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1 Dusa McDuff, homotopy properties of symplectomorphism groups

It's a great pleasure to be here and welcome John into this club that none of us want to belong to.

I want to talk about homotopy groups of symplectomorphism groups. This question is motivated by some amazing results I'll tell you about.

Some of the things that are known about symplectomorphism groups, well, $Symp(\mathbb{CP}^2, \omega) \cong PU(3)$ (homotopy equivalent). For $\mathbb{CP}^2$ blown up at a point. YOu have to be careful about what you want, $\int_{line} \omega_\epsilon = 1$ and $\int_{exceptional} \omega_\epsilon = \epsilon$ for $0 < \epsilon < 1$ we have $Symp(\mathbb{CP}^1 \# \bar{\mathbb{C}}\mathbb{P}^1, \omega_\epsilon)$ is $U(3)$ for ϵ less than one half.

For $Symp(S^2 \times S^2, \omega_\lambda = \lambda\sigma_1 \oplus \sigma_2)$ When λ is one, its identity component is $SO(3) \times SO(3)$. The rational homotopy is worked out above one. Between one and two, Anjos Granja worked out that you have

$$\begin{tikzcd}
SO(3) \arrow[r, hook, "\Delta"] \arrow[d] & SO(3) \times SO(3) \arrow[d, dotted] & \\
SO(3) \times S^1 & \text{amalgamated free product} \arrow[r] & \mathscr{G}_\lambda
\end{tikzcd}$$

How much of this structure remains above dimension four? This is harder there because these results rely on low dimensional methods.

Reznikov showed $B(PU(n+1)) \to BSymp\mathbb{CP}^n$ induces an injection on $H_*(\ , \mathbb{Q})$ so a surjection on $H^*(\ , \mathbb{Q})$.

The proof involved constructing characteristic classes on the Lie algebra of $Symp\mathbb{CP}^n$. The point is that this result,

Proposition 1 *(Kedre, M.)*
$B(PU(n+1)) \to BDiff(\mathbb{CP}^n)$ (that act trivially on H_*) then this also induces an injection on $H_*(\ ,\mathbb{Q})$.

The proof is to construct some characteristic classes on $BDiff_H\mathbb{CP}^n$.

Start off with (M,a) which is c-symplectic. This is $a \in H^2$ with $a^n \neq 0$ and M $2n$ dimensional. Let me assume $\pi_1(M) = 0$ for simplicity. If you have a bundle
$$\begin{tikzcd} M \arrow[r] & P \arrow[d] \\ & B \end{tikzcd}$$
with fiber M and the fundamental group of the base acting trivially on $H_*(M,\mathbb{Q})$ then a has a canonical extension $\tilde{a} \in H^2(P,\mathbb{R})$ with $[unintelligible] = \int_M \tilde{a}^{n+1} = 0$.

Given any extension b of a you have $\int_M b^{n+1} = c \in H^2(B)$ and then $\int_M (b - \pi^*(c'))^{n+1} =$ [At this point Dusa erased the board].

So you can get $\tilde{c}_k = \int_M \tilde{a}^{n+k} \in H^{2k}(BDiff_H M)$ for the M bundle over these diffeomorphsims. There are enough classes to pull back to the generators. This uses hardly anything in the symplectic structure.

Look at the Leray Serre spectral sequence to do this. d_2 is zero because there's nothing in H^1. So $d_3(a^{n+1}) = 0$ since $a^{n+1} = 0$. This is $d_3(a)(n+1)a^n$. Multiplication by a^n gives an isomorphism between parts of the spectral sequence, so d_3 is zero below where you want it to be.

I want to spend a little time now talking about symplectomorphism groups of blowup manifolds. First of all I want to present an idea that I saw in Kedra. We want to create elements in $\pi_*(Symp(\widetilde{M,\omega_\epsilon}))$. The $(\tilde{M},\epsilon)$ is a one point blowup. You remove an ϵ-ball and then collapse the boundary, identifying the boundary of the ball with $\mathbb{CP}^{n-1}$. If ϵ is small enough, you can do this. If ϵ is large other factors come into play.

Start off with $\Delta \subset M \times M$. Take the product form $\omega \times \omega$. You can blow up along the diagonal by some amount. Then you get $\tilde{P}$ sitting over M with fiber $\tilde{M}$. So you get a map from M into $BSymp(\tilde{M},\omega_\epsilon)$.

We can first of all look at the exact sequence, you have $Symp(M) \to M$ by $\Phi \to \Phi(p)$. and then you can put this into $Symp(M,p) \to Symp(M) \to M \to BSymp^U(M,p) \leftarrow \phi : d\phi_p \in U(n)$ Then the group $BSymp(M,p)$ is essentially the same as $BSymp^U(\tilde{M},\Sigma) \to BSymp(\tilde{M}_\epsilon)$. The question is what the image is. But $ev_*(\pi_{2k})$ has finite image. You'd somehow, if you pulled back, you'd get cohomology of infinite order, and yet it's finite. This is fairly understandable. You can say $f_*(\pi_1 Symp(M))$ is in the center. One of the later maps, however, is not well understood except in the symplectic case. Something comes from $SEmb(\Sigma,\tilde{M})/U$ which is often contractible for M^4.

So in $\tilde{M}^4$ the exceptional divisor is a two-sphere and if $[\omega]$ is integral and $\epsilon = \frac{1}{N}$, then $[\omega_\epsilon] \in H^2(M, \frac{1}{N}\mathbb{Z})$. Then $\tilde{\omega}_\epsilon(\Sigma)$ is minimal. Then $Mod^J([\Sigma])/reparam$ is compact. So for every a.c.s. K there exists an embedded S^2 in the class $[\Sigma]$. This tells you this is contractible. Kedra showed that if you start off with a $K3$ surface you start with something of finite rank but this over here has infinite rank so you get a lot of stuff over here.

There are three propositions. One of them is about π_1. The first one is that ker $f_* : \pi_1 M \to \pi_1 BDiffM^2$ is contained in the center of π, M. The best result would be that ker f_* is equal to $im\ ev_* : \pi_1 DiffM \to \pi_1(M)$. I think this kernel is contained in the image of the evaluation map, I think you have to extend to smooth self-maps isotopic to the identity. I think the kernel is contained in this group. It's hard to find examples. There's nothing about symplectic manifolds here at all.

For π_2 you get, you seem to need to use something. If you start off with (M, a) c-symplectic, then there's an injection of $H_2^S(M, \mathbb{Q})$, of $\pi_2(M) \otimes \mathbb{Q} \oplus \mathbb{Z} \to \pi_2 BDiff\tilde{M}$. Basically you have an injection, and then you have extra elements, when I was doing the construction with the diagonal, you use the symplectic blowup along the diagonal. I can look over $S^2 \times M$ and then look inside at the graph of σ. I have a section of a product manifold. The choice of unitary framing gives you a separate $\mathbb{Z}$.

If you just apply this for $\mathbb{CP}^2$ with σ a point map, then there exists an element of $\pi_1 Diff(\mathbb{CP}^2 \# \overline{\mathbb{CP}}^2)$ that is not in $\pi_1 Symp(\mathbb{CP}^2 \# \overline{\mathbb{CP}}^2$, any form). You can try to do a similar thing for $S^2 \times S^2$, which has the antidiagonal in it, which you can collapse to get a singular orbifold Y, and blow up along that to get $S^2 \times S^2$. This gives you an element in the diffeomorphism group not in the symplectic group, but it is a sum of two such, so it's not particularly new.

Anyway, so there's, I'd like to say a little bit about the proof in a minute. The last thing is, what can you say in the higher groups. Here I seem to need to use the symplectic structure.

Proposition 2 *If (M, ω) is symplectic with $\pi_1 M = 0$ then ther exists a homomorphism $H_{2k}(M, \mathbb{R}) \to H_{2k}(BSymp_0(\tilde{M}, \omega_\epsilon))$ whose kernel is contained in $\{\gamma \in H_{2k} : \int_\gamma a^k = 0$ for all $a \in H^2(M)\}$. The proof uses Gromov Witten invariants so it requires the symplectic structure.*

You have a bundle with fiber $\tilde{M}$ and the exceptional divisors sit inside as a subbundle. You can't identify it using [unintelligible], but you can kind of do that with Gromov-Witten invariants. So $GW_{0,2}^E = 1$. Take a K that is integrable near Σ. This gives lines in the exceptional divisor. Take two copies of E which intersect the exceptional divisor. You know that the Gromov Witten invariant is one because it's one line through two points in this curve. If you can find some E here, a cycle $Z \subset \tilde{Q}$ so that $Z \cap$ fiber is E. In each fiber there is at least one J-holomorphic curve. That has evaluation maps into $\tilde{Q}$. That's a cycle that's basically like a section. It's a class with the dimension of the base whose image lies above. That's a homological way of detecting this cycle.

Since the two-cycles have this canonical extension, if you start with a class that is dectectable with this method, you get something above where $\tilde{a}^k$ doesn't vanish, but if it were a boundary it would not vanish.

You might get by with pure homology. The cycles could be anything as long as they have the right intersection property. I don't quite know the status in the general case.

When I started this, I thought you'd need the symplectic structure. As always, you start off with $S^2 \to M$ and then you have $M \times S^2$ over S^2 with the graph section, along which we'll blow up. We can always look at $a + \pi^*(\text{area form})$. So we can choose this nondegenerate, and also locally symplectic near the section. We can blow up along this section, and we can calculate the volume $\tilde{P}, \tilde{\Omega}_\epsilon$. It is absolutely not the volume of a product. That would be $vol(\tilde{M}_\epsilon)\times$ the area of the base. You can just calculate and find that it can't be that, unless it's the trivial section.

That's basically all I have to say. Something about the role of the different elements.

2 Dennis

So this lecture, its first part is dedicated to John Morgan, I want to convince him that infinity structures are interesting. A while back we had similar interests in mathematics. I would have a picture of a certain algebraic topology thing, and have a hard lemma, and he proved the lemma because I wouldn't, and published it, making it available. Anyway, that was great, John, thank you.

So, ∞ versions of usual structures. I want to give examples. A vague definition is, the usual constraints that describe the structure are relaxed and replaced a potentially infinite hierarchy of deformations or homotopies. This could be in various contexts. The hierarchy is something like a resolution of the the constraints.

There's nothing wrong with this mothematically, so, do you like it yet? Later, there are a couple examples of this whose names are familiar, Lie_∞, A_∞, later I'll discuss Lie_∞ and ∞-action, and I'll describe it as an algebraic analogue of a G-action on spaces, which I will now describe.

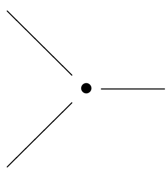
This will be thi first example. Take the group $G = \mathbb{Z}_2$. I want the infinity version of an action of $\mathbb{Z}_2$. Take two copies of a space X and an isomorphism $\sigma : X \to X$. Over two points in X you construct the unit interval, formally you take the mapping cone of σ, which gives you a bundle over the interval, and then you perform σ again giving you a bundle over the circle. If this were an action of $\mathbb{Z}_2$ this would extend over the disk, the monodromy would be trivial, so extend over the disk. On the boundary, it is symmetric, now add another copy, so you get a bundle over the 2-sphere, and then fill that, and continue thusly. If you keep going to infinity you get a contractible space, so you get a bundle over a contractible space with an actual involution, so we have fiber X and base $\mathbb{RP}^\infty$.

The $\infty - G$-action on X is a bundle with fiber X over B_G. So $\mathbb{RP}^\infty = B_{\mathbb{Z}_2}$. This loses information, if you have an actual action you can form an associated bundle. So the Borel construction is the infinity version of the actual G-action. Suppose G were a free group $\mathbb{Z}$, it would be like a bundle over a circle, and for $\mathbb{Z} \star \mathbb{Z}$ is a bundle over the figure eight, so it's the same in the ∞ case and in the usual case. One can think about this.

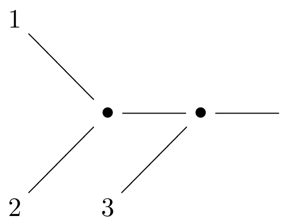
This resonates with the idea of R-modules and free or projective resolutions of them, because, when you have a discrete group, you take the group ring, acting trivially on the Z module and that corresponds to the cells of BG. More generally, if you had a resolution of module, you have a complex which is isomorphic, a resolution is just a free complex (projective complex) isomorphic to your object. You replaced your object with a free one where you can do homotopy. Do you like it yet?

[You're heading in my direction.]

Let's go back to the infinity version of a Lie algebra. By the end I want to relate this to symplectic topology, how moduli spaces get compactified. Maybe you have a Lie algebra, you have the bracket map

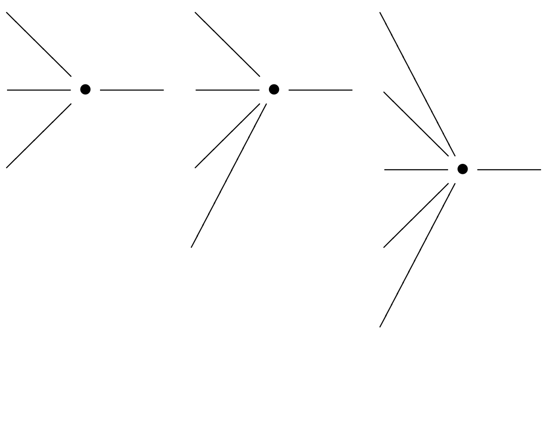


and you can make a picrture of Jacobi:



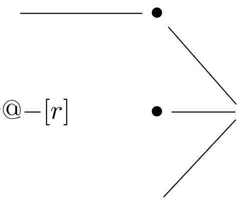
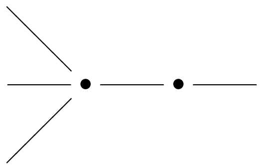
with two others, the sum of this and the cyclic permutations of it are zero.

So you're now going to have more operations, three, four, five to one:



and so on.

I also want a one to one.Then you are going to have unfolding equations among the operations. If you have a certain selection of graphs of a certain type, then you could have some kind of unfolding equation. This is generalized unfolding. This is quadratic because there's only one composition. So you can also unfold with the one-to one, like if I were unfolding the three to one, I could get terms like

&ar@$-$[r]  and 

I should have said at the begining that the one to one is δ and unfolding it gives

is zero so $\delta^2 = 0$. Then the unfolding of the three to one m_3 gives $[\delta, m_3] =$ Jacobi.

I haven't finished showing that this is a resolution, but there's a theorem that it is, Ginzburg and Kapranov. So if I move δ over into the structure of the space as a differential, then you've destroyed, except for δ this is a free structure. Every relation uses δ. There are no internal relations among these objects, all the relations have to do with δ. Now, this leads to, this has an important consequence, which is the following, suppose I have (V, d) and another complex (W, d), Suppose you have an ordinary chain map that commutes with d and is an isomorphism on homology. Then you can transport such a structure. Think of it, if you're over a field, let's work over $\mathbb{Q}$. You define the operations inductively, you go over, bracket, and come back. Up to homotopy, they will satisfy the appropriate equations to transport the structure. This is the key property we gain, that we have this transportive structure through very rough maps.

Now. An example of this, there's a very small complex, take the homology of this complex, with differential zero, then the bracket becomes an actual bracket, but then you get all these higher order tensors. I'm trying to pull out, squeeze out the juice and get something invariant. These are like correlation operators, you might say. They are not individually well-defined. The higher terms transform, the way if you change variables in a multivariable power series nonlinearly. The thing from before gives a $d^2 = 0$ on $\wedge^c V^*\{-1\}$ and extend them to derivations. These are all extended to be coderivations on the coalgebra $\wedge^c V\{-1\}$. This has a nice slick $d^2 = 0$ and in the dual language, the commutative algebra, you have a derivation on the functions on V, it's like an infinitesimal [unintelligible]on V, it's a formal manifold. These things is what we were using in 1970. These were the objects of rational homotopy theory. Most of what I've been saying was known then but not understood in this way. So one of the new things that, well, what is sort of new is the perspective that such an object is an ∞ version of a Lie algebra. This is tantamount to showing that if you look at the space of trees with n inputs and one output and you think of this as a complex where you have lengths on these edges and as lengths go to zero you collapse to a local stratum, then the homology of this complex for each n is concentrated in the top degree, the trivalent degree. Make a boundary operator by collapsing edges. A cell is labelled by a tree. There's an orientation, and an induced one. When you collapse two edges, you get two terms, but in different orders so they cancel. You put brackets in all possible places and then reduce modulo the Jacobi, this resolves the Lie object. This is Ginzberg-Kapranov, and it's a nontrivial theorem. I'm keeping my eye open for an easy proof.

I just wanted to mention, now you can define the ∞ version of a Lie_∞-algebra on a differential graded algebra (F, d). This will be $F\tilde{\otimes}(V^+, d)$ and then $\tilde{d}(1 \otimes --) = (1 \otimes d --)$ and $\tilde{d}(-- \otimes 1) = d - \otimes 1 + A$. For each monomial it will give a derivation of the differential algebra. This is like A_{ij}^k. So you get four terms, and you get $dA + \frac{1}{2}[A, A] = 0$. This is in defree one totally. It's like a connection. It interacts with d through this equation. This is actually the algebraic analogue, and we knew this in the 1970s, this is like B_G and then this is like a fibration over B_G with fiber F and this is the twisting cochain. It looks like a connection. If you had an actual action you'd have this but at the infinitessimal level.

The only thing that wasn't understood about this in the 1970s was this conceptual understanding that this was a resolution of a Lie algebra.

So now I want to mention that, let's do this same process with, instead of trees, let's do it with a number of inputs and a number of outputs. So we would have all of the n to one, and one to n's and then we can get Jacobi and coJacobi by unfolding each of those. We would have a Lie and coLie algebra, you also have the last tree with four things, two inputs and two outputs. You get five terms, and if you have a vector space with bracket, cobracket, and this five term relation, this is the Drinfeld compatibility condition and defines a Lie bialgebra. The next stage, there are analogues of these other steps. Gan showed that if you take a complex of trees then the homology is concentrated in one degree and it's a Lie bialgebra. If you have (V, d) then you have an operation for every "star-shaped" tree, then you have a free thing whose homology is the things of a Lie bialgebra. We can again transport this structure, you move down to homology, and you can move it across.

In some joint work with Moira Chas, this kind of structure, in string topology, you look at the free loop space of a manifold, this would only be for a manifold, it has a circle action, and then you can form the homotopy theoretical quotient, using the ∞ version of the S^1-action, and then you can take homology or just work at the chain level. The equivariant chains on LM has the structure of an ∞-Lie bialgebra. You hav contact homology in symplectic topology, manifolds with contact boundary. You look at genus zero curves running from the negative to positive boundary, rigid ones, and then count them to give the matrix coefficients of these operators. This structure, if you look in the one dimensional moduli space, you get $\delta X = X \cdot X$ and then later $[\textit{unintelligible}] = 0$. If you can get rid of some constants you can get a ∞ Lie bialgebra. I'm working now on the correct way to deal with these constants. Of course, ultimately you want to interpret all of the moduli spaces, the whole structure. Then there's this interesting conjecture, I first heard it some years ago from Eliashberg, when you look at symplectic field theory at level two. Level one is this infinity discussion with only one output, level two is described in a different formalism, you can change it into this formalism if you play around with it, then the conjecture is that when you apply this to T^*M, you should get the same ∞ Lie bialgebra. The one structure uses transversality and classical topology while the other uses PDEs. So the bad news is that my structure is just a very special case of this other structure, but the good news is that it's probably easier to compute.

It's still not understood at the chain level how to construct Poincaré duality, that's where these graphs with no extra structures at the vertices. Now draw graphs locally in the plane, The inputs and outputs can be deployed in any manner. If you just do one output, and you unfold that, this leads exactly to the theory of A_∞ algebras. Since the other one exactly replicates homotopy theory in topology, this one replicates a noncommutative version, if you can imagine that. If you look at relative contact homology, with boundary on Lagrangian submanifolds there's an analogous construction, there's some work in progress with Michael Sullivan, but in the classical cases of knots and Legendrian knots, there's a pretty good understanding of this according to my expert here. Again, this structure exists on both sides, on the string side, now you have the manifold with submanifolds, and arcs between submanifolds, and you get structures there that seem to fit this yoga, and the structures on the other side fit too, you could imagine a similar connection, there's some work by Lenny Ng there. Michael has interpreted his work in terms of string topology operations. When you do this in a setting where you're really doing topology, it seems like maybe you're doing operations of string topology.

[If you do these graphs with Ciemann surfaces do you get [unintelligible]? Des it give information about the homology of Deligne Mumford moduli space?] One knows that the Deligne Mumford compactification of a disk with points on the boundary is exactly the A_∞. If I look at closed strings in moduli space, but the way I get a Lie bialgebra is by going through moduli space. String topology will give operations on the cells of moduli space. When you compactify there are two issues. First you have to decompose into compositions, then you have to show that the singularity can develop. It starts with joint work with Moira Chas, but it's not all that. You get a Lie bialgebra out of it as the top stratum.

3 Witten

Thank you. I'm really happy to be here to help celebrate John's birthday. A few years ago we were studying some things, I wish that for your birthday I could have given you a more precise connection.

Let me know in the back if I write too small or speak too softly.

I'll be talking about Geometric Langlands. This began with number fields. But geometers have constructed analogues in the hopes of more intuition for why it's true. So for a curve over a finite field, or, what I'll talk about today, over a complex Riemann surface. Why would I as a physicist be working on it? There are hints it has to do with duality in Gauge theory. One hint comes from the statement of the Langlands program. The Langlands correspondence relates $Hom(\pi_1(C),^L G)$ which is minimal energy stuff. The right hand side involves what are called automorphic forms of G, a geometric analogue, which are closely related to a current algebra, or Kac-Moody (affine Lie) algebras on a surface C. So it has to do with conformal field theory and Kac-Moody algebras, WZW-models. Both involve something recognizable in gauge theory. I would have spent a lot of time on this point in years past.

I've mentioned two Lie groups ${}^LG$ and G. The relation in the simply laced case is from interchanging roots and weights. So $U(n)$ goes to itself, $SU(n)$ goes to $PSU(n)$, E_8 goes to itself. Every other Lie group has some difficulty. $SO(2n+1)$ goes to $Sp(n)$ and G_2 goes to itself.

That correspondence arises in two areas of wisdom: one is the Langlands program (1970) and the second is quantum gauge theory, which goes back to 1976, Goddard [unintelligible]Olive and [unintelligible]-Olive on four dimensional gauge theory. They considered gauge theory with gauge group G so that the charges are in representations of G. In the strong force this is in $SU(3)$ and that's where the quarks live, in rpresentations. And then the magnetic charges are in representations of another group. That was ${}^LG$; I first heard this from Atiyah in 1977. I won't explain today what we did, it would take us too far afield. It's been a tantalizing puzzle that this arises in two places. There are numerous other reasons to expect a connection, but I don't have time to talk about them right now.

So in the first paper they reintroduced the dual group, with some interpretation. The second paper I mentioned, for a gauge theory with gauge group G and "coupling constant" e they proposed an equivalent description via a dial theory where the gauge group is ${}^LG$ and the ${}^Le = \frac{2\pi\hbar c}{e}$. You can set c to one but don't put $\hbar$ to zero without talking about it first. (The constant is $\frac{1}{4e\hbar}\int TrF\wedge *F$). This would exchange electric and magnetic. The magnetic charges in the classical limit are solved by PDEs, and they write books about this. Being able to [unintelligible]is what you would do if you studied gauge theory quantum mechanically, not classically.

Subsequent work made it clear, by around 1981, and here there was Olive-W., and Osborn, it was clear that the right case would be the maximally supersymmetric case of $N = 4$ supersymmetry. Now, $N = 4$ supersymmetry is the maximal possible in dimension 4 and is uniquely determined by the choice of a Lie group, which we will think of as simple, and a complexified form of e. The M-O considered only that L^2 norm of the curvature, but the second Chern class of the bundle $\int Tr\ F\wedge F$ should also be considered, multiplied by $\theta/16\pi^2$. There are a lot more supersymmetric terms. I won't explain that today.

Now, M-O assumed θ was zoro, and you should combine e and θ to got $\tau = \frac{\theta}{2\pi} + \frac{4\pi i}{e^2}$. This is defined to be in the upper half plane. Then there is an elementary symmetry, $\tau \to \tau + 1$. We only care about the action mod $frm{-}e\pi$ Then the integrality of the Chern class means we only care about θ mod 2π. So M-O conjecture was that τ goes to $-1/\tau$. This is $SL_2(\mathbb{Z})$ so another hint of Langlands. Why is $N = 4$ so important? There's a lot of stuff that goes into a high degree of supersymmetry. I can write a table.

Hodge theory is a suprsymmetric theory, where S is a one-manifold, X is whatever, and Φ :

		Hodge Theory	Number of supersymmetries		Maximal dimensio
	$\mathbb{R}$	generic manifold	d, d^*	$H = dd^* + d^*d$	3
$S \to X.$	$\mathbb{C}$	K ahler	4	$\partial, \bar\partial, \partial^*, \bar\partial^*$	4
	$\mathbb{H}$	hyperK ahler	8	$Sp(2) \cong SO(5)$	6
	$\mathbb{O}$	there is a theory for each G.	16	$Q_1, \ldots, Q_16$	10

It stops there, I won't lecture next time about a theory with 32 supercharges.

You might ask, can you replace S by S_d which is a d-manifold and find a suitable Lagrangian so that if you specialize it so that it will reduce to this. What is the maximum dimension, you may ask, to which a particular Hodge theory may be lifted, classically? It's two more than the dimensions of $\mathbb{R}, \mathbb{C}, \mathbb{H}, \mathbb{O}$ because those dimensions are the signatures of the Lorentzian metric on that size of space.

What group acts on the cohomology of a K ahler manifold? $SL(2) \sim SO(3)$. So for the six case we have $SO(5)$. In the octonionic case we have $SO(9)$ on what I had better not call the cohomology of an octonionic manifold. But, even on the harmonic forms you have it. The $SO(2)$ action is the degree of a harmonic form. I'm missing something on the K ahler case having to do with spinors in four dimensions.

Now, if you ask quantum mechanically, in the octonionic case, there are three answers. It's four for quatum mechanical gauge theory, an exotic conformal field theory where it's six, and with string it's ten. We only need the gauge theory level for the rest of this talk. For three we could go to Σ models. If there's an n-dimensional version of the Langlands program it won't be found by this method because this will only go to ten.

So, exchanging G and ${}^LG$ is nice, but what in the story of electric-magnetic duality is closer to the Langlands program? I'm going to describe the duality between electric and magnetic charges. This story goes back to around 1979 or 1980. The first part on the electric side is in any dimension, but then we'll have fun in four dimensions. I'm writing it as M_4, and we're doing gauge theory. We have S a one-manifold in M_4. We take the holonomy of the connection around the loop S, taking its trace in a representation R of G, that is, $Tr_R\ Hol(A, S)$ or as physicists write it $w_R(s) = Tr_R Pexp \int_S AS\mu dx^\mu$. So this is $\int(\mathscr{D}A \ldots)exp(I) \cdot w_R(s)$. Physicists had to calculate the average values of an expectation value for a large loop in space. In the theory of strong interactions people did lots of computer evaluations. This decays exponentially with the area of the loop.

That's the Wilson loop operator. If there's duality it will turn the Wilson loop operator into something else. Now, really in four dimensions, we'll do something else, something of a very different kind. This was an order operator, a quantization of something classical. A disorder operator is giving a recipe which should be followed. Classically it looks totally different from the order case but quantum mechanically you get something with the same properties. In the order case we didn't carry out the same path integral, we added an extra factor. Here you carry out a path integral but change the space of fields. Before A was a connection on a G bundle over M_4. Here it's a connection on a G-bundle over $(M_4 - S)$ and it has a singularity along S. I want to talk about what kind of singularities this might have in codimension three. I'm going to draw a three manifold with a bad point with a singularity, take a normal slice to S. It will be a conical singularity. By this I mean near the singular point the gauge field will be invariant under scaling. So the local behavior of A near p is invariant under scaling. That means the gauge field will be a pullback from an S^2. In fact it's a pullback of a solution of the Yang Mills equations on S^2.

The Yang Mills equations are complicated in higher dimensions but in two dimensions they can be solved simply. If $G = U(1)$ then $F = dA$ is the curvature. The Yang Mills equations say $d(*F) = 0$ so $*F$ is constant, and it is given by the Chern class. So take a basic solution, I'll call it A_0, which is the connection on a line bundle over S^2 whose first Chern class is 1. Then you can multiply by an integer n, that is take the nth power of the line bundle. For any G the solutions of Yang Mills on S^2 are $\rho(A_0)$ where $\rho : U(1) \to G$. So in one direction this is trivial. It's slightly harder but still simple to find such a ρ.

I'm going a little too slow.

[No.]

Okay, so the Wilson operators are classified by R a representation of G, which is determined by a highest weight $w : T \to U(1)$. We only care about ρ up to trace, so this is like caring only up to $\rho : U(1) \to T$.

Then the Langlands interchanges Wilson and 'tHooft, which is this disorder side but can't be written down by a formula. For the Wilson you do the path integral over the space of connections with an extra parameter.

You do the integral over the space of connections with a singularity and you describe what happens on the singularity.

Dualities in many cases, even in Abelian versions or the Ising model, replace operators with formulas with ones with recipes, order operators with disorder operators.

The Wilson operators are obvious classical objects. The 'tHooft operators are less obvious but turn into the geometric Hecke operators of the geometric Langlands program.

I'm going to have to cut a lot of corners. Say we have a Riemann surface C. Imagine things are independent of time. So our three manifold is $C \times I$. So C is the Riemann surface on which we're doing the Langlands program and I is an interval. Now A is a connection on a bundle $E \to W$. We can restrict to $C_y = C \times \{y\}$ and then we get a bundle $E_y \to C_y$. E_y has an automatic holomorphic structure with $\bar{D}$ given by $d\bar{Z}(\frac{\partial}{\partial z} + A^{(y)}\bar{z})$. This gives a one real dimensional family of holomorphic C-bundles $E_y \to C$. What if you get a bad point, where you have a 'tHooft operator? Well, you'll jump by what a geometric Langlands person would call a Hecke operator of type ρ.

I will say one thing that is more precise. I said this as if you had any family of connections. I could narrow this down by asking the connection to obey some equations. Some natural ones in four dimensions are the instanton equations, and in three dimensions there are close cousins, $F = *D\phi$, the Bogmolny equation. Then you find out that E_y is constant away from singularities. It's locally constant, jumps at the 'tHooft operations.

If we omitted the projections of the bad point to C we would not see this, if we omit the bad point from the Riemann surface. That's why the 'tHooft is constant on the complement of a point in C. That's how they set it up in geometric Langlands.

I should give you a hint of how to go from all of this to the Langlands program. First you have to twist it to make it a topological field theory. $N = 2$ super Yang Mills in dimension four has one twist and gives Donaldson theory. $N = 4$ super Yang Mills has three twists and two are similar to Donaldson. the other twist is not, and gives geometric Langlands. This is

1995 (Marcus). As for how, the details are too long to explain. Take M_4 to be $\Sigma \times C$, where C is where we do geometric Langlands, and make a reduction to an effective two dimensional world.

Here you get a two dimensional theory which is a *sigma* model with target $M_H(G, C)$ the moduli space of Higgs G-bundles over C. The electric magnetic duality turns into mirror symmetry. You show that the 0-brane is an electric eigenbrane and so the dual brane corresponds to [unintelligible]sheaves.

[Is the twisted geometric procedure, could you say something about what it is?]

If it's $\mathbb{R}^4$, you have $SO(4)$ acting, and you take a diagonal embedding of $SO(4)$ into the product of the two groups, and you can generalize to an arbitrary four manifold. You get three choices, one of which is with the geometric Langlands program.

[What are the other two?]

They have to do with counting solutions to PDEs, they're qualitatively similar to Donaldson invariants and Seiberg-Witten invariants say they contain the same information but there are other interesting properties.

[If the third twist leads to four-manifold invariants?]

I would guess they wouldn't be very interestiong even if you could do it?

[[unintelligible]]

For the moment it's generated new things to do with boundary conformal field theory, rather than new ways to construct them.