UNIVERSITY OF PADOVA
HIGHER STRUCTURES IN ALGEBRAIC ANALYSIS

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1. Ezra Getzler: Chern-Weil theory for perfect complexes

Thank you very much. Actually I may not as it were get to that. As I was preparing the talk, I realized I didn't understand the formulas as well as I thought. I want to give a survey of the perspective we are developing. A lot of the territory I'll cover will be similar to Dominic's territory. He likes minimal resolutions; I'll take really big ones with hopes that there's more room to move. Everything will be in the context of categories of fibrant objects, which were introduced long ago in K-theory. I'm a big fan of them now. An example is the fibrant objects in a closed model category. The point is that there are much smaller examples. Closed model categories have lots of limits and colimits. In differential geometry, we don't have coproducts, in particular. You want to strip down homotopy theory to its absolute essence. I'll have $\mathcal{V}$ the category of "spaces" which should have two subcategories, that of weak equivalences $\mathcal{W}$. This subcategory should satisfy the two out of three axiom (or left and right cancellation) and should include all isomorophisms.

Then we have the fibrations $\mathcal{F}$ which are also a subcategory including all isomorphisms. Unlike in a closed model category, we don't assume that all morphisms have pullbacks. But fibrations have pullbacks and the pullback of a fibration is a fibration.

There are two more axioms: there should be a terminal object e in $\mathcal{V}$, so every object has a unique morphism to e; that should always be a fibration.

Going back to model categories, we can factor every morphism into a trivial cofibration followed by a fibration. We don't have cofibrations but we can ask for factorization into a weak equivalence followed by a fibration. Brown proved something that you can call Brown's lemma. With these axioms you can assume that, well, let me first say, morphisms in $\mathcal{F}\cap\mathcal{W}$ are trivial fibrations, and pullbacks of trivial fibrations are trivial fibrations.

Brown's lemma says that you can factorize a map $X \to Y$ into $X \to P \to Y$ where $P \to Y$ is a fibration and $X \to P$ is a section of a trivial fibration.

To give an example, if we have simplicial sets and fibrations are Kan fibrations and weak equivalences weak equivalences. We take P to be the fibered quotient of X with the path space of Y, $X \times_Y Map(\Delta^1, Y)$.

We want to do derived algebraic geometry bearing this in mind.

I should give some examples more relevant to derived algebraic geometry. I won't get to the real examples just yet. For now, let me take nilpotent L_∞ algebras. Weak equivalences are quasiisomorphisms and fibrations are surjective morphisms of L_∞ algebras. Maybe if you're not interested, you can spend the rest of the talk proving this. These can be thought of as fibrant cocommutative coalgebras, but not *all* fibrant cocommutative coalgebras.

Now I'm going to define ∞-groupoids or higher stacks. I need a notion of a topology. Some people say "admissible morphisms." Here's a set of axioms for a topology on a category of fibrant objects. I mean something like a Grothiendieck pretopology. When we take covers in algebraic geometry, we take sets of morphisms, but I'll take single morphisms to simplify things.

I have a subcategory of covers $\mathcal{C}$. My covers will lie between, they will all be fibrations and they'll include all trivial fibrations. Now I have two axioms remaining. The pullback of a cover sholud be a cover, that's the main axiom of a topology. An unexpected axiom that seems to hold for almost all Grothiendieck pretopologies of interest but usually isn't part of the definition—so I apologize for calling it a topology—if gf and f are covers, then so is g.

For an example, take $\mathcal{V}$ to be schemes and $\mathcal{F}$ all morphisms, $\mathcal{W}$ isomorphisms. This is a rather trivial example. Here I'm using that every morphism can be pulled back for schemes, avoiding my reason for this formalism (that not every morphism can be pulled back). Then take $\mathcal{C}$ to be surjective submersions. The point is that this class of covers satisfies the extra axiom. There's a chapter in the thousands-of-pages stack book, there's a chapter that just shows that this axiom holds for one after another pretopology.

So I'm going to introduce a category of ∞-groupoids, a full subcategory of the category of simplicial objects in $\mathcal{V}$. Take $s_\infty\mathcal{V}$, the full subcategory of ∞-groupoids, and here I'm inspired by Duskin and Rezk, and this exact definition is from Pridham. Duskin's idea was to introduce the analogue of the Kan condition for groupoids. Rezk gives an auxilliary condition, Reedy fibrancy. This is what I mean when I say I'm taking a very big realization. The realizations will be fatter than what one might first write down.

An ∞-groupoid, for each n I have an object X_n in $\mathcal{V}$ along with all the face and degeneracy maps between objects. Since I'm calling objects in $\mathcal{V}$ spaces, this is a simplicial space. Now the main condition is the following. This imitates the Kan condition. We have a simplicial subset of the simplicial n-simplex, $\Lambda^n_i \subset \Delta^n$, where we take the union of all but the i face. Then I'll justify this in a moment, we can take all the maps from the horn to X. This is a finite limit that might not exist, later I'll give an additional axiom. Then I have a map from X_n to this, to $Map(\Lambda^n_i, X)$, which is restriction, and then the axiom is that this is a cover for $n > 0$ and $0 \le i \le n$.

This won't make sense without Reedy fibrancy. In the context of ∞-categories, it's was introduced in (88?). It's hard to show that it holds.

The condition is that $X_n \to Map(\partial\Delta^n, X)$ is a fibration for all $n \ge 0$. If this condition holds up to $n-1$, then we can define the space of maps from the horn. It's established in a number of papers, Dugger et al. It's already visible in Verdier's seminar. Since this is assumed, the other axiom makes sense, we have the horns as finite limits.

I have to define fibrations and weak equivalences. The fibrations are defined a lot like the horn thing. When is $X \to Y$ a fibration? The condition is that the map from X to

$$Map(\Lambda^n_i, X) \times_{Map(\Lambda^n_i, Y)} Y_n$$

is a cover plus the Reedy fibrancy condition, that the following is a fibration:

$$X_n \to Map(\partial\Delta^n, X) \times_{Map(\partial\Delta^n, Y)} Y_n$$

At the bottom, $X_1 \to X_0$, both of the maps are covers. You want to think that the source and target are covers if you think of these as Lie groupoids. The whole point is to find the right extension of Ehressman (in the 50s) to higher n.

The first new thing you get is that the product of source and target is a fibration. In the scheme world this won't be a condition but in manifolds this won't be possible. This leaves Lie groupoids out but you can do some analytic things. You can work in loci, which are the completion of manifolds, if you wanted to work with Lie groupoids.

The weak equivalences, it's hard to say what that is. If you presume in addition fibrancy, then there's a simple closed diagnositc for when a map is a weak equivalence. The weak equivalences are, if you take the fibered product $X_n \times_{Y_n} Y_{n+1}$ where the map $Y_{n+1} \to Y_n$ is the last face map. We can map this to $Map(\partial\Delta^n, X) \times_{Map(\partial\Delta^n, Y)} Map(\Lambda^{n+1}_{n+1} Y)$.

The idea is that I have a horn taking values in Y, and I lift the boundary of the last face to X. Anyway, this map should be a cover for all $n \geq 0$. In sets, this is the statement that the relevant homotopy groups vanish.

This is, it turns out, a lift of that condition in our world.

Theorem 1.1. *The category $s_\infty\mathcal{V}$ is a category of fibrant objects.*

The proposal is that derived stacks should just fit into this language. Looking at Toën Vezzosi, they have some more structure and I'm throwing some of it away and trying to work internally.

Oh geez, that's the first page of my notes. I should say that once you have Reedy fibrancy, if K is a finite simplicial complex and X is an ∞-groupoid, then we can form an ∞-groupoid of maps from K to X whose n-simplices are $Map(K \times \Delta^n, X)$. You can form loop spaces and so on. This uses Reedy fibrancy.

So I want to give some examples. How do you get Reedy fibrancy in practice? Let me assume I have a finite dimensional differential graded algebra A (I'm actually interested in differential graded Banach algebras). I want a derived stack of the invertible elements of A. This may relate in some way to the talk on Friday as well as derived quot. I want to show a technical trick that gives Reedy fibrancy.

Your first guess might be to take as n-simplices, let me introduce $\mathbb{MC}(A)$, which will be a curved L_∞ algebra. Take $A_1 \to A_2 \to \cdots$ and I take $A_2[1]$, I want to take the stupid truncation in degree 2 and higher, shifted down by one. I have A^1 parameterizing a family of curved L_∞ structures on this thing. This I want to think of as derived Maurer Cartan. Think of the Dolbeault resolution of $End(E)$ where E is a holomorphic vector bundle. That's the example I have. I want to abstract it. I have a differential graded algebra. Forget commutativity. The point is, if I have a curved L_∞ algebra. The places where the curvature is 0 is the usual Maurer Cartan locus, solutions to $d\omega + \omega^2 = 0$ where $\omega \in A^1$.

Your first guess is to take the Maurer Cartan locus of $C^*(\Delta^n) \otimes A$. I want normalized cochains to make this finite. For $n = 0$ you get Maurer Cartan things, and for $n = 1$ intertwiners. This is a complete Segal category although I haven't introduced those.

But that's not the answer. The answer is that I want to take Maurer Cartan solutions in something like this but with a replacement $\tilde{\Delta}$ for Δ. So Δ^n is the nerve of $[n]$ but $\tilde{\Delta}$ is a groupoid, has morphisms going up *and* down. This is the same thing, it's an infinite dimensional simplicial complex but still contractible and with plenty of nice property. Joyal calls $\tilde{\Delta}^1$ the groupoid interval.

I haven't defined the category of fibrant objects. The fibrations are affine maps of these guys which are surjective. This is a very strong condition. There are very few fibrations. The covers require in addition that we go to a point in the classical locus and get a complex, and we get cohomology, that should be an isomorphism. On loci it should be a surjective submersion. [Missed weak equivalences]. The theorem is that this is an ∞-groupoid. The source and target maps from $\mathbb{N}_1 A \to \mathbb{N}_0 A$ should be a cover. Incidentally, this is the condition in Segal spaces that makes them complete Segal spaces. Everything else in this theorem is soft. This is the only place you have to think. Everything else is a universal formal consequence. I have three minutes because I started late. Five, thank you.

I have two topics to cover in the last five minutes, negative five minutes. I want to mention one thing which I like a lot. What about the image of $\mathbb{N}_n A$ in $N_n A$? In sets, it's Joyal's notion and above it Rezk's. You get a fibration. When does this hold? Actually let me not do this. The main point is the other way to ensure Reedy fibrancy. We can use complex manifolds as derived stacks for this. That's a lot harder. If you think about it, a complex manifold is some pseudoconvex charts glued together, that's a groupoid, the atlas. The task is to produce a Reedy fibrant resolution. Even for a chart it's not so straightforward. Take $U \subset \mathbb{C}^n$ pseudoconvex (or convex in the real world). How do I make this a derived stack? I need to find a Reedy fibrant guy. I need $P_n U$ which satisfies $P_0 U = U$, $P_1 U \to U \times U$ should be a fibration. I don't have many fibrations. It's going to be a little tricky to arrange this. I didn't say anything about Chern-Weil, so finding an explicit resolution is a matter of imitating some work I did on infinity groupoids a few years ago, using the Dupont gauge and essentially if you have a nilpotent L_∞ algebra then $L \otimes C^*(\Delta^n)$ is again a nilpotent L_∞ algebra. You extend to the curved case by the same formulas and need a parameterized version. The explicit and very complicated formulas. For $n = 1$ you get Bernoulli numbers. You glue, again, using the fact that you have a category of fibrant objects. Once you've done it once, you never have to do it again. It changes the notions of what presentations of manifolds you work with, but it's very explicit.

What I want to finally suggest is, for example, let's look at the special case of the $n \times n$ matrices. I already have one; I get Maurer Cartan solutions in $n \times n$ matrices with values in the thickened n-simplex. I think of this as a simplicial differential graded algebra and then focus on its noncommutative forms. The first Chern class is the determinant, which is a complicated subject in itself. I'd better stop there.

2. Philip Boalch: Natural flat connections and wild mapping class groups

I'd like to thank the organizers for asking me to speak here. Most of what I'm talking about won't be so derived. Let me start by talking about nonabelian Hodge moduli spaces. The simplest case starts by taking a smooth compact algebraic curve. Then you want to attach to that a connected reductive group G with a maximal torus T. So we have $H^1(\Sigma, G)$. There are various ways to think about what this set is. This is pairs (in the de Rham perspective) consisting of a principal G bundle P over our curve Σ and an algebraic connection on P. There's the Betti picture, representations of $\pi_1(\Sigma)$ in Σ modulo conjugation.

People here would probably prefer to say we have an analytic isomorphism of algebraic stacks. If you put in a stability condition, you have another perspective, that means that you can make a solution to Hitchin's equations. That's a hyperKähler manifold. Then these perspectives become different algebraic structures on the same underlying manifold. The de Rham picture is due to Donaldson-Corlette, and there's another complex perspective, the Dolbeault, where you have a Higgs field instead. Then there is the Kobayashi Hitchin correspondence for Higgs pairs. That makes this into a hyperKähler manifold. The correspondence to de Rham is nonabelian Hodge.

I want to think about what happens when you vary the curves. It's possible to integrate in the Betti setting. The mapping class group acts there. There's an algebraic connection on bundles of bases, a nonabelian Gauss-Manin connection, in de Rham.

So there are various extensions of this picture.

There's this construction problem. This says "does there exist a projective variety X so that the the de Rham moduli space of X is not equivalent to the moduli space of some curve Σ?" As far as I know, there are no examples.

We can look at non-compact Σ, let's give points $a_1, \dots, a_n$. Then you're looking at the fundamental group of the punctured thing. We could be viewed as fixing conjugacy classes of monodromy around local punctures. This is still not quite, we fix the real part of the eigenvalues and the imaginary part. We want a quaternionic triple, so we need to fix a weight. This is the third in the triple that go to the real and imaginary part of the eigenvalues. Then we need to look at the class of the centralizer in G^m of the weight ϕ. We want to look at first order poles in all three pictures.

These aren't the most general solutions to Hitchin's equations that appear. There might be higher order poles in the wild picture. This goes, we need to fix at each pole elements Q in, $t((z))/t[[z]]$. The coordinate independent perspective is to say this is $t(K)/t(\mathcal{O})$. We can change the moduli problem we have and look at connections that look like dQ plus irst order poles.

[At this point I stopped taking notes.]

3. Andrei Caldararu: The de Rham complex from the point of view of twisted derived intersections

Thank you very much for the opportunity to be here today. This is joint work with Arinkin and my student Hablicsek. The paper is on the arXiv:1311.2629.

Let me explain the main object of study. You have some variety X, affine or maybe some open subset in a projective variety. You have a function $f : X \to \mathbb{C}$ and you want to study the critical locus of f. One thing you want to study is the twisted de Rham cohomology of X, which I will denote $\Omega^\bullet_{X,d+\wedge df}$. This goes from $\Omega^0 \to \cdots$ via $d + \wedge df$ which you can easily check squares to zero.

These appear naturally, if you study matrix factorizations of this f, that's a model for singularity categories of the singular fibers of f. These cohomology groups compute the cyclic cohomology of the category of matrix factorization. It's the same role de Rham cohomology plays for the usual variety of X.

A particularly simple example is, if $f = 0$, then this computes the usual de Rham cohomology of X. Then if X were compact, we'd have a very useful way of computing it. We'd have the Hodge theorem that this was the same as $\bigoplus H^p(X, \Omega^q)$. My goal for today is to generalize this theorem in the case of twisted de Rham cohomology.

Now one way you could rephrase this theorem is that we have actually two complexes, $\Omega^\bullet_{X,d}$ and $\Omega^\bullet_{X,0}$, and the statement is that these two have the same hypercohomology. Somehow the point is that you can get rid of the d and at the level of hypercohomology you're not losing it. Here X must be compact, otherwise this fails. For the twisted case there is a result that was announced by Barannikov and Kontsevich and then proved by Sabbah, that says in the twisted case, we need to assume that the critical locus of f is compact. Then these two vector spaces $R\Gamma(\Omega^\bullet_{X,d+\wedge df})$ and $R\Gamma(\Omega^\bullet_{X,\wedge df})$. Of course, it's much easier to compute on the right hand side.

The result says that the function $X \to \mathbb{C}$ with proper critical locus over your base, you get the same hypercohomology groups. The piece that's missing is you'd like some explicit calculation similar to the Hodge decomposition. It replaces a calculation of homology of some complicated complex to doing it for some kind of sheaves. You always have a Hodge to de Rham spectral sequence but it degenerates here.

So first of all can we find an algebraic proof of the Sabbah theorem and can we find one where we get a calculation like the Hodge decomposition. There is an algebraic proof of [missed] but I will give a particular case, with an extra assumption you will get the cohomology of a bunch of sheaves that you can compute.

Now I want to say that for the original result there's a nice algebraic proof using characteristic p methods going back to Deligne and Illusie, I think 1988. You have a complex of sheaves which is not a complex of $\mathcal{O}_X$ modules. On the other hand you have a much nicer category. If X is over a field $\mathbf{k}$ of characteristic $p > dim\ X$, perfect, we can use the Frobenius morphism, which, the relative Frobenius is from X to $X \times_{\mathbf{k}} \mathbf{k}$, this is twisted to be made $\mathbf{k}$-linear. Then here's what Deligne and Illusie proved. They proved that if you take the de Rham complex of X with the non-linear differential d and push it forward to X', (since the field is perfect the map is an isomorphism, just not $\mathbf{k}$-linear), you get something $\mathcal{O}_{X'}$-linear. If X lifts to $W_2(k)$, (this is a ring, no longer a field) which is easy to satisfy, then $F_*(\Omega^\bullet_{X,d}) \cong \bigoplus_i \Omega^i_{X'}[-i]$.

This is the heart of their result, it gives the Hodge theorem immediately. You say that the de Rham cohomology of X is the global sections $R\Gamma(X, \Omega^\bullet_{X,d})$ which is $R\Gamma(X', F_*\Omega^\bullet_{X,d}) \cong R\Gamma(X', \Omega^\bullet_{X',0})$ and here we're not $\mathbf{k}$ linear and you get $R\Gamma(X, \Omega^\bullet_{X,0})$

Vanishing for arbitrary characteristic sufficiently large gives a characteristic zero calculation.

Since many people aren't familiar with characteristic p methods, I thought it might be good to give an explicit example.

If X is $\mathbb{A}^1$ which is not proper, but let's ignore that, $\mathcal{O}_X$ is $\mathbf{k}[x]$, and $F : X \to X'$ goes by $\mathbf{k}[x^p] \subset \mathbf{k}[x]$, with $Oo_{X'} = \mathbf{k}[x^p]$. We have $f \mapsto f'dx$ and look at $0 \to \mathbf{k}[x] \to \mathbf{k}[x]dx \to 0$. You want to compare this to $0 \to \mathbf{k}[x^p] \xrightarrow{0} \mathbf{k}[x^p]dx^p \to 0$. There's an obvious map on the left but on the right you ctake $gd(x^p)$ to $gx^{p-1}dx$. We're choosing explicit representatives, like choosing harmonic representatives. Finding this is only possible under the extra assumption. This will always be satisfied for any variety coming from characteristic zero.

Now let's move on to the, there's a more geometric way in terms of derived geometry to try to understand this result, namely I'd like to realize these two complexes that I'm trying to compare, the statement I'll make is the following. If

I try to understand X', I have two complexes, $\Omega^\bullet_{X',0}$. This can be understood in terms of the self intersection of X' inside its cotangent bundle. This intersection gives you a complex that is essentially the dual to this one, via a standard Koszul resolution. The one side is dual to the structure sheaf of the other side. Now this is no news. The more interesting news is that the Frobenius pushwordward of the de Rham complex on X can be related to the structure sheaf of a slightly different gadget, which is the cotangent bundle twisted by $\mathcal{D}$.

In characteristic p the sheaf of differential operators $\mathcal{D}$ on X has lots of elements in the center. In particular, the center can be identified with $\mathcal{O}_{T^*X'}$. This naturally lives on this bigger space, T^*X'. Moreover, this $\mathcal{D}$ is an Azumaya algebra, in the sense that it is over T^*X' and it's always nontrivial. This natural twisting is there staring at you and is not there in characteristic zero. Twisted spaces behave almost exactly like regular spaces. The point is that if we do the derived intersection inside the tangent bundle, without the twisting we get one complex and with the twisting we get the other. So computing the two derived intersections is the same, this is one way of understanding Deligne and Illusie's statement.

Morally, why is that true? The reason it's true is the following. When you compute the derived intersection of something with itself, you only need to know something about the infinitesimal neighborhood of X'. If you know that d is 0 on some formal neighborhood of X', you're done. That's not true, but Ogus and Vologodsky proved that $\mathcal{D}$ restricted to $X'^{(1)}$ is trivial as an Azumaya algebra if and only if X lifts to W_2. This says to first order the Azumaya algebra is trivial around the zero section and for reasons that will become obvious later, the computation only cares about the first order neighborhood of the zero section.

I would like to do the same exact story in the twisted de Rham case. I need to somehow put my function f into this picture. It's fairly easy to generalize the geometric picture. My function f gives rise to a section, X_f is the graph of df contained inside T^*X, and then do the Frobenius twist and you get X'_f contained inside T^*X'. The picture is kind of like this. Before I had the cotangent bundle of X' and I was doing the intersection of two Lagrangians, both the zero section. Now I'm just intersecting with a different Lagrangian. If I compute $\Omega_{X',\wedge df'}$, this is related to the intersection of X' and X'_f in T^*X', these two Lagrangian sections, the structure sheaf is the dual of this Ω. If I twist things as I was doing before where I replace $\mathcal{O}$ with $\mathcal{D}$, then we get the Frobenius pushforward of $\Omega_{X,d+\wedge f}$. The cotangent bundle of X' has a quantization going from $\mathcal{O}_X$ to $\mathcal{D}$. I look at what happens under the deformation, under the quantization. It is just adding the extra d. The problem is, you want to show that these are quasiisomorphic. We saw one example. What is the corresponding story in the twisted case?

Now we have a very concrete abstract problem. Namely, here's the abstract problem. We have a space S inside which we're doing intersections and we can compute the derived intersection of X with Y inside S. Or we could instead, we have an Azumaya algebra on S. Call it $\mathcal{A}$. Let's assume, I don't want to ask, this gives us Azumaya algebras on X and Y by restriction. I could compute the derived fiber product of this but that's not very interesting. What happens if $\mathcal{A}$ is trivial along X, so we have $(X, \mathcal{O}_X)$ and $(Y, \mathcal{O}_Y)$. I'm trying to intersect the same things as before but in $S, \mathcal{A}$ instead of S.

The question is to compare the derived intersection W in S with the derived intersection $\bar{W}$ in $(S, \mathcal{A})$. The point somehow is that it's fairly easy to see, let me say it this way. I can take the fiber product restricted to X and to Y. The fiber product would be the old W with the twisting from $\mathcal{A}$. Because it was already trivial on X or on Y it would pull back as trivial on W. However, the point is that the two trivializations need not agree on the intersection. The picture is that I have the two subvarieties. If I restrict my Azumaya algebra to either one it's trivial but on the intersection they may not agree. Any two differ by a line bundle of the ambient space. The difference of the two trivializations of $A|_W$ is a line bundle on W. This tells me whenever I have a problem of this sort, I have a natural line bundle on the intersection of X and Y. In the problem we're interested in, the line bundle is trivial in the classical sense. If we think of the classical intersection it's trivial. In the derived sense the line bundle may be non-trivial. This will be essentially the Deligne Illusie story.

What I wanted to emphasize, when we think of line bundles in the usual setting, that may be misleading. If we think $\mathcal{O}_W$ is $\cdots \oplus \wedge^2 E \oplus E \oplus \mathcal{O}_{W^0}$. What is the Picard group of W? It's $H^1(W^0, \mathcal{O}_W^*)$ which contains $H^2(W^0, E)$. The complex has the same cohomology sheaves as all these sheaves but it stops being a formal complex. One thing you can do is tensor with a line bundle. You can also take a complex which stops being formal. H^2 is a way to amalgamate the two terms of the structure complex.

So now we would want a theorem that says that the line bundle is trivial sometimes. Here's the theorem. It's a formality theorem.

Theorem 3.1. *Let W be the derived intersection in S with W^0 the classical intersection. If:*

a. *W^0 is scheme-theoretic smooth,*
b. *$X \to S$ is split to first order,*
c. *the sequence $0 \to N_{W^0/Y} \to N_{X/Z}|_{W^0} \to E \to 0$ splits, and*
d. *the two trivializations of $\mathcal{A}$ on the classical intersection are the same.*

then the derived intersection over S is the total space of $E[-1]$.

The point is that inside, S can be either ordinary or twisted. So the whole point is that the result of the calculation does not care about whether your scheme is twisted at all. The third condition is nontrivial and must be checked. The second condition needs to be understood in the appropriate category in the sense that if $X \to S$ is a map of ordinary schemes, it means the ordinary thing, but if these are Azumaya schemes (X and Y are not twisted, by the way) then I need to consider X and Y in this setting. The derived intersection, the point is, doesn't care about the twisting. It only cares about the intersection of X and Y in the ordinary case.

One other way to say this is that the line bundle is trivial.

I had two different derived schemes. One had a twisting. The difference is a line bundle. The statement is that the bundle is trivial.

Putting all of this together, apply this to the setup where S is the cotangent bundle of X' with either $\mathcal{O}$ or $\mathcal{D}$. Now X' is X' and Y is the graph X'_f, the graph of df. Now W^0 is the critical locus of f. Now W is the derived critical locus. The conditions, well, condition d. is automatically true. The sheaf of differential operators always becomes trivial on zero sections and the graph of a one-form. My assumptions are

a. $Crit\ f$ is smooth,
b. X lifts to $W_2(\mathbf{k})$, and

c. $Crit\ f$ inside X is split to first order.

Then $R\Gamma(X, \Omega^\bullet_{d+\wedge df}) \cong R\Gamma(X, \Omega^\bullet_{\wedge df}) \cong \bigoplus H^p(Crit\ f, \Omega^q_{Crit\ f})$. Thank you.

4. Behrang Noohi: Introduction to topological stacks

Thank you very much for the invitation. Those of you who have looked at the abstract will have realized that this was supposed to be a short course. I hope this is not too disappointing. The content is easy. I condensed some easy material to make it look like a research talk.

Here's what I want to talk about. Homotopy types of topological stacks, I don't know if that's the title I gave.

The outline is to start with some setup, then talk about some old ideas, then the classifying space of a topological stack, and then the singular chains. This part is joint in progress with Tom Coyne. The last part is higher stacks, really just a question.

Okay, so let me fixe the setup. I'm going to use an unorthodox approach to stacks. I'll take a stack to be a presheaf of groupoids on the category of topological spaces with the descent condition. For me it's a one-stack. There's no infinity going on. I work on the site of topological spaces or compactly generated Hausdorff spaces, whatever. I'll work in presheaves rather than things fibered in groupoids. The gadgets I'm interested in are topological stacks. A topological stack comes from a topological groupoid, associated to a topological groupoid. I will make an assumption. I'll assume that $R \to X$ is a local Serre fibration, using either the source or the target. For every y in R there are neighborhoods of y and its image in R and X such that if I restrict my map to $V \to U$ I get a Serre fibration. A lot of the things I want will still be true but let's assume it so I don't have to say it.

Some standard properties of topological stacks. First of all, they form a 2-category. 2-products exist. Of course, the famous Yoneda lemma, every topological space gives a topological stack. This is a fully faithful embedding.

What I want to do is extend ideas to the other side of this. If I have a group acting on X then I get the quotient stack X/G. I want to be able to get equivariant information about X captured by this theory.

That's what I want to do. I'll tell you a few different ways to think about this problem.

These were known before stacks were defined. Already a few decades ago people more or less knew about these. Let me give you some examples of homotopy invariants you can associate with these guys. So now $\mathcal{X}$ will be a topological stack. I can use Yoneda to think of S^n as a topological stack and look at maps $S^n \to \mathcal{X}$. You can easily talk about homotopy and pointed homotopy and talk about and define π_n which satisfies the ordinary expected properties. You could try to define homology or cohomology. You can look at cohomology by looking at the simplicial space associated to your stack. SUppose you have these quotient stacks. Then you can get the simplicial space $X_\bullet$ where $X_n = R \times_X R \times_X \cdots R$ and to this (I learned this from Kai) that you can construct a bicomplex, taking the chains, and take the Tot and get homology and cohomology. This is something that has been known for a long time, maybe not phrased in the language of stacks.

Instead of taking the bicomplex, you could take the geometric realization of $X_\bullet$ and get an honest space and homotopy invariants of $|X_\bullet|$ should be regarded as the homotopy invariants of $\mathcal{X}$. That's how you make sense of homotopy invariants for topological stacks. This is not very satisfactory, for a couple of reasons at least. One reason it's not satisfactory is functoriality. To get the right theory you'd need functoriality with respect to Morita morphisms. Some people call these bimodules. Not every morphism of stacks comes from a morphism of groupoids. You need to check functoriality. It can be done, of course, but it's not convenient, and if you want to do something fancy it gets quite complicated.

How do you go back and forth between $|X_\bullet|$ and $\mathcal{X}$ itself? You could define the homotopy groups this way or take the homotopy groups of $|X_\bullet|$. You expect them to be the same, but you really have to work at it. A map from $S^n \to \mathcal{X}$ does not come from a map of groupoids.

So you want a more functorial approach that tells you how $\mathcal{X}$ and $|X_\bullet|$ are related in a concrete way. Working on a project with Kai and Ping Xu and Greg Ginot, it turned out this wasn't really the right way, to use these ad hoc approaches. This question came up in Kai's office eight years ago. Is it true that for every topological stack there exists a topological space X with a map $X \to \mathcal{X}$ such that the map is a trivial Serre fibration? I keep changing my mind as to whether the answer is yes or no. I don't know the answer to this question. What I do know is the following.

Theorem 4.1. *There is a natural map from $||X_\bullet|| \to \mathcal{X}$ where $||\ ||$ is the fat realization where we keep degeneracies such that for all paracompact spaces T and every map $T \to \mathcal{X}$ we get that the space of lifts is contactible*

$$\begin{tikzcd} & {||X_\bullet||} \arrow[d, "\phi"] \\ T \arrow[r] \arrow[ur, dotted] & \mathcal{X} \end{tikzcd}$$

This theorem was an attempt to answer the question (call the question *). A corollary of this which is easy is that

Corollary 4.1. *ϕ is a universal weak equivalence.*

The meaning of that is that $||X_\bullet|| \to \mathcal{X}$, the fiber product over a map $S \to \mathcal{X}$, the pullback is a weak equivalence of topological spaces. In particular, all the fibers are contractible. It looks like what you need to be a trivial Serre fibration. It's somewhat satisfactory.

Corollary 4.2. *ϕ is a trivial weak Serre fibration*

What does this mean?

$$\begin{tikzcd} A \arrow[r] \arrow[d, "\text{CW inclusion}"] & \mathcal{X} \arrow[d] \\ B \arrow[r] \arrow[ur, dotted] & \mathcal{Y} \end{tikzcd}$$

when the square 2-commutes, the bottom triangle commutes and the upper triangle homotopy commutes fiberwise. The only problem is that I have the homotopy. We're almost there but I don't know how to get rid of the H. That's a bit unfortunate.

I'm not going to prove this, I'll just give you the ingredients of the proof. The proof of the theorem, the main two ingredients of the proof are the ideas of Haefliger on $||X_\bullet||$ and Dold's paper on partitions of unity, an annals paper from the 50s. These are old ideas, my only observation was that you get this map which has this property. The proof turned out to be quite technical. Elementary but you have to use 15th century—I mean 50s mathematics. Okay so that's the classifying space, what it does for us. Let me make a definition here. If I have a universal weak equivalence $\phi : X \to \mathcal{X}$ with X a space, then I call (X, ϕ) a classifying space for $\mathcal{X}$. Let me tell you how this could be used to prove things. I'll show you a couple of simple examples. It's easy to see using what I've erased that $\pi_n(X) \to \pi_n(\mathcal{X})$ is an isomorphism. Another application and this was one of our motivations, we wanted a Thom isomorphism for stacks. Suppose you have a vector bundle $\mathcal{E}$ over $\mathcal{X}$. You could define homology with the bicomplex, and you could formulate the Thom isomorphism. Showing it though, I don't think it will be that easy. Once you have a classifying space, it's almost trivial. Take a classifying space of $\mathcal{X}$ and take the pullback. So the homology is the same, the complement of the zero section as well. Then the Thom isomorphism on the pullback gives the Thom isomorphism on the stacks.

This gives you a bit more functoriality. That's why I like it. Let me say a few words about "functoriality." I put it in quotes because it's not exactly what I want. If I choose classifying spaces for $\mathcal{X}$ and $\mathcal{Y}$ with a map $f : \mathcal{X} \to \mathcal{Y}$, can I get a map lifting it? I take the fiber product, take a classifying space for the fiber product, and then for that choice I can lift the map upstairs. If you manage to do the lift, then two lifts are the same in the homotopy category. They are not homotopic, you may need to invert some quasiisomorphism to make them equal. The homotopy (respecting the diagram) is unique up to higher homotopy and so on. So taking the classifying space from topological stacks to the homotopy category of topological spaces is a functor. But I need to pass to the homotopy category.

Let me make a few remarks about this functor. It's the right adjoint to the inclusion of Top into $TopSt$. It's not right to say it's the right adjoint because the category Top is incorrect. It's true after some localization. That was my attempt at making this an honest functor. That's all I can say. I can say that ϕ is the counit of the adjunction. There's also a diagram version. If you start with a diagram $\mathcal{D}$, you need a condition, maybe locally finite, then you can do this uniformly and find a diagram of classifying spaces.

I was desperate to make this functorial. At the end of the day I don't have enough functoriality.

You could do some stuff using these classifying spaces. They do make your life somewhat easier. They are not quite satisfactory because as I said, they are not functorial enough.

(finde)

Now I want to present a different approach which is more functorial. This is singular chains on topological stacks, which is joint with Tom Coyne. It's very easy. I have a topological stack $\mathcal{X}$ and I want to define singular chains. So what I do, I think of $\mathcal{X}$ as a functor from topological spaces to groupoids and get a simplicial groupoid, looking at Δ^{op} in Top^{op}. This captures the homotopy type of $\mathcal{X}$. You want to be very classical and work with simplicial spaces. You can really construct a simplicial space. Take the nerve $N(\mathcal{X}|_{\Delta^{op}})$ which is a simplicial set and take the diagonal. That's the singular chains on $\mathcal{X}$. It takes morphisms to morphisms and two-morphisms to homotopies. This is the most obvious thing you could do and you could hope that it gives you the right homotopy types. This agrees with the usual simplicial set associated to a space, $S(X)$ when $\mathcal{X}$ is a space

X. It does extend the ordinary defnition. But you have to make sure it has the right properties. Here's the list you want it to satisfy.

(1) Does $S(\mathcal{X})$ have the same weak homotopy type as the classifying space?
(2) If I have a weak equivalence of topological stacks $\mathcal{X} \to \mathcal{Y}$, is the induced map on simplicial sets a weak equivalence?
(3) Is $S(\mathcal{X})$ a Kan complex? It's always a Kan complex when X is a space.
(4) More generally, if $\mathcal{X} \to \mathcal{Y}$ is a (weak) Serre fibration, is $S(\mathcal{X}) \to S(\mathcal{Y})$ a (weak) Kan fibration?
(5) Is there a natural weak equivalence $|S(\mathcal{X})| \to \mathcal{X}$?

Let me make some observations. First of all, two implies one, by taking $\mathcal{X}$ to be the classifying space of $\mathcal{Y}$. Four implies three by taking $\mathcal{Y}$ to be a point. Four also implies two. Maybe I should say a few words about why. I can always come up with a square above a morphism of stacks where above each stack I have a classifying space with a weak Serre fibration. If I apply the singular functor I get a square

$$\begin{tikzcd} X \arrow[r] \arrow[d, "\phi_X"'] & Y \arrow[d, "\phi_Y"] \\ \mathcal{X} \arrow[r] & \mathcal{Y} \end{tikzcd} \qquad \begin{tikzcd} S(X) \arrow[r] \arrow[d] & S(Y) \arrow[d] \\ S(\mathcal{X}) \arrow[r] & S(\mathcal{Y}) \end{tikzcd}$$

So then S preserves the fiber. You have to be careful with this because the original square is two commutative so you get a homotopy, but eliding that, you get the fibers to be contractible and move back and forth. So all I need to do is prove four. I'm surprised no one has noticed this. Four is not true. Everything pretty much breaks down. I have just a few minutes to fix that. I'll have to speed up a little.

Definition 4.1. *A morphism of topological stacks is a Reedy fibration if when I restrict it to Δ^{op} I get a Reedy fibration of simplicial groupoids. These make sense for simplicial objects in any simplicial category. It's the definition Ezra gave this morning in terms of matching spaces. Copy the definition Ezra gave except now we're in groupoids.*

You only have to check it up to two. You only need to check the lifting for paths. Maybe to two simplices. Anyway, the theorem is that if $f : \mathcal{X} \to \mathcal{Y}$ is a Reedy fibration and a (weak) Serre fibration then the induced map on singular complexes is a (weak) Kan fibration. So the answer is yes if you also have Reedy.

So now the arguments I gave could be modified but you get the same statements, roughly. You need for three to have $\mathcal{X}$ be Reedy fibrant in order to have $S(\mathcal{X})$ Kan.

Let me say one word about five. For five, unfortunately I need a condition. The answer is yes if $\mathcal{X}$ admits a classifying space which is a Serre fibration. Then we know how to prove five. Then, though, instead of using the ordinary realization, you use the fat one.

I guess I should stop. I wanted to say a few words about infinity stacks. You can make sense in higher stacks as well. You could do the same thing. What I have proved so far, I just indicated that you have a good definition. To formulate one last question. The generalization, I wanted to know the answer. If I find the answer I'll be happy. Question: is this true for ∞-topological stacks, meaning the noes coming from simplicial spaces?

5. Alice Rizzardo: Representability of cohomological functors over extension fields

So thanks to the organizers for giving me the opportunity to speak. For the duration of this talk, X and Y will be smooth projective and $D^b(X)$ will be derived category of coherent sheaves. We'll talk about $D^b(X) \to D^b(Y)$. If this functor F is fully faithful, it's isomorpyhic to a [missed]-transform. If p_1 and p_2 are the projections from $X \times Y$ to X and Y, then $\Phi_E = R_{\rho_{i*}} E \otimes p_i^*()$. You don't need all the hypotheses. Most of what I'm saying is joint with [missed]. One nice theorem is that for X a projective scheme such that $\mathcal{O}_X$ has no zero dimensional torsion and Y quasicompact and separated, then every fully faithful functor $Perf(X) \to DQCoh(Y)$ is isomorphic to the restriction of a Formier-Mukai functor associated to an object in the derived category of quasicoherent sheaves on $X \times Y$. This is all I'm going to say about relaxing hypotheses on $X \times Y$. You could also keep smooth projective and relax fully faithful. In the original paper of Orlov, he thought it was still true. He's changed his mind since then maybe. Some other people relaxed to a slightly weaker case than asking X to be full. I'll give you a counterexample in the case where our functor is not fully faithful.

Let me begin with F from $D^b(X) \to D^b(Y)$, an exact functor, and I'm going to pull back $D^b(\eta)$ where η is a generic point of y. I'll go to function fields and dualize for technical reasons. That total functor is H and a question I can ask is whether H is representable. Why do I care? If yes, I get $\tilde{E}$ in $D^b(X_{K(Y)})$ such that $H(C)$ is $[missed] j^*C, \tilde{E}$ where j is base change.

Then we can lift to E in $D^b(X \times Y)$ and then Y and Φ_E are isomorphic after pulling back to η.

This would be nice but I haven't told you whether H is representable. The answer is sometimes. I'll state the theorem in this precise situation. H is representable for the degree $K(Y) \leq 1$ or $K(Y) = [missed]$ so Y is a curve or a rational surface. The idea of the proof is as follows. I have my functor $D^b(X) \to mod_{K(Y)}$. If this were just to K then this is representable. I'm going to a bigger field. But I can take this to Mod_K (Big M means not necessarily finitely generated). The proof still goes through and I get $A \in D^b(QCoh(X))$ but also because I was actually going to modules over $K(Y)$ I also get an action of $K(Y)$. This isn't quite what I wanted, I wanted something in $D^b(QCoh(X)_{K(Y)})$. There's a functor from the latter to the former. Is it essentially surjective? I was able to prove it at the time with the conditions on the field above that put you in the curve or rational surface setting. I thought maybe there was a better way.

It turns out that you can do better but not that much better. There's an obstruction to lifting in Hochschild cohomology. Our theorem says, take $\mathcal{C}$ to be a K-linear Grothiendieck category and B a K-algebra. I have this functor forget from $D^b(\mathcal{C}_B)$ to $D^b(\mathcal{C})_B$. If B has Hochschild dimension less than or equal to two, then forget is essentially surjective. Less than or equal to one, you get full, and zero you get an equivalence of categories. In particular, you could get this in more generality, but in particular, the Hochschild dimension is just Chern Simons degree. I could substitute this with Chern Simons degree less than or equal to two and with higher degree you get an obstruction.

So for $M \in D(\mathcal{C})_B$, I'll lift at the A_∞ level. So an object in $D(\mathcal{C}_B)$ can be thought of as an object in $D_\infty(B, \mathbb{C})$ (I've inverted quasiisomorphisms) and the inclusion is an equivalence of categories. So start with fibrant $M \in C(\mathcal{C})$ and then $B \to Hom_{C(\mathcal{C})}(M, M)$ is compatible with multiplication up to homotopy. What does it mean to put an A_∞ structure on M? It's the same as having an A_∞ morphism $B \to Hom(M, M)$. We can do this step by step, starting with the one we have already and at each step we get an obstruction which lives in $HH^n(B, Ext^*_{\mathcal{C}}(M, M))_{-n-2}$. In particular this will be true if B has Hochschild dimension less than or equal to two.

Now you can ask what happens for higher Chern Simons degree. Things really do go wrong.

Theorem 5.1. *Take Y smooth projective over K, not a point, $\mathbb{P}^1$, or an elliptic curve. There exists a finitely generated field extension L/K of degree three on an object $Z \in D^b(QCoh(Y))$ which does not lift to $D^b(QCoh(Y)_L)$.*

What can we do with this? Take X smooth projective with function field L, and then start with $Perf(X)$, pull back to a generic point, this goes to $D(L)$, and then take, to the derived category of quasicoherent sheaves over Y, by taking L to Z which is enough to define my functor since Y has global dimension zero. Now this functor is not the restriction of an FM transform in the non-fully faithful case.

Let me see how much I can proove of this.

First of all, let me start with part two, showing that the composite functor is not a transform. I'll forget that I defined it on Perf, I'll take it on quasicoherent, so what happens if I compose $\Phi_v \circ \eta_*(L)$? This si the same as $\Phi_{(i_\eta \times id)^*} L$ and that base lives in $DQCoh(SpecL \times Y)$ which is $D(QCoh(Y)_L)$. But on the other side you get Ψ (the functor) composed with [missed] which is Z and this is something I said you couldn't do.

For part one I'll do proof by example because maybe it'll be more interesting. My example will be $X = Y = P^3$. So $L = k(x, y, z)$. I'll consider $\mathcal{O}_X \oplus \mathcal{O}_X(1)$. I'll go to $Mod(kQ)$, where Q is the quiver with four arrows.

I'll take the following representations where the arrows are 1, x, y, and z from L to L. Now $Z = R \oplus R[1]$ in $D^b(Mod(kQ))$.

Now I get

$$L \to End_{D^b(Mod(kQ))}(Z) = \begin{pmatrix} End\ R & 0 \\ Ext^1(R, R) & EndR) \end{pmatrix}$$

My map is $\begin{pmatrix} \phi_{11} & 0 \\ \phi_{21} & \phi_{22} \end{pmatrix}$.

You need ϕ_{21} to be a derivation and Z lifts to $D^b(Mod(kQ)_L)$ to ϕ_{22} and inner derivation. So you look at $HH^1(L, Ext^1_{kQ}(R, R))$ which is $HH^3(L, Hom_{kQ}(R, R) = HH^3(L, L)$ which is nonzero by the Hochschild-Kostant-Rosenberg theorem.

6. Jesse Wolfson: The Index Map in Algebraic K-Theory

Thank you, and I want to thank the organizers for letting me speak even after snowfall prevented me last week. I'm talking today about part of an ongoing project. With some luck in short succession a few more papers will show up after the first one later this week.

So R will be a ring. and the basic objects I want to consider are $R((t))$ with the t-adic topology. This is working in the category of Tate modules, and a countable Tate module is a topological direct summand of the Laurent series. This is the background, and let me get a few definitions up on the board. The first definition is that a lattice is a submodule $L \subset R((t))$ such that the quotient is a discrete projective R-module, and we require that L is isomorphic to the topological dual of another projective module. For example, the topological dual $k[t]^\vee$ is the module $k[[t]]$. This is the type of thing. The basic lattice we'll consider is the submodule $R[[t]]$ of formal power series. There's an unfortunate thing all the things called Tate modules. If you've seen a different notion put it out of your mind for todays talk. The main object of study is the Sato Grassmanian, which I'll define to be $Gr(R((t)))$ which is the set of lattices.

The key fact about lattices is that if $L \subset L'$ are lattices, then the quotient L'/L is a finitely generated projective R-module. This motivates the following definition, if L and L' are a pair of lattices, then $L - L'$ we'll define this to be $N/L - N/L'$ as a point in the K-theory of R where N is a lattice containing both.

There's a canonical map from the classifying space classifying R-modules into K-theory. This is an infinite loop space so I can add and subtract things.

The goal of this talk is to show that $L \mapsto R[[t]] - L$ gives a natural map from $Gr(R((t))))$ to K_R.

Why is this interesting and why do I care? This Grassmanian is very rich and interesting in geometry. This is the set of R points of an Ind-scheme. You can think of this as being a good place to study the KP and the [missed] hierarchies. This object also plays an important role in the representation theory of loop groups. There's also something important in index theory. Today I'll talk something about index theory and give a perspective on how that map looks from that perspective.

Okay, so I'll take a brief detour into Hilbert space. $\mathcal{H}$ will be a complex separable Hilbert space. Recall that a bounded operator A on $\mathcal{H}$ is Fredholm if the dimensions of its kernel and cokernel are both finite. We'll write $Fred(\mathcal{H})$ for the space of Fredholm operators. The clasical theorem of Atiyah and Janich is that there exsits a map from the space of operators to $K^{top}_{\mathbb{C}}$. This is sort of my guide for how to think about this area. If we think of a polarized Hilbert space, for example $L^2(S^1, \mathbb{C})$ with polarization from $L^2(D^2, \mathbb{C})$, then we can think of the automorphisms in $\mathcal{H}$ whose projections onto the polarization is Fredholm and the projection onto the complement is H.S. There's also a version of the Grassmanian due to Segal-Wilson.

An easy corollary of the Atiyah-Janich theorem is that there's a homotopy commuting square

$$\begin{tikzcd} CL_{res}(H, H_+) \arrow[r, "g \mapsto gH_+"] \arrow[d, "g \mapsto \pi_{H_+} g \pi_{H_+}"'] & Gr(H, H_+) \arrow[d, "L \mapsto H_+ - L"] \\ Fred(H_+) \arrow[r, "\text{index}"'] & K^{top}_{\mathbb{C}} \end{tikzcd}$$

All these maps are equivalences.

The theorem that tells you how to think about this is that for R there is a homotopy commuting square

$$\begin{tikzcd} Aut(R(\mathcal{H})) \arrow[r, "j \mapsto jR[[t]]"] \arrow[d] & Gr(R((t))) \arrow[d] \\ \text{index}\ \Omega K^\vee_{\mathrm{Tate}} r & K_R \end{tikzcd}$$

So this is a more general version of the previous square.

Theorem 6.1. *(Saito)*
Index is an equivalence.

Proposition 6.1.
$$\Omega(BGL^+_{res}) \cong Fred(\mathcal{H})$$

Proposition 6.2. $\Omega BAut(\mathbb{R}((t))^+) \cong \Omega K_{Tate(R)}$

These are the propreties I want this map to have. [Missed some discussion]

I'll come up with a map to K-theory. I'll avoid homotopy theory by using a convenient model. This construction showed up in a couple of talks, Waldhausen's construction of K-theory. If X is a simplicial set, then $Dec(X)$ is a simplicial set whose n simplices are the $n+1$-simplices of X. The face maps shift up by one, d_i is d_{i+1}. We have to have a starting and ending point. So we also have an endpoint, so on $n$simplices we have $X_{s+1} \to X_n$ and $\delta_1 : DecX \to X_0$ via d_1^{n+1}. This S is a deformation retract.

Let me recall Waldhausen's construction. Think of $\mathcal{C}$ being finitely generated projecive R-modules. Now $s\mathcal{C}$ is diagrams, a sequence of inclusion admissible, see, just kernels, inclusions of direct summands. Then we'll choose quotients for all these inclusions just as a way of sort of canonically fixing the data. You have a nested sequence of subobjects. The ith face map equals forgetting the ith row and the ith column counting from the top. The theorem, this is the amazing thing, due to Waldhausen, is that if we geometrically realize this simplicial set, it's an infinite loop space, so that's quite strong, and two, the loop space of the geometric realization is the K-theory of our exact categories.

This gives us a way to get maps into K-theory. If X is a simplicial set, then a homotopy commuting square

$$\begin{tikzcd} {|X|} \arrow[r] \arrow[d] & {|Decs.\mathcal{C}|} \arrow[d] \\ \star \arrow[r] & {|s.\mathcal{C}|} \end{tikzcd}$$

determines a map $|X| \to \Omega|s.\mathcal{C}|$.

Now I'll build a map and then I'll be done.

So I have to write down a nasty definition. I wrote down a definition of a difference of two lattices. I want to make a choice that includes all choices and it'll even out in the wash. This will be technical, I'm sorry, it'll go fast. So $\mathcal{L}$ will be a subset of $Dec^2 s.Tate(R) \times^{S_0,S_0}_{Decs.Tate(R)} Dec^2 s.Tate(R)$. The subset will be [missed]. Once you have this definition, the map to K-theory follows formally. The key fact is the quotient of lattices by sublattices, you'll get a map into $Decs.\mathcal{P}(R)^2$ and if we apply [missed] on both factors we can get down to $s.\mathcal{C}$. So key is that both factors are equal. We geometrically realize and this is an infinite loop space so we have a contractible space of contraction maps and if we compose that with these maps, because the two factors are equal, we have a homotopy commuting square of the desired form

$$\begin{tikzcd} {|\mathcal{L}|} \arrow[r] \arrow[d] & {|Decs.\mathcal{C}|^2} \arrow[d] \\ \star \arrow[r] & {|s.\mathcal{C}|} \end{tikzcd}$$

The fact then is that any two lattices admit a common envelope.

If I forget all the chains of common enveloping lattices, this gives me a map from $\mathcal{L}$ to the Grassmannian of my Tate module. So I get things that break up in terms of nerves of posets so the fibers are contractible. So I get this map to the K-theory space. If you run through the definitions up on the board, you'll see that this comes from taking a lattice, taking a common enveloping lattice, and taking the quotient, and the construction ensures that our choices give canonically equivalent answers. That's the map, that's a little bit of K-theory and index theory in this context, and thank you for listening to my talk.