stretches of laziness on my part.... Nonetheless, I feel I should describe a few new papers on topological quantum field theories.

The real reason for physicists' interest in topological quantum field theories (TQFTs) is that we sorely need a mathematical framework that quantum gravity might fit into. It's likely, however, that quantum gravity won't be much like the TQFTs people have studied so far. The existing TQFTs tend to be "exactly soluble" and have finite-dimensional state spaces; quantum gravity is likely to be different. Still, any experience in studying quantum field theories that don't rely on "fixed background structures" like a fixed spacetime metric is likely to be worth having. Also, quantum gravity appears to be tied mathematically to simpler TQFTs in a variety of ways. In particular, the Ashtekar formulation of quantum gravity is closely related to a 4-dimensional TQFT variously known as "$B \wedge F$ theory," "BF theory," "topological 2-form gravity" or "topological quantum gravity". This in turn is closely related to Chern–Simons theory in 3 dimensions.

Let me just say what the heck BF theories *are*, and then list a few references on them. The easiest way to describe them is by giving the Lagrangian. Say spacetime is an n-dimensional orientable manifold M and we have a principal G-bundle E over M, where G is a Lie group whose Lie algebra is equipped with an invariant trace on it. The two fields in BF theory are a connection A on E — which we can think of locally as a $\mathrm{Lie}(G)$-valued one-form — and a $\mathrm{Lie}(G)$-valued $(n-2)$-form called B. If F denotes the curvature of A, which is a $\mathrm{Lie}(G)$-valued 2-form, we can take the wedge product $B \wedge F$ and get a $\mathrm{Lie}(G)$-valued n-form, which gives the Lagrangian

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

an n-form we can integrate over M to get the action. Since we don't need any metric to integrate an n-form over our spacetime M, this action is "generally covariant". (Note also that it's gauge-invariant.) If we vary B and F to get the classical equations of motion, varying B gives us

$$F = 0,$$

that is, the connection A is flat, and varying A gives us

$$d_A B = 0,$$

that is, the exterior covariant derivative of B vanishes.

In 4 dimensions we can soup up our BF theory a bit by adding terms proportional to

$$\mathrm{tr}(B \wedge B)$$

and

$$\mathrm{tr}(F \wedge F)$$

to the Lagrangian. If we take as our Lagrangian

$$\mathrm{tr}(B \wedge F) + a\,\mathrm{tr}(B \wedge B) + b\,\mathrm{tr}(F \wedge F),$$

the third term, when integrated over M, is proportional to an invariant called the second Chern class of E, that is, it's independent of the connection A, so it really doesn't affect the equations of motion at all! In a sense it's utterly useless. The second term does something, though; our equations of motion become

$$F = -2aB, \quad d_A B = 0.$$

Note that if we plug the first equation into the Lagrangian, we get that for solutions of the equations of motion, the action is a constant times the second Chern class (if a is nonzero).

Horowitz showed, in this four-dimensional case, that if a is nonzero, there is a single state of the *quantum* version of BF theory when spacetime has the form $\mathbb{R} \times S$ (S being some oriented 3-manifold), and that this state, thought of as a wavefunction on the space of connections on S, is just the exponential of the Chern–Simons action. This is how Chern–Simons theory gets into the game.

Moreover, Baulieu and Singer showed that if you take the boring-looking "FF theory" with Lagrangian $\mathrm{tr}(F \wedge F)$, and quantize it using the BRST approach, you get something that Witten proved was closely related to Donaldson theory — an invariant of 4-manifolds. So there seems to be a relation between this stuff and Donaldson theory. It is a rather mysterious one as far as I'm concerned, though, because it seems you could just as well have used *zero* as a Lagrangian, rather than $\mathrm{tr}(F \wedge F)$, and done the same things Baulieu and Singer did. (At least, that's how it seems to me, and I got Scott Axelrod to agree with me on that yesterday.) In other words, Donaldson theory seems to have more to do with the geometry of the space of connections on M than it does with the "FF" Lagrangian per se. But still, there are other relationships between Donaldson theory and Chern–Simons theory (which I don't understand well enough to want to discuss), so perhaps it is not too silly to say that BF theory is related to Donaldson theory in some poorly understood manner.

Now for some references: the reference that got me started on these was

1) Gary Horowitz, "Exactly soluble diffeomorphism-invariant theories", *Commun. Math. Phys.* **125** (1989), 417–437. (Listed in "Week 19")

I got more interested in them when I read

2) Paolo Cotta-Ramusino and Maurizio Martellini, "BF Theories and 2-knots" in *Knots and Quantum Gravity*, ed. J. Baez, Oxford U. Press, Oxford, 1994. (Listed in "Week 23")

which indicated that BF theories may give invariants of surfaces embedded in 4-dimensional manifolds, much as Chern–Simons theory gives invariants of knots in 3-dimensional manifolds. Actually, BF theories make sense in any dimension, and in dimension 3 they appear to give knot invariants, including the Alexander-Conway polynomial:

3) A. S. Cattaneo, P. Cotta-Ramusino, and M. Martellini, "Three-dimensional BF theories and the Alexander–Conway invariant of knots", available as `hep-th/9407070`.

Another nice-looking paper on BF theories is the following:

4) Henri Waelbroeck, "$B \wedge F$ theory and flat spacetimes", available as gr-qc/9311033.

Waelbrock also has a recent paper with Zapata on a Hamiltonian formulation of the theory on a lattice:

3) Henri Waelbrock and J. A. Zapata, "A Hamiltonian formulation of topological gravity", available as gr-qc/9311035.

The paper by Baulieu and Singer relating FF theory to Donaldson theory is:

4) L. Baulieu and I. M. Singer, "Topological Yang–Mills symmetry", *Nucl. Phys. (Proc. Suppl.)* **B5** (1988), 12–19.

BF theory in 2 dimensions is also called "topological Yang–Mills theory", and it's discussed in various places, including

5) Edward Witten, "On quantum gauge theories in two dimensions", *Commun. Math. Phys.* **141** (1991), 153–209.

and

6) Matthias Blau and George Thompson, "Topological gauge theories of antisymmetric tensor fields", *Ann. Phys.* **205** (1991), 130–172.

Week 37

August 10, 1994

Mainly this week I have various bits of news to report from the 7th Marcel Grossman Meeting on general relativity. It was big and had lots of talks. Bekenstein gave a nice review talk on entropy/area relations for black holes, and Strominger gave a talk in which he proposed a solution to the information loss puzzle for black holes. (Recall that if one believes, as most people seem to believe, that black holes radiate away all their mass in the form of completely random Hawking radiation, then there's a question about where the information has gone that you threw into the black hole in the form of, say, old issues of *Phys. Rev. Lett.*. Some people think the information goes into a new "baby universe" formed at the heart of the black hole — see "Week 31" for more. The information would still, of course, be gone from *our* point of view in this picture. Strominger proposed a set up in which one had a quantum theory of gravity with annihilation and creation operators for baby universes, and proposed that the universe (the "metauniverse"?) was in a coherent state, that is, an eigenstate of the annihilation operator for baby universes. This would apparently allow handle the problem, though right now I can't remember the details.) There were also lots of talks on the interferometric detection of gravitational radiation, other general relativity experiments, cosmology, etc.. But I'll just try to describe two talks in some detail here.

1) L. Lindblom, "Superfluid hydrodynamics and the stability of rotating neutron stars", talk at MG7 meeting, Monday July 5, 1994, Stanford University.

Being fond of knots, tangles, and such, I have always liked knowing that in superfluids, vorticity (the curl of the velocity vector field) tends to be confined in "flux tubes", each containing an angular momentum that's an integral multiple of Planck's constant, and that similarly, in type II superconductors, magnetic fields are confined to magnetic flux tubes. And I was even more happy to find out that the cores of neutron stars are expected to be made of neutronium that is *both* superfluid *and* superconductive, and contain lots of flux tubes of both types. In this talk, which was really about a derivation of detailed equations of state for neutron stars, Lindblom began by saying that the maximum rotation rate of a rotating neutron star is due to some sort of "gravitational radiation instability due to internal fluid dissipation". I didn't quite understand the details of that, which weren't explained, but it motivated him to study the viscosity in neutron star cores, which are superfluid if they are cool enough (less than a billion degrees Kelvin). There are some protons and electrons mixed in with the neutrons in the core, and both the protons and neutrons go superfluid, but the electrons form a normal fluid. That means that there are actually *two* kinds of superfluid vortices — proton and neutron — in addition to the magnetic vortices. These vortices mainly line up along the axis of rotation, and their density is about 10^6 per square centimeter. Rather curiously, since the proton, neutron, and electron fluids are coupled due to β decay (and the reverse process), even the neutron vortices have electric currents associated to them and generate magnetic fields. This means that the electrons scatter off the neutron vortex cores as well as the proton vortex cores, which is one of the mechanisms that yields viscosity.

2) Abhay Ashtekar, "Mathematical developments in quantum general relativity, a sampler", talk at MG7 meeting, Tuesday July 6, 1994, Stanford University.

This talk, in addition to reviewing what's been done so far on the "loop representation" of quantum gravity, presented two new developments that I found quite exciting, so I'd like to sketch what they are. The details will appear in future papers by Ashtekar and collaborators.

The two developments Ashtekar presented concerned mathematically rigorous treatments of the "reality conditions" in his approach to quantum gravity, and the "loop states" used by Rovelli and Smolin. First let me try to describe the issue of "reality conditions". As I described in "Week 7", one trick that's important in the loop representation is to use the "new variables" for general relativity introduced by Ashtekar (though Sen and Plebanski already had worked with similar ideas). In the older Palatini approach to general relativity, the idea was to view general relativity as something like a gauge theory with gauge group given by the Lorentz group, $\mathrm{SO}(3,1)$. But to do this one actually uses two different fields: a "frame field", also called a "tetrad", "vierbein" or "soldering form" depending on who you're talking to, and the gauge field itself, usually called a "Lorentz connection" or "$\mathrm{SO}(3,1)$ connection". Technically, the frame field is an isomorphism between the tangent bundle of spacetime and some other bundle having a fixed metric of signature +—, usually called the "internal space", and the Lorentz connection is a metric-preserving connection on the internal space.

The "new variables" trick is to use the fact that $\mathrm{SO}(3,1)$ has as a double cover the group $\mathrm{SL}(2,\mathbb{C})$ of two-by-two complex matrices with determinant one. (For people who've read previous posts of mine, I should add that the Lie algebra of $\mathrm{SL}(2,\mathbb{C})$ is called $\mathfrak{sl}(2,\mathbb{C})$ and is the same as the complexification of the Lie algebra $\mathfrak{so}(3)$, which allows one to introduce the new variables in a different but equivalent way, as I did in "Week 7".) Ignoring topological niceties for now, this lets one reformulate *complex* general relativity (that is, general relativity where the metric can be complex-valued) in terms of a *complex-valued* frame field and an $\mathrm{SL}(2,\mathbb{C})$ connection that is just the Lorentz connection in disguise. The latter is called either the "Sen connection", the "Ashtekar connection", or the "chiral spin connection" depending on who you're talking to. The advantage of this shows up when one tries to canonically quantize the theory in terms of initial data. (For a bit on this, try "Week 11".) Here we assume our 4-dimensional spacetime can be split up into "space" and "time", so that space is a 3-dimensional manifold, and we take as our canonically conjugate fields the restriction of the chiral spin connection to space, call it A, and something like the restriction of the complex frame field to a complex frame field E on space. (Restricting the complex frame field to one on space is a wee bit subtle, especially because one doesn't really want a frame field or "triad field", but really a "densitized cotriad field" — but let's not worry about this here. I explain this in terms even a mathematician can understand in my paper "Strings, loops, knots and gauge fields" (hep-th/9309067). The point is, first, that the A and E fields are mathematically very analogous to the vector potential and electric field in electromagnetism — or really in $\mathrm{SL}(2,\mathbb{C})$ Yang–Mills theory — and secondly, that if you compute their Poisson brackets, you really do see that they're canonically conjugate. Third and best of all, the constraint equations in general relativity can be written down very simply in terms of A and E. Recall that in general relativity, 6 of Einstein's 10 equations act as *constraints* that the metric and its time derivative must satisfy at $t = 0$ in order to get a solution at later times. In quantum gravity, these constraints are a big technical problem one has to deal with, and the point of Ashtekar's new variables is precisely that the constraints simplify in terms of these variables. (There's more on these constraints in "Week 11".)

The price one has paid, however, is that one now seems to be talking about *complex-valued* general relativity, which isn't what one had started out being interested in. One needs to get back to reality, as it were — and this is the problem of the so-called "reality conditions". One approach is to write down extra constraints on the E field that say that it comes from a *real* frame field. These are a little messy. Ashtekar, however, has proposed another approach especially suited to the quantum version of the theory, and in his talk he filled in some of the crucial details.

Here, to save time, I will allow myself to become a bit more technical. In the quantum version of the theory one expects the space of wavefunctions to be something like L^2 functions on the space of connections modulo gauge transformations — actually this is the "kinematical state space" one gets before writing the constraints as operators and looking for wavefunctions annihilated by these constraints. The problem had always been that this space of L^2 functions is ill-defined, since there is no "Lebesgue measure" on the space of connections. This problem is addressed (it's premature to say "solved") by developing a theory of generalized measures on the space of connections and proving the existence of a canonical generalized measure that deserves the name "Lebesgue measure" if anything does. One can then define L^2 functions and work with them. For compact gauge groups, like $\mathrm{SU}(2)$, this was done by Ashtekar, Lewandowski and myself; see e.g. the papers "Spin network states in gauge theory" (gr-qc/9411007) and "Generalized measures in gauge theory" (hep-th/9310201). In the case of $\mathrm{SU}(2)$, Wilson loops act as self-adjoint multiplication operators on the resulting L^2 space. But in quantum gravity we really want to use gauge group $\mathrm{SL}(2,\mathbb{C})$, which is not compact, and we want the adjoints of Wilson loop operators to reflect that fact that the $\mathrm{SL}(2,\mathbb{C})$ connection A in quantum gravity is really equal to $\Gamma + iK$, where Γ is the Levi–Civita connection on space, and K is the extrinsic curvature. Both Γ and K are real in the classical theory, so the adjoint of the quantum version of A should be $\Gamma - iK$, and this should reflect itself in the adjoints of Wilson loop operators.

The trick, it turns out, is to use some work of Hall which appeared in the *Journal of Functional Analysis* in 1994 (I don't have a precise reference on me). The point is that $\mathrm{SL}(2,\mathbb{C})$ is the complexification of $\mathrm{SU}(2)$, and can also be viewed as the cotangent bundle of $\mathrm{SU}(2)$. This allows one to copy a trick people use for the quantum mechanics of a point particle on $\mathbb{R}^n$ — a trick called the Bargmann–Segal-Fock representation. Recall that in the ordinary Schrödinger representation of a quantum particle on $\mathbb{R}^n$, one takes as the space of states $L^2(\mathbb{R}^n)$. However, the phase space for a particle in $\mathbb{R}^n$, which is the cotangent bundle of $\mathbb{R}^n$, can be identified with $\mathbb{C}^n$, and in the Bargmann representation one takes as the space of states $HL^2(\mathbb{C}^n)$, by which I mean the *holomorphic* functions on $\mathbb{C}^n$ that are in L^2 with respect to a Gaussian measure on $\mathbb{C}^n$. In the Bargmann representation for a particle on the line, for example, the creation operator is represented simply as multiplication by the complex coordinate z, while the annihilation operator is d/dz. Similarly, there is an isomorphism between $L^2(\mathrm{SU}(2))$ and a certain space $HL^2(\mathrm{SL}(2,\mathbb{C}))$. Using this, one can obtain an isomorphism between the space of L^2 functions on the space of $\mathrm{SU}(2)$ connections modulo gauge transformations, and the space of holomorphic L^2 functions on the space of $\mathrm{SL}(2,\mathbb{C})$ connections modulo gauge transformations. Applying this to the loop representation, Ashtekar has found a very natural way to take into account the fact that the chiral spin connection A is really $\Gamma + iK$, basically analogous to the fact that in the Bargmann representation multiplication by z is really $q + ip$ (well, up to various factors of $\sqrt{2}$, signs and the like).

Well, that was pretty sketchy and probably not especially comprehensible to anyone who hasn't already worried about this issue a lot! In any event, let me turn to the other good news Ashtekar reported: the constuction of "loop states". Briefly put (I'm getting worn out), he and some collaborators have figured out how to *rigorously* construct generalized measures on the space of connections modulo gauge transformations, starting from invariants of links. This begins to provide an inverse to the "loop transform" (which is a construction going the other way).

Week 38

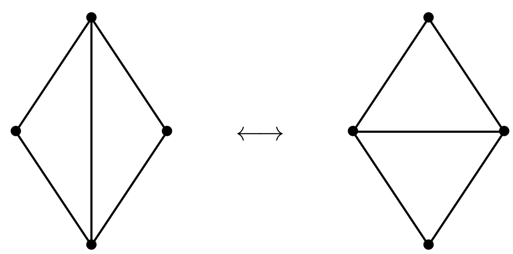
August 19, 1994

I've been busy, and papers have been piling up; there are lots of interesting ones that I really should describe in detail, but I had better be terse and list them now, rather than waiting for the mythical day when I will have time to do them justice.

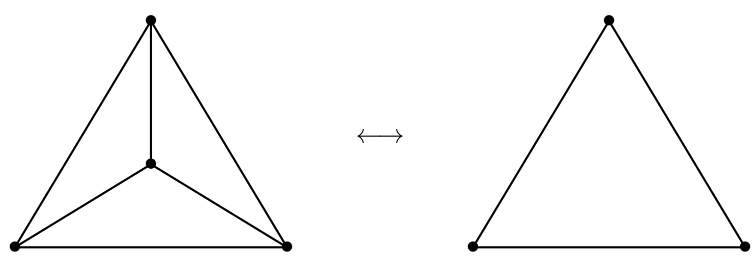
So:

1) B. Durhuus, H. P. Jakobsen and R. Nest, "Topological quantum field theories from generalized 6j-symbols", *Rev. Math. Physics* **5** (1993), 1–67.

In "Week 16" I explained a paper by Fukuma, Hosono and Kawai in which they obtained topological quantum field theories in 2 dimensions starting with a triangulation of a 2d surface. The theories were "topological" in the sense that the final answers one computed didn't depend on the triangulation. One can get between any two triangulations of a surface by using a sequence of the following two moves (and their inverses), called the (2,2) move:



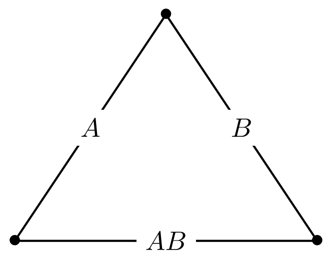
and the (3,1) move:



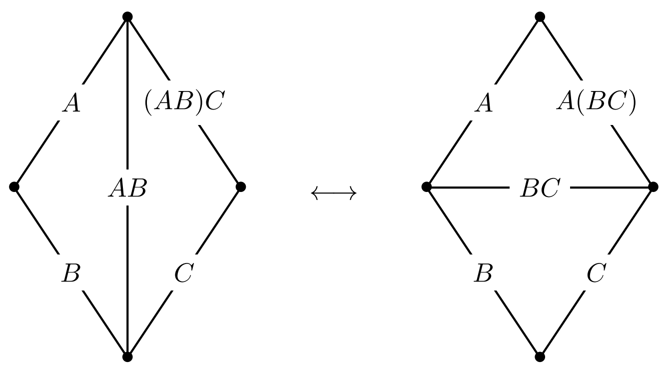
Note that in either case these moves amount to replacing one part of the surface of a tetrahedron with the other part! In fact, similar moves work in any dimension, and they are often called the Pachner moves.

The really *wonderful* thing is that these moves are also very significant from the point of view of algebra. . . and especially what I call "higher-dimensional algebra" (following Ronnie Brown), in which the distinction between algebra and topology is largely erased, or, one might say, revealed for the sham it always was.

For example, as explained more carefully in "Week 16", the (2,2) move is really just the same as the *associative* law for multiplication. The idea is that we are in a 2-dimensional spacetime, and a triangle represents multiplication: two "incoming states" go in two sides and their product, the "outgoing state", pops out the third side:

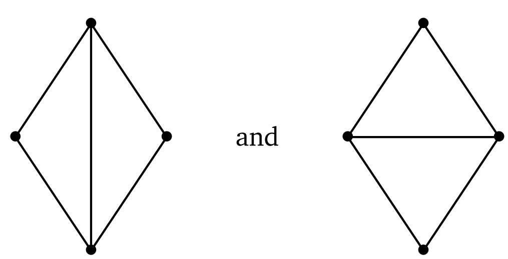


Then the (2,2) move represents associativity:



Of course, the distinction between "incoming" and "outgoing" sides of the triangle is conventional, and the more detailed explanation in "Week 16" shows how that fits into the formalism. Roughly speaking, what we have is not just any old algebra, but an algebra that, thought of as a vector space, is equipped with an isomorphism between it and its dual. This isomorphism allows us to forget whether we are coming or going, so to speak.

Hmm, and here I was planning on being terse! Anyway, the still *more* interesting point is that when we think about 3-dimensional topology and "3-dimensional algebra," we should no longer think of



as representing *equal* operations (the 3-fold multiplication of A, B, and C); instead, we should think of them as merely *isomorphic*, with the tetrahedron of which they are the front and back being the isomorphism. The basic philosophy is that in higher-dimensional algebra, as one ascends the ladder of dimensions, certain things which had been regarded as *equal* are revealed to be merely isomorphic. This gets tricky, since certain *isomorphisms* that were regarded as equal at one level are revealed to be merely isomorphic at the next level... leading us into a subtle world of isomorphisms between isomorphisms between isomorphisms... which the theory of n-categories attempts to systematize. (I should note, however, that in the particular case of associativity this business was worked out by Jim Stasheff quite a while back: it's the homotopy theorists who were the ones with the guts to deal with such issues first.)

Now, it turns out that in 3-dimensional algebra, the isomorphism corresponding to the (2,2) move is not something marvelously obscure. It is in fact precisely what physicists call the "$6j$ symbol", a gadget they've been using to study angular momentum in quantum mechanics for a long time! In quantum mechanics, the study of angular momentum is just the study of representations of the group $\mathrm{SU}(2)$, and if one has representations A, B, and C of this group (or any other), the tensor products $(A \otimes B) \otimes C$ and $A \otimes (B \otimes C)$ are not *equal*, but merely *isomorphic*. It should come as no surprise that this isomorphism is represented by physicists as a big gadget with 6 indices dangling on it, the "$6j$ symbol".

Quite a while back, Regge and Ponzano tried to cook up a theory of quantum gravity in 3 dimensions using the $6j$ symbols for $\mathrm{SU}(2)$. More recently, Turaev and Viro built a 3-dimensional topological quantum field theory using the $6j$-symbols of the *quantum group* $SU_q(2)$, and this led to lots of work, which the above article explains in a distilled sort of way.

The original Ponzano–Regge and Turaev–Viro papers, and various other ones clarifying the relation of the Turaev–Viro theory to quantum gravity in spacetimes of dimension 3, are listed in "Week 16". It's also worth checking out the paper by Barrett and Foxon listed in "Week 24", as well as the following paper, for which I'll just quote the abstract:

2) Timothy J. Foxon, "Spin networks, Turaev–Viro theory and the loop representation", available as gr-qc/9408013.

We investigate the Ponzano–Regge and Turaev–Viro topological field theories using spin networks and their q-deformed analogues. I propose a new description of the state space for the Turaev–Viro theory in terms of skein space, to which q-spin networks belong, and give a similar description of the Ponzano–Regge state space using spin networks. I give a definition of the inner product on the skein space and show that this corresponds to the topological inner product, defined as the manifold invariant for the union of two 3-manifolds. Finally, we look at the relation with the loop representation of quantum general relativity, due to Rovelli and Smolin, and suggest that the above inner product may define an inner product on the loop state space.

(Concerning the last point I cannot resist mentioning my own paper on knot theory and the inner product in quantum gravity, "Quantum gravity and the algebra of tangles".)

In addition to the papers by Turaev–Viro and Fukuma–Shapere listed in "Week 16", there are some other papers on Hopf algebras and 3d topological quantum field theories that I should list:

3) Greg Kuperberg, "Involutory Hopf algebras and three-manifold invariants", *Internat. Jour. Math* **2** (1991), 41–66.

   "A definition of $\#(M, H)$ in the non-involutory case", by Greg Kuperberg, unpublished.

Greg Kuperberg is one of the few experts on this subject who is often found on the net; he is frequently known to counteract my rhetorical excesses with a dose of precise information. The above papers, one of which is sadly still unpublished, make it beautifully clear how "algebra knows more about topology than we do", since various basic structures on Hopf algebras have a pleasant tendency to interact just as needed to give 3d topological quantum field theories.

4) John W. Barrett and Bruce W. Westbury, "Spherical categories", available as hep-th/9310164.

   John W. Barrett and Bruce W. Westbury, "Invariants of piecewise-linear 3-manifolds", *Trans. Amer. Math. Soc.* **348** (1996), 3997–4022. Also available as hep-th/9311155.

   John W. Barrett and Bruce W. Westbury, "The equality of 3-manifold invariants", available as hep-th/9406019.

Let me quote the abstract for the first one; the second one gives a construction of 3-manifold invariants, and the third shows that the authors' 3-manifold invariants agree with Kuperberg's when both are defined.

> *This paper is a study of monoidal categories with duals where the tensor product need not be commutative. The motivating examples are categories of representations of Hopf algebras and the motivating application is the definition of $6j$-symbols as used in topological field theories.*
>
> *We introduce the new notion of a spherical category. In the first section we prove a coherence theorem for a monoidal category with duals following MacLane (1963). In the second section we give the definition of a spherical category, and construct a natural quotient which is also spherical.*
>
> *In the third section we define spherical Hopf algebras so that the category of representations is spherical. Examples of spherical Hopf algebras are involutory Hopf algebras and ribbon Hopf algebras. Finally we study the natural quotient in these cases and show it is semisimple.*

5) Louis H. Kauffman and David E. Radford, "Invariants of 3-Manifolds derived from finite dimensional Hopf algebras", available as hep-th/9406065.

This paper also relates 3d topology and certain finite-dimensional Hopf algebras, and it shows they give 3-manifold invariants distinct from the more famous ones due to Witten (and a horde of mathematicians). I have not had time to think about how they relate to the above ones, but I have a hunch that they are the same, since all of them make heavy use of special grouplike elements associated to the antipode.

6) Louis Crane and Igor Frenkel, "Four dimensional topological quantum field theory, Hopf categories, and the canonical bases", available as hep-th/9405183.

Work in 4 dimensions is, as one expects, still more subtle than in 3, since again various things that were equalities becomes isomorphisms. In particular, this means that various things one thought were vector spaces — which are *sets* that have *elements* that you can *add* and *multiply by numbers*, and which satisfy *equations* like

$$A + B = B + A$$

are now reinterpreted as "2-vector spaces", which are *categories* that have *objects* that you can *direct sum* and *tensor with vector spaces*, and which have certain *natural isomorphisms* like the isomorphism

$$A \oplus B \cong B \oplus A.$$

In particular, using Lusztig's canonical basis, Crane and Frenkel start with quantum groups (which are Hopf algebras of a certain sort) and build marvelous "Hopf categories" out of them. While they do not construct a 4d TQFT in this paper, they indicate the game plan in terms clear enough that they will probably now have to race other workers in the field to see who can get the first interesting 4d TQFT... or perhaps something a bit subtler than a 4d TQFT (e.g. Donaldson theory).

Finally, let me turn to a subject that is closely related (though unfortunately this has not yet been made sufficiently clear), namely, holonomy algebras and the loop representation of quantum gravity. Let me simply list the references now; many of these papers were discussed at my session on knots and quantum gravity at the Marcel Grossman conference, so I promise to explain at some later time (and in some papers I'm writing) a bit more about how the loop representation of a gauge theory is interesting from the viewpoint of higher-dimensional algebra!

7) A. Ashtekar, J. Lewandowski, D. Marolf, J. Mourao and T. Thiemann, "A manifestly gauge-invariant approach to quantum theories of gauge fields", contribution to the Cambridge meeting proceedings, available as hep-th/9408108.

   Jerzy Lewandowski, "Topological measure and graph-differential geometry on the quotient space of connections", *Proceedings of "Journees Relativistes 1993"*, available as gr-qc/9406025.

   Abhay Ashtekar, Donald Marolf and Jose Mourao, "Integration on the space of connections modulo gauge transformations", available as gr-qc/9403042.

   A. Ashtekar and R. Loll, "New loop representations for 2+1 gravity", available as gr-qc/9405031.

   R. Loll, "Independent loop invariants for 2+1 gravity", available as gr-qc/9408007.

   R. Loll, J.M. Mouro and J.N. Tavares, "Generalized coordinates on the phase space of Yang–Mills theory", available as gr-qc/9404060.

   C. Di Bartolo, R. Gambini and J. Griego, "The extended loop representation of quantum gravity", available as gr-qc/9406039.

   Rodolfo Gambini, Alcides Garat and Jorge Pullin, "The constraint algebra of quantum gravity in the loop representation", available as gr-qc/9404059.

Week 39

September 24, 1994

I want to say a bit about Alain Connes' book, newly out in English, and then some about Yang–Mills theory in 2 dimensions.

1) Alain Connes, *Noncommutative Geometry*, Academic Press, Cambridge, Massachusetts.

You know something is up when a prominent mathematical physicist (Daniel Kastler) says "Alain is great. I am just his humble prophet." (This happened at a conference at Penn State I just went to.) What is noncommutative geometry and what's so great about it?

Basically, the idea of noncommutative geometry is to generalize geometry to "quantum spaces". For example, the ordinary plane has two functions on it, the coordinate functions x and y, which commute: $xy = yx$. We can think of x and y as representing the position and momentum of a classical particle. But when we consider a quantum-mechanical particle, we must give up commutativity and instead impose the "canonical commutation relations" $xy - yx = i\hbar$, where $\hbar$ is Planck's constant. Now x and y are not really functions on any space at all, but simply elements of a noncommutative algebra. Still, we can try our best to *pretend* that they are functions on some mysterious sort of "quantum space" in which knowing one coordinate of a point precisely precludes us from knowing the other coordinate exactly, by the Heisenberg uncertainty principle. Mathematically, noncommutative geometry consists of 1) expressing the geometry of spaces algebraically in terms of the commutative algebra of functions on them, and 2) then generalizing the results to classes of noncommuative algebras.

The main trick invented by Connes was to come up with a substitute for the "differential forms" on a space. Differential forms are the bread and butter of modern geometry. If we start with a commutative algebra A (say the algebra of smooth functions on some manifold like the plane), we can form the algebra of differential forms over A by introducing, for each element f in A, a formal symbol df, and imposing the following rules:

- $d(f + g) = df + dg$
- $d(cf) = cdf$ (c a constant)
- $d(fg) = (df)g + fdg$
- $fdg = (dg)f$
- $dfdg = -dgdf$.

More precisely, the differential forms over A are the algebra generated by A and these differentials df, modulo the above relations. This gives a purely algebraic way of understanding what those mysterious things like $dxdydz$ in integral signs are.

Now, the last two of the five rules listed above fit nicely with the commutative of A when it *is* commutative, but they jam up the works horribly otherwise. So: how to generalize differential forms to the noncommutative case? There are various things one can do if A is commutative in some generalized sense, such as "supercommutative" or "braided commutative" (which I call "R-commutative" in some papers on this subject). However, if A is utterly noncommutative, it seems that the best approach is Connes', which is first to *throw out* the last two relations, obtaining something folks call the "differential envelope" of A or the "universal differential graded algebra" over A — which is pleasant but quite boring by itself — and then to consider "chains" which are linear maps F from this gadget to the complex numbers (or whatever field you're working in) satisfying the cyclic property

$$F(uv) = (-1)^{ij} F(vu)$$

where u is something that looks like $f_0 df_1 df_2 \ldots df_i$, and v is something like $g_0 dg_1 dg_2 \ldots dg_j$. There are charming things one can do with chains that wind up letting one do most of what one could do with differential forms. More precisely, just as differential forms allow you entry into the wonderful world of de Rham cohomology, chains let you develop something similar called cyclic homology (and there is a corresponding cyclic cohomology that's even more like the de Rham theory).

Connes, being extremely inventive and ambitious, has applied noncommutative differential geometry to many areas: index theory, K-theory, foliations, Penrose tilings, fractals, the quantum Hall effect, and even elementary particle physics. Perhaps the most intriguing result is that if one develops the Yang–Mills equations using the techniques of noncommutative geometry, but with a very simple "commutative" model of spacetime, namely a two-sheeted cover of ordinary spacetime, the Higgs boson falls out rather magically on its own. This has led Kastler and other physicists to pursue a reformulation of the whole Standard Model in terms of noncommutative geometry, hoping to simplify it and even make some new predictions. It is far too early to see if this approach will get somewhere useful, but it's certainly interesting.

I haven't read this book, just part of the French version on which it's based (with extensive additions), but my impression is that it's quite easy to read given the technical nature of the subject.

2) Gregory Moore, "2d Yang–Mills theory and topological field theory", available as `hep-th/9409044`.

This is a nice review of recent work on 2d Yang–Mills theory. While Yang–Mills theory in 4 dimensions is the basis of our current theories of the strong, weak, and electromagnetic forces, and mathematically gives rise to a cornucopia of deep results about 4-dimensional topology, 2d Yang–Mills theory has traditionally been considered "trivial" in that one can exactly compute pretty much whatever one wants. However, Witten, in "On quantum gauge theories in two dimensions" (see "Week 36"), showed that precisely because 2d Yang–Mills theory was exactly soluble, one could use it to study a lot of interesting mathematics problems relating to "moduli spaces of flat connections." (More about those below.) And Gross, Taylor and others have recently shown that 2d Yang–Mills theory, at least working with gauge groups like $\mathrm{SU}(N)$ or $\mathrm{SO}(N)$ and taking the "large N limit", could be formulated as a string theory. So people respect 2d Yang–Mills theory more these days; its complexities stand as a strong clue that we've just begun to tap the depths of 4d Yang–Mills theory!

I can't help but add that Taylor and I did some work a while back in which we formulated $\mathrm{SU}(N)$ 2d Yang–Mills theory for *finite* N as a string theory. This was meant as evidence for my proposal that the loop representation of quantum gravity is a kind of string theory, a proposal described in "Week 18". For more on this sort of thing, try my paper in the book *Knots and Quantum Gravity* (see "Week 23") — which by the way is finally out — and also the following:

3) J. Baez and W. Taylor, "Strings and two-dimensional QCD for finite N", available as hep-th/9401041.

When it comes to "moduli spaces of flat connections", it's hard to say much without becoming more technical, but I certainly recommend starting with the beautiful work of Goldman:

4) William Goldman, "The symplectic nature of fundamental groups of surfaces", *Adv. Math.* **54** (1984), 200–225.

   William Goldman, "Invariant functions on Lie groups and Hamiltonian flows of surface group representations", *Invent. Math.* **83** (1986), 263–302.

   William Goldman, "Topological components of spaces of representations", *Invent. Math.* **93** (1988), 557–607.

The basic idea here is to take a surface S with a particular G-bundle on it, and carefully study the space of flat connections modulo gauge transformations, which will be a finite-dimensional stratified space. If you fix G and S, no matter what bundle you pick, this space will appear as a subspace of a bigger space called the moduli space of flat connections, which is the same as $\mathrm{Hom}(\pi_1(S), G)/\mathrm{Ad}G$. There is an open dense set of this space, the "top stratum", which is a symplectic manifold. Geometric quantization of this manifold has everything in the world to do with Chern–Simons theory, as summarized so deftly by Atiyah:

5) Michael Atiyah, *The Geometry and Physics of Knots*, Cambridge U. Press, Cambridge, 1990.

On the other hand, lately people have been using 2d Yang–Mills theory, BF theory, and the like (see "Week 36") to get a really thorough handle on the cohomology of the moduli space of flat connections. For a mathematical approach to this problem that doesn't talk much about gauge theory, try:

6) Lisa C. Jeffrey, "Group cohomology construction of the cohomology of moduli spaces of flat connections on 2-manifolds", available as alg-geom/9404012.

Week 40

October 19, 1994

When I was an undergraduate I was quite interested in logic and the foundations of mathematics — I was always looking for the most mind-blowing concepts I could get ahold of, and Gödel's theorem, the Löwenheim–Skolem theorem, and so on were right up there with quantum mechanics and general relativity as far as I was concerned. I did my undergrad thesis on computability and quantum mechanics, but then I sort of lost interest in logic and started thinking more and more about quantum gravity. The real reason was probably that my thesis didn't turn out as interesting as I'd hoped, but I remember feeling at the time that logic had become less revolutionary than in it was in the early part of the century. It seemed to me that logic had become a branch of mathematics like any other, studying obscure properties of models of the Zermelo–Fraenkel axioms, rather than questioning the basic presumptions implicit in those axioms and daring to pursue new, different approaches. I couldn't really get excited about the properties of super-huge cardinals. Of course, I knew a bit about intuitionistic logic and various forms of finitism, but these seemed to be the opposite of daring; instead, they seemed to appeal mainly to grumpy people who didn't trust abstractions and wanted to do everything as conservatively as possible. I was pretty interested in quantum logic, too, but I tended to think of this more as a branch of physics than 'logic' proper.

Anyway, it's now quite clear to me that I just hadn't been reading the right stuff. I think Rota has said that the really interesting work in logic now goes under the name of 'computer science', but for whatever reason, I didn't dig into the *Journal of Philosophical Logic*, other logic journals, or proceedings of conferences on category theory, computer science and the like and find the stuff that would have excited me. It goes to show that one really needs to keep digging! Anyway, I just went to a conference called the Lambda Calculus Jumelage up in Ottawa, thanks to a kind invitation by Prakash Panangaden and Phil Scott, who thought my ideas on category theory and physics might interest (or at least amuse) the folks who attend this annual bash. It became clear to me while up there that logic is alive and well!

Of course, I don't actually understand most of what these people are up to, so take what I say with a large grain of salt. My goal here is more to draw attention to some interesting-sounding ideas than to explain them.

One interesting subject, which I think I'm finally beginning to get an inkling of, is "linear logic". This was introduced in the following paper (which I haven't gotten around to looking at):

1) Jean-Yves Girard, "Linear Logic", *Theoretical Computer Science* **50** (1987), 1–102.

When I first heard about linear logic, it made utterly no sense. It seemed to be a logic suitable for use in some completely different universe than the one I inhabited! For example, there were the familiar logical connectives "and" and "or", but they had weird alternate versions called "tensor" and "par", the latter written with an upside-down ampersand. There was also an alternate version of the material implication "$\to$", and a strange operation called "!" (pronounced "bang") that somehow mediated between the logical connectives I knew and loved and their eerie alter egos.

I understand a wee bit about these things now; one can get a certain ways just by getting used to "tensor", since the rest of the weird connectives are defined in terms of this one and the familiar ones. (I won't worry about the "!" here.) One key idea, which finally penetrated my thick skull, is that there is a good reason why "tensor" does not satisfy the following deduction rule so characteristic of "and":

$$\frac{S \vdash p \qquad S \vdash q}{S \vdash p \& q}$$

meaning: if from the set of premisses S we can deduce p, and from S we can also deduce q, then from S we can deduce $p\&q$. The point is that in linear logic one should not think of S as a *set* of premisses, but rather as a **multiset**, meaning that the same premise can appear twice. The idea is that if we use one premise in S to deduce something, we *use it up*, and we can only use it again if S has several copies of that premise in it. As they say, linear logic is "resource-sensitive" (which is apparently why computer scientists like it). So the idea is that in linear logic,

$$S \vdash p\&q$$

means something like "from the premisses S one can deduce p if one feels like it, or alternatively one can deduce q if one feels like it, but not necessarily both at once, since there may not be enough copies of the premisses to do that." On the other hand,

$$S \vdash p \otimes q$$

is stronger, since it means something like "from the premisses S one can deduce both p and q at once, since there are enough copies of all the premisses in S to do it." Thus "&" satisfies the above deduction rule in linear logic just as in classical logic, but "tensor" does not; instead, it satisfies

$$\frac{S \vdash p \qquad T \vdash q}{S \cup T \vdash p \otimes q}$$

where $S \cup T$ denotes the union of the multisets S and T (so that if both S and T have one copy of a premiss, $S \cup T$ has two copies of it).

Well, let me leave it at that. I should add that there is a paper available online,

2) Vaughan Pratt, "Linear logic for generalized quantum mechanics", by available at http://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.49.649.

which relates linear logic and quantum logic, and which is part of a body of work relating linear logic and category theory, with the key idea being that "linear logic is a logic of monoidal closed categories in much the same way that intuitionistic logic is a logic of Cartesian closed categories" — here I quote

3) Richard Blute, "Hopf algebras and linear logic", *Mathematical Structures in Computer Science*, **6** (1996), 189–217. Available as https://citeseerx.ist.psu.edu/viewdoc/summary?doi=10.1.1.114.8038.

I suppose to most people, explaining linear logic in terms of monoidal closed categories may seem like using mud to wipe one's windshield. However, to some of us monoidal closed categories are rather familiar things, and in fact anyone who knows about vector spaces, linear maps, and the vector spaces $\mathrm{Hom}(V, W)$ and $V \otimes W$ knows a really good example of a monoidal closed category. Thus monoidal closed categories can be viewed as an abstraction of linear algebra, and indeed this is how "linear logic" got its name.

It seems that I should read the following papers, too, before I really understand the connection between linear logic and category theory:

4) R. A. G. Seely, Linear logic, *-autonomous categories and cofree coalgebras, in *Categories in Computer Science and Logic*, Contemp. Math. **92**, American Mathematical Society, Providence, Rhode Island, 1989.

5) D. Yetter, "Quantales and (noncommutative) linear logic", *Journal of Symbolic Logic* **55** (1990), 41–64.

A terse summary of linear logic in terms a categorist might like can be found in Section 3.5 of Pratt's paper cited above. I should add that Pratt has lots of other interesting papers available online.

Week 41

October 17, 1994

In the beginning of September I went to a conference at the Center for Gravitational Physics and Geometry at Penn State. This is the center run by Abhay Ashtekar, and it has Jorge Pullin and Lee Smolin as faculty, and Roger Penrose as a part-time visitor — so it's a great place to visit if you're interested in quantum gravity. There are a lot of good postdocs and such there, too. I've been too busy to say much so far about what happened at this conference, but I'd like to now.

One talk I enjoyed a lot was Steve Carlip's, on the entropy of black holes. This has subsequently come out as a preprint, available electronically:

1) Steve Carlip, "The statistical mechanics of the (2+1)-dimensional black hole", available as gr-qc/9409052.

It's well-known by now that in certain situations it makes sense to speak of the "entropy" of a black hole, but the real meaning of this entropy is still mysterious. In particular, since the entropy of a black hole is (often, but not always) proportional to the area of its event horizon, it would be very satisfying if the entropy corresponded somehow to degrees of freedom that "lived at the event horizon". Steve Carlip has done a pretty credible calculation along these lines (though not without various subtle difficulties) in the case of a black hole in 3-dimensional spacetime.

I should say a little bit about gravity in 3 dimensions and why people are interested in it. 3-dimensional gravity is drastically simpler than 4-dimensional gravity, since in 3 dimensions the vacuum Einstein's equations say the spacetime metric is *flat*, at least if the cosmological constant vanishes. Thus there can be no gravitational radiation (and in quantum theory no "gravitons"), and the metric produced by a static point mass is not like the Schwarschild metric, instead, on space it is just like that of a cone. Things are a bit different if the cosmological constant is nonzero; in particular, there are black-hole type solutions. But there is still no gravitational radiation.

Basically, people are interested in 3-dimensional quantum gravity because it's simple enough that one can compute something and hope it sheds some light on the 4-dimensional world we live in. For some issues this appears to be the case: primarily, conceptual issues having to do with theories in which there is no "background metric". Unfortunately, there are *several different ways* to set up 3-dimensional quantum gravity, corresponding to different approaches people have to 4-dimensional quantum gravity. For this, check out Carlip's paper "Six ways to quantize (2+1)-dimensional gravity," mentioned in "Week 16". However, I think the "best" way to quantize gravity in 3 dimensions is the way involving Chern–Simons theory, because this way is the most closely related to Ashtekar's approach to quantizing gravity in 4 dimensions, hence it sheds the most light on the things I'm interested in — and I also think it's the most beautiful. In this approach, you can compute a lot of things, and basically what Carlip has done is to show that associated to the event horizon there are degrees of freedom which should give entropy proportional to its area.

I suppose I can't say how he does it much more clearly than he says it, so I'll quote the introduction, taking the liberty of turning some of his LaTeX into English. If you get scared by the "Virasoro operator L_0" below, never fear — in this context, it just amounts to the angular momentum operator, which generates rotations about the origin. So:

> *The basic argument is quite simple. Begin by considering general relativity on a manifold M with boundary. We ordinarily split the metric into true physical excitations and "pure gauge" degrees of freedom that can be removed by diffeomorphisms of M. But the presence of a boundary alters the gauge invariance of general relativity: the infinitesimal transformations [. . .] must now be restricted to those generated by vector fields [. . .] with no component normal to the boundary, that is, true diffeomorphisms that preserve the boundary of M. As a consequence, some degrees of freedom that would naively be viewed as "pure gauge" become dynamical, introducing new degrees of freedom associated with the boundary.*
>
> *Now, the event horizon of a black hole is not a true boundary, although the black hole complementarity approach of Susskind et al. suggests that it might be appropriately treated as such. Regardless of one's view of that program, however, it is clear that in order to ask quantum mechanical questions about the behavior of black holes, one must put in "boundary conditions" that ensure that a black hole is present. This means requiring the existence of a hypersurface with particular metric properties—say, those of an apparent horizon.*
>
> *The simplest way to do quantum mechanics in the presence of such a surface is to quantize fields separately on each side, imposing the appropriate correlations as boundary conditions. In a path integral approach, for instance, one can integrate over fields on each side, equate the boundary values, and finally integrate over those boundary values compatible with the existence of a black hole. But this process again introduces boundary terms that restrict the gauge invariance of the theory, leading once more to the appearance of new degrees of freedom at the horizon that would otherwise be treated as unphysical.*
>
> *My suggestion is that black hole entropy is determined by counting these would-be gauge degrees of freedom. The resulting picture is similar to Maggiore's membrane model of the black hole horizon, but with a particular derivation and interpretation of the "membrane" degrees of freedom.*
>
> *The analysis of this phenomenon is fairly simple in 2+1 dimensions. It is well known that (2+1)-dimensional gravity can be written as a Chern–Simons theory, and it is also a standard result that a Chern–Simons theory on a manifold with boundary induces a dynamical Wess–Zumino–Witten (WZW) theory on the boundary. In the presence of a cosmological constant $\Lambda = -1/L^2$ appropriate for the (2+1)-dimensional black hole, one obtains a slightly modified* $\mathrm{SO}(2,1) \times \mathrm{SO}(2,1)$ *WZW model, with coupling constant*
>
> $$k = \frac{L\sqrt{2}}{8G}$$
>
> *This model is not completely understood, but in the large k — i.e., small Λ — limit, it may be approximated by a theory of six independent bosonic oscillators. I show below that the Virasoro operator L_0 for this theory takes the form*
>
> $$L_0 \sim N - \left(\frac{r}{4G}\right)^2,$$

> *where N is a number operator and r is the horizon radius. It is a standard result of string theory that the number of states of such a system behaves asymptotically as*
> $$n(N) \sim \exp(\pi\sqrt{4}N)$$
> *If we demand that L_0 vanish — physically, requiring states to be independent of the choice of origin of the angular coordinate at the horizon — we thus obtain*
> $$\log n(r) \sim \frac{2\pi r}{4G},$$
> *precisely the right expression for the entropy of the (2+1)-dimensional black hole.*

Also, Carlo Rovelli spoke about describing the dynamics of quantum gravity coupled to a scalar field in terms of "spin network" states. I think this was based on work he did in collaboration with Lee Smolin, and I don't think it's out yet. I'm just about to finish up a little paper on spin network states myself, since they seem like very useful things in quantum gravity. The simplest sort of spin network is just a trivalent graph (i.e., 3 edges adjacent to each vertex) with edges labelled by "spins" $0, \frac{1}{2}, 1, \frac{1}{2}, \ldots$, and satisfying the "triangle inequality" at each vertex:

$$j_1 + j_2 \leqslant j_3, \quad j_2 + j_3 \leqslant j_1, \quad j_3 + j_1 \leqslant j_2,$$

where j_1, j_2, j_3 are the spins labelling the edges adjacent to the given vertex. Really, the spins should be thought of as irreducible representations of $\mathrm{SU}(2)$, and the triangle inequalities is necessary for the representation j_3 to appear as a summand in the tensor product of the representations j_1 and j_2. (If the last sentence was meaningless to you, reading "Week 5" will help a little, though probably not quite enough.)

Penrose introduced spin networks as part of a purely combinatorial approach to spacetime in the paper:

2) Roger Penrose, "Angular momentum; an approach to combinatorial space time", in *Quantum Theory and Beyond*, ed. T. Bastin, Cambridge University Press, Cambridge, 1971. Available as https://math.ucr.edu/home/baez/penrose/.

It is somehow satisfying, therefore, to see that spin networks arise naturally as a convenient description of states in the loop representation of quantum gravity, which **starts** mainly with Einstein's equations and the principles of quantum mechanics. Certainly there is a lot more we need to learn about them.... One place worth reading about them is:

3) Louis Crane, "Conformal field theory, spin geometry, and quantum gravity", by *Phys. Lett.* **B259** (1991), 243–248.

I will be coming out with a paper on them next week if I get my act together, and I may say a bit more about them in future "Week s".

Rovelli also mentioned an interesting paper he wrote about the problem of time in quantum gravity with the operator-algebra/noncommutative- geometry guru Alain Connes:

4) A. Connes and C. Rovelli, "Von Neumann algebra automorphisms and time-thermodynamics relation in general covariant quantum theories", available as gr-qc/9406019.

The problem of time in quantum gravity is a bit tricky to describe, since it takes different guises in different approaches to quantum gravity, but I have attempted to give a rough introduction to it in "Week 11" and "Week 27". One way to get a feeling for it is to realize that anything you are used to doing with Hamiltonians in quantum mechanics or quantum field theory, you CAN'T do in quantum gravity, at least not in any simple way, because there is no Hamiltonian in general relativity, but only a "Hamiltonian constraint" — which in quantum gravity becomes the Wheeler–DeWitt equation

$$H\psi = 0.$$

Now, people know there is a mystical relationship between time and temperature that might be written

$$it = \frac{1}{kT}$$

where t is time, T is temperature, and k is Boltzmann's constant. This equation is a bit of an exaggeration! But the point is that in quantum theory, when there is a Hamiltonian H around one evolves states using the operator

$$\exp(-itH)$$

while the Gibbs state, that is, the equilibrium state at temperature T, is given by the density matrix

$$\exp(-H/kT).$$

It is this fact that relates statistical mechanics and quantum field theory so closely.

Now, in quantum gravity things aren't so simple, since there isn't a Hamiltonian (just a Hamiltonian constraint). However, people *do* know that there are all sorts of funny relationships between statistical mechanics and quantum gravity. For example, an accelerating observer in Minkowski space will see the vacuum as a heat bath with temperature proportional to her acceleration, so in curved spacetime, where there are no truly inertial frames, there really is no well-defined notion of a vacuum; in some vague sense, all there are is "thermal" states. This fact is also somehow related to Hawking radiation, and to the notion of black hole entropy... but really, there is a lot that nobody understands about all these connections!

In any event, Rovelli was prompted to use thermodynamics to **define** time in quantum gravity as follows. Given a mixed state with density matrix D, *find* some operator H such that D is the Gibbs state $\exp(-H/kT)$. In lots of cases this isn't hard; it basically amounts to

$$H = -kT \ln D$$

Of course, H will depend on T, but this really is just saying that fixing your units of temperature fixes your units of time!

Operator theorists have pondered this notion very carefully for a long time and generalized it into something called the Tomita–Takesaki theorem, which Connes and Rovelli explain. This gives a very general way to cook up a Hamiltonian (hence a notion of time evolution) from a state of a quantum system! For example, one can use this trick to start with a Robertson–Walker universe full of blackbody radiation, and recover a notion of "time". This is very intriguing, and it may represent some real progress in understanding the deep relations between time, thermodynamics, and gravity. There are, of course, lots of problems and puzzles to deal with.

Another intriguing talk at the conference was given by Viqar Husain, on the subject of the following paper:

5) Viqar Husain, "The affine symmetry of self-dual gravity", available as hep-th/9410072.

Let me simply quote the abstract, since I don't feel I really understand the essence of this business well enough to say anything useful yet:

> *Self-dual gravity may be reformulated as the two dimensional chiral model with the group of area preserving diffeomorphisms as its gauge group. Using this formulation, it is shown that self-dual gravity contains an infinite dimensional hidden symmetry algebra, which is the Affine (Kac–Moody) algebra associated with the Lie algebra of area preserving diffeomorphisms. This result provides an observable algebra and a solution generating technique for self-dual gravity.*

A couple more things before I wrap this up.... First, in case any mathematicians out there are wondering what this "knots and quantum gravity" business is all about, here's something I wrote to review the subject:

6) John Baez, "Knots and quantum gravity: progress and prospects", available as gr-qc/9410018.

My abstract:

> *Recent work on the loop representation of quantum gravity has revealed previously unsuspected connections between knot theory and quantum gravity, or more generally, 3-dimensional topology and 4-dimensional generally covariant physics. We review how some of these relationships arise from a 'ladder of field theories' including quantum gravity and BF theory in 4 dimensions, Chern–Simons theory in 3 dimensions, and the G/G gauged WZW model in 2 dimensions. We also describe the relation between link (or multiloop) invariants and generalized measures on the space of connections. In addition, we pose some research problems and describe some new results, including a proof (due to Sawin) that the Chern–Simons path integral is not given by a generalized measure.*

Finally, let me draw people's attention to "Matters of Gravity", the newsletter Jorge Pullin puts together at considerable effort, to keep people informed about general relativity and the like, experimental and theoretical:

7) "Matters of Gravity", a newsletter for the gravity community, Number **4**, edited by Jorge Pullin, 24 pages in Plain TeX, available as gr-qc/9409004.

Here's the table of contents of this issue:

- Editorial.

- Gravity News:
  - Report on the APS topical group in gravitation, Beverly Berger.
- Research briefs:
  - Gravitational microlensing and the search for dark matter, Bohdan Paczynski.
  - Laboratory gravity: the G mystery, Riley Newman.
  - LIGO project update, Stan Whitcomb.
- Conference Reports
  - PASCOS '94, Peter Saulson.
  - The Vienna Meeting, P. Aichelburg, R. Beig.
  - The Pitt binary black hole grand challenge meeting, Jeff Winicour.
  - International symposium on experimental gravitation at Pakistan, Munawar Karim.
  - 10th Pacific coast gravity meeting, Jim Isenberg.

Week 42

November 3, 1994

String theory means different things to different people. The original theory of strings – at least if I've got my history right – was a theory of hadrons (particles interacting via the strong force). The strong force wasn't understood too well then, but in 1968 Veneziano cleverly noticed when thumbing through a math book that Euler's beta function had a lot of the properties one would expect of the formula for how hadrons scattered (the so-called S-matrix). Later, around 1970, Nambu and Goto noticed that this function would come out naturally if one thought of hadrons as different vibrational modes of a relativistic string.

This theory had problems, and eventually it was supplanted by the current theory of the strong force, involving quarks and gluons. The gluons are another way of talking about the strong force, which is a gauge field. The biggest puzzle about this approach to hadrons is, "how come we don't see quarks?" This is called the puzzle of confinement. In the late 1970's, one proposed solution was that as you pulled the quark and the antiquark in a meson apart, the strong force effectively formed an elastic "string" with constant tension. This would mean that pulling them apart took energy proportional to how far you pulled them apart. Past a certain point, the energy would be enough to create a new quark-antiquark pair and *snap* — the string would split into two new strings with quark and antiquark on each end. So here the "string" idea is revived but as an approximation to a theory of gauge fields. One can even try to derive approximate string equations from the equations for the strong force: the Yang–Mills equations. In my paper on strings, loops, knots and gauge fields (see "Week 18"), I gave references to some early papers on the subject:

1) Y. Nambu, "QCD and the string model", *Phys. Lett.* **B80** (1979), 372–376.

   "Gauge fields as rings of glue", A. Polyakov, *Nucl. Phys.* **B164** (1979), 171–188.

   Y. Nambu, "The quantum dual string wave functional in Yang–Mills theories", *Phys. Lett.* **B80** (1979), 255–258.

   F. Gliozzi and M. Virasoro, "The interaction among dual strings as a manifestation of the gauge group", *Nucl. Phys.* **B164** (1980), 141–151.

   A. Jevicki, "Loop-space representation and the large-N behavior of the one-plaquette Kogut-Susskind Hamiltonian", *Phys. Rev.* **D22** (1980), 467–471.

   Y. Makeenko and A. Migdal, "Quantum chromodynamics as dynamics of loops", *Nucl. Phys.* **B188** (1981), 269–316.

   Y. Makeenko and A. Migdal, "Loop dynamics: asymptotic freedom and quark confinement", *Sov. J. Nucl. Phys.* **33** (1981), 882–893.

These papers make very interesting reading even today. Anyone who knows particle physics will recognize most of these names! Strings were big back then. But then they went out of fashion, because the string models predicted a massless spin-2 particle — and there's no such thing in particle physics. Later, when people were trying to cook up "theories of everything" including gravity, this flaw was again seen as a plus, since the hypothesized "graviton" meets that description.

The modern, more technical subject of string theory is a lot more fancy than these early papers. In particular, the recognition that conformal invariance was a very good thing when studying strings propagating on fixed background metric (like that of Minkowski space) pushed string theorists into a careful study of 2-dimensional conformal invariant quantum field theories. (Here the 2 dimensions refer to the surface the string traces out as it moves through spacetime.) Conformal field theory then developed a life of its own! By now it's pretty intimidating to the outsider. Mathematicians might find the following summary handy:

2) Krzysztof Gawedzki, "Conformal field theory", *Seminaire Bourbaki*, Asterisque **177–178** (1989), 95–126.

while physicists might try

3) Michio Kaku, *Introduction to Superstrings*, Springer, Berlin, 1988.

   Michio Kaku, *String Fields, Conformal Fields, and Topology*, New York, Springer, Berlin, 1991.

Kaku's books are a decent overview but rather sketchy in spots, since they cover vast amounts of territory.

Then there is another kind of sophisticated modern string theory, "string field theory", which doesn't assume the strings are moving around on a spacetime with a background geometry. This is clearly more like what one wants to do if one is using strings to explain quantum gravity. I don't understand this nearly as well as I'd like to, but the guru on this subject is Barton Zwiebach, so if one was really gutsy one would, after a suitable warmup with Kaku, plunge in and read something like

4) Ashoke Sen and Barton Zwiebach, "Quantum background independence of closed string field theory", available as hep-th/9311009.

   Ashoke Sen and Barton Zwiebach,"Background independent algebraic structures in closed string field theory", available as hep-th/9408053.

Unfortunately I'm not quite up to it yet. . . .

Then, in a different direction, a bunch of folks from general relativity pursued some ideas about string and loops to the point of developing the "loop representation of quantum gravity." I'm referring to

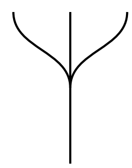
5) C. Rovelli and L. Smolin, "Loop representation for quantum general relativity", by*Nucl. Phys.* **B331** (1990), 80–152.

though it's important to credit some of the people who kept alive the idea that one should study gauge fields as being "loops of string", or more technically, "Wilson loops":

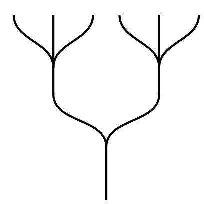
6) R. Gambini and A. Trias, "Gauge dynamics in the C-representation", *Nucl. Phys.* **B278** (1986), 436–448.

Now what's frustrating here is that I understand the loop representation business, but not the "background-free closed string field theory" business, even though they have the same historical roots and are both trying to deal with quantum gravity (among other forces) in a way that assumes that loops are the basic objects. Alas, the two strands speak in different languages! Heavy-duty mathematicians like Getzler, Kapranov and Stasheff know how to think about closed string fields in terms of "operads", and that stuff seems like it should be simple enough to understand, but alas, when I read it I get snowed in detail (so far).

Let me digress to mention what an "operad" is. An "operad" is basically a cool way to handle sets equipped with lots of n-ary operations. These operations might be "parametrized" in various ways. The operad elegantly keeps track of these parametrizations. So, for each n, an operad has a set $X(n)$ which we think of as all the n-ary operations. Think of something in $X(n)$ as a black box that has n "input" tubes and one "output" tube, or a tree-shaped thing



with n branches and one root (here $n = 3$). Then suppose we have a bunch of these black boxes. Say we have something in $X(n_1)$, something in $X(n_2)$, ... and so on up to something in $X(n_k)$. Thus we've got a pile of black boxes with a total of $n_1 + \ldots + n_k$ input tubes and k output tubes. Now if we *also* have a guy in $X(k)$, which has k input tubes, we can hook up all the output tubes of all the boxes in our pile to the input tubes of this guy, to get a monstrous machine with $n_1 + \ldots + n_k$ input tubes and one output. In short, there is an operation from $X(n_1) \times \ldots \times X(n_k) \times X(k) \to X(n_1 + \ldots + n_k)$. For example, if we take the tree up there, which represents something in $X(3)$, and another thing in $X(3)$, we can hook up their outputs to the inputs of something in $X(2)$, to get something that looks like



which is in $X(6)$. The closed string field theorists like operads because there are lots of parametrized ways of gluing together Riemann surfaces with punctures together. It's a handy language, apparently... I am a bit more familiar with operads (though not much) in the context of homotopy theory, where they can be used to elegantly summarize the operations one has floating around in an infinite loop space. *Very* roughly, an infinite loop space is a space that looks like the space of loops of loops of loops of loops... of loops in some topological space, where you get to make the "dot dot dot" part go on as long as you want! A beautifully unpretentious and utterly readable book on these spaces, operads, and much much more, is:

7) J. F. Adams, *Infinite Loop Spaces*, Princeton U. Press, Princeton, 1978.

Lest "infinite loop spaces" seem abstruse, I should emphasize that the book is really a nice tour of a lot of modern homotopy theory. As he says, "my object has been a more elementary exposition, which I hope may convey the basic ideas of the the subject in a way as nearly painless as I can make it. In this the Princeton audience encouraged me; the more I found means to omit the technical details, the more they seemed to like it." A lot of the general mathematical machinery he discusses, especially in the chapter called "Machinery", is really too nice to be left for only the homotopy theorists!

Anyway, once you have gotten the hang of operads you can try the work of a reformed homotopy theorist, Jim Stasheff, on string field theory:

8) Jim Stasheff, "Closed string field theory, strong homotopy Lie algebras and the operad actions of moduli spaces", available as hep-th/9304061.

Actually Graeme Segal, another string theory guru, also used to do homotopy theory. He's the one who's famous for:

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

So it's possible that these guys didn't really quit homotopy theory, but just figured out how to get physicists interested in it. Notice all those loops! :-)

But where was I... romping through various approaches to string theory, taking a detour to mention loops, but all the while sneaking up on my goal, which is to list a few papers that lend evidence to the thesis of my paper Strings, Loops, Knots and Gauge Fields, namely that a profound "string/gauge field duality" is at work in many physical models, and that the loop representation of quantum gravity, and string theory, may eventually not be seen as so different after all.

Let's see what we've got here:

10) "A reformulation of the Ponzano–Regge quantum gravity model in terms of surfaces", Junichi Iwasaki, University of Pittsburgh, 11 pages in LaTeX format available as gr-qc/9410010.

I've discussed the Ponzano–Regge model quite a bit in "Week 16" and "Week 38". It's an approach to quantum gravity that is especially successful in 3 dimensions, and involves chopping spacetime up into simplices. The exact partition function, as they say, can be computed using this combinatorial discrete approximation to the spacetime manifold. (In quantum field theory, when you know enough about the partition function you can compute the expectation values of observables to your heart's content.) Anyway, here Iwasaki does the kind of thing I was pointing towards in my paper, namely, to rewrite the theory, which starts out as a gauge theory, as a theory of surfaces ("string worldsheets") in spacetime.

Meanwhile, more work has been done on the same kind of idea for good old quantum chromodynamics, though here there *is* a background geometry, and one approximates the spacetime manifold by a discrete lattice not because one expects to get the *exact* answers out that way, but just because it's a decent approximation that makes things a bit more manageable:

11) B. Rusakov, "Lattice QCD as a theory of interacting surfaces", available as hep-th/9410004.

Ivan K. Kostov, "$\mathrm{U}(N)$ gauge theory and lattice strings", available as hep-th/9308158.

Also, if there were any gauge theory that deserved to be a string theory, it's probably Chern–Simons theory, which has so much to do with knots... and indeed something like this seems to be the case, though it's all rather subtle and mysterious so far:

12) Michael R. Douglas, "Chern–Simons-Witten theory as a topological Fermi liquid", available as hep-th/9403119.

Frequently, when there is a whole lot of frenetic, sophisticated-sounding activity around a certain idea, like this relation between strings and gauge fields, there is a simple truth yearning to be known. Sometimes it takes a while! We'll see.

Week 43

November 5, 1994

It is very exciting, yet somewhat scary, as work continues on the loop representation of quantum gravity. On the one hand, researches are busy making it mathematically rigorous; on the other hand, they are beginning to understand its physical significance. The reasons for excitement are obvious, but the scary part is that until the final touches are put on the mathematical rigor, we don't know if the theory really exists!

Of course there is the whole separate issue of whether the theory will find experimental confirmation. If the theory were experimentally confirmed, questions of mathematical rigor wouldn't be quite such a big deal. But experimental verification will probably take a long time! Also, we don't really expect a theory of "pure gravity" to be experimentally confirmed. One will need to figure out how all the other particles and fields fit in — except perhaps for very general, qualitative issues. (See the paper mentioned at the very end of this article for some of *those*.) So here the suspense is of a long-term sort. Luckily, the question of whether the theory makes mathematical sense is already very interesting, since so many theories of quantum gravity have already been shot down on that basis, and the loop representation approach seems so pretty. Either it will make sense, or we will run into some obstacle, which is bound to be enlightening.

Let me briefly review the loop representation, without too many technical details. For more details try the original paper by Rovelli and Smolin (see "Week 42" for a reference), the book by Ashtekar (see "Week 7"), or, especially if you're a mathematician, my review article "Knots and quantum gravity: progress and prospects".

There are 3 basic steps in the "canonical quantization" of general relativity. At each step there is a vector space of quantum states, but only in the last do we really need a Hilbert space of states, since only when we're done do we want to be able to compute expectation values of observables, which takes an inner product.

In what follows I'll talk about the simplest situation, where we have the *vacuum* Einstein equations

$$G = 0$$

where G is the "Einstein tensor" cooked up from the curvature of spacetime. Say spacetime is of the form $\mathbb{R} \times S$, where $\mathbb{R}$ is the real numbers (time) and S is a 3-dimensional manifold (space). We will think of S as the "$t = 0$ slice" of $\mathbb{R} \times S$.

I) The first stage is to get the space of "kinematical states". In the quantum mechanics of a point particle on the line, the space of wavefunctions is a space of functions on the real line. Similarly, in quantum gravity we naively expect kinematical states to be functions on the space of Riemannian metrics on the 3-dimensional manifold S we're taking to be "space". In the loop representation one does something a bit more clever, but let's move on and then come back to that.

II) The second stage is getting the space of "diffeomorphism-invariant states". In fact, Einstein's equations in coordinates look like

$$G_{\mu\nu} = 0$$

where the indices μ, ν range from 0 to 3. It's customary to work in coordinates x_μ where x_0 is "time" and the other three coordinates are the "space" coordinates on S. Then classically, the equations $G_{0\mu} = 0$ serve as *constraints* on the initial data for Einstein's equations, while the remaining equations describe time evolution. I.e., only for certain choices of a metric and its first time derivative at $t = 0$ can we get a solution of Einstein's equations. In fact, $G_{0\mu}$ can be calculated knowing only the metric and its first time derivative at $t = 0$, and the equations saying they are zero are the constraints that this data must satisfy to get a solution of Einstein's equations.

Following the usual recipes of quantum theory, we want to turn these constaints into *operators* on the kinematical Hilbert space of stage I, and then demand that the states relevant for physics be annihilated by these operators. The "diffeomorphism-invariant subspace" is the subspace of the kinematical state space that is annihilated by the constraints corresponding to G_{0i} where $i = 1, 2, 3$. Let us put off for a moment why it's called what it is!

III) The third and final stage is getting the space of "physical states". Here we look at the subspace of diffeomorphism-invariant states that are also annihilated by the constaint corresponding to G_{00}. The equation saying that a diffeomorphism-invariant state is annihilated by this constraint is called the "Wheeler–DeWitt equation", and this is generally regarded as the fundamental equation of quantum gravity.

Now, it should make some sense why we call the "physical states" what we do. These are quantum states satisfying the quantum analogues of the constraints that the *classical* initial data must satisfy to be initial data for a solution of Einstein's equations. But why do we impose the constraints $G_{\mu\nu} = 0$ in two separate stages, and call the states in part II "diffeomorphism-invariant states"?

This is a very important question which gives quantum gravity much of its curious character. In classical general relativity, G_{0i} not only gives one of Einstein's equations, namely $G_{0i} = 0$, it also "generates diffeomorphisms" of the 3-dimensional manifold S representing space. If you don't quite know what this means, let me simply say that in classical mechanics, observables give rise to one-parameter families of symmetries. For example, momentum gives rise to spatial translations, while energy (aka the Hamiltonian) gives rise to time translations. We say that the observable "generates" the one-parameter family of symmetries. This is (roughly) what I mean by saying that G_{0i} generates diffeomorphisms of S. Similarly, G_{00} generates diffeomorphisms of the spacetime $\mathbb{R} \times S$ corresponding to time evolution.

A similar thing happens in quantum theory. BUT: in quantum theory, if a state is annihilated by some observable, it implies that the state is invariant under the one-parameter family of symmetries generated by that observable. This is not true in classical mechanics. Indeed, it's rather odd. But what it implies is that in step II we are really restricting ourselves to kinematical states that are invariant under diffeomorphisms of the spatial manifold S. This is why we call them "diffeomorphism-invariant" states. Similarly, in step III we're further restricting ourselves to states that are invariant under time evolution. The final "physical states" are, at least heuristically, invariant under *all diffeomorphisms of spacetime*. (So maybe the physical states are the ones that really should be called "diffeomorphism-invariant" — but it's too late now.) While this may seem odd, all it really means is that in the quantum theory of gravity — at least when one does it this way — the physical states describe only those aspects of the world that are independent of any choice of coordinate system. That has a certain charm, philosophically speaking. It is, however, not something physicists are used to.

Now, the general scheme outlined above has been around ever since the work of DeWitt:

1) Bryce S. DeWitt, "Quantum theory of gravity, I-III", *Phys. Rev.* **160** (1967), 1113–1148, **162** (1967) 1195–1239, 1239–1256.

However, the problem has always been making the scheme mathematically rigorous, or else to do some kind of calculations that shed some light on the meaning of it all! There are lots of problems. Let me not delve into them now, but simply cut directly to the "new variables" idea for handling these problems. The key idea of Ashtekar was to use as basic variables, not the metric on S and its first time derivative, but the "chiral spin connection" on S and a "complex frame field". To describe these would require a digression into differential geometry that I'm not in the mood for right now, especially since I already explained this stuff a bit in "Week 7". (There I call the chiral spin connection the "right-handed" connection.)

I do, however, want to emphasize that the new variables rely heavily upon some of the basic group-theoretic facts about 3 and 4 dimensions. The group of rotations in 3d space is called $\mathrm{SO}(3)$, because mathematically these are 3×3 orthogonal matrices with determinant 1. Now, a key fact in math and physics is that this group has the group $\mathrm{SU}(2)$ of 2×2 complex unitary matrices with determinant 1 as a "double cover". This means roughly that there are two elements of this other group corresponding to each element of $\mathrm{SO}(3)$. It's this fact that allows the existence of spin-$\frac{1}{2}$ particles!

Now, $\mathrm{SU}(2)$ is sitting inside a bigger group, $\mathrm{SL}(2, \mathbb{C})$, the group of all 2×2 complex matrices with determinant 1, not necessarily unitary. Just as $\mathrm{SU}(2)$ is used to describe the symmetries of spin-$\frac{1}{2}$ particles in space, $\mathrm{SL}(2, \mathbb{C})$ describes the symmetries of spin-$\frac{1}{2}$ particles in spacetime. The reason is that $\mathrm{SL}(2, \mathbb{C})$ is the double cover of the group $\mathrm{SO}(3, 1)$ of Lorentz transformations.

Given a Riemannian metric on the space S, there is always an "$\mathrm{SO}(3)$ connection" describing how objects rotate when you move them around a loop, due to the curvature of space. This is called the Levi–Civita connection. With a little work we can also think of this as an $\mathrm{SU}(2)$ connection. However, Ashtekar works instead with the chiral spin connection, which is an $\mathrm{SL}(2, \mathbb{C})$ connection cooked up from the Levi–Civita connection and the first time derivative of the metric (which turns out to be closely related to the "extrinsic curvature" of S as it sits in the spacetime $\mathbb{R} \times S$.)

The great advantage of Ashtekar's "new variables" is that the Hamiltonian and diffeomorphism constraints are simpler in these variables. Unfortunately, they lead to a curious new issue which at first seemed very nasty — the problem of "reality conditions". This has a lot to do with going from $\mathrm{SU}(2)$, which is a "real" group in a technical sense, to $\mathrm{SL}(2, \mathbb{C})$, which is a "complex" group that's roughly twice as big. Essentially, Ashtekar's formalism seems at first to be better suited to general relativity with a complex-valued metric than to good old "real" general relativity. For quite a while people didn't know quite what to do about this, so a lot of work on the new variables more or less ignores this issue. Luckily, there is now a very elegant approach to handling it, worked out by Ashtekar and collaborators. They are coming out with a couple of papers on this, hopefully by mid-November:

2) Abhay Ashtekar, Jerzy Lewandowski, Donald Marolf, José Mourão and Thomas Thiemann, "Coherent state transforms for spaces of connections", available as gr-qc/9412014.

   Abhay Ashtekar, Jerzy Lewandowski, Donald Marolf, José Mourão and Thomas Thiemann, "Quantization of diffeomorphism invariant theories of connections with local degrees of freedom", available as gr-qc/9504018.

The first paper constructs a kind of transform that takes functions on the space of $\mathrm{SU}(2)$ connections on S into functions on the space of $\mathrm{SL}(2,\mathbb{C})$ connections on S. The "kinematical states" in Ashtekar's approach are, roughly speaking, functions of the latter kind. (Really they are more like "measures".) This is some really pretty mathematics — it's a kind of generalization of the Bargmann–Segal transform to the case of functions on spaces of connections.

Physically, the transform allows us to relate Ashtekar's approach to the traditional "metric" approach much more clearly, since, as I described, $\mathrm{SU}(2)$ connections are closely related to metrics on S. The second paper should treat the physics behind this in more detail, and also describe a rigorous construction of "loop states" — a large class of diffeomorphism-invariant states which Rovelli and Smolin have claimed are actually *physical* states. (For more on these, see below.) This means that to check Rovelli and Smolin's claim, the main thing we need is a rigorous treatment of the Hamiltonian constraint in quantum gravity.

Unfortunately, this is where it gets scary, since the Hamiltonian constraint is a very tricky thing. For more on it, try:

3) M. Blencowe, "The Hamiltonian constraint in quantum gravity", *Nucl. Phys.* **B341** (1990), 213–251.

   Bernd Bruegmann and Jorge Pullin, "On the constraints of quantum gravity in the loop representation", *Nucl. Phys.* **B390** (1993), 399–438.

   Bernd Bruegmann, *On the Constraints of Quantum General Relativity in the Loop Representation*, Ph.D. Thesis, Syracuse University, 1993.

The loop states of Rovelli and Smolin are in one-to-one correspondence with *knots* in space, or more precisely, isotopy classes of knots. (Roughly, two knots are isotopic if you can get one from the other by applying a diffeomorphism of space that can be continuously deformed to the identity.)

What is the physical meaning of these loop states? Roughly it's this. Say you take a spin-$\frac{1}{2}$ particle and move it around in a path that traces out a knot. When you do this using the Levi–Civita connection, it comes back "rotated" by some $\mathrm{SU}(2)$ matrix. If you take the trace of this matrix (sum of diagonal entries) and divide by two, you get a number between -1 and 1. This number is called a "Wilson loop".

This should remind you of the Bohm–Aharonov effect where a split electron beam takes two paths from A to B. Depending on the magnetic flux through the loop, one can have constructive or destructive interference in the split beam experiment. Mathematically, one can imagine moving the electron around a loop that starts at A, goes to B by one path, and goes back to A by the other path. If this phase corresponding to going is 1 we get total constructive interference in the split beam experiment, while if it's -1 we get total destructive interference. So, just as the Bohm–Aharonov effect measures interference effects due to the magnetic field, the the above Wilson loop sort of measures the interference effects due to **gravity**!

In the Rovelli–Smolin loop state corresponding to a particular knot K, the expectation value of a Wilson loop around any knot K' will be 1 if K and K' are isotopic, and 0 otherwise! That's the physical meaning of the loop states: they describe quantum states of geometry in terms of the resulting interference effects on spin-$\frac{1}{2}$ particles.

Now, there is a more general kind of diffeomorphism-state than than the loop states. These are the spin network states! Here one fancies up the Wilson loop idea and imagines a graph embedded in space — i.e. a bunch of edges and vertices — where each edge is labelled by a spin that can be 0,$\frac{1}{2}$,1,$\frac{1}{2}$, etc. In the simplest flavor of spin network, one only allows 3 edges to meet at each vertex, and requires j_3 to be of the form

$$j_3 = |j_1 - j_2|, |j_1 - j_2| + 1, \ldots, j_1 + j_2 - 1, j_1 + j_2.$$

where j_1, j_2, j_3 are the spins labelling the edges adjacent to the given vertex. For example, we can have the the three spins be $\frac{1}{2}$,3, and $\frac{5}{2}$, because it's possible for a spin-$\frac{1}{2}$ particle and a spin-3 particle to interact and form a spin-$\frac{5}{2}$ particle. Here by "possible" I simply mean that it doesn't violate conservation of angular momentum. Mathematicians would say the spins should be thought of as irreducible representations of $\mathrm{SU}(2)$, and the condition above is just the condition that the representation j_3 appears as a summand in the tensor product of the representations j_1 and j_2.

Just as we can compute a kind of "Wilson loop" number from a knot that a spin-$\frac{1}{2}$ particle goes around, we can compute a number from a spin network. I've thought about spin networks for quite a while, since they are very important in topological quantum field theories. A great introduction to how they show up in TQFTs, by the way, is:

4) Louis Crane, Louis H. Kauffman, and David N. Yetter, "State-sum invariants of manifolds, I", available as hep-th/9409167.

This explains how to cook up 3d quantum gravity (or more precisely, the Turaev–Viro model) and a 4d TQFT field theory called the Crane–Yetter model using spin networks.

However, Rovelli's talk on spin network states in quantum gravity (see "Week 41"), followed by some good conversations, got me motivated to write up something on spin network states:

5) John Baez,"Spin network states in gauge theory", available as gr-qc/9411007.

Basically, I show that in the loop representation of any gauge theory, states at the kinematical level can be described by spin networks, slightly generalized. Heck, I'll quote my abstract:

Given a real-analytic manifold M, a compact connected Lie group G and a principal G-bundle $P \to M$, there is a canonical 'generalized measure' on the space

> *A/G of smooth connections on P modulo gauge transformations. This allows one to define a Hilbert space $L^2(A/G)$. Here we construct a set of vectors spanning $L^2(A/G)$. These vectors are described in terms of 'spin networks': graphs φ embedded in M, with oriented edges labelled by irreducible unitary representations of G, and with vertices labelled by intertwining operators from the tensor product of representations labelling the incoming edges to the tensor product of representations labelling the outgoing edges. We also describe an orthonormal basis of spin networks associated to any fixed graph φ. We conclude with a discussion of spin networks in the loop representation of quantum gravity, and give a category-theoretic interpretation of the spin network states.*

I'm now hard at work trying to show that spin networks also give a complete description of states at the diffeomorphism-invariant level. Well, actually right NOW I'm goofing off by writing this darn thing, but you know what I mean.

Rovelli and Smolin have come out with one of their papers on spin networks and they should be coming out with another soon. These are not about the rigorous mathematics of spin network states, but how to use them to really understand the *physics* of quantum gravity. The first one out is:

6) Carlo Rovelli and Lee Smolin, "Discreteness of area and volume in quantum gravity", available as gr-qc/9411005.

This is perhaps the most careful computation so far that derives *discreteness* of geometrical quantities directly from Einstein's equations and the principles of quantum theory! Let me quote the abstract:

> *We study the operator that corresponds to the measurement of volume, in non-perturbative quantum gravity, and we compute its spectrum. The operator is constructed in the loop representation, via a regularization procedure; it is finite, background independent, and diffeomorphism-invariant, and therefore well defined on the space of diffeomorphism invariant states (knot states). We find that the spectrum of the volume of any physical region is discrete. A family of eigenstates are in one to one correspondence with the spin networks, which were introduced by Penrose in a different context. We compute the corresponding component of the spectrum, and exhibit the eigenvalues explicitly. The other eigenstates are related to a generalization of the spin networks, and their eigenvalues can be computed by diagonalizing finite dimensional matrices. Furthermore, we show that the eigenstates of the volume diagonalize also the area operator. We argue that the spectra of volume and area determined here can be considered as predictions of the loop-representation formulation of quantum gravity on the outcomes of (hypothetical) Planck-scale sensitive measurements of the geometry of space.*

Week 44

November 6, 1994

SPECIAL EDITION: THE END OF DONALDSON THEORY?

I got some news today from Allen Knutson. Briefly, it appears that Witten has come up with a new way of doing Donaldson theory that is far easier than any previously known. According to Taubes, many of the main theorems in Donaldson theory should now have proofs that are $1/1000$th as long!

I suppose to find this exciting one must already have some idea of what Donaldson theory is. Briefly, Donaldson theory is a theory born in the 1980s that revolutionized the study of smooth 4-dimensional manifolds by using an idea from physics, namely, the self-dual Yang–Mills equations. The Yang–Mills equations describe most of the forces we know and love (not gravity), but only in 4 dimensions can one get solutions of them of a special form, known as self-dual solutions. (In physics these self-dual solutions are known as instantons, and they were used by 't Hooft to solve a problem plaguing particle physics, called the $\mathrm{U}(1)$ puzzle.)

Mathematically, 4-dimensional manifolds are very different from manifolds of any other dimension! For example, one can ask whether $\mathbb{R}^n$ admits any smooth structure other than the usual one. (Technically, a smooth structure for a manifold is a maximal set of coordinate charts covering the manifold which have smooth transition functions. Loosely, it's a definition of what counts as a smooth function.) The answer is no — *except* if $n = 4$, where there are uncountably many smooth structures! These "exotic $\mathbb{R}^4$'s" were discovered in the 1980's, and their existence was shown using the work of Donaldson using the self-dual solutions of the Yang–Mills equation, together with work of the topologist Freedman. More recently, a refined set of invariants of smooth 4-manifolds, the Donaldson invariants, have been developed using closely related ideas.

Some references are:

1) Simon K. Donaldson and Peter B. Kronheimer, *The Geometry of Four-Manifolds*, Oxford University Press, Oxford, 1990.

Simon K. Donaldson, "Polynomial invariants for smooth four-manifolds", *Topology* **29** (1990), 257–315.

Daniel S. Freed and Karen K. Uhlenbeck, *Instantons and Four-Manifolds*, Springer, Berlin, 1984.

Charles Nash, *Differential Topology and Quantum Field Theory*, Academic Press, London, 1991.

This is an extremely incomplete list, but it should be enough to get started. Or, while you wait for the new, simplified treatments to come out, you could make some microwave popcorn and watch the following video:

2) Simon K. Donaldson, "Geometry of four dimensional manifolds", videocassette (ca. 60 min.), color, American Mathematical Society, Providence, Rhode Island, 1988.

Now, what follows is my interpretation of David Dror Ben-Zvi's comments on a lecture by Clifford Taubes entitled "Witten's Magical Equation", these comments being kindly passed on to me by Knutson. I have tried to flesh out and make sense of what I received, and this required some work, and I may have screwed up some things. Please take it all with a grain of salt. I only hope it gives some of the flavor of what's going on!

So, we start with a compact oriented 4-manifold X with L a complex line bundle over X having first Chern class equal to w_2, the second Stiefel–Whitney class of TX, modulo 2. If X is spin (meaning that the $w_2 = 0$), take the bundle of spinors over X. Otherwise, pick a Spin-c bundle and take the bundle of complex spinors over X. Note that Spin-c structure is enough to define complex spinors on X, and it will always exist if w_2 is the $\mod 2$ reduction of an integral characteristic class. For more on this sort of stuff, try:

3) H. Blaine Lawson, Jr. and Marie-Louise Michelson, *Spin Geometry*, Princeton U. Press, Princeton, 1989.

In either case, take our bundle of spinors, tensor it with the square root of L, and call the resulting bundle B. (Perhaps someone can explain to me why L has a square root here; it's obvious if X is spin, but I don't understand the other case so well.) The data for our construction are now a connection A on L, and a section ψ of the self-dual part of B. (Note: I'm not sure what the "self-dual part of B" is supposed to mean. I guess it is something required to make the right-hand side of the formula below be self-dual in the indices a, b.) Consider now two equations. The first is the Dirac equation for ψ. The second is that the self-dual part F^+ of the curvature of A be given in coordinates as

$$F^+_{ab} = -\frac{1}{2}\langle \psi, e^a e^b \psi \rangle$$

where the basis 1-forms e^a, e^b act on ψ by Clifford multiplication.

Next form the moduli space M of solutions (A, ψ) modulo the action of the automorphisms of L. The wonderful fact is that this moduli space is always compact, and for generic metrics it's a smooth manifold. Still more wonderfully (here I read the lines between what was written), it is a kind of substitute for the moduli space normally used in Donaldson theory, namely the moduli space of instantons. It is much nicer in that it lacks the singularities characteristic of the other space.

What this means is that everything becomes easy! Apparently Taubes, Kronheimer, Mrowka, Fintushel, Stern and the other bigshots of Donaldson theory are frenziedly turning out new results even as I type these lines. On the one hand, the drastic simplifications are a bit embarassing, since the technical complications of Donaldson theory were the stuff of many erudite and difficult papers. On the other hand, Donaldson invariants were always notoriously difficult to compute. Taubes predicted that a purely combinatorial formula for them may be around within a year. (Here it is interesting to note the work of Crane, Frenkel, and Yetter in that direction; see "Week 2" and "Week 38".) This is sure to lead to a deeper understanding of 4-dimensional topology, and quite possibly, 4-dimensional physics as well.

Week 45

November 12, 1994

DONALDSON THEORY UPDATE

In the previous edition of "This Week's Finds" I mentioned a burst of recent work on Donaldson theory. I provocatively titled it "The End of Donaldson Theory?", since the rumors I was hearing tended to be phrased in such terms. But I hope I made it clear at the conclusion of the article that this recent work should lead to a lot of *new* results in 4-dimensional topology! An example is Kronheimer and Mrowka's proof of the Thom conjecture.

Many thanks to my network of spies for obtaining a preprint of the following paper:

1) Peter B. Kronheimer and Tomasz S. Mrowka, "The genus of embedded surfaces in the projective plane".

Let me simply quote the beginning of the paper:

> *The genus of a smooth algebraic curve of degree d in $\mathbb{CP}^2$ is given by the formula $g = (d-1)(d-2)/2$. A conjecture sometimes attributed to Thom states that the genus of the algebraic curve is a lower bound for the genus of any smooth 2-manifold representing the same homology class. The conjecture has previously been proved for $d \leqslant 4$ and for $d = 6$, and less sharp lower bounds for the genus are known for all degrees [references omitted]. In this note we confirm the conjecture.*
>
> *Theorem 1. Let S be an oriented 2-manifold smoothly embedded in $\mathbb{CP}^2$ so as to represent the same homology class as an algebraic curve of degree d. Then the genus g of S satisfies $g \geqslant (d-1)(d-2)/2$.*
>
> *Very recently, Seiberg and Witten [references below] introduced new invariants of 4-manifolds, closely related to Donaldson's polynomial invariants [reference omitted], but in many respects much simpler to work with. The new techniques have led to more elementary proofs of many theorems in the area. Given the monopole equation and the vanishing theorem which holds when the scalar curvature is positive (something which was pointed out by Witten), the rest of the argument presented here is not hard to come by. A slightly different proof of the Theorem, based on the same techniques, has been found by Morgan, Szabo and Taubes.*

The reference to Donaldson's polynomial invariants appears in "Week 44". The references to the new Seiberg–Witten invariants are:

2) Edward Witten,"Monopoles and four-manifolds", available as `hep-th/9411102`.

   Edward Witten and Nathan Seiberg, "Electric-magnetic duality, monopole condensation, and confinement in $N = 2$ supersymmetric Yang–Mills theory", available as `hep-th/9407087`.

   Edward Witten and Nathan Seiberg, "Monopoles, duality and chiral symmetry breaking in $N = 2$ supersymmetric QCD", available as `hep-th/9408099`.

Differential geometers attempting to read the second two papers will find that they contain no instance of the term "Donaldson theory", and they may be frustrated to find that these are very much *physics* papers. They concern the ground states of supersymmetric Yang–Mills theory in 4 dimensions with gauge group $\mathrm{SU}(2)$. The "ground states" of a field theory are its least-energy states, which represent candidates for the physical vacuum. In certain theories there is not a unique ground state, but instead a "moduli space" of ground states. Seiberg and Witten study these moduli spaces of ground states in both the classical and quantum versions of $\mathrm{SU}(2)$ supersymmetric Yang–Mills theory in 4 dimensions. They also consider the theory coupled to spinor fields, which they call "quarks", using the analogy of the theory to quantum chromodynamics, aka "QCD".

I haven't had time to go through their papers, since this isn't my main focus of interest. Perhaps the most useful thing I can do at this point is to use Kronheimer and Mrowka's clear description of their moduli space (which is presumably closely related to Seiberg and Witten's moduli spaces) to simplify and fill in the holes of what I wrote in "Week 44". I will aim my exposition to mathematicians, but make some elementary digressions on physics to spice things up.

We start with a compact oriented Riemannian 4-manifold X, and assume we are given a $\mathrm{Spin}^{\mathbb{C}}$ structure on X. Recall the meaning of this. First, the orthonormal frame bundle of X has structure group $\mathrm{SO}(4)$, and a spin structure would be a double cover of this which is a principal bundle with structure group given by the double cover of $\mathrm{SO}(4)$, namely $\mathrm{SU}(2) \times \mathrm{SU}(2)$. Thus we get two principal bundles with structure group $\mathrm{SU}(2)$, the left-handed and right-handed spin bundles. Using the fundamental representation of $\mathrm{SU}(2)$, we obtain two vector bundles called the bundles of left-handed and right-handed spinors. This "handedness" or "chirality" phenomenon for spinors is of great importance in physics, since neutrinos are left-handed spinors — meaning, in down-to-earth terms, that they always spin clockwise relative to their direction of motion. The fact that the laws of nature lack chiral symmetry came as quite a shock when it was first discovered, and part of Seiberg and Witten's motivation in their second paper is to study mechanisms for "spontaneous breaking" of chiral symmetry. This means simply that while the theory has chiral symmetry, its ground states need not.

A $\mathrm{Spin}^{\mathbb{C}}$ structure is a bit more subtle, but it allows us to define bundles of left-handed and right-handed spinors as $\mathrm{U}(2)$ bundles, which Kronheimer and Mrowka denote by $W+$ and $W-$. The determinant bundle L of $W+$ is a line bundle on X. The first big ingredient of the theory is a hermitian connection A on L. In physics lingo this is the vector potential of a $\mathrm{U}(1)$ gauge field. This gives a Dirac operator D_A mapping sections of $W+$ to sections of $W-$. The connection A has curvature F, and the self-dual part F^+ of F can be identified with a section of $\mathfrak{sl}(W+)$. (This is just a global version of the isomorphism between the self-dual part of $\Lambda^2\mathbb{C}^4$ and $\mathfrak{sl}(2,\mathbb{C})$.)

The second big ingredient of the theory is a section ψ of $W+$, i.e. a left-handed spinor field. There is a way to pair two sections of $W+$ to get a section of $\mathfrak{sl}(W+)$, which we write as $\sigma(.,.)$ and which is conjugate-linear in the first argument and linear in the second. This is a global version of the similar pairing

$$\sigma(.,.)\colon \mathbb{C}^2 \times \mathbb{C}^2 \to \mathfrak{sl}(2,\mathbb{C})$$

where $\sigma(v,w)$ given by taking the traceless part of the 2×2 matrix $v^*\otimes w$. Here v^* is the element of the dual of $\mathbb{C}^2$ coming from v via the inner product on $\mathbb{C}^2$.

To get the magical moduli space, we consider solutions (A, ψ) of

$$\begin{aligned} D_A\psi &= 0 \\ F^+ &= i\sigma(\psi, \psi). \end{aligned}$$

Here we are thinking of F^+ as a section of $\mathfrak{sl}(W+)$. These are pretty reasonable equations for some sort of massless left-handed spinor field coupled to a $\mathrm{U}(1)$ gauge field. Let M be the space of solutions modulo gauge transformations. Kronheimer and Mrowka show the "moduli space" M is compact.

One can also perturb the equations above as follows. If we have any self-dual 2-form δ on X we can consider

$$\begin{aligned} D_A\psi &= 0 \\ F^+ + i\delta &= i\sigma(\psi, \psi). \end{aligned}$$

and get a moduli space $M(\delta)$. This will still be compact if δ is nice (here I gloss over issues of analysis).

Now, if X has an almost complex structure, Kronheimer and Mrowka show that one can pick a $\mathrm{Spin}^{\mathbb{C}}$ structure for X such that, for "good" metrics and generic small δ, $M(\delta)$ is a compact 0-dimensional manifold. Using this fact and some geometrical yoga, it follows that the number n of points in $M(\delta)$, counted mod 2, is independent of (such) δ. (This is essentially a glorified version of the fact that, when you look at the multiple images of an object in a warped mirror and slowly bend the mirror, the images generically appear or disappear in pairs.) Moreover, if the self-dual Betti number b^+ of X is > 1, the space of good metrics is path-connected, and n mod 2 is independent of the choice of good metric. Kronheimer and Mrowka call this a "simple mod 2 version of the invariants of Seiberg and Witten". It is one ingredient of their proof of the Thom conjecture.

Week 46

December 12, 1994

I will be on sabbatical during the first half of 1995. I'll be roaming hither and thither, and also trying to get some work done on n-categories, quantum gravity and such, so this will be the last "This Week's Finds" for a while. I have also taken a break from being a co-moderator of `sci.physics.research`.

So, let me sign off with a roundup of diverse and sundry things! I'm afraid I'll be pretty terse about describing some of them. First for some news of general interest, then a little update on Seiberg–Witten theory, then some neat stuff on TQFTs, n-categories, quantum gravity and all that, and then various other goodies. . . .

1) Gary Stix, "The speed of write", *Scientific American*, Dec. 1994, 106–111.

   Jacques Leslie, "Goodbye, Gutenberg: pixilating peer review is revolutionizing scholarly journals", *Wired* 2.10, Oct. 1994. Available as https://www.wired.com/1994/10/ejournals/.

Among other things, the above articles show that Paul Ginsparg is starting to get the popular recognition he deserves for starting up `hep-th`. In case anyone out there doesn't know yet, `hep-th` is the "high-energy physics — theoretical" preprint archive, which revolutionized communications within this field by making preprints easily available worldwide, thus rendering many (but not all) aspects of traditional journals obsolete. The idea was so good it quickly spread to other subjects. Within physics it went like this:

- High Energy Physics — Theory (`hep-th`), started 8/91
- High Energy Physics — Lattice (`hep-lat`), started 2/92
- High Energy Physics — Phenomenology (`hep-ph`), started 3/92
- Astrophysics (`astro-ph`), started 4/92
- Condensed Matter Theory (`cond-mat`), started 4/92
- General Relativity & Quantum Cosmology (`gr-qc`), started 7/92
- Nuclear Theory (`nucl-th`), started 10/92
- Chemical Physics (`chem-ph`), started 3/94
- High Energy Physics — Experiment (`hep-ex`), started 4/94
- Accelerator Physics (`acc-phys`), started 11/94
- Nuclear Experiment (`nucl-ex`), started 11/94
- Materials Theory (`mtrl-th`), started 11/94
- Superconductivity (`supr-con`), started 11/94

Similar archives are sprouting up in mathematics (see below — but also note the existence of the American Mathematical Society preprint server, described later in this week's finds).

There are many ways to access these preprint archives, since Ginsparg has kept up very well with the times — indeed, so much better than I that I'm afraid to go into any details for fear of making a fool of myself. The *dernier cri*, I suppose, is to access the archives using the World-Wide Web, which is conveniently done by opening the document

```
http://xxx.lanl.gov
```

If this makes no sense to you, my first and very urgent piece of advice is to learn about the World-Wide Web (WWW), Mosaic, and the like, since they are wonderful and very simple to use! In the meantime, however, you can simply send mail to various addresses with subject header

```
help
```

and no message body, in order to get information. Some addresses are:

- `acc-phys@xxx.lanl.gov` (accelerator physics)
- `astro-ph@xxx.lanl.gov` (astrophysics)
- `chem-ph@xxx.lanl.gov` (chemical physics)
- `cond-mat@xxx.lanl.gov` (condensed matter)
- `funct-an@xxx.lanl.gov` (functional analysis)
- `gr-qc@xxx.lanl.gov` (general relativity / quantum cosmology)
- `hep-lat@ftp.scri.fsu.edu` (computational and lattice physics)
- `hep-ph@xxx.lanl.gov` (high energy physics phenomenological)
- `hep-th@xxx.lanl.gov` (high energy physics formal)
- `hep-ex@xxx.lanl.gov` (high energy physics experimental)
- `nucl-th@xxx.lanl.gov` (nuclear theory)
- `nucl-ex@xxx.lanl.gov` (nuclear experiment)
- `mtrl-th@xxx.lanl.gov` (materials theory)
- `supr-con@xxx.lanl.gov` (superconductivity)
- `alg-geom@publications.math.duke.edu` (algebraic geometry)
- `auto-fms@msri.org` (automorphic forms)
- `cd-hg@msri.org` (complex dynamics & hyperbolic geometry)

- `dg-ga@msri.org` (differential geometry & global analysis)
- `nlin-sys@xyz.lanl.gov` (non-linear systems)
- `cmp-lg@xxx.lanl.gov` (computation and language)
- `e-mail@xxx.lanl.gov` (e-mail address database)

One might also want to check out the:

> *Directory of Electronic Journals, Newsletters, and Academic Discussion Lists: Send e-mail to `ann@cni.org` at the Association of Research Libraries, +1 (202) 296-2296, fax +1 (202) 872 0884.*

Let me say a bit about how the AMS preprint server works. Assuming you are hip to the WWW, just go to

```
http://e-math.ams.org
```

You will then see a menu, and you can click on "Mathematical Preprints", and then "AMS Preprint Server", where preprints are classified by subject. Alternatively, click on "New Items This Month (all Subjects)".

On a related note, you can also get some AMS stuff using telnet by doing

```
telnet e-math.ams.org
```

and using

```
e-math
```

as login and password. This doesn't seem to get you to the preprints, though. For gopher fans,

```
gopher e-math.ams.org
```

has roughly similar effects.

2) Carlo Rovelli and Lee Smolin, "Spin networks in quantum gravity".

This paper is closely related to the earlier one in which Rovelli and Smolin argue that discreteness of area and volume arise naturally in the loop representation of quantum gravity, and also to my own paper on spin networks. (See "Week 43" for more on these, and a brief intro to spin networks.) Basically, while my paper shows that spin networks give a kind of basis of states for gauge theories with arbitrary (compact, connected) gauge group, in this paper Rovelli and Smolin concentrate on the gauge groups $\mathrm{SL}(2,\mathbb{C})$ and $\mathrm{SU}(2)$, which are relevant to quantum gravity, and work out a lot of aspects particular to this case, in more of a physicist's style. This makes spin networks into a practical computational tool in quantum gravity, used to great effect in the paper on the discreteness of area and volume.

3) Abhay Ashtekar, "Recent mathematical developments in quantum general relativity", available as gr-qc/9411055 (discussed in "Week 37").

   Abhay Ashtekar, Jerzy Lewandowski, Donald Marolf, Jose Mourao and Thomas Thiemann, "Coherent state transforms for spaces of connections", available as gr-qc/9412014 (discussed in "Week 43").

These are two papers on the loop representation of quantum gravity which I talked about in earlier "finds", and are out now. The former is a nice review of recent mathematically rigorous work; the latter takes a tremendous step towards handling the infamous "reality conditions" problem.

4) Abhay Ashtekar and Jerzy Lewandowski, "Differential geometry on the space of connections via graphs and projective limits", available as hep-th/9412073.

I've spoken quite a bit about doing rigorous functional *integration* in gauge theory using ideas from the loop representation; this paper treats functional *derivatives* and other things that are more differential than integral in nature. This is crucial in quantum gravity because the main remaining mystery, the Wheeler–DeWitt equation or Hamiltonian constraint, involves a differential operator on the space of connections. (For a wee bit more, try "Week 11" or "Week 43", where the Hamiltonian constraint is simply written as $G_{00} = 0$.)

Let me quote their abstract:

> *In a quantum mechanical treatment of gauge theories (including general relativity), one is led to consider a certain completion,* A*, of the space of gauge equivalent connections. This space serves as the quantum configuration space, or, as the space of all Euclidean histories over which one must integrate in the quantum theory.* A *is a very large space and serves as a "universal home" for measures in theories in which the Wilson loop observables are well-defined. In this paper,* A *is considered as the projective limit of a projective family of compact Hausdorff manifolds, labelled by graphs (which can be regarded as "floating lattices" in the physics terminology). Using this characterization, differential geometry is developed through algebraic methods. In particular, we are able to introduce the following notions on* A*: differential forms, exterior derivatives, volume forms, vector fields and Lie brackets between them, divergence of a vector field with respect to a volume form, Laplacians and associated heat kernels and heat kernel measures. Thus, although* A *is very large, it is small enough to be mathematically interesting and physically useful. A key feature of this approach is that it does not require a background metric. The geometrical framework is therefore well-suited for diffeomorphism invariant theories such as quantum general relativity.*

6) A. P. Balachandran, L. Chandar, Arshad Momen, "Edge states in gravity and black hole physics", available as gr-qc/9412019.

Ever since it started seeming that black holes have an entropy closely related to (and often proportional to) the area of their event horizons, many physicists have sought a better understanding of this entropy. In many ways, the nicest sort of explanation would say that the entropy was due to degrees of freedom living on the event horizon. A concrete calculation along these lines was recently made by Steve Carlip (see "Week 41") in the context of 2+1-dimensional gravity. The mechanism is mathematically very similar to what happens in (a widely popular theory of) the fractional quantum Hall effect! In both cases, Chern–Simons theory on a 3d manifold with boundary gives rise to an interesting field theory on the boundary, or "edge". The above paper clarifies this, especially for those of us who don't understand the fractional quantum Hall effect too well. Let me quote:

> *Abstract: We show in the context of Einstein gravity that the removal of a spatial region leads to the appearance of an infinite set of observables and their associated edge states localized at its boundary. Such a boundary occurs in certain approaches to the physics of black holes like the one based on the membrane paradigm. The edge states can contribute to black hole entropy in these models. A "complementarity principle" is also shown to emerge whereby certain "edge" observables are accessible only to certain observers. The physical significance of edge observables and their states is discussed using their similarities to the corresponding quantities in the quantum Hall effect. The coupling of the edge states to the bulk gravitational field is demonstrated in the context of (2+1) dimensional gravity.*

I can't resist adding that I have a personal stake in the notion that a lot of interesting things about quantum gravity will only show up when we consider it on manifolds with boundary, including the area-entropy relations. The loop representation of quantum gravity has a lot to do with knots and links, but on a manifold with boundary it has a lot to do with *tangles*, which can contain arcs that begin and end at the boundary. I wrote a paper on this a while back:

7) John Baez, "Quantum gravity and the algebra of tangles", *Jour. Class. Quantum Grav.* **10** (1993), 673–694. Available as hep-th/9205007.

and I'll be coming out with another in a while, co-authored with Javier Muniain and Dardo Piriz. The importance of manifolds with boundary for cutting-and-pasting constructions is also well-known in the theory of "extended" TQFTs (topological quantum field theories). These cutting and pasting operations should allow one to describe extended TQFTs in n dimensions purely algebraically using "higher-dimensional algebra". So part of the plan here is to understand better the relation between quantum gravity, TQFTs, and higher-dimensional algebra. Along these lines, a very interesting new paper has come out:

8) Louis Crane and David Yetter, "On algebraic structures implicit in topological quantum field theories", available as hep-th/9412025.

This makes more precise some of Louis Crane's ideas on "categorification". Nice TQFTs in 3 dimensions have a lot to do with Hopf algebras (like quantum groups), or alternatively, their categories of representations, which are certain braided monoidal categories. In this paper it's shown that nice TQFTs in 4 dimensions have a lot to do with Hopf categories, or alternatively, their categories of representations, which are certain braided monoidal 2-categories.

9) V. M. Kharlamov and V. G. Turaev, "On the definition of 2-category of 2-knots", *Les Rencontres Physiciens-Mathématiciens de Strasbourg-RCP25* **45** (1993), 151-166. Available as http://www.numdam.org/item/RCP25_1993_45_151_0/.

This preprint, which I obtained through my network of spies, *seems* to be implicitly claiming that the work of Fischer describing 2d surfaces in $\mathbb{R}^4$ via on braided monoidal 2-categories (see "Week 12") is a bit wrong, but they don't come out and say quite what if anything is really wrong.

10) Greg Kuperberg, "Non-involutory Hopf algebras and 3-manifold invariants", available as q-alg/9712047.

I noted the existence of a draft of this paper, and related work, in "Week 38". Let me quote:

> *Abstract: We present a definition of an invariant $\#(M, H)$, defined for every finite-dimensional Hopf algebra (or Hopf superalgebra or Hopf object) H and for every closed, framed 3-manifold M. When H is a quantized universal enveloping algebra, $\#(M, H)$ is closely related to well-known quantum link invariants such as the HOMFLY polynomial, but it is not a topological quantum field theory.*

Okay, now for some miscellaneous interesting things...

11) J. Lambek, "If Hamilton had prevailed: quaternions in physics", in *Mathematical Conversations*, Springer, Berlin, pp. 259–274.

Lambek is mainly known for work in category theory, but he has a strong side-interest in mathematical physics. This paper is, first of all, a "nostalgic account of how certain key results in modern theoretical physics (prior to World War II) can be expressed concisely in the labguage of quaternions, thus suggesting how they might have been discovered if Hamilton's views had prevailed." But there is a very interesting new thing, too: a way in which the group $\mathrm{SU}(3) \times \mathrm{SU}(2) \times \mathrm{U}(1)$ shows up naturally when considering Dirac's equation a la quaternions. This group is the gauge group of the Standard Model! Lambek modestly says that there does not appear to be any significance to this coincidence... but it *would* be nice, wouldn't it?

12) Nina Byers, "The life and times of Emmy Noether; contributions of E. Noether to particle physics", available as hep-th/9411110.

Bert Schroer, "Reminiscences about many pitfalls and some successes of QFT within the last three decades", available as hep-th/9410085.

Roman Jackiw, "My encounters — as a physicist — with mathematics", available as hep-th/9410151.

These are some interesting historical/biographical pieces.

13) Karl Svozil, "Speedup in quantum computation is associated with attenuation of processing probability", available as hep-th/9412046.

The subject of quantum computation has become more lively recently. I haven't had time to look at this paper, but quoting the abstract:

> *Quantum coherence allows the computation of an arbitrary number of distinct computational paths in parallel. Based on quantum parallelism it has been conjectured that exponential or even larger speedups of computations are possible. Here it is shown that, although in principle correct, any speedup is accompanied by an associated attenuation of detection rates. Thus, on the average, no effective speedup is obtained relative to classical (nondeterministic) devices.*

Week 47

January 17, 1995

Hi. I know I'm supposed to be taking a break from "This Week's Finds," but it's a habit that's hard to quit. I just want to briefly mention a couple of new electronic venues for mathematics and physics. First, there are a couple new preprint servers along the lines of `hep-th`:

- Nuclear Experiment (`nucl-ex@xxx.lanl.gov`), started 12/94
- Quantum Physics (`quant-ph@xxx.lanl.gov`), started 12/94

To check these out, send email with subject header "help" and no message body to the address listed, or use more slick modern method.

There are also a bunch of math preprint servers I have been neglecting to mention. The newest one is devoted to "quantum algebra" (quantum groups and the like) and knot theory! Here are some:

- Algebraic Geometry (`alg-geom@eprints.math.duke.edu`), started 2/92
- Functional Analysis (`funct-an@xxx.lanl.gov`), started 4/92
- Differential Geometry and Global Analysis (`dg-ga@msri.org`), started 6/94
- Automorphic Forms (`auto-fms@msri.org`), started 6/94
- Complex Dynamics and Hyperbolic Geometry (`cd-hg@msri.org`), started 6/94
- Quantum Algebra (Including Knot Theory) (`q-alg@eprints.math.duke.edu`), started 12/94

Also, there is a new electronic journal on the theory and applications of category theory. This is a subject dear to my heart since much of the most interesting work on topological quantum field theories uses category theory in an interesting way, so I hope mathematicians and mathematical physicists interested in category theory turn to this journal:

```
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Week 48

February 26, 1995

There are a few things I've bumped into that I feel I should let folks know about, so here's a special issue from Munich, where the Weissbier is very good. (And not at all white, but that's another subject.)

One of the most exciting aspects of mathematics over the last few years — in my utterly biased opinion — has been how topological quantum field theories have revealed the existence of deep connections between 3-dimensional topology, complex analysis, and algebra, particularly the algebra of quantum groups.

The most interesting topological quantum field theory in this game is Chern–Simons theory. This a field theory in 3 dimensions, and the reason it's called "topological" is that you don't need any metric or other geometrical structure on your 3d spacetime manifold for this theory to make sense. Thus it admits *all* coordinate transformations (or more precisely, all diffeomorphisms) as symmetries. In particular, this means that the quantity folks like to compute whenever they see a quantum field theory — the partition function, which you get by doing a path integral a la Feynman — is an invariant of 3-dimensional manifold you happen to have taken as "spacetime".

Now computing path integrals is often a very dubious and tricky business. They are integrals over the space of all possible histories of the classical fields corresponding to your quantum field theory. This is a big fat infinite-dimensional space, of the sort to which ordinary integration theory doesn't really apply. If you aren't very careful, path integrals often give infinite answers. So one very nice thing is that, suitably interpreted, the path integrals in Chern–Simons theory actually make rigorous sense!

A key advance here was Atiyah's axioms for topological quantum field theories (or TQFTs). These axioms formalize exactly what properties you'd hope path integrals would have in the case of a diffeomorphism-invariant theory. If, by no matter what devilish tricks, one can get a theory that satisfies these axioms, it's in many ways just as good if one had figured out how to make sense of the path integrals by honest labor, because all the manipulations one would normally want to do are then permitted. The marvelous thing about Chern–Simons theory is that one can show the TQFT axioms hold starting from some beautiful algebraic structures called quantum groups. Corresponding to every "semisimple Lie group" — examples being the groups $\mathrm{SU}(n)$ of unitary complex nxn matrices with determinant 1 — there is a quantum group, which is not really a group, but a so-called "quasitriangular Hopf algebra." Amusingly these quantum groups really depend on Planck's constant $\hbar$, and reduce to the ordinary groups in the "classical limit" $\hbar \to 0$!

Now, where these quantum groups come from has always been a bit of a puzzle. They can be rigorously shown to exist, that's for sure. There are also many good algebraic reasons why they "should" exist. But it is still a bit remarkable that they have the exactly the properties needed to get Chern–Simons theory to be a TQFT. So people have tried in many ways to turn the tables on them, and get *them* from the *path integrals*. Lots of these approaches have succeeded, but most of them involve a few subtleties here and there, so mathematicians, who only feel happy when everything is *blindingly obvious* (to them, that is, not you), have continued to seek the most beautiful, elegant way of getting at them.

1) Daniel Freed, "Quantum groups from path integrals", available as q-alg/9501025.

This is a nice expository treatment of the work of Free and Quinn on topological quantum field theories, particularly Chern–Simons theory with finite gauge group. In this case, the path integral reduces to a finite sum and one really can get the quantum group from the path integral very beautifully. But there are some differences in this case of finite gauge group. For example, the resulting "finite quantum group" does *not* depend on $\hbar$; it's just the "quantum double" of the group algebra of the group. For more on what this has to do with marvelous algebraic things like n-categories, the reader should check out the paper by Freed cited in "Week 12", which has subsequently been published:

2) Daniel Freed, "Higher algebraic structures and quantization", *Commun. Math. Phys.* **159** (1994), 343–398.

and also

3) Daniel Freed and Frank Quinn, "Chern–Simons theory with finite gauge group", *Commun. Math. Phys.* **156** (1993), 435–472.

Now in addition to the path-integral approach to quantum field theory there is another, the so-called Hamiltonian approach, which is very much like the approach people usually learn in a first course on quantum mechanics: if you know the wavefunction of your system at $t = 0$, the Hamiltonian tells you how it evolves in time from then on. Now this has special subtleties in diffeomorphism-invariant theories. When there is no unique best coordinate system, there's no unique best notion of "time evolution". This leads to the so-called "problem of time", very important in quantum gravity, but rather easier to deal with in toy models like Chern–Simons theory.

Now if we take a 3-dimensional spacetime and look at the $t = 0$ slice, we will with some luck get a 2-dimensional manifold, such as a sphere, torus, or more general n-holed doughnut. This is where the complex analysis comes in, because the complex plane is 2-dimensional, and we can cover these surfaces with coordinate systems that look locally like the complex plane, making them into "Riemann surfaces" upon which we can merrily proceed with complex analysis. In particular, the phase space of classical Chern–Simons theory is something of which complex analysts have long been enamored, namely the "moduli space of flat bundles" over our Riemann surface. (Let me reassure physicists that this "flat" business is merely a weird way of saying that in Chern–Simons theory the analog of the magnetic field vanishes.)

Now starting from the description of the classical phase space for Chern–Simons theory one should be able to get ahold of the quantum theory by some "quantization" business just as one does in elementary quantum mechanics, where the "classical phase space" is the space of p's and q's, and to quantize one merely decrees that these no longer commute:

$$pq - qp = -i\hbar.$$

So one should be able to get ahold of quantum groups this way too: starting with the "moduli space of flat bundles" and "quantizing" it. I had long why nobody did this in the way which seemed most tempting and plausible to me. To lapse into jargon for a bit here, since the quantum group is obtained from the group by deformation quantization, where the Poisson structure of the group itself is described by some "classical r-matrices", and since Chern–Simons theory is in some sense obtained by quantizing the moduli space, where the Poisson structure was explicitly described by Goldman, it seemed natural to me that somehow the classical r-matrices should be *derivable* from Goldman's formulas. But after a few wimpy tries at figuring it out, with not much success, I gave up. Luckily it turns out someone else succeeded nicely, as I found out in a talk by Alekseev here at the Mathematisches Institut of the Universität München:

4) V. V. Fock and A. A. Rosly, "Poisson structures on moduli of flat connections on Riemann surfaces and r-matrix", math/9802054.

They figured out a beautiful formula relating the classical r-matrices and the Poisson structure on moduli space. Using this, Alekseev, Grosse, and Schomerus were able to get at quantum groups quite directly from deformation quantization of moduli space, though there are a few important points left to nail down:

5) Yu. Alekseev, H. Grosse, and V. Schomerus, "Combinatorial quantization of the Hamiltonian Chern–Simons theory, I & II". Available as hep-th/9403066 and hep-th/9408097.

In fact Schomerus told me about this while I was in Cambridge Mass. over Christmas, but somehow I didn't pick up on the coolest thing about it, that it gets the quantum groups quite naturally from Chern–Simons theory, via the relation between the Poisson brackets on moduli space and the classical r-matrices.

Let's see. This is getting technical so I'll give in and get technical. One other paper that I found out about while here studies rather similar issues, but from the viewpoint of geometric quantization rather than deformation quantization. Axelrod, Della Pietra and Witten did some very fundamental work on geometric quantization of Chern–Simons theory:

6) Scott Axelrod, Steve Della Pietra and Edward Witten, "Geometric quantization of Chern–Simons gauge theory",*Jour. Diff. Geom.* **33** (1991), 787–902. Also available as https://projecteuclid.org/euclid.jdg/1214446565.

but this only treated the case of Riemann surfaces, not the Riemann surfaces with punctures that you need to think about when sticking *knots* in your 3-manifold. Martin Schottenloher, one of the professors, here, gave me a nice review of the work of one of his students on the case with punctures:

7) Martin Schottenloher, "Metaplectic quantization of the moduli space of flat and parabolic bundles (after Peter Scheinhost)", in *Public. I. R. M. A. Strasbourg*, **45** (1993), 43–70. Also available as http://www.numdam.org/item/RCP25_1993_45_43_0/.

This uses a lot of complex geometry that's beyond my ken, but one very exciting remark penetrated my thick skull, namely that you really need to take into account the so-called "metaplectic correction", as one usually does in geometric quantization, and that in the case of no punctures, this has the sole effect of accomplishing the mysterious "level shift"

$(k \to k + N)$ that pervades $\mathrm{SU}(N)$ Chern–Simons theory. (Of course, I bet this must be what's going on for other gauge groups too.) Also, when there are punctures, you apparently really need the metaplectic correction to get the right answers from geometric quantization; the ad hoc level shift ain't enough.

Well, I have some other things, but this issue seems more or less devoted to Chern–Simons theory so far, and only Chern–Simons freaks are likely to have read this far, so I'll put off the rest for later.

Week 49

February 27, 1995

For the last year or so I've been really getting interested in n-categories as a possible tool for unifying a lot of strands in mathematics and physics. What's an n-category? Well, in a sense that's the big question! Roughly speaking, it's a structure where there are a bunch of "objects", and for any pair of objects x, y a bunch of "morphisms" from x to y, written $f\colon x \to y$, and for any pair of morphisms $f, g\colon x \to y$ a bunch of "2-morphisms" from f to g, written $F\colon f \Rightarrow g$, and for any pair of 2-morphisms $F, G\colon f \Rightarrow g$ a bunch of "3-morphisms" from F to G, $\ldots$ and so on, up to n-morphisms. Ordinary categories, or 1-categories, have been studied since the 1940s or so, when they were invented by Eilenberg and Mac Lane. Anyone wanting to get going on those could try:

1) Saunders Mac Lane, *Categories for the Working Mathematician*, Springer, Berlin, 1988.

Roughly, categories give a general framework for dealing with situations where you have "things" and "ways to go between things", like sets and functions, vector spaces and linear maps, points in a space and paths between points, etc. That's a pretty broad territory! n-categories show up when you also have "ways to go between ways", "ways to go between ways to go between ways", etc. That may seem a little weird at first. But in fact they show up in a lot of places if you look for them. Perhaps the most obvious place is topology. If you think of a point in a space as an object, and a path between two points as a morphism:

$$x \xrightarrow{f} g$$

you are easily tempted to think of a "path of paths" as a 2-morphism. Here a "path of paths" is just a continuous 1-parameter family of paths from x to y, which you can think of as tracing out a 2-dimensional surface, as follows:

$$\begin{tikzcd} x \arrow[r, bend left=50, "f"{name=U}] \arrow[r, bend right=50, "g"'{name=D}] & y \arrow[Rightarrow, from=U, to=D, "F"] \end{tikzcd}$$

And one can keep on going and look at "paths of paths of paths", etc. In fact, people in homotopy theory do this all the time.

There is another example, equally primordial, which is a bit more "inbred" in flavor. In other words, if you already know and love n-categories, there is a wonderful example of an $(n + 1)$-category which you should know and love too! Now this isn't so bad, actually, because a 0-category is basically just a *set*, namely the set of objects. Since everyone knows and loves sets, everyone can start here! Okay, there is a wonderful 1-category called Set, the category of all sets. This has sets as its objects and functions between sets as its morphisms. So now that you know an example of a 1-category, you know and love 1-categories, right? Well, it turns out there is this wonderful thing called Cat, the 2-category of all 1-categories. (Usually people restrict to "small" 1-categories, which have a mere *set* of objects, so that set theorists don't start freaking out at a certain point.) To understand why Cat is a 2-category is a bit of work, but as objects it has categories, as morphisms it has the usual sort of morphisms between categories, so-called "functors", and as 2-morphisms it has the usual sort of things that go between functors, so-called "natural transformations". These are the bread and butter of category theory; just take my word for it if you haven't studied them yet! Okay, so Cat is a 2-category, so now you know and love 2-categories, right? (Well, I haven't even told you the definitions, but just nod your head.) Guess what: there is this wonderful thing called 2Cat, the 3-category of all 2-categories! And so on.

So in short, the detailed theory of n-categories at each level automatically leads one to get interested in $(n+1)$-categories. Now for the bad news: so far, people have only figured out the right definition of n-category for $n = 0, 1, 2, 3$. By the "right" definition I mean the ultimate, most general definition, which should be the most useful in many ways. So far people only know about "strict" n-categories for all n, which one can think of as a special case of the ultimate ones; the ultimate 1-categories are just categories, the ultimate 2-categories are often called bicategories (see the reference to Benabou in "Week 35"), and ultimate 3-categories are usually called tricategories (see the reference to the paper by Gordon, Power and Street) in "Week 29". Tricategories were just defined last year! They have a whole lot to do with knots, Chern–Simons theory, and other 3-dimensional phenomena, as one might expect. If we could understand the ultimate 4-categories — tetracategories? — it would probably help us with some of the riddles of topology and physics in 4 dimensions. (Indeed, what little we *do* understand is already helping a bit.)

So anyway, I have been trying to learn about these things, and had the good luck to meet James Dolan via the net, who has helped me immensely, since he eats, lives and breathes category theory, and he is now at Riverside hard at work figuring out the ultimate definition of n-categories for all n. (Although when he reads this, he will not be hard at work; he will be goofing off, reading the news.)

He and I have have written one paper so far espousing our philosophy concerning n-categories, topology, and physics:

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

One of the main themes of this paper is what I sometimes jokingly call the "periodic table". Say you have an $(n+k)$-category with only one object, one morphism, one 2-morphism, ... and only one $(k-1)$-morphism. Then all the interest lies in the k-morphisms, the $(k+1)$-morphisms, and so on up to the $(k+n)$-morphisms. So there are n interesting levels of morphism, and we can actually think of our $(n+k)$-category as an n-category of a special sort. Let's call this kind a "k-tuply monoidal n-category". Now we can make a chart of these:

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
$k=5$	" "	" "	" "

First, I should emphasize that some parts of the chart as I've drawn it here are a bit conjectural; since we don't know what the most general 7-categories are like, for example, we don't really know for sure what 5-tuply monoidal 2-categories are like. The exact status of all the entries on the table is made more clear in the paper. For now, let me just say, first, that these various flavors of n-categories turn out to be of great interest in topology — some have already been used a lot in topological quantum field theory and knot theory, other less, so far, but they all seem to have lot to do with generalizations of knot theory to different dimensions. Second, it seems that the nth column "stabilizes" by the time you get down to the $(n+2)$nd row. This very interesting pattern turns out also to have a lot to do with knots and their generalizations, and also to a subject called stable homotopy theory.

Now it also appears that there is a nice recipe for hopping down the columns. (Again, we only understand this *perfectly* in certain cases, but the pattern seems pretty clear.) In other words, there's a nice recipe to get a (k+1)-tuply monoidal n-category from a k-tuply monoidal one. It goes like this. Hang on to your seat. You start with a k-tuply monoidal n-category $\mathcal{C}$. It's a special sort of $(n+k)$-category, so its an object in $(n+k)\mathsf{Cat}$. But $(n+k)\mathsf{Cat}$, remember, is an $(n+k+1)$-category. Now look at the largest sub-$(n+k+1)$-category of $(n+k)\mathsf{Cat}$ which has $\mathcal{C}$ as its only object, $1_{\mathcal{C}}$ (the identity of $\mathcal{C}$) as its only morphism, $1_{1_{\mathcal{C}}}$ as its only 2-morphism, $1_{1_{1_{\mathcal{C}}}}$ as its only 3-morphism, and so on, up to $1_{1_{1_{\dots}}}$ as its only k-morphism. Let's call this $\mathcal{C}'$. If one keeps count, this should be a $(k+1)$-tuply monoidal n-category. That's how it goes.

Now say we do this to an example. Say we do it to the category $\mathcal{C}$ of all representations of a finite group G. This is in fact a monoidal category, so the result $\mathcal{C}'$ is a braided monoidal category. It is, in fact, just the category of representations of the "quantum double" of G, which is an example of what one might call a "finite quantum group". These play a big role in the study of Chern–Simons theory with finite gauge group (see the papers by Freed and Quinn in "Week 48"). One can also get the other quantum groups with the aid of this "quantum double" trick. A good description of this case appears in:

3) Christian Kassel and Vladimir Turaev, "Double construction for monoidal categories", *Acta Mathematica* **175** (1995), 1–48. Also available as `https://projecteuclid.org/euclid.acta/1485890865`.

So this is rather remarkable: starting from a finite group, and all this n-categorical abstract nonsense, out pops precisely the raw ingredients for a perfectly respectable 3-dimensional topological quantum field theory! Understanding *why* this kind of thing works is part of the aim of Dolan's and my paper, though there are some important pieces of the puzzle that we don't get around to mentioning there.

Right now I'm busily working out the details of how to get braided monoidal 2-categories from monoidal 2-categories by the same trick, with the aid of Martin Neuchl and Frank Halanke here. These should have a lot to do with 4-dimensional topological quantum field theories (see e.g. the paper by Crane and Yetter cited in "Week 46"). And here I can't resist mentioning a very nice paper by Neuchl and Schauenburg,

4) Martin Neuchl and Peter Schauenburg, "Reconstruction in braided categories and a notion of commutative bialgebra", *Jour. Pure and Applied Algebra* **124** (1998), 241–259.

Let me conclude by describing this. I always let myself get a bit more technical at the end of each issue, so I'll do that now. The relationship between Hopf algebras and monoidal categories is given by "Tannaka–Krein reconstruction theorems", which give conditions under which a monoidal category is equivalent to the category of representations of a Hopf algebra, and actually constructs the Hopf algebra for you. In physics people use related but fancier "Doplicher–Haag–Roberts" theorems to reconstruct the gauge group of a quantum field theory. This paper starts with the beautiful Tannaka–Krein theorem in

5) Peter Schauenburg, "Tannaka duality for arbitrary Hopf algebras", *Algebra-Berichte* **66** (1992).

Leaving out a bunch of technical conditions that make the theorem actually *true*, it says roughly that when you have a braided monoidal category $\mathcal{B}$, a category $\mathcal{C}$, and a functor $f\colon \mathcal{C} \to \mathcal{B}$, there is a coalgebra object a in $\mathcal{B}$, the universal one for which f factors through the forgetful functor from $a\mathsf{Comod}$ (the category of a-comodule objects in $\mathcal{B}$) to $\mathcal{B}$. The point is that the ordinary Tannaka–Krein theorem is a special case of this one where $\mathcal{B}$ is the category of vector spaces. The point of the new paper is as follows. Suppose $\mathcal{C}$ is actually braided monoidal and f preserves the braiding and monoidal structure. Then we expect a to actually be something like commutative bialgebra object in $\mathcal{B}$. The paper makes this precise. There are actually some sneaky issues involved in doing so. In particular, the "quantum double" trick for categories makes an appearance here. I guess I'll leave it at that!

Week 50

March 12, 1995

Your roving mathematical physics reporter is now in Milan, where (when not busy eating various kinds of cheese) he is discussing BF theory with his hosts, Paolo Cotta-Ramusino and Maurizio Martellini. This is rather a long way to go to stumble on the October 1994 issue of the *Journal of Mathematical Physics*, but such is life. This issue is a special issue on "Topology and Physics", a nexus dear to my heart, so let me say a bit about some of the papers in it.

1) Edward Witten, "Supersymmetric Yang–Mills theory on a four-manifold", *Jour. Math. Phys.* **35** (1994), 5101–5135. Available as `hep-th/9403195`.

This paper concerns the relation between supersymmetric Yang–Mills theory and Donaldson theory, discovered by Seiberg and Witten, which not too long ago hit the front page of various newspapers. (See "Week 44" and "Week 45" for my own yellow journalism on the subject.) I don't have anything new to say about this stuff, of which I am quite ignorant. If you are an expert on $N = 2$ supersymmetric Yang–Mills theory, hyper-Kähler manifolds, cosmic strings and the renormalization group, the paper should be a piece of cake. Seriously, he does seem to be making a serious effort to communicate the ideas in simple terms to us mere mortals, so it's worth looking at.

2) Louis Crane and Igor Frenkel, "Four-dimensional topological quantum field theory, Hopf categories, and the canonical bases", *Jour. Math. Phys.* **35** (1994), 5136–5154. Also available as `hep-th/9405183`.

I discussed this paper a wee bit in "Week 38". Now you can actually see the pictures. As we begin to understand n-categories (see "Week 49") our concept of symmetry gets deeper and deeper. This isn't surprising. When all we knew about was 0-categories — that is, sets! — our concept of symmetry revolved around the notion of a "group". This is a set G where you can multiply elements in an associative way, with an identity element 1 such that

$$1g = g1 = g$$

for all elements g of G, and where every element g has an inverse g^{-1} with

$$gg^{-1} = g^-1g = 1.$$

For example, the group of rotations in n-dimensional Euclidean space. When we started understanding 1-categories — that is, categories! — the real idea behind groups and symmetry could be more clearly expressed. Sets have elements, and they are either equal or not — no two ways about it. Categories have "objects", and even though two objects aren't equal, they can still be "isomorphic". An object can be isomorphic to itself in lots of different ways: these are its symmetries, and the symmetries form a group. But this is really just the tip of a still mysterious iceberg.

For example, in a 2-category, even though two objects aren't equal, or even isomorphic, they can be "equivalent", or maybe I should say "2-equivalent". This is a still more general notion of "sameness". I won't try to define it just now, but I'll just note that it arises from the fact that in a 2-category one can ask whether two morphisms are isomorphic! (For people who followed "Week 49" and know some category theory, let me note that the standard notion of equivalence of categories is a good example of this "equivalence" business.)

As we climb up the n-categorical ladder, this keeps going. We get ever more subtle refinements on the notion of "sameness", hence ever subtler notions of symmetry. It's all rather mind-boggling at first, but not really very hard once you get the hang of it, and since there's lots of evidence that n-dimensional topological quantum field theories are related to n-categories, I think these subtler notions of symmetry are going to be quite interesting for physics.

And now, to wax technical for a bit (skip this paragraph if the last one made you dizzy), it's beginning to seem that the symmetry groups physicists know and love have glorious reincarnations, or avatars if one prefers, at these higher n-categorical levels. Take your favorite group — mine is $\mathrm{SU}(2)$, which describes 3d rotational symmetry hence angular momentum in quantum mechanics. Its category of representations isn't just any old category, it's a symmetric monoidal category! See the chart in "Week 49" if you forget what this is. Now, there are more general things than groups whose categories of representations are symmetric monoidal categories — for example, cocommutative Hopf algebras. And there are other kinds of Hopf algebras — "quasitriangular" ones, often known as "quantum groups" — whose categories of representations are *braided* monoidal categories. The cool thing is that $\mathrm{SU}(2)$ has an avatar called "quantum $\mathrm{SU}(2)$" which is one of these quantum groups. Again, eyeball the chart in "Week 49". Symmetric monoidal categories are a special kind of 4-categories, which is why they show up so much in 4d physics, while braided monoidal categories are a special kind of 3-categories, which is why they (and quantum groups) show up in 3d physics. For example, quantum $\mathrm{SU}(2)$ shows up in the study of 3d quantum gravity (see "Week 16"). Now the even cooler thing is that while a quaistriangular Hopf algebra is a set with a bunch of operations, there is a souped-up gadget, a "quasitriangular Hopf category", which is a *category* with an analogous bunch of operations, and these have a *category* of representations, but not just any old 2-category, but in fact a *braided monoidal* 2*-category*. If you again eyeball the chart, you'll see this is a special kind of 4-category, so it should be related to 4d topology and — this is the big hope — 4d physics. Now the *really* cool thing, which is what Crane and Frenkel show here, is that $\mathrm{SU}(2)$ has yet another avatar which is one of these quasitriangular Hopf categories.

Regardless of whether it has anything to do with physics, this business about how symmetry groups have avatars living on all sorts of rungs of the n-categorical ladder is such beautiful math that I'm sure it's trying to tell us something. Right now I'm trying to figure out just what.

3) Raoul Bott and Clifford Taubes, "On the self-linking of knots", *Jour. Math. Phys.* **35** (1994), 5247–5287.

I'd need to look at this a few more times before I could say anything intelligent about it, but it looks to be a very exciting way of understanding what the heck is really going on as far as Vassiliev invariants, Feynman diagrams in Chern–Simons theory, and so on are concerned — especially if you wanted to generalize it all to higher dimensions.

Alas, I'm getting worn out, so let me simply *list* a few more papers, which are every bit as fun as the previous ones... no disrespect intended... I just have to call it quits soon. As always, it should be clear that what I write about and what I don't is purely a matter of whim, caprice, chance, and my own ignorance.

4) Christopher King and Ambar Sengupta, "An explicit description of the symplectic struture of moduli spaces of flat connections", *Jour. Math. Phys.* **35** (1994), 5338–5353.

   Christopher King and Ambar Sengupta, "The semiclassical limit of the two-dimensional quantum Yang–Mills model", *Jour. Math. Phys.* **35** (1994), 5354–5363.

5) D. J. Thouless, "Topological interpretations of quantum Hall conductance", *Jour. Math. Phys.* **35** (1994), 5362–5372.

6) J. Bellisard, A. van Elst, and H. Schulz-Baldes, "The noncommutative geometry of the quantum Hall effect", *Jour. Math. Phys.* **35** (1994), 5373–5451.

7) Steve Carlip and R. Cosgrove, "Topology change in (2+1)-dimensional gravity", *Jour. Math. Phys.* **35** (1994), 5477–5493.