DENNISFEST NOTES

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1. Ralph Cohen

Dennis has had a huge influence on a wide variety of fields, in particular the study of chain complexes of various sorts. My talk will be algebraic, a little algebra is good for the soul. This is a tale of two algebras. The first one is the cochains of a manifold $C^*(M)$, the coefficients will always be a fixed field. Dennis' work taught us a lot about cochains, how you can recover the rational homotopy by looking at the cochains.

Dennis said that looking at just the homology is like taking a picture of your wife and throwing your wife away. According to Mike Mandell, you can recover the homotopy type of your wife with the cochains.

Another algebra that comes about is the chains of the based loop space $C_*(\Omega M)$. These are maps from the circle to the manifold. I'll put a dot to mean base-point preserving: $Map_\cdot(S^1, M)$. This has a multiplication, you can concatenate, it's a group up to homology. Understanding this algebra and modules over it tells us about diffeomorphisms. This can be done for any space so far.

One thing that was observed in the early 80s, although in some sense it goes back further than that, is the relationship between the algebras and their Hochschild homology. Think of this as an algebraic invariant of your ring or algebra. One theorem is that $HH_*(C_*\Omega M, C_*\Omega M)$ is the homology of the free loop space H_*LM, where $LM = Map(S^1, M)$, without the basepoint condition. This was observed by a bunch of people, including Goodwillie, unintelligble, others, Bott and Samuelson knew about this back in the 50s. Then Jones and others did the same thing for the cochains and found it's not the same thing, $HH_*(C^*M, C^*M) = H^*LM$. The first is true in full generality, the second if M is simply connected. Neither statement have anything to do with being a manifold. But I'll use the manifold in a moment.

If you think of an aspherical manifold, so the universal cover is contractible, then you can replace the chains of the loop space with the fundamental group. So $C^*M = A$ and $C_*\Omega B$ are "Koszul dual" algebras. To tell you what I mean, I'll introduce some notation.

For this talk, I'll say A and B are Koszul dual if, well, they both have to be augmented $A \overset{\epsilon_A}{\to} k$, $B \overset{\epsilon_B}{\to} k$, so the ground field is a module over the algebra. The main property is that $Rhom_A(k,k) \cong B$, the right derived homomorphisms, and similarly $Rhom_B(k,k) \cong A$. If you're not comfortable with deriving, you don't have your deriving license, you can think, take a free acyclic resolution and take the honest homomorphisms.

This is an algebra, basically by composition. This is what I mean by Koszul dual.

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This Hochschild homology duality occurs for any pair of Koszul dual algebras. That is to say, the Hochschild homologies of A and B will be dual to each other.

About ten years ago at Ib's sixtieth birthday, well, nine years ago, sorry Ib, I asked the question, what is the effect of Koszul duality in K-theory? It turns out the answer is more complicated than I guessed it wsa, and this was worked out by Klein and then Blumberg-Mandell. I wanted to bring up a new question, which is, what is the effect of Koszul duality in topological field theories?

In particular, how does it relate to string topology that Dennis and Moira introduced. It'll take me some time to come back to a couple or three conjectures. Before I go further, I need to input Poincaré duality. Of course, the way we learn about Poincaré duality, there's an orientation class $[M]$ represented by a chain, and this has a property that $C^*M \overset{\cap[M]}{\to} C_*M$ you get an isomorphism from the cochains to the chains. This tells you that C^*M is self-dual up to homotopy. One of things I remember about the first times I talked to Dennis, you might call this Frobenius up to homotopy. He had a student, Thomas Tradler, who wrote a very nice thesis about these ideas. One of the things I want to talk about is an idea about what that means.

I want to reformulate this in more complicated language, why not. Let me recall an ancient theorem of Eilenberg and Maclane. If G is a topological group, then C_*G is an algebra and what they proved is that $Tor_{C_*G}(k,k) \cong H_*BG$, the homology of the classifying space. If G is a discrete group, this is the statement that the "group homology" of G is the homology of the classifying space. Similarly, the corresponding Ext group is the cohomology.

You want to pretend then that the loop space is a group up to homotopy, so $H_*(M) = Tor_{C_*\Omega M}(k,k)$ and $Ext_{C_*\Omega M}(k,k) \cong H^*M$. These are derived functors for tensor and *hom*, and I want to rewrite this on the chain level. There is a class $[M] \in k \otimes^L_{C_*\Omega M} k$, resolve, take a free resolution, and tensor, so that $Rhom_{C_*\Omega M}(k,k) \overset{\cap[M]}{\to} k \otimes^L_{C_*\Omega M} (k,k)$ is an isomorphism.

The theorem, written down in the more modern language by Dwyer, unintelligble, unintelligble, unintelligble, but if you go back to Andrew Ranicki, it's implicit but not explicit there. If P is any module over the chains of the loop space, a differential graded modul, bounded below in a weak sense, then the same cap product homomorphism induces an equivalence of $Rhom_{C_*\Omega M}(k,P) \overset{\cong}{\to} k \otimes^L_{C_*\Omega M} P$. The usual version has P a module over $k[\pi_1 M]$.

An example of this, worked out by Eric Malm, who's here, is that you take P to be the chains of the loop space with the adjoint module structure, $(C_*\Omega M)^{Ad}$, where you think of it as a group and act by conjugation. What this theorem implies is that there is an isomorphism between the Hochschild cohomology of the chains of the loop space and the Hochschild homology of chains of the loop space, and remember, $HH_*(C_*\Omega M)$ is the chains on the loop space. On the left hand side this is a cohomology theory, so you have a cup product, and this corresponds to the Chas-Sullivan loop product that they described in their original preprint, and the right hand side has a circle action by the work of Connes, corresponding to the action by rotating loops. This gives you a Batalin-Vilkovisky structure, also described geometrically in the Chas-Sullivan paper. Much of the structure comes from Poincaré duality if you'll allow this derived Poincaré duality.

Ib gave a particular complex for computing homology that's quite useful, but if P is a bimodule over A, then you can take, using the same notation, the derived tensor product $A \otimes^L_{A\otimes A^{op}} P$, this is the Hochschild chains $CH_*(A, P)$, or I could take $Rhom_{A\otimes A^{op}}(A, P) \cong CH^*(A, P)$.

What I was saying before is that string topology, I want to convince you that the string topology of a manifold, which i'll call $\mathcal{S}_M$, is a positive boundary (I know Graeme Segal refers to this as noncompact) field theory. I'll say what this means. Field theories are many versions, many flavors of the same version, so let me be more explicit. So a topological field theory is a functor F from the cobordism category $Bord$ (Ib called it C), I want it to be in dimension 2 and I'll put a + and tell you what I mean, to differential graded algebras, $Bord^2_+ \to DGAs$. These are examples of (∞)-2 categories. I won't say about the ∞ but a 2 category has morphisms between morphisms. In $Bord^2_+$, the objects are collections of points, 0-manifolds. The morphisms are 1-manifolds between those points, and the 2-morphisms are 2 manifolds between the 1-manifolds. Everything is oriented. The surfaces need to have nonempty boundary, let's say, incoming, in each component. On the DGA side, everything is over some field k. The one-morphisms are bimodules, and the two-morphisms are maps of bimodules. All of this, I'm suppressing the ∞. There is a coherent, homotopy theoretic way of thinking about things, but it'll come up in a couple minutes.

The functor has to be monoidal, so it takes disjoint unions to tensor products. An important example in the one-manifold case is the circle which is a cobordism from the empty set to itself. If I apply F to it, it'll be a bimodule over F of the empty set, so this is a chain complex over k, often called the state space. In the case of string topology, $\mathcal{S}_M(S^1)$ is the chains of the loop space.

Why talk in these terms? There's a terrific theorem that classifies these things. The cobordism hypothesis, I'll use a special case of it (Lurie, Costello). Lurie gave a sketch of a proof of a theorem, the sketch is a hundred pages. It's a beautiful and deep collection of ideas. In this case of two dimensions, the details are there because of prior work of Kevin Costello, and others have worked it out, Chris Schommer-Pries. Let $\mathcal{C}$ be any symmetric monoidal ∞-2 category. Then a monoidal functor that takes disjoint unions of manifolds to tensor products from $Bord^2_+ \to \mathcal{C}$ (a $\mathcal{C}$-valued topological field theory) is determined by its value on a point, as a zero manifold, up to natural isomorphism. The value of a point is an object of $\mathcal{C}$. Furthermore, these objects arising from a field thorey is a Calabi-Yau object (I'll define it in a bit) and every such Calabi-Yau object arises as the value of a point in a field theory like this. Field theories in this setting are classified by these Calabi-Yau objects.

[Is this fully dualizable? No, that would be for framed cobordisms with no positive boundary condition.]

Definition 1. *(Sketch, Lurie) A Calabi-Yau object in $\mathcal{C}$ consists of the following data:*

1. *A dualizable object X, meaning there is a dual object $X^\vee$ and a dualizing map. We'd want a map from the tensor product to the ground field and the other way around, so there are $X \otimes X^\vee \to \mathbf{1}$ and $\mathbf{1} \to X \otimes X^\vee$ establishing the duality. Let's call these evaluation and coevaluation.*
2. *This'll get a little abstract but I'll parse it out. ev_X and $coev_X$ are adjoint morphisms, so there are unit and counit of the adjunction. If I write $\chi_X =$*

$ev_X \circ coev_X : \mathbf{1} \to \mathbf{1}$ [sic], there is a trace map $tr : \chi_X \to id_{\mathbf{1}}$ which is the counit of the adjuction and is $SO(2)$-equivariant.

In the example of $\mathcal{C}$ being a dga, every algebra is dualizable, you take the opposite algebra. The unit is k, so we need these evaluation and coevalution maps $A \otimes A^{op}$ to the ground field, that's a bimodule, that's a module over $A \otimes A^{op}$, which is a bimodule over A, so I take A to be that module, and similarly, you take the same thing in the opposite direction.

This induces a homomorphism to k-modules, chain complexes, ev_A and similarly the other way, k-mod to $A \otimes A^{op}$-modules via coevaluation.

How are these adjoint? If P is an $A \otimes A^{op}$-module and V is a k-module, then the adjunction says that $Rhom_k(ev_A(P), V) \cong Rhom_{A\otimes A^{op}}(P, coev_A(V))$. This is the adjunction property. Evaluation $ev_A(P)$, let me tell you coevalution first. $coev_A(V)$ is $V \otimes A$ (everything is free over k and this will be an $A \otimes A^{Op}$-module using the structure of A itself. Similarly, the evaluation of P is $P \otimes^L_{A\otimes A^{op}} (A)$. By the way, we already saw that this is $CH_*(A, P)$. By the way, what's kind of cool is that the Euler characteristic of the ground field is $A \otimes_{A\otimes A^{op}} A$, which is $CH_*(A, A)$, take P to be $A \otimes A^{op}$, and V be k. What is this adjunction in this case? It means that $Rhom_k(A \otimes A^{op}) \otimes^L_{A\otimes A^{op}} A$ which is A. So $Rhom_k(A, k) \cong Rhom(A \otimes A^{op}, A)$, so we have $A^* \cong A$, so self dual up to homotopy. This reflects the notion that Dennis asked about years ago.

Let me state a couple of theorems, and then I'll quit. The cochains is a Calabi-Yau object in these dga category, and the resulting field theory is string topology, when you pass to homology. The other result is back to our other algebra, the chains of the based loop space which does not satisfy this, is a Calabi-Yau object in the opposite dga category. You still have all of these things, but the trace becomes a cotrace. Again, the resulting field theory, how can that be, and I wanted to end with these conjectures:

If A and B are Koszul dual, and this could be in an abstract category and A is a Calabi-Yau object, then B is Calabi-Yau in the opposite category. This conjecture, I think, we likely have a proof of. The other is, the corresponding field theories are "dual." Remember the circle gives you some kind of Hochschild homology, every operation from A is the dual of the one from B, given that it's opposite, it goes in the opposite direction. I think it's likely to be true but not nearly there. I'll quit here. Thank you.

[Does this shed light on mirror symmetry?]

This should be a baby form. If you did this with, if your 2-category was the category of categories, then it's a statement about the Fukaya category, and whatever the Koszul dual of the Fukaya category would be, that would be a sort of baby version of mirror symmetry.

[Why positive boundary?]

If you remove this, well, see, a field theory might not be finite dimensional. I can say more later, without that condition you force finite dimensionality. Chas-Sullivan, you get a Frobenius algebra, you have a trace map, you can also say multiplication and comultiplication and the comultiplication is a map of bimodules. You can say the exact same words but the coalgebra structure doesn't have a counit, so it's not finite dimensional.

[What about positive-positive boundary? Is there a Lurie-type theory?]

In his manuscript he doesn't talk about the possibility. David is shaking his head. [I don't think there's a classification theorem.]

[When you say that the chains on the loop space are Koszul dual, you're assuming simply connected. Is that integral to your statements?]

Yes, but, unintelligble.

2. John Morgan, Rational homotopy theory, I

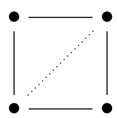
A couple of caveats. I took my instructions to be to give a minicourse aimed at the non-experts. I don't know if I'll be successful, but that's how I interpreted my instructions. Almost everything I say dates from 35 years ago, but in my second talk I'll try to make some connections. Today will be very elementary, I'll start at the beginning, pre-Dennis.

I first met Dennis, if memory serves, in 1966. I think of him (as I thought of him) as master of the evocative, mysterious, phrase. I could quote some but I don't think I will.

I'll start with the pre-Dennis background. It all starts with something that Ralph alluded to, the cup product for cochains. X is our space, and we have the cup product $C^p(X) \otimes C^q(X) \to C^{p+q}(X)$, and if you have two cochains α and β and want to evaluate $\alpha \cup \beta$ on $\sigma : \langle v_0, \ldots, v_{p+q} \rangle$, we should get an integer, and we evaluate α on $\sigma|_{\langle v_0, \ldots v_p \rangle}$ and multiply it by β evaluated on the back, $\sigma|_{\langle v_p, \ldots, v_{p+q} \rangle}$. You have some easy things you'd like, the Leibniz rule, that $d(\alpha \cup \beta) = d\alpha \cup \beta + (-1)^p \alpha \cup d\beta$, it's associative, it has a unit. This tells us that we have a ring structure on $H^*(X, \mathbb{Z})$. What is not clear is that $[\alpha] \cup [\beta] = (-1)^{pq} [\beta] \cup [\beta]$. At the level of chains, this is not true, this is highly non-commutative. This led to a question that must have been extant in the 40s and 50s. Maybe there's a formula that has all of these properties and also commutativity on the chain level. Maybe there's a better formula? Well, in fact, Steenrod gave the answer to that question. Steenrod showed that there can't be a better formula, as I've set it up here. It goes to how you prove that the ring structure is commutative. This goes back to the idea of a chain approximation to the diagonal.

Let's shift gears from a topological space to, say, a regular cell complex. The boundary of each open cell is a union of lower cells, the closure is acyclic, that's technically all I need.

So I have the diagonal $\Delta : X \to X \times X$. I can replace my singular chains with cellular chains. That computes just as well as the singular chains the singular homology or cohomology. We'll use this structure to compute homology. So I have C^*X and $C^*X \otimes C^*X$. You imagine using the diagonal to pull back. But the diagonal is not a cellular map. If X is the interval, you can draw this, the one-cell on the diagonal doesn't go to a linear combination of one-cells in the product:



But what you can do is take a cellular approximation of the diagonal. So $c^d \mapsto K(c) \in C_d(X \times X)$ I want this to respect the boundary, so $\partial K(c) = K(\partial c)$ and if c is a zero cell, then $K(c) = c \times c$. Inductively you can always do this. Therefore, being a cycle in an acyclic case, you bulild up and you get a chain approximation to the diagonal. This gives you a chain approximation to the chains on X, so you can use $K : C_*X \to C_*(X \times X) \to C_*(X) \to C_*(X)$. If I use singular chains, then $H^*(X) \otimes H^*(X) \to H^*(X \times X) \xrightarrow{\Delta^*} H^*(X)$ is the cup product. I have $K(c)$ in $C_*(\bar{c} \times \bar{c})$ and $s(\bar{c}) \in Sing_*(\bar{c} \times \bar{c})$. I have a singular element and a cellular element and because things are acyclic, these are homologous. Then $K(c)$ is chain-homotopic in singular homology to the actual diagonal. In particular, they represent the same homoloogy class, so when I pull back I get the same map in cohomology from the cohomology $K^* : H^*(X) \times H^*(X) \to H^*(X)$ is the cup product.

What does this have to do with cohomology? well, there is the sign switching map $T : X \times X \to X \times X$ which maps $(a, b) \mapsto (b, a)$, but on the other hand there's $T \circ X$, a new chain approximation to the diagonal, and this new chain approximation also induces the cup product. The problem with noncommutativity, the simplices have finite extent. Differential forms are supported near a point. You can work close enough to the diagonal that up to homotopy it doesn't matter (in cohomology).

Steenrod observed, this was just the beginning of an infinite process. Let's think of the chain approximation as a dot. Then I have the switch of factors. That's a different approximation, they're homotopic. I can connect them by a homotopy. Of course, I can switch K_1 to get a different homotopy to get a circle of maps. So this circle of maps extends over a 2-disk of maps. But there's no reason not to flip the 2-disk and so on. So you end up with a cellular map $S^\infty \times X \to X \times X$ which is $\mathbb{Z}_2$-equivariant. This acts as the antipodal map on S^∞ and the switching map on $X \times X$. This will induce something on the equivariant cohomology, $H^*_{\mathbb{Z}/2}(X \times X) \to H^*_{\mathbb{Z}/2}(S^\infty \times X)$, but this is free, so it's $H^*(\mathbb{RP}^\infty \times X)$ but by Kunneth that's $H^*(X)[\alpha]$. I can take $\beta \otimes \beta$ in $H^*_{\mathbb{Z}/2}(X \times X)$ and see what we get, and we'll get $\sum Sq^{n-i}\beta\alpha^i$. These are the Steenrod squares and they're natural and so on.. If you had a commutative cochain, all of these would vanish and these are obstructions. You can't, then, make a commutative cochain model. These obstructions generally do not vanish. These are $\mathbb{Z}_2$-classes. You can do something similar for other finite cyclic groups, but we can ask, can we solve the cyclic cochain problem over $\mathbb{Q}$? In some sense, Quillen showed the answer is yes. It's not exactly as explicit as we could hope for. What he showed wa that the rational homotopy category, and I'll say what that is in a minute, of simply connected spaces is equivalent to the category of cocommutative coassociative coalgebras. You dualize one of these objects and you end up with a graded commutative associative algebra.

What I want to do first of all is talk about the rational homotopy category, and give a flavor of Quillen's argument to let you comparewith what Dennis had to say later about the same subject. So this idea really goes back to Serre, where he proved the higher homotopy groups of spheres are finite.

We'll look at the category of simply connected spaces, where those are the objects and the morphisms are continuous maps but I want to introduce a relationship, saying that some maps are quasiisomorphisms and formally inverting them. This isn't the usual way of doing things but because of various theorems this will work. Let's say that $f : X \to Y$ is a quasiisomorphism if $f_* : H_*X \to H_*Y$ is an isomorphism, or equivalently, that $f_\# : \pi_*(X) \to \pi_*(Y)$ is an isomorphism

[Alarm]. Is that something we need to worry about? [You made a false statement] [Laughter.] [Arms up like a robot] WHAT ARE YOU TALKING ABOUT? [Laughter.]

So we have topological spaces and maps are compositions where some of the arrows are quasiisomorphisms that go the wrong way. This leads naturally to the notion of $\mathbb{Z}$ localized at 2 homotopy. So I can talk about rational homotopy theory, where $f : X \to Y$ is a quasiisomorphism if and only if the induced map $H_*(X, \mathbb{Q}) \to H_*(Y, \mathbb{Q})$ is an isomorphism, or equivalently that $\pi_*(X) \otimes \mathbb{Q} \to \pi_*(Y) \otimes \mathbb{Q}$ is an isomorphism. So the first thing to check is that those are equivalent. This goes back to Serre.

Let's start by computing $H^*(K(\mathbb{Z}, n), \mathbb{Q})$. We get either $P[a]$ with the degree of a equal to n if n is even or the exterior algebra $\wedge^*(a)$ if n is odd. So in general, if F is a free Abelian group, then $H^*(K(F, n); \mathbb{Q})$ is $P[(F \otimes \mathbb{Q})^*]$ or $\wedge^*(F \otimes \mathbb{Q})^*$.

The proof is by induction. If $n = 1$ then $K(\mathbb{Z}, 1)$ is the circle and $K(\mathbb{Z}, 2)$ is $\mathbb{CP}^\infty$. We'll use a spectral sequence with contractible total space, fiber $K(\mathbb{Z}, n)$, and base $K(\mathbb{Z}, n+1)$. There's only one non-trivial differential, which goes from $(0, n)$ to $(n+1, 0)$, and you show by induction that the powers of this element are nontrivial and account for everything.

Maybe I should say why this doesn't work over $\mathbb{Z}$. At the next step, if you go to a $\mathbb{CP}^\infty$ bundle over $K(\mathbb{Z}, 3)$, you start to build the differential, the derivative of a^2 is $2ab$, and you see these coefficients coming in, and you can estimate, as Serre did, when the first time that the p will show up in the homotopy groups of spheres. Over the rationals, this doesn't happen. The higher homotopy groups of spheres are completely understood rationally and not at all integrally.

I wanted to show that these two things are the same, and I've done the first computation. So if π is a torsion group, the reduced cohomology of $(K(\pi, n), \mathbb{Q})$ is zero. A finite Abelian group has no rational homology.

Let's go from π_* to H_* I have an isomorphism at the level of rational homotopy. The fiber will have torsion homotopy groups, each of which have trivial rational cohomology, so when you stick them together, you get a rational homology equivalence.

For the other direction, you use Hurewicz. The relative homotopy groups of the fiber and the relative homology groups of the fiber are related by the morphism, and the first nonzero one is torsion, and you can keep going.

Here's Quillen's isomorphism. The $Ho_{\mathbb{Q}}$ category will be equivalent to the category of $\mathbb{Q}$-dg coalgebras. I'll write down some things in the middle, you can tune them out if they're not familiar, I won't use them. So first, you go by Kan to simplicial sets, taking the singular complex. A simplicial set has face maps and degeneracy maps. We could have a simplicial object in any category. This is well-known to recreate the homotopy type of X. Milnor described how to realize this. This is a good equivalence.

Next there's something called the Kan group functor to simplicial groups. This is a model for the chains on the loops space, and then you can go to simplicial Hopf algebras, by associating a group to its completed group ring $\widehat{\mathbb{Q}[G]}$. There you can look at the primitives x so that $x \otimes 1 + 1 \otimes x$, and these are a Lie algebra, and then the Eilenberg Zilber shuffle gives differential graded Lie algebras. You sue $\wedge^*(L^*), \partial^* + [\ ,\ ]^*$.

$$Ho_{\mathbb{Q}} \xrightarrow{\text{Kan}} Sset \longrightarrow SGrp \longrightarrow SHopf \longrightarrow SLie \longrightarrow DGLie \longrightarrow DGcoAlg$$

$$X \longrightarrow Sing_*(X)$$

That's the backgrond and it was supposde to take twenty minutes. Now Dennis comes into the picture. I'm imagining the first thing he said to himself is, there's an easier way to get to a dg algebra, take differential forms. Often in geometry you know something about the geometric forms and get something about cohomology. So you should be able to use the differential forms instead of this machinery.

Secondly, I want to build what he called the minimal model to get a handle on what rational homotopy theory *looks like*. These are the two main themes of his excursion into rational homotopy theory.

There are two problems: these are only defined on smooth things, and are defined over $\mathbb{R}$ instead of $\mathbb{Q}$. Let's write down an algebra of forms on Δ^n. We know this sits in $\mathbb{R}^{n+1}$. What algebra should we associate? We'll take $P[t_0,\ldots,t_n]/\sum_0^1 0t_i$. So we can take

$$\Omega^*_{\mathbb{Q}}(\Delta^n) = (P\mathbb{Q}[t_0,\ldots,t_n]/\sum t_i = 0) \otimes \wedge^*_{\mathbb{Q}}(dt_i)/\sum dt_i = 0.$$

This is collections of k-forms on σ so that any time τ is a face of σ, $\omega_\sigma|\omega_\tau$

Let's think of a one-form on the circle, viewed as a triangle. There are one-forms on each simplex but when they meet you have a question about how they match up at the zero simplices, but this is no condition. This is a differential graded algebra that calculates the cohomology of X. There's a map from $\Omega^*_{\mathbb{Q}}(X) \to C^*(X,\mathbb{C})$ by integration that commutes with D and is an isomorphism on homology. This is a commutative model for cochains.

[How obvious is it that these two rings are equivalent?].

One remark: what's the connection to smooth forms? If you'll pass from $\mathbb{Q}$ to $\mathbb{R}$. If you take some C^∞ triangulation $X \to M$. The C^∞ forms on M, you have, the rational differential forms on X and eventually $\Omega^*_{p,C^\infty}(M)$.

We have a nice picture now that is more geometric and fits better with smoothness when you have a smooth manifold.

I have a question. I am not aware that anyone has ever shown that Quillen's equivalence is equivalent to Sullivan's equivalence. Surely if you trace through this, you ought to be able to prove that these give the same equivalence up to equivalence. Is that true?

Let me finish briefly with a remark about the difference between rational and real homotopy theory. Real says that a map induces an isomorphism on real homotopy, but that's the same as rational. Simply connected four-manifolds, the rational equivalence is the rational equivalence of the quadratic form, whereas real equivalence is real equivalence of the quadratic form.

Next time I'm going to go through how to read off a lot of invariants. On the last day I'll talk about classifications up to ambiguity and so on.

[Why can't you do this with integer coefficients?]

Which model of the wedge do you use? The quotient or the sub thing. You'll end up with something acyclic [and won't be able to prove de Rham's theorem.]

3. Kevin Costello

Last time we talked about the definition of an elliptic dg Lie algebra on a manifold X which is a model for a formal elliptic derived moduli problem. Today I want to talk about classical field theories and maybe a little bit about quantization. For me a classical field theory on X is such a moduli problem along with additional geometric structure.

Before the definition, I want to motivate with zero dimensional field theory. In this case, we have M a manifold, the "space of fields," along with $f : M \to \mathbb{R}$, the "action functional." Classical field theory knows about the critical points of f in M. I like derived things so what I'd like to do, and Batalin and Vilkovisky tell us that we should do, is we should consider instead the derived critical locus. It took me a long time to understand that this is what they were doing. I can look at the graph Γ_{df} of df inside T^*M, and the critical locus is this graph, intersected with the zero section: $\Gamma_{df} \cap M$. We're working in this derived world, and algebras of functions will be dg algebras, and so we'll define the algebra of functions $\mathcal{O}(Crit^h(f))$ by saying, I look at functions on the graph and I tensor them over functions on the cotangent bundle with functions on M, $\mathcal{O}(\Gamma_{df}) \otimes^L_{\mathcal{O}(T^*M)} \mathcal{O}(M)$, with the derived tensor product. There's a concrete way of doing it with the Koszul resolution.

We find that functions on the critical locus looks like this:

$$\mathcal{O}(Crit^h) = \cdots \to \Gamma(M, \wedge^2 M) \stackrel{\vee df}{\to} \Gamma(M, TM) \stackrel{\vee df}{\to} \mathcal{O}(M)$$

This is what you find when you take the naive Koszul resolution. So let's rewrite this, you can think of this as sections of the tangent bundle and its powers, you can think of this as functions on the cotangent bundle, so this is $\mathcal{O}(T^*[-1]M)$ with differential $\vee df$.

An observation: this dga has a natural Poisson bracket of cohomological degree one. How is it defined? This is called the Schouten-Nijenhuis bracket, and it's defined, if $X \in \Gamma(M, TM)$ and $f \in \mathcal{O}(M)$ then $\{X, f\} = Xf$, $\{X, Y\} = [X, Y]$, and the Leibniz rule determines the rest.

Writing it in this way, as the shifted cotangent bundle with the differential df, the cotangent bundle of a manifold is not just Poisson, it's symplectic. What this is showing is that the derived critical locus of a function is equipped with a symplectic form of degree -1. What do we mean by this? It's a closed and nondegenerate two-form. I'm looking at the degree -1 forms.

[Do you see the bracket abstractly?] I don't have a down-to-earth one, I have a fancy one using derived geometry.

So $\Omega^2(Crit^h(M))$ is a cochain complex. In here we'll have a cycle $\omega \in Z^{-1}(\Omega^2(Crit^h(M))$ with $d_{dR}\omega = 0$ and ω is non-degenerate.

In principle, I'm not sure if physicists will like this, I suspect no, is that to do classical field theory and to try to quantize, we don't actually need the action functional. Instead, all we need is the -1-symplectic manifold of the derived critical locus.

This may seem funny at first, but it turns out that this data, the derived critical locus determines the action functional up to an additive constant. So if you give me one of these symplectic manifolds, this gives me a field theory, I don't need the functional.

Now let me give you the definition I'd like to use.

Definition 2. *(Tentative)*
A classical field theory on X is a formal elliptic moduli problem with a symplectic form of degree -1.

We defined these formal elliptic moduli problems in terms of Lie algebras. So the question becomes, "What does a symplectic form become? The answer was worked out by Kontsevich. Let M be a formal moduli problem, so functions on M are the completed symmetric algebra $\widehat{Sym^*}V$ with V a graded vector space, and a differential d on this. The Lie algebra associated to this $\mathfrak{g}_M$ is $V^*[-1]$ (or L_∞ algebra). We know that if we change coordinates on M, this should change to an equivalent, homotopy equivalent L_∞ algebra. [This is isomorphic by a non-linear L_∞ isomorphism.]

It's equivalent in this world. There's no difference after changing coordinates.

Now suppose M is symplectic. Then we can choose Darboux coordinates, so ω has constant coefficients, and so ω is in $\wedge^2 V$ of some degree. Let's translate this into the Lie algebra. So V is isomorphic to the dual of the Lie algebra, with a shift that I will get wrong, so $V \cong \mathfrak{g}_M^*[-1]$ and $\wedge^2 V \cong Sym^2(g_M^*)[-2]$, so the wedge becomes a symmetric pairing on g.

There are more properties. We also said that the two-form had to be a cycle, so $d_{\mathrm{internal}}\omega = 0$, and this becomes the statement that we have an invariant pairing on $\mathfrak{g}$. Okay, following this? Dennis is being suspiciously quiet.

[The question burning in my head right now, the deformation theory is a homotopy invariant of the Lie algebra, what is the notion here?]

I'm not sure how to parse the question.

Let's give a definition:

Definition 3. *Let X be a manifold, and last time we had this idea, so let L be an elliptic dg Lie or L_∞ algebra on X. This is a graded vector bundle whose sections $\mathcal{L}$ is a dg Lie algebra where the differential makes it an elliptic complex and the bracket is a bidifferential operator. Wa want to say what an invariant pairing on such a guy is. Well, what we need to do, this pairing, we can think of as a map for $\mathfrak{g}$ to its dual. You don't want to take the dual because you'll have compactly supported sections. So let $L^!$ be $L^* \otimes \Omega^{top} X$.*

So $\mathcal{L}^! = \Gamma(X, L^!)$ plays the role of the dual, and if X is compact then $\mathcal{L}^!$ is quasiisomorphic to the dual of $\mathcal{L}(X)$. If X is noncompact, you have to worry about compactly supported sections. An invariant pairing is an isomorphism to the dual, so it's an isomorphism of $\mathcal{L}(X)$-modules from $\mathcal{L}(X)$ to $\mathcal{L}^!(X)$ which is:

1. *given by a differential operator D, and also, we have the formal adjoint, involving dualizing and twisting, and so also,*
2. *the formal adjoint $D^* = D$.*

So we can finally make our definition of classical field theory precise. A classical field theory is an elliptic moduli problem, an elliptic L_∞ algebra $\mathcal{L}(X)$ on X with an invariant symmetric pairing in this sense, and there's various funny things with the shifts, and this needs to be of degree -3, which corresponds to symplectic of degree -1, because when we passed from the moduli space, the -1 becomes a -3.

If you were a physicist, you might come by your field theory by writing down your action functionals and considering their derived critical loci. I'm more of a geometer.

The first example is Chern-Simons. X is a 3-manifold, so if $\mathfrak{g}$ is a Lie algebra with an invariant pairing, then I can look at the de Rham complex of X with coefficients in $\mathfrak{g}$, which controls the moduli problem of flat G-bundles when you're deforming the trivial one.

Now, just like I said, you'll see there's an evident pairing of the right degree, you have

$$\Omega^0(X)\otimes\mathfrak{g} \qquad \Omega^1(X)\otimes\mathfrak{g} \qquad \Omega^2(X)\otimes\mathfrak{g} \qquad \Omega^3(M)\otimes\mathfrak{g}$$

The pairing is $\langle\alpha\otimes E,\beta\otimes F\rangle=\int(\alpha\wedge\beta)\langle E,F\rangle_{\mathfrak{g}}$ for α and β in $\Omega(M)$ and E and F in $\mathfrak{g}$.

You can do this in other dimensions, but then you'd need a graded Lie algebra with the right dimensions. Kontsevich did it for dimension two for his formality theorem. Dimension four is a supersymmetric theory with $n=4$.

[So you can do this in any dimension] The ones we like, the Lie algebras we like, are in graded 0.

[Do physicists study these theories?] [Parts of them show up as parts of supergravity theories.]

Kontsevich's paper of 1994 does it in every dimension greater than 2 and constructs the quantum theory.

So we have this definition. How do we get things with symplectic forms? The most naive thing is to take the cotangent bundle of something.

Definition 4. *Let $\mathcal{L}$ be an elliptic moduli problem on X. Then we can consider the* cotangent field theory *defined by, take the cotangent in the world of Lie algebras, take $\mathcal{L}\oplus\mathcal{L}^![-3]$, shifted to put it in the correct degree. This is automatically, has a pairing of degree -3, so it defines a classical field theory.*

This construction corresponds to $T^*[-1]$ of the formal moduli problem. One thing I wanted to explain is, I've been struggling to understand supersymmetry, they write down crazy formulas, but you take some elliptic equation and you do this to it, and it gives you a field theory.

Let's take the self-duality equation $F(A)_-=0$, where A is a connection on a G bundle on the 4-manifold X. You can take the cotangent theory, and this turns out to be what's called self-dual Yang-Mills theory, a degenerate limit as the coupling constant goes to zero. Another example, in mirror symmetry, you have these A model and B model, the A model is Gromov-Witten invariants and the B-model is more mysterious. This is one of the things that John mentioned. Let X be a complex manifold, Σ a surface. We want to consider locally constant maps from Σ to X. What do we mean by this? Locally constant means your map is killed by the de Rham differential. These are maps of ringed spaces from (Σ,Ω^*_Σ) to $(X,\Omega^{0,*}_X)$ that are maps of ringed spaces. The cotangent theory is the B-model.

I wanted to talk about quantization but I'm not sure I have time to get into it.

[Take ten minutes]

I have another lecture. Let me say that for any system of elliptic equations you can try to quantize, I've been working on this for a while, and I have this definition of what it means to quantize. A quantization of a cotangent field theory gives a volume form on the elliptic moduli problem.

[Only a cotangent field theory?] In general it's more subtle, some kind of half-density. Here the terms beyond $\hbar$ will vanish, and you'll only get the first power of $\hbar$ appearing, so we can treat things nonperturbatively.

As I mentioned in the first lecture, one example I know how to do is holomorphic maps to a complex manifold, and the Witten genus is the volume of the resulting moduli space.

Maybe I'll end by saying what it means to quantize and we'll start from there next time. If $\mathcal{L}$ has an invariant pairing of degree -3, then cochains on this Lie algebra, I can think of this as functions on the corresponding moduli problem. This object has a Poisson bracket of degree 1. If I have an open subset of my manifold $U \subset X$, I can consider cochains on $\mathcal{L}(U)$, and the definition of quantization is very like deformation quantization, I want to deform this in a way that to first order gives the bracket.

Roughly speaking, quantization is a differential D on $C^*(\mathcal{L}(U))[[\hbar]]$ such that:

(1) D is the original differential mod $\hbar$, and
(2) the failure of being a derivation is measured by the bracket $D(\alpha\beta) = (D\alpha)\beta + \alpha D(\beta) + \hbar\{\alpha, \beta\}$.

[Where is the invariant pairing?] In the Poisson bracket. I'll stop there and pick up again.