TOPOLOGY IN AUSTRALIA AND SOUTH KOREA 2018

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1. April 22: Kathryn Hess: Creating and exploiting model categories I

I'd like to thank the organizers and professor Oh. This is my first time in Korea and I've been here for approximately 15 hours. I hope that between jet lag and an allergy pill, this goes okay. I'll start with an introduction. We have some high level homotopy theorists here but also people who do low dimensional topology so I want to not assume people know too much.

I want to start, then, with an introduction, what model categories are, why we care about them, and so on. Then about creation of them, tips and tricks for showing that they exist and so on. Then I want to talk about exploiting model categories in all kinds of contexts, in topology, in algebra, in algebraic geometry. If this subject intrigues you, I'd be happy to provide references afterwards, Dwyer and Spalinski, a wonderful book by Emily Riehl, and so on.

I will assume that you know something about categories. I'll assume some very basic category theory. So $\mathcal{C}$ will be a category with object class $\operatorname{Ob}\mathcal{C}$ and morphisms $\operatorname{Mor}\mathcal{C}$. Then given c and d in $\operatorname{Ob}\mathcal{C}$, I will write $\mathcal{C}(c,d)$ for the set of morphisms from c to d.

1.1. **Section 0: Motivation.**

So I'll start with section 0, which is why we care about this at all. So I'll start with the classical homotopy theory of topological spaces. If I start with two continuous maps f and g in $\operatorname{Top}(X,Y)$, then I say that $f \simeq g$, that f is homotopic to g, if, well, there are two options. You could write these two as $X \coprod X \xrightarrow{f+g} Y$, and if I look at the inclusion of $X \coprod X \to X \times I$, where I is the interval, that there exists an extension making the diagram commute:

$$\begin{tikzcd}
X \coprod X \arrow[rr, "f+g"] \arrow[dr, "i"'] & & Y \\
& X \times I \arrow[ur, "\exists H"'] &
\end{tikzcd}$$

but I could also use the path object, $\operatorname{Map}(I,Y)$ and ask for existence of a lifting

$$\begin{tikzcd}
X \arrow[rr, "{(f,g)}"] \arrow[dr, "\exists K"] & & Y \times Y \\
& PY \arrow[ur, "{p=(\mathrm{ev}_0,\mathrm{ev}_1)}"'] &
\end{tikzcd}$$

Let me make some observations about this. The maps $i_0 : X \to X \times I$ which takes x to $(x,0)$ and the similar map i_1 are *Hurewicz cofibrations.* That is, we say

$j : A \to Z$ is a Hurewicz cofibration if there always exists a lift H in any diagram of the following sort (homotopy extension):

$$\begin{tikzcd} A \arrow[r] \arrow[d, "j"'] & PY \arrow[d, "\mathrm{ev}_0"] \\ Z \arrow[r] \arrow[ur, dotted, "\exists H"] & Y \end{tikzcd}$$

Or dually, ev_0 and ev_1 are Hurewicz fibrations: we say that $p : E \to B$ is a Hurewicz fibration if there is a lifting in any diagram of the following sort (homotopy lifting):

$$\begin{tikzcd} X \arrow[r] \arrow[d, "i_0"'] & E \arrow[d, "p"] \\ X \times I \arrow[r] \arrow[ur, dotted, "\exists K"] & B \end{tikzcd}$$

Note that we have a factorization

$$\begin{tikzcd} X \coprod X \arrow[rr] \arrow[dr, "\text{homotopy equivalence}"] & & X \\ & X \times I \arrow[ur, "\text{Hurewicz cofibration}"] & \end{tikzcd}$$

and dually

$$\begin{tikzcd} X \arrow[rr] \arrow[dr, "\text{Hurewicz fibration}"] & & X \times X \\ & PX \arrow[ur, "\text{homotopy equivalence}"] & \end{tikzcd}$$

I want to mention a theorem of Strøm. Let me use $\twoheadrightarrow$ to denote a Hurewicz fibration and $\rightarrowtail$ to denote a Hurewicz cofibration. So Strøm noted that you can factor any math into a cofibration followed by a homotopy equivalence that is a fibration or a homotopy equivalence which is a cofibration followed by a fibration.

$$\begin{tikzcd} & \bullet \arrow[dr, two heads, "\cong"] & \\ X \arrow[ur, tail, "\cong"] \arrow[rr, "f"] \arrow[dr, tail] & & Y \\ & \bullet \arrow[ur, two heads] & \end{tikzcd}$$

It turns out that the Hurewicz cofibrations have the left lifting property with respect to *all* Hurewicz fibrations that are homotopy equivalences, not just these path ones. Dually, the Hurewicz fibrations satisfy the right lifting property with respect to all Hurewicz cofibrations that are homotopy equivalences. So you start with things that lift against a very small class of maps and then show that they lift against a much larger class.

Let me show how similar notions arise in a different context. To keep things simple, let's specialize to chain complexes (unbounded) over a field $\mathbb{K}$. I'll call that $\mathrm{Ch}_{\mathbb{K}}$. If I have f and g in $\mathrm{Ch}_{\mathbb{K}}(C, C')$, then $f \cong g$, they are "chain homotopic", if I have some maps $\{h_n : C_n \to C'_{n+1}\}$ for $n \in \mathbb{Z}$ such that $d_{n+1}h_n + h_{n-1}d_n = f_n - g_n$ for all n.

That's the usual definition. One reason it's not entirely satisfactory is that this is not actually a map of chain complexes. How do you put this in the world of chain complexes? You could take the direct sum $C \oplus C$ and get a map $f + g$ to C', and factor this through $C \otimes I$ (let me tell you what I is in a second) and then ask for a factorization

$$\begin{tikzcd}
C \oplus C \arrow[rr] \arrow[dr] & & C' \\
& C \otimes I \arrow[ur, "\exists H"] &
\end{tikzcd}$$

Here I_n is $\mathbb{K} \cdot a \oplus \mathbb{K} \cdot b$ in $n = 0$, is $\mathbb{K} \cdot t$ in degree $n = 1$, and is otherwise 0. It's an exercise to see that a chain map as described there is the same as a chain homotopy.

You can also do a path version, so you ask for K:

$$\begin{tikzcd}
C \arrow[rr, "{(f,g)}"] \arrow[dr, "\exists K"'] & & C' \times C' \\
& \mathrm{Hom}(I, C') \arrow[ur] &
\end{tikzcd}$$

Let's make some observations. The analogues of Hurewicz cofibrations (respectively Hurewicz fibrations) are degreewise injective (degreewise surjective) chain maps. The analogues of all the other observations in the topological context hold. For example, not to belabor the point, but, for all f we have two factorizations

$$\begin{tikzcd}
& \bullet \arrow[dr, two heads, "\cong"] & \\
X \arrow[ur, tail, "\cong"] \arrow[rr, "f"] \arrow[dr, tail] & & Y \\
& \bullet \arrow[ur, two heads] &
\end{tikzcd}$$

via chain versions of the mapping cylinder and mapping path space.

This all works in this algebraic context, all of the constructions are similar. So you can ask what is the bigger picture, what is the common context, the common framework, for both of these, and can it be generalized or formulated to apply to chain complexes with structure, differential graded algebra or commutative algebra or Lie algebra or whatever, so for example if you are interested in differential graded categories, operads in chain complexes, or to algebraic varieties or manifolds, et cetera.

So I'd like to tell you about the framework of model categories, one of the frameworks that lets people talk about homotopy theory in a wide variety of contexts using the same machinery, that really generalizes the classical picture that I just reminded you of.

1.2. **Model categories.** My career is long enough that when I was a graduate student, I had to explain to homotopy theorists what model categories were. Now you'll hear from Dominic and maybe Aaron that these are sort of old-fashioned. I think there is still room for both.

The goal is to define a homotopy-like equivalence relation on morphisms in a category. We'll need extra structure on the category to get the extra structure, and we'll use that to "invert" the homotopy equivalences, and if we have two categories with this kind of structure we'll compare them.

So I need to introduce some categorical notions. The first one is one that came up implicitly, the left and right lifting properties. Let $\mathcal{C}$ be a category and S a class of morphisms $S \subset \operatorname{Mor}\mathcal{C}$, and then $\mathrm{LLP}(S)$ is the set of morphisms where for every commuting diagram like this, there is a commuting lift:

$$\begin{tikzcd} \bullet \arrow[r] \arrow[d, "f"'] & \bullet \arrow[d, "s\in S"] \\ \bullet \arrow[r] \arrow[ur, dotted, "\exists"] & \bullet \end{tikzcd}$$

Similarly, we say $RLP(S)$ is the set of f such that there is always a lift as follows:

$$\begin{tikzcd} \bullet \arrow[r] \arrow[d, "s\in S"'] & \bullet \arrow[d, "f"] \\ \bullet \arrow[r] \arrow[ur, dotted, "\exists"] & \bullet \end{tikzcd}$$

Definition 1.1. Let $\mathcal{C}$ be a category. A *weak factorization system* in $\mathcal{C}$ consists of a pair of classes $(\mathcal{L}, \mathcal{R})$ of morphisms, so that every f in $\mathcal{C}$ can be factored

$$\begin{tikzcd} \bullet \arrow[rr, "\forall f\in \operatorname{Mor}\mathcal{C}"] \arrow[dr, "\exists \ell\in\mathcal{L}"'] & & \bullet \\ & \bullet \arrow[ur, "\exists r\in\mathcal{R}"'] & \end{tikzcd}$$

and moreover $\mathcal{L} = LLP(\mathcal{R})$ and $\mathcal{R} = RLP(\mathcal{L})$.

This is what we saw in the topological case, with the Hurewicz fibrations and cofibrations.

Now we can define what a model category is.

Definition 1.2. A *model category* consists of a category $\mathcal{M}$ that is bicomplete (has all limits and colimits)—sometimes you don't need all limits and colimits, but let's take the strong definition for now, along with three classes of distinguished morphisms $(\mathcal{W}, \mathcal{C}, \mathcal{F})$, the weak equivalences, cofibrations, and fibrations, satisfying the following axioms.

(1) The class of weak equivalences satisfies "2 out of 3", meaning that if I have two composable morphisms f and g, and two of f, g and the composite gf are weak equivalences, then so is the third.
(2) We have two weak factorization systems $(\mathcal{C}, \mathcal{F}\cap\mathcal{W})$ and $(\mathcal{C}\cap\mathcal{W}, \mathcal{F})$.

If you look at the classical article by Dwyer and Spalinski, you'll see unfoldings of what all of these things mean. This is a compact description, due to Joyal and Tierney, I believe.

Remark 1.1.
- The classes $\mathrm{LLP}(S)$ and $\mathrm{RLP}(S)$ are closed under retracts, which implies that $\mathcal{F}$, $\mathcal{C}$, $\mathcal{F}\cap\mathcal{W}$, and $\mathcal{C}\cap\mathcal{W}$ are closed under retracts.
- In general $\mathrm{LLP}(S)$ is closed under pushouts along morphisms in the underlying category, whence $\mathcal{C}$ and $\mathcal{C}\cap\mathcal{W}$. There is a dual result for RLP that works for $\mathcal{F}$ and $\mathcal{F}\cap\mathcal{W}$.
- The classes $\mathrm{RLP}(S)$ and $\mathrm{LLP}(S)$ contain isomorphisms.

A bit of terminology and notation. I'll use the same kind of arrows as before, so that $\rightarrowtail\in\mathcal{C}$ and $\twoheadrightarrow\in\mathcal{F}$, and $\xrightarrow{\sim}$ for weak equivalences. I'll call a morphism that is a weak equivalence and a cofibration an *acyclic cofibration* and similarly for fibrations.

Given X in M, if the unique map from the initial object $\varnothing \to X$ is a cofibration, then X is called *cofibrant* and similarly if the unique map from $X \to e$ (the terminal object) is a fibration, then X is fibrant.

One thing that will be important to compute is the fibrant and cofibrant objects in your category.

One more thing. A model category is a category with certain structure, so $(\mathcal{W}, \mathcal{C}, \mathcal{F})$ is a *model structure* on $\mathcal{M}$.

It's an exercise to see that any two of the classes here determine the third. It's easy to see that $\mathcal{W}$ and $\mathcal{C}$ determine $\mathcal{F}$, and that $\mathcal{W}$ and $\mathcal{F}$ determine $\mathcal{C}$. What's a little more difficult is that $\mathcal{C}$ and $\mathcal{F}$ determine $\mathcal{W}$.

This overdetermination gives you a lot of control.

There are variations, fibration categories, cofibration categories, when your structure is less rich.

We had a model structure on topological spaces with homotopy equivalences and the Hurewicz fibrations and cofibrations.

You could put other model structures on topological spaces, you could put a structure where the weak equivalences are weak homotopy equivalences, and use Serre fibrations and cofibrations, where these are maps with a lifting property against cylinders on spheres and respectively retracts of cell attachments. This is more commonly used.

I advertised this as a way to get a homotopy relation. So what I want to do is try to get a definition analogous to what we had in topological spaces and chain complexes. So now I'll fix $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$.

We can do this two ways. Let me do it with cylinders. Let X be an object of $\mathcal{M}$. A cylinder is a factorization of the fold map $X \coprod X \to X$ into a map j followed by a weak equivalence p. If $j \in \mathcal{C}$ we call this a *good* cylinder. If in addition $p \in \mathcal{F}$ we call this a *very good* cylinder.

We can define homotopy now in terms of cylinders. We say that f and g are *left homotopic* if and only if there is a cylinder on X and a morphism from $\mathrm{Cyl}(X) \to Y$ such that

$$\begin{tikzcd} X \coprod X \arrow[rr, "f+g"] \arrow[dr, "j"'] & & Y \\ & \mathrm{Cyl}(X) \arrow[ur, "H"'] & \end{tikzcd}$$

commutes. This left homotopy is *good* or *very good* if the cylinder is good or very good. This is just following our noses and doing an abstract analogue of what we did in topological spaces.

A couple of properties. We'd like this relation to be well-behaved.

- If f and g are left homotopic, then there exists a good left homotopy from f to g, and it's even very good if Y is fibrant. This is not a very hard exercise.
- The second property is that, it's not hard to see that this relation is reflexive and symmetric. If X is cofibrant, then left homotopy is an equivalence relation on $\mathcal{M}(X, Y)$ for all Y.

This allows us to talk about the set of left homotopy classes. If X is cofibrant, we write $\pi^\ell(X, Y) = \mathcal{M}(X, Y)/\sim^\ell$.

Remark 1.2. It turns out for example, that if I have a cofibrant object X and an acyclic cofibration $p : E \to B$, then postcomposition induces an isomorphism on left homotopy classes $\pi^\ell(X, E) \to \pi^\ell(X, B)$ which takes $[f]$ to $[pf]$.

There is a dual situation in terms of path objects. If I start with an object Y, then a *path object* on Y is an object PY factoring $Y \to Y \times Y$ into a weak equivalence $j : Y \to PY$ followed by a map $p : PY \to Y \times Y$. Just as in the other context, if $p \in \mathcal{F}$ we call this good, and if $p \in \mathcal{F}$ and $j \in \mathcal{C}$ we call it very good. Then we can define right homotopy as the existence of a path object on Y along with a map H fitting into

$$\begin{tikzcd} X \arrow[rr, "{(f,g)}"] \arrow[dr, "H"'] & & Y \times Y \\ & PY \arrow[ur, "p"'] & \end{tikzcd}$$

Dually, if Y is fibrant then $\sim^r$ is an equivalence relation, which gives a notion of *right homotopy classes* of morphisms $\pi^r(X,Y) = \mathcal{M}(X,Y)/\sim^r$.

Moving back to the topological context, when X is cofibrant and Y is fibrant then everything agrees.

Proposition 1.1. *If X is cofibrant and Y is fibrant then the notions of right and left homotopy are exactly the same.*

This is reassuring. You want to think that X is projective and Y is injective or that X is a cell complex and Y is anything. So when X is cofibrant and Y is fibrant, then we will write $\sim$ for both $\sim^\ell$ and $\sim^r$, for the homotopy classes, either left or right, for maps from X to Y. So at least when X and Y are nice enough you get homotopy classes.

Just to conclude I'll give one example of this, an abstract version of the Whitehead lemma.

Let X and Y be both cofibrant and fibrant. These are particularly nice objects in a model category. Then $f : X \to Y$ is a weak equivalence if and only if it's a homotopy equivalence, i.e., there is a homotopy inverse $g \in \mathcal{M}(Y,X)$ such that $gf \sim \mathrm{Id}_X$ and $fg \sim \mathrm{Id}_Y$.

So being a weak equivalence is the same as being a homotopy equivalence for these fibrant and cofibrant objects. I'll talk about inverting them next time and comparing them. Then we'll start talking about creating them.

2. April 24: Kathryn Hess II

Two line reminder of where we were yesterday. I defined the notion of a model category, consisting of a bicomplete category $\mathcal{M}$ along with three classes satisfying certain axioms. This led to two distinct notions of homotopy between morphisms in the category, left and right homotopy, using cylinders and path objects. At the end I mentioned that looking at bifibrant objects you end up with an equivalence relation. If f and g go from X to Y where X is cofibrant and Y is fibrant, then $f \sim_\ell g$ if and only if $f \sim_r g$, and so we get a unique equivalence relation when we are looking at morphisms from a cofibrant to a fibrant.

So now what I want to do is use the structure of a model category and the homotopy that we have to formally invert all the weak equivalences. I want to create a category where these are all isomorphisms. I really want to invert them.

The goal now is to invert the class of weak equivalences formally in $\mathcal{M}$, forming some sort of localization of the category $\mathcal{M}$ at $\mathcal{W}$. The notation will be either $\mathrm{Ho}\,\mathcal{M}$ or $\mathcal{M}[\mathcal{W}^{-1}]$. This is a generalization of localization of a ring. This is the *homotopy category* of $\mathcal{M}$. I need to demonstrate existence.

I want to show that this depends only on the class of weak equivalences. But it will *use* the cofibrations and the fibrations.

We'll use what are called fibrant and cofibrant replacements. If I have an arbitrary object, I need to talk about taking that object and replacing it with an object which is weakly equivalent but of the right type. You can think of these replacements as something like injective and projective resolutions of modules.

How do we get these? The idea is the following. I start with an object X in $\mathcal{M}$ and look at the unique morphism $\varnothing \to X$, and one of my factorizations gives me $\varnothing \rightarrowtail X^c \twoheadrightarrow X$ with the second map a weak equivalence. So this gives me X^c which is equivalent to X and cofibrant. On the other hand I can factor $X \to e$, the unique map, into $X \rightarrowtail X^f \twoheadrightarrow e$ where the first map is a weak equivalence. So the first of these X^c is a cofibrant replacement, and X^f is a fibrant replacement.

Sometimes one asks for these to be functorial, and I might not do that. But even without functoriality, given $g \in \mathcal{M}(X,Y)$, I can consider the following. I want a map from X^c to Y^c.

$$\begin{tikzcd} \varnothing \arrow[rr] \arrow[d] & & Y^c \arrow[d] \\ X^c \arrow[r] \arrow[urr, dotted, "\exists g^c"] & X \arrow[r, "g"] & Y \end{tikzcd}$$

because $\varnothing \to X^c$ is a cofibration and $Y^c \to Y$ is an acyclic fibration. So this might not be unique but I have it. It's unique "enough" i.e. up to homotopy in an appropriate sense.

Similarly, I have a lift

$$\begin{tikzcd} X \arrow[r, "g"] \arrow[d] & Y \arrow[r] & Y^f \arrow[d] \\ X^f \arrow[rr] \arrow[urr, dotted, "\exists g^f"] & & e \end{tikzcd}$$

Some properties, g^c is a weak equivalence if and only if g is if and only if g^f is, by two out of three.

Also, they "behave well" with respect to left and right homotopy and with respect to composition (up to homotopy). I don't want to make these statements precise.

This says we can take a morphism between objects and replace it with a morphism between cofibrant objects or between fibrant objects, or we can do first one and then the other replacement.

Now I'll give one possible construction of this homotopy category. What will I do? I'll have the objects be the objects of $\mathcal{M}$. I'll say that the morphisms $\operatorname{Ho}\mathcal{M}(X,Y)$ are $\pi(X^{cf}, Y^{cf})$. Note that if X is already cofibrant, then the composite $X \to X^f$ will still be a cofibration. So X^{cf} will be both cofibrant and fibrant (and the same for Y).

This is well defined, composition works well, composition in this category, if I have a homotopy class of g and a homotopy class of h, then I compose them to the homotopy class of $h \circ g$. The properties that I didn't spell out exactly tell us that this is well-defined, it's nice and associative, and the identity is the class of the identity of the fibrant cofibrant replacement.

You'd like a way to compare this to the original category. There is a *comparison functor* $\gamma : \mathcal{M} \to \operatorname{Ho}\mathcal{M}$, where $\gamma(X) = X$ and $\gamma(g) = [\gamma^{cf}]$. More explicitly, what am I doing here?

$$\begin{tikzcd} X^{cf} \arrow[r, "g^c f"] & Y^{cf} \\ X^c \arrow[u, "\sim"] \arrow[d, "\sim"] \arrow[r, "g^c"] & Y^c \arrow[u, "\sim"] \arrow[d, "\sim"] \\ X \arrow[r, "g"] & Y \end{tikzcd}$$

What are the properties of γ that show it is a localization?

First note that $\gamma(\mathcal{W}) \subset \operatorname{Iso}\operatorname{Ho}\mathcal{M}$. This is not hard once you see where these maps come from. The Whitehead lemma says a weak equivalence between X^{cf} and Y^{cf} is a homotopy equivalence and becomes an isomorphism in the homotopy category.

What is the universal property in this context? If I have a functor $F : \mathcal{M} \to \mathcal{D}$ sending $\mathcal{W}$ to isomorphisms in $\mathcal{D}$, then there should be a unique extension $\hat{F}$ of F over the homotopy category to make the diagram commute: $\hat{F}\gamma = F$.

It's a little work to show that, but it's not ridiculously difficult. That's the sense in which this category is actually a localization.

The reason it's useful to depend on the cofibrations and fibrations is that it lets you get around the normal way you might want to think to do this, which is to write down chains of maps and formal inverses. You don't know if you have a set of morphisms like that. This lets you get around that without going through a formal process of inversion.

I talked about model categories and getting a homotopy category, we want to compare two of these now. I'll have some sort of functor between the categories, and I'd ask what kind of properties this should satisfy in order to preserve these homotopy categories.

I won't go into too much detail, but will only give some basic compatibilities so I can get to creating model structures.

Theorem 2.1. *Let $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ and $(\mathcal{M}', \mathcal{W}', \mathcal{C}', \mathcal{F}')$ be model categories. I'll start with an* adjunction *$F : \mathcal{M} \to \mathcal{M}'$ and $\mathcal{M} \leftarrow \mathcal{M}' : G$ be an adjoint pair. I want to give conditions on when this induces an adjunction of homotopy categories.*

If F preserves cofibrations and G preserves fibrations, then there is an induced adjunction on the level of homotopy categories, then there is an induced adjunction $\mathbb{L}F : \operatorname{Ho}\mathcal{M} \leftrightarrows \operatorname{Ho}\mathcal{M}'\mathbb{R}G$, the total left derived functor *of F and* total right derived functor *of G.*

Let me say a word about what these do on objects, $\mathbb{L}F(X) = F(X^c)$ and $\mathbb{R}G(Y) = G(Y^f)$. These are examples of left and right derived functors. If you have a functor sending weak equivalences to weak equivalences, then $\gamma' \circ F(\mathcal{W}) \subset \operatorname{Iso}(\operatorname{Ho}\mathcal{M}')$ where $\gamma' : \mathcal{M}' \to \operatorname{Ho}\mathcal{M}'$ and we know $\mathbb{L}F = \hat{F}$. Similarly for G.

We're interested in when we have two different categories which will have equivalent homotopy categories.

So if in addition, for every cofibrant object X of $\mathcal{M}$ and every fibrant object Y in $\mathcal{M}'$, if you consider a map in $\mathcal{M}'$ from $F(X)$ to Y, that map is a weak equivalence if and only if the transpose is also a weak equivalence. So I'm saying $g : F(X) \to Y$ is a weak equivalence if and only if $g^{\#} : X \to G(Y)$ is a weak equivalence in $\mathcal{M}$.

Under these hypotheses, the adjunction $\mathbb{L}F \dashv \mathbb{R}G$ is an equivalence of categories. So you get an equivalence of homotopy theories.

Because we have an adjunction, there are actually alternative formulations that can be useful for doing this kind of verification.

Remark 2.1. Since $F \dashv G$ and we have the lifting conditions, the first condition (and this is a nice little exercise), this is equivalent to F preserving both cofibrations and acyclic cofibrations, and this is equivalent to the dual condition on the other side, that G preserves fibrations and acyclic fibrations.

This is something you can get your hands dirty on.

Let me give one example. Let R be a commutative ring. I am going to consider the category of chain complexes over R this time, unbounded chain complexes, this is a slightly more delicate situation, there are various model categories one can consider. I'll use the model structure where weak equivalences are quasi-isomorphisms (there are also ones where the weak equivalences are chain homotopy equivalences), $\mathcal{F}$ will be degreewise surjective and $\mathcal{C}$ degreewise injective maps with degreewise projective cokernel. [sic]

This is not a terribly difficult thing to prove, that this gives a model structure. Let's consider the following functor, $- \otimes_R M$ for M a left R-module. This has a right adjoint, and for N a right R-module, let $N[0]$ be the chain complex with N in degree 0. I evaluate and get

$$H_i(\mathbb{L}(- \otimes_R M)(N[0])) = \mathrm{Tor}_i^R(M, N).$$

I can't stop without mentioning one of my favorite examples, $|\ | : \mathrm{sSet} \leftrightarrows \mathrm{Top} : S_*$ gives an equivalence of homotopy categories. If I have a pair $F \dashv G$ which satisfies the condition in the first part of the theorem, I call it a *Quillen pair*, an adjunction which induces one at the level of homotopy categories. If it satisfies the second condition as well, it's a Quillen equivalence.

A lot of what goes on here is the search for Quillen pairs that are actually Quillen equivalences.

Are there any more questions on this particular chapter before I go on to creation of model structures?

2.1. **Creation of model structions.** I hope I've convinced you that these are interesting to study. If so you'll be excited to see more. For motivation, for example, if you care about differential graded things, then you might be interested not just in chain complexes, but in algebras or modules over them, or coalgebras. I have Ch_R, and I could be interested in dg algebras over R. I have a free forgetful adjunction. I have an adjunction, can I bring the model structure from chains to algebras. For coalgebras, you have a cofree functor which turns out to be right adjoint to the forgetful functor, so if you want to use this adjunction, you have a different adjunction you could work with. So you could say, what about differential graded Hopf algebras? There it's not even directly related to chain complexes, you have to pass through first algebras (or coalgebras) and then to Hopf algebras by an adjunction with the opposite handedness.

You have two ways of doing that, can you say something about the two different ways, if it's possible to get model category structures going both ways around, will it give the same thing?

Fix an algebra, a differential graded algebra and look at modules or a differential graded coalgebra and look at comodules over it. Then you have an adjunction with chain complexes over R and A-modules, tensoring with A or forgetting, and you could ask about whether you can move the model structure along that adjunction. Dually, you could look at comodules, where forgetful is the left adjoint and tensoring is the right adjoint.

This is related to the next point. If you think about homotopy descent theory, which is what motivated me to think about this kind of creation, restrict to chain complexes to keep it simple, but given a morphism of differential graded algebras, you could consider the construcution, looking at $B \otimes_A B$, which is a B-bimodule. This has a comultiplication, a diagonal or coproduct, in the world of B-bimodules, and this thing is isomorphic to $B \otimes_A A \otimes_A B$, and then I can use φ to get to $B \otimes_A B \otimes_A B$ and then this is isomorphic to $(B \otimes_A B) \otimes_B (B \otimes_A B)$. This seems kind of stupid but it gives a coproduct, this is a B*-coring*.

But this plays an essential role in descent theory. We have an adjunction between A-modules and B-modules, where these are $-\otimes_A B$ and φ^*, forgetting. You compare [missed] to the category of *descent data*, $\mathrm{Desc}(\varphi)$, where here we have, well, you have a canonical comparison functor, which is the category of B modules together with a coaction of this B-coring.

So this is the category of $(B \otimes_A B)$-comodules in Mod_B. Let me spell this out a little more explicitly. You have M (a B-module), and $\rho : M \to M \otimes_B (B \otimes_A B)$, satisfying some sort of coassociativity and counitality. If you look at the standard definition of descent data, this is an action of this coring. The descent data takes N to $N \otimes_A B$, along with the canonical action ρ_N which takes

$$N \otimes_A B \cong N \otimes_A A \otimes_A B \to N \otimes_A B \otimes_A B \cong (N \otimes_A B) \otimes_B (B \otimes_A B)$$

and the descent theory problem asks when this is equivalent to the category of modules. You can ask when this gives an equivalence of the respective homotopy categories if you have a model structure on both sides.

The standard (classical) descent question asks when the canonical comparison functor is an equivalence of categories, and the homotopical version asks, if Mod_A and $\mathrm{Desc}(\varphi)$ are model categories, you can ask when this is part of a Quillen equivalence.

It's a very natural way of loosening up the approach to descent. This can be moved far beyond chain complexes to other ground categories, but we will always need a comodule context.

Okay, and before I finish today, I want to give one more motivational example. This is when you are thinking about categorise of diagrams in a model category. You might want a nice homotopy invariant way to compute limits and colimits in the model category. Then you need a nice model structure on the category of diagrams in a model category so that you can come up with good definitions. If I have $\mathcal{D}$ a small category and $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ a model category, I want to put a model category structure on functors from $\mathcal{D}$ to $\mathcal{M}$. Let $\mathcal{D}_0$ be the discrete category underlying $\mathcal{D}$. The objects are the same and I only have identity morphisms. In that case, the functors give me a product of mode structures component by component. This is easy. Then let ι denote the inclusion of $\mathcal{D}_0$ to $\mathcal{D}$. Then $\mathcal{M}^{\mathcal{D}_0}$ has both left and right adjoints via Kan extension. You can ask whether either of these adjunctions gives a model structure on $\mathcal{M}^{\mathcal{D}}$.

3. April 25: Joan Licata: Front projection via Morse theory and monodromy

The question I want to talk about is a small part of a big question. If you start with an understanding of something in $\mathbb{R}^3$, do you understand it in other three-manifolds? The part of this I want to address is about Legendrian knot theory. It's convenient for me to follow Youngjin's talk yesterday. I can treat it as motivation. If you start with a Legendrian knot in a three-manifold, how do you study it? You have a nice projection in $\mathbb{R}^3$ that lets you do some nice algebraic invariants, so I'll talk some about front projections and then use that to give me an angle on other three-manifolds.

So in $\mathbb{R}^3$ we have a one-form $dz - ydx$, and we're not interested in the form so much but rather its kernel, and together these two things make up a *contact manifold*. So what's the kernel of this one-form? It's a 2-plane field, but it's a *non-integrable* 2-plane field. It's not the tangent plane of any surface. Unlike for a vector field, where you can always do this locally, you can't always do this, and in this case this is completely non-integrable, there is no small open set where this is possible. So whenever we have such a thing we call it a contact structure.

Inside a manifold with a contact structure you can find a knot theory that is compatible. We're interested in the case where the curve is tangent to the plane at every point. So K is Legendrian inside a contact manifold if TK is always in the plane field, and that's another way of saying that $\alpha(TK) = 0$.

When you have a Legendrian knot you immediately get a special projection that makes it easy to study. What we saw yesterday is that when you take the projection to the xz plane, you get a "front projection" and the curves you get are smooth except for isolated cusps, and when you look at $\alpha(TK) = 0$, we find out that we can recover the y coordinate exactly from the projection, from the dz/dx slope. This is great. The question I have, if you're studying knots in a 3-manifold, I want a projection that has the same property, I want to be able to recover the knot exactly.

So this is our motivating question, the question of front projections in an arbitrary contact M^3.

In some settings there is a more general notion of front projection. There are a whole family of 3-manifolds that are jet spaces. This works in jet spaces in odd dimensions in general. I want something in arbitrary contact manifolds. There are reasons that such things shouldn't exist, because there are non-jet space contact manifolds. But there are also reasons that such things should exist because things like this work for S^3 which is not a jet space. So this is joint with Gay, with Mathews, and some with Durst-[unintelligible].

So I want to start with a description of a way to construct arbitrary three-manifolds, through an open book construction. A beautiful theorem says that when you construct a 3-manifold by this construction you are getting a contact manifold. I want to add a little bit of extra data, introducing Morse structures, and then we get nice structures, front projections. This is a broad audience so I want to draw a lot of pictures and so on, if you want details talk to me after.

So now I'll sneak in some Lagrangian geometry. I want to give a model of knot projection as hitting a fly with a flyswatter. You push it with a flyswatter until it hits a wall and then it becomes flat. I can decompose $\mathbb{R}^3$ as the disjoint union of $z = c$ for $c \in \mathbb{R}$. If we look at these slices we get $d\alpha|_{z=c}$ which is a symplectic form.

There is a part of this which is not unique, but $y\partial_y$ is a Liouville vector field for $d\alpha|_{z=c}$ and $d(\omega(v,\cdot)) = \omega$, this is a Liouville vector field.

When I think about my front projection, instead of thinking of it as an abstract plane, I want to think of it as a constant. So $y\partial_y$ is every transverse to $y = c$. We have some, so front projection is the image of my knot under this flow to the plane $y = c$. What this is supposed to hint at is that if you have the right kind of flow, you get a nice projection.

An open book is transverse to a 3-manifold. An open book is, I'll start with a surface, I have a surface Σ^2 and no boundary. I have a homemomorphism ϕ from Σ^2 to itself which is the identity on the boundary. So I'll take this crossed with the interval. I have a manifold with boundary, I have this map that lets me glue the bottom to the top by identifying $(x,1)$ with $(\phi(x),0)$, now I've take the boundary component and crossed it with I. So I have a toroidal boundary component. If I change my t parameter, I move around the outside of the tube [pictures] and to get a closed 3-manifold I collapse this to a single curve, and I get a closed 3-manifold. Now I get a collection of closed curves which is the binding, and the rest of this is the pages.

We say a contact form α is supported by this open book structure if we have two properties. We want $\alpha(TB) > 0$ (this is oriented), compatibility with the binding, and $d\alpha|_{\Sigma_t}$ is a symplectic form, an area form.

What prevents a plane field from being integrable is twisting. Your contact planes twist when you go along any curve in the y direction. You can't have tangent planes to the pages, [missed some], and what the open book does is gather all the twisting into a neighborhood of the binding.

Let's put in an important theorem.

Theorem 3.1 (Thurston–Winkelnkempen, Giroux)**.** *If you have an open book, you can always find a compatible contact form.*

We have the *Giroux correspondence* which we don't need today that lets us pass back and forth between contact manifolds and open books.

Anyway I want to move to the unit sphere in $\mathbb{R}^4$, which is $\{(r_1,\theta_1,r_2,\theta_2) : r_1^2 + r_2^2 = 1\}$. If r_1 is zero, you get a circle, and then you get a family of tori, and then when you get to $r_1 = 1$, you go back to a circle component. So if you just take a half-interval then you get a solid torus. So $\{0 \le r_i \le \frac{1}{\sqrt{2}}\}$ is a solid torus.

This is a Heegaard splitting, and what we'd like to do is choose a standard contact form. So α is $r_1^2 d\theta_1 + (1 - r_1^2)d\theta_2$ and the contact structure, we can understand it very concretely in terms of these [unintelligible]. So these contact planes meet the core curves transversally. If you stop at a fixed torus, then your $d\theta_1$-$d\theta_2$ slope will be constant. As you move from one of these curves to the other curve inside S^3, I can picture these as embedded as the Hopf link, and as I move along this radial line, we can see that $\frac{d\theta_2}{d\theta_1}$ of $\ker\alpha$ varies with r_1, and this sholud remind you of what we've already seen.

If I drow the Hopf link like this, you can see a surface bounded by these two curves [picture] and an open book for this manifold is given as follows. Start with an annulus, cross it with the interval, and glue the top to the bottom, and the gluing map will be a positive Dehn twist, where we wrap once around in the right-handed direction. If we apply this map and do this collapsing operation, we get these annuli which are the annuli which give the pages in the decomposition.

If we have a Legendrian curve in this manifold, it already gives us a hint as to what we can do. So you can push it away from the binding, and then we can project it to the torus, $r_1 = \frac{1}{\sqrt{2}}$, let me cut this torus open for you, this is our $r_1 = 1/\sqrt{2}$-torus, and we get something that looks very much like the pictures we've already seen. [pictures].

Here you'll see slopes bounded between 0 and $-\infty$. This was exploited by Oszvath–Szabo and Thurston to give some Heegaard–Floer homology. Getting back to the flyswatter model, I want to say that you're going along vector field parallel to [unintelligible].

Theorem 3.2 (Gay–L.). *Every contact three-manifold is a compactification of solid tori each contactomorphic to* $\{0 \leq r_1 < \frac{1}{\sqrt{2}}\}$.

If you are willing to glue together along a more complicated map than a homeomorphism, you can get any contact 3-manifold.

As a consequence of the theorem, we get the front projections we wanted. If we identify the front projections, for each of my solid tori, I get a projection to a torus that acts just like what I had in $\mathbb{R}^3$.

So let me explain a little bit about where this comes from. Given a three-manifold with an open book structure, we add a little bit of extra structure, (F, V), so F is a function from M to $\mathbb{R}$, with a variety of conditions that I won't write down, going for the big picture. So F restricted to a page is Morse. So V is gradient-like for this Morse function, so I mean that it could be the gradient vector field for restriction to a page, and is also the Liouville vector field for $d\alpha|_{\Sigma_t}$. When you have an open book and move through it with this t structure, we get an evolving family of Weinstein structures on the pages. So $d\alpha|_{\Sigma_t}$ is a symplectic form, and any time you have a contact manifold presented as an open book, you can always find an F and V that realizes it in this way.

The upshot is that we get a one-parameter family of Weinstein structures, of Morse functions on the pages, with a gradient-like vector field, and so I get a one-parameter family. So getting back to S^3, I want my Morse function to have a single index 0 critical point, it should be Morse–Smale, no index 2-critical points, this is not so hard to arrange. So I can write the flowlines on the page. I can draw in the other flowlines and they aren't interesting. The other flow lines go from the index 0 point to the boundary. We care that we have a graph embedded on our surface. Let t evolve, and what could happen? I encode this with the one-parameter family of graphs. [pictures]

Define the skeleton of the manifold as the union of flow-lines from index 0 to index 1. So now what if we have a Legendrian knot in the 3-manifold.

We don't have a condition that the binding is connected. We can look at the preimage of the flow by V back in the manifold. So now $M \smallsetminus Skel$ is the disjoint union over preimages under the flow by V to B_i. So what you get is this open solid torus. In infinite time you get this skeleton, which compactifies this otherwise disjoint pieces. You want to translate this into something for front projections.

But I'll pause and tell us something about how this lets us study 3-manifolds. What I'd like to do is build a torus which is a place we'll do a front projection to. Take a curve near each boundary component, and what I end up with is two circles, and I record the intersection with the flow lines from the index 1 points to the boundary. Now we let t vary, and since the monodromy is constant at the boundary, these circles persist as t changes, and one one of the components [pictures] nothing interesting happens. So what happens is that the point moves along the curve and you get, in general, collections of tori with pictures on them, you call this a Morse diagram. We can give a combinatorial characterization of the kind of drawing that give you a Morse diagram. The proposition is that up to a reasonable notion of isotopy, the Morse diagram determines the contact manifold, so if I just draw you something like [pictures].

You have these decorated tori, and I want to think that projection is pushing a knot until you hit a surface. So these tori are the boundary of specific components. If I push my Legendrian until it lands on this surface, that's my front projection.

[pictures]

You get pictures and the slopes are between 0 and negative ∞, the curves are smooth with cusps, and this is exactly motivated by and in parallel to the usual version.

Proposition 3.1 (with Gay, Mathews, Durst, Kegel)**.** *In a manifold $M(\Sigma, \phi, F, V)$ we have front projections for Legendrian knots, tangles, that completely determine the original Legendrian,*

- *We have a Reidemeister theorem*
- *We can compute various things, $[K] \in H_1(M)$, and if it is nulhomologous, you can compute $tb(K)$ and you can get $rot(K)$*

I think I'll stop there.

4. Chang-Yeon Chough: The comparison theorem for algebraic stacks

Thanks to the organizers, to bringing me to this meeting of two different hemispheres. Before you get annoyed by the term algebraic stacks, I need to explain some background materials. Today I'll begin with something like this. I'll explain the comparison theorem for schemes first, which uses model category theory a lot.

So if you have a scheme over $\mathbb{C}$ another thing you can do is you can look at $\mathbb{C}$ points, $X(\mathbb{C})$, and you get, you have a theorem, the Riemann existence theorem, which tells us the comparison between the geometry on X which is algebraic, and this topological space, there is an equivalence of categories between finite étale coverings of my scheme X and the finite coverings of the usual topological space $X(\mathbb{C})$. When it comes to étale topology this tells you how to recover the finite coverings of this object. In terms of fundamental groups, this tells you that $\pi_1^{\text{ét}} \cong \pi_1(X(\mathbb{C}))$, and when you think of a multiplicative group scheme on the algebraic side corresponding to the circle, this is given by an exponential which is not algebraic, and so you should take the profinite completion on the right hand side. You look at the collection of these finite quotients. So this other π_1 is in the sense of [unintelligible]and Grothendieck.

We also know that the étale cohomology of X recovers the Betti cohomology of $X(\mathbb{C})$. We wonder if we can generalize this to higher data, but we don't have higher π_n to attach to a scheme. So instead we'll give a space which captures the homotopy of the scheme you start with.

So the statement is, on the one side you have this topological space $X(\mathbb{C})$ and on the other side you have some "space" associated to X, which I'll call Artin–Mazur's étale homotopy type. There is a map from $X(\mathbb{C})$ to this Artin–Mazur space. This becomes an equivalence after profinite completion. Let's name this (this is not the original notation) $\hat{h}_{AM}(X)$.

So what I'm going to do is lift this comparison theorem to algebraic stacks. If you unravel what this means, this amounts to this algebraic data giving you something on π_1, on the profinite completions, and that the homology groups, with finite coefficients, give you an isomorphism.

The goal is to extend this equivalence to algebraic stacks. Since we'll work at the level of "spaces" we'll attach some space. Let me fix a scheme X, and I'll show you how Artin and Mazur constructed an object capturing the homotopy of X. For that I'll bring it down to topological spaces.

The Cech nerve of my covering, which has $\coprod U_i$ in degree 0, has $\coprod U_i \cap U_j$ in degree 1, and these are related by maps, and this is organized into a simplicial topological space. Then you take the connected component functor to this picture, and get $\pi_0(N\mathcal{U})$, this is functorial and at each level you get a set, so this is a simplicial set. Then you take the geometric realization to get a topological space.

Theorem 4.1. *(Borsuk) Let X be a paracompact topological space. Let's say I'm given $\mathcal{U} = \{U_i \to X\}$, a good cover of X where good means that a finite intersection of U_i is empty or contractible (you can't always do this). Then $X \cong |\pi_0(N\mathcal{U})|$.*

From a computational point of view this might not be easy to understand, but we can approximate this space with something combinatorial.

Now we leave topological spaces for algebraic geometry and try to mimic this for my scheme. Every scheme will be locally Noetherian and every morphism locally of finite type. So replace the space with a scheme. You need a covering, of course you use an étale cover. You try to get an object, you want something like going from étale coverings of X to simplicial sets, something like $\mathcal{U} \mapsto \pi_0(N\mathcal{U})$.

From now on I will be a little vague about the difference between topological spaces and simplicial sets. So hopefully this captures the homotopy of X, but this fails, you can make whatever you want, but this should work, for example, you will try to check the cohomology attached to this gadget, and when you compute the cohomology, this is the Cech cohomology of your scheme. This, then, fails. Even at the level of cohomology, we want sheaf cohomology, not Cech cohomology, and they don't always coincide.

The failure is that when you look at this formation of $\pi_0(N\mathcal{U})$, this object is somehow rigid in the following sense. What happens in degree zero determines all the higher behavior of your simplicial scheme. So your cover might be nice, but the intersections might not be nice. An étale cover, the U_i are good, but the intersections might not be. So you need to fix the failure on the intersections, and one way to do that, you start with the same object, but to fix the failure on the intersections, think about the unit circle and the covering by two sets.

So you refine this cover, and so get an étale cover of the étale cover. And you do this in a compatible manner, replacing the nerve with a new thing. So this is organized with hypercovers. You roughly understand it as some kind of covering which rectifies the failures on these intersections.

A hypercover will be a simplicial scheme satisfying certain properties. So you replace this with hypercoverings, and take π_0, and this is where we are using the locally Noetherian assumption.

You do this thing and for some technical reason you should mod out by simplicial homotopies between hypercoverings, $HR(X)$ is the category of hypercoverings of

X modulo simplicial homotopies. If you do that, this index category becomes cofiltered, this is a theorem in Artin–Mazur, but you pay something, you mod out up to homotopy, and the price is that the target is the homotopy category of simplicial sets. And this one works. This is the so-called Artin–Mazur étale homotopy type of the scheme X. Remember space means simplicial set, this is not really a space but a collection of spaces.

As always, you want to check cohomology first. If you compute cohomology of this gadget,

$$\operatorname*{colim}_{V \in HR(X)} H^n(\pi_0(V), A)$$

is isomorphic to the étale cohomology of the scheme X, $H^n_{\text{ét}}(X, \underline{A})$.

This is the Verdier hypercovering theorem.

So Artin–Mazur's étale homotopy type, it has the right cohomology. The π_1 is really not a group but a pro-group, and the higher π_n will be a pro-Abelian group. The profinite completion will recover the étale fundamental group of the scheme.

I understand this functor as a space which recovers well-known algebraic invariants, this functor attached to my scheme.

So let's come back to the goal of the talk, which is to extend this equivalence to algebraic stacks.

You first need to attach a space to algebraic stacks.

Of course the very first thing you're going to try is to mimic Artin–Mazur's idea. There are a couple of issues. The first one is that it's heavily dependent on the small étale topology, if you start with a Deligne–Mumford stack, there's no problem, but for algebraic stacks this small étale topology is obsolete. The other thing, this object is in the pro category of the homotopy category of simplicial sets. Instead of this, if you want to do homotopy theory, you'd like to land in the homotopy category of something, maybe the homotopy category of pro-simplicial sets, to land in the derived category of a certain object.

After this seminal work, Friedlander developed a kind of étale homotopy theory for schemes and simplicial schemes. He used the notion of rigid hypercoverings to lift this object to pro-simplicial sets, and lifted the theory to simplicial schemes, around 1982. But not for algebraic stacks. For them, I'll used a modern reformulation of the Artin–Mazur theory due to Bornea and Tomer Schlank. Here is a 21st century thing. Say T is $X_{\acute{e}t}$, the category of small étale sheaves on X. Thanks to the locally Noetherian assumption, you can take $T \xrightarrow{\pi_0} \text{Set}$, and if your scheme is represented this is the usual thing.

Then you enlarge the category to the category of simplicial objects on each site, and this functor extends, and enlarge these categories further into pro-categories

$$\operatorname{Pro}(T^{\Delta^{\text{op}}}) \xrightarrow{\pi_0} \operatorname{Pro}(\text{Sset})$$

The upshot is that there are model category structures on both of these in such a way that this π_0 becomes left Quillen. They start with some sort of category of fibrant objects, actually something a little stronger, a weak fibration category, and you start with weak equivalences and fibrations, but your category with those two classes cannot be extended to a model category, you get a model category on the pro-category.

The reason why we have to go to the pro-category is, there are so many things to say, I told you you are going to start with the weak equivalences and fibrations, I'll choose the local ones, so when you pull back to stalks you get normal weak equivalences and Kan fibrations, and I'll say it's a local weak equivalence (fibration) if it is so on every stalk, at least if you have enough points.

Look at a certain simplicial sheaf F, there is a unique map to the final object, a local weak equivalence and local fibration simultaneously, this generalizes hypercoverings as used before.

If you take the local weak equivalences and local fibrations as your choices for the model category structure, it won't work. There's a counterexample, with a finite group, using BG for a profinite group which is not finite. You cannot take these two classes as part of your model category structure. These are enough to get you the structure you want on the pro-category.

The upshot is that $\mathbb{L}\pi_0(*)$ (where $*$ is the final object, the scheme X), this recovers the Artin–Mazur étale homotopy type.

This is more than a reformulation of Artin–Mazur's theory. All the technical things and the magic are in the model structure. When you take the cofibrant replacement of this final object, the hypercovering is encoded there.

That's the case for schemes. Now you want to do it for stacks, and here comes my part. Let me fix a base scheme X, well, maybe I don't have enough time. They started with the small étale topos of this scheme X. You could use a different topology, like the big étale topos. You can still use the machinery of Bornea–Schlank to develop a homotopy theory, and generalize this to simplicial schemes and simplicial spaces. I'll try to explain that part in the proof of the theorem.

Let's say $\mathcal{X}$ is an algebraic stack. Recall what we are trying to do, establish a comparison theorem for algebraic stacks. I'll use their machinery. You apply the machinery to the big étale topos on the stack $\mathcal{X}$. There's the big étale topology, the objects are [unintelligible]certain scheme to $\mathcal{X}$ and the topology is the usual topology on schemes, and you look at the associated topos, and that's T.

If you use the big étale topology, you can attach a certain topology to your stack to get a certain topos, and apply the same procedure. This one still works and the connected component functor, you derive this and you'll get something. You'll get an object related to the stack $\mathcal{X}$ and define this to be the homotopy type of the stack $\mathcal{X}$.

In this case I define the homotopy type of $\mathcal{X}$ to be this derived object $\mathcal{L}\pi_0(*)$, with respect to this topos. So the question is whether this is the right answer.

There are many nice properties, even at the level of topoi. For simplicial objects in a topos you already have some homotopy theory, developed in my paper.

So so far what we've done is, given an algebraic stack we've attached a space, and now we go into a goal, which is to establish the comparison theorem for algebraic stacks, and I'll show you why this object is reasonable, or is on the right track.

Here's your étale homotopy type $h(\mathcal{X})$ in that sense, and you compare that to $h(\mathcal{X}^{\top})$, the homotopy type of the topological stack. You can get a stack by [unintelligible], and there's a map from $h(\mathcal{X}^{\top}) \to h(\mathcal{X})$, and the claim is that this is an equivalence after profinite completion. There is a category of profinite spaces and a model category structure in the sense of Quick, and I'll say that this map induces a weak equivalence of profinite spaces, and you try to reduce this question to that of schemes. Say you choose a smooth hypercover of the stack $\mathcal{X}$, just like you do cohomological descent for your stack $\mathcal{X}$. That kind of thing is okay, you can apply that kind of idea to simplicial objects, and on the left side you had, say $h(\mathcal{U}(\mathcal{C}))$, and for simplicity you could imagine you had a simplicial scheme, and so you get an equivalence, and a map at the level of schemes, and you reduce the question from the bottom to the top level

$$\begin{tikzcd} h(U(\mathcal{C})) \arrow[r] \arrow[d, "\sim"] & h(U) \arrow[d, "\sim"] \\ h(\mathcal{X}^\top) \arrow[r] & h(\mathcal{X}). \end{tikzcd}$$

and how do you deal with this? These should be hocolims of the pieces in each degree. These exist thanks to the way I developed the theory. This is simplicial descent so you reduce further to schemes.

$$\begin{tikzcd} \operatorname{hocolim} h(U_n(\mathcal{C})) \arrow[r] \arrow[d, "\sim"] & \operatorname{hocolim}(h(U_n)) \arrow[d, "\sim"] & \\ h(U(\mathcal{C})) \arrow[r] \arrow[d, "\sim"] & h(U) \arrow[d, "\sim"] & \\ h(\mathcal{X}^\top) \arrow[r] & {} & h(\mathcal{X}). \end{tikzcd}$$

and you want the equivalence up to profinite completion. But you need to interchange the profinite completion and the hocolim.

$$\begin{tikzcd} \operatorname{hocolim} \hat{h}(U_n(\mathcal{C})) \arrow[r, "\text{Artin–Mazur}\sim"] \arrow[d] & \operatorname{hocolim}(\hat{h}(U_n)) \arrow[d] \\ \operatorname{hocolim} h(U_n(\mathcal{C}))^\wedge \arrow[r] \arrow[d] & \operatorname{hocolim}(h(U_n))^\wedge \arrow[d] \\ \hat{h}(U(\mathcal{C})) \arrow[r] \arrow[d, "\sim"] & \hat{h}(U) \arrow[d, "\sim"] \\ \hat{h}(\mathcal{X}^\top) \arrow[r] & \hat{h}(\mathcal{X}). \end{tikzcd}$$

In my other paper I proved that this profinite completion admits a right adjoint, so this homotopy colimit in this simplicial model category can be described as colimits, and then left adjoints commute with colimits and you get the isomorphism of the top vertical maps.

Or the way that this is left adjoint to something can be stated at the level of model category structures, and these commutativities are interchange of the left derived functor of completion and the homotopy colimit. That's a general result in model category theory.

These model category statements and methods are hidden in all of these vertical equivalences, which let us reduce this equivalence at the level of stacks, which might be nasty, to the level where it's not so hard to understand where we have an answer from Artin–Mazur.

In the remaining three minutes I'll give an example, otherwise people will be upset. So for example, think about the multiplicative group scheme $\mathbb{G}_m$ and its classifying stack. This is algebraic but not Deligne–Mumford. So you try to compute $h(\mathbb{B}\mathbb{G}_m)$, and roughly you look at $\mathcal{G}_m(\mathbb{C})$, and this is S^1 up to homotopy, and so this is the classifying space of that, so this should be $K(\mathbb{Z}, 2)$ up to finite completion. So if you compute $H^*(\mathcal{B}\mathbb{G}_m, \mathbb{Q}_\ell)$, this should be $H^*(BS^1, \mathbb{Q}_\ell)$ and this is the polynomial ring $\mathbb{Q}_\ell[c]$ where c is the universal Chern class in degree 2. Both sides I think are well-known to topologists, algebraic geometers, whoever.

Okay, I'll stop here.

5. April 26: Kathryn Hess III

This is joint with Kędziorek, Riehl, and Shipley, and we have sort of described a common way of creating model sturctures through an adjunction on either side. I want to talk about a necessary condition and show that the necessary condition is very often sufficient.

I made a mistake about model categories for chain complexs over a commutative ring. The cofibrations are not degreewise injections with projective cokernel but are contained in that class.

Okay, let's start with some terminology. Let $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ be a model category, and let $\mathcal{N}$ and $\mathcal{K}$ be bicomplete categories. I'll suppose that I have adjunctions $F : \mathcal{M} \to \mathcal{N}$ (the left adjoint to U) and $G : \mathcal{M} \to \mathcal{K}$ (right adjoint to V). A model structure $(\mathcal{W}_\mathcal{N}, \mathcal{C}_\mathcal{N}, \mathcal{F}_\mathcal{N})$ is *right-induced* by $F \dashv U$ if $\mathcal{W}_\mathcal{N} = U^{-1}(\mathcal{W})$ and $\mathcal{F}_\mathcal{N} = U^{-1}(\mathcal{F})$.

We say that a model structure $(\mathcal{W}_\mathcal{K}, \mathcal{C}_\mathcal{K}, \mathcal{F}_\mathcal{K})$ is *left-induced* by $V \vdash G$ if $\mathcal{W}_\mathcal{K} = V^{-1}(\mathcal{W})$ and $\mathcal{C}_\mathcal{K} = V^{-1}(\mathcal{C})$.

Thanks to results of Kan from way back, there are well-known conditions on which we have the right-induced structure in the case when $\mathcal{M}$ is "cofibrantly generated." But nothing is fibrantly generated so you don't really have the dual thing.

I want to say that the simplicial set structure is left induced from topological spaces either from the Hurewicz or the Serre structure.

Let me note a consequence. If the right induced structure on $\mathcal{N}$ exists, then we know that its acyclic fibrations are the things in the preimage of the acyclic fibrations of $\mathcal{M}$, $U^{-1}(\mathcal{F} \cap \mathcal{W})$. Oops, that's not what I wanted to say, true but irrelevant. The acyclic cofibrations have the left lifting property with respect to $\mathcal{F}_\mathcal{N}$ which is $U^{-1}(\mathcal{F}_\mathcal{M})$. In particular it follows that things with the left lifting property with respect ot $U^{-1}\mathcal{F}$ are necessarily in $U^{-1}\mathcal{W}$.

Dually, if the left-induced structure on $\mathcal{K}$ exists, then the maps with the right-lifting property with respect to the new cofibrations are necessarily in the preimage of the weak equivalences.

We call these the *acyclicity conditions.* These are a necessary consequence of the existence of these structures.

My goal right now is to tell you under what conditions these are not only necessary but sufficient to conclude the existence of these model structures.

One remark I want to make before saying the theorem that answers this question. If you have the right or left induced structures, then the adjunctions you started with become Quillen pairs. With respect to the right induced structure on $\mathcal{N}$, the pair $F \dashv U$ is a Quillen pair which then induces an adjunction on the homotopy categories. We've defined things so that U preserves fibrations and acyclic fibrations.

Similarly with respect to the left-induced structure on $\mathcal{K}$, the other adjunction $V \dashv G$ is a Quillen pair.

Okay, so what does the acyclicity theorem say?

Theorem 5.1 (HKRS, 2017)**.** *Assume the model category is* accessible *(I'll say this in a minute, but many of our favorite categories are accessible, so this isn't a strong restriction). So* $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ *is an accessible model category and let* $\mathcal{N}$ *and* $\mathcal{K}$ *be* locally presentable categories*. Locally presentable means locally small, bicomplete, and with a set of small objects which generate all the objects under filtered colimits of a maximal size.*

Consider adjunctions $F \dashv U$ *and* $V \dashv G$*. If the acyclicity conditions are satisfied, then the desired model categories exist.*

(1) *If* $LLP(U^{-1}(\mathcal{F})) \subset U^{-1}(\mathcal{W})$ *then the right induced structure on* $\mathcal{N}$ *exists.*
(2) *If* $RLP(V^{-1}(\mathcal{C})) \subset V^{-1}(\mathcal{W})$ *then the left induced model structure on* $\mathcal{K}$ *exists.*

Proving the existence of the necessary weak factorization systems is the hard part. Once you've done that the rest is, well, not an exercise, but not particularly difficult. It's hard work that was done originally by Bourke and Garner, and Emily realized what we did in the article is not quite right, and so Garner with Kędziorek and Riehl fixed this in a preprint on the arxiv.

Proposition 5.1. *Suppose* $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ *is a model category and let* $\mathcal{N}$ *and* $\mathcal{K}$ *be bicomplete categories, with the same adjunctions* $F \dashv U$ *and* $V \dashv G$ *as before, satisfying the acyclicity conditions.*

If you have the weak factorization systems, then you're in business. So,

$$(LLP(U^{-1}(\mathcal{F} \cap \mathcal{W})), U^{-1}(\mathcal{F} \cap \mathcal{W})) \text{ and } (LLP(U^{-1}(\mathcal{F})), \mathcal{F});$$

if these are weak factorization systems, then the right induced model structure exists.

One of these is usually for free and the other is usually pretty darn hard.

For the other side, if you have weak factorizations

$$(V^{-1}(\mathcal{C} \cap \mathcal{W}), RLP(V^{-1}(\mathcal{C} \cap \mathcal{W})) \text{ and } (V^{-1}(\mathcal{C}), RLP(V^{-1}(\mathcal{C}));$$

then the left-induced structure on $\mathcal{K}$ *exists.*

Remark 5.1. The model category $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ is *accesible* if $\mathcal{M}$ is locally presentable and (here's where accesibility comes in) the weak factorization systems are functorial, naturally in the morphism you're factoring, via functors that are themselves accessible (I need to say what that means). You want to preserve λ-filtered colimits for some regular cardinal λ.

It's a reasonable sort of property. For some examples for those familiar with the model category literature, this contains all the examples we're used to except topological spaces. This includes any locally presentable (enriched) cofibrantly generated model category. So all combinatorial model categories are accessible.

One thing I didn't mention when I stated the theorem, these structures are also accessible, so you can iterate the process.

Remark 5.2. The model structures obtained by the acyclicity theorem are accessible. So you can iterate this process.

This turns out to be something that one does. You use one adjunction to get a model structure, and then you can do this again et cetera, and lead things along this way.

In order to show the existence of these model structures, you have what look like these simple kinds of conditions to chck. The apparent simplicity is only *apparent*, so I want to give a couple of tools for establishing acyclicity.

The first one is a generalization of (a dual of) Quillen's path object argument. This is one particular criterion given by Quillen maybe fifty years ago to prove the existence of model structures.

Since we're principally interested in left induction, we formulate it in that context but it can be easily dualized. Let me call this the *cylinder object argument*.

Suppose we have $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ a model category, and some adjunction $V \dashv G$ to $\mathcal{K}$. If the following conditions hold then we get an acyclicity condition:

(1) For every X in the objects of $\mathcal{K}$, I want a map $\epsilon_X : QX \to X$ such that $V(QX)$ is cofibrant in $\mathcal{M}$ and $V(\epsilon_X)$ is a weak equivalence. If I were trying to get a model structure, then this would be a cofibrant replacement. So this is "some sort of" cofibrant replacement. I don't ask about functoriality.

(2) For every $f \in \mathcal{K}(X, Y)$, there exists some $Qf \in \mathcal{K}(QX, QY)$ such that the diagram

$$\begin{tikzcd} QX \arrow[r, "Qf"] \arrow[d, "\epsilon_X"] & QY \arrow[d, "\epsilon_Y"] \\ X \arrow[r, "f"] & Y \end{tikzcd}$$

commutes.

(3) For every X in $\mathcal{K}$ there exists a factorization of the fold map

$$\begin{tikzcd} QX \coprod QX \arrow[rr, "\nabla"] \arrow[dr, "j"'] & & QX \\ & \mathrm{Cyl}(QX) \arrow[ur, "p"'] & \end{tikzcd}$$

Then the acylicity condition holds: $RLP(V^{-1}(\mathcal{C})) \subset V^{-1}(\mathcal{W})$.

If we're in the situation where $\mathcal{M}$ is accessible, $\mathcal{K}$ is locally presentable, then we have the factorization system and the model structure exists.

Corollary 5.1. *Under the conditions above, if $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ is accessible and $\mathcal{K}$ locally presentable, then the left-induced structure exists.*

There's one more tool that we have that is also quite useful, related to bialgebras and Hopf algebras, where you have two ways to get there from chain complexes. There is a nice general framework that we call the square theorem. Again, the proof of the cylinder object argument is something I'd be happy to show somebody during the lunch break. The proof is not very hard.

Theorem 5.2 (The square theorem)**.** *Let a square of adjunctions be given.*

$$\begin{tikzcd} \mathcal{M} \arrow[d, "F"] & \mathcal{K} \arrow[l, "V"'] \arrow[d, "F'"] \\ \mathcal{N} & \mathcal{P} \arrow[l, "V'"'] \end{tikzcd}$$

[ed.: only left adjoints pictured for my ease in TeX]

Suppose all categories are locally presentable, that $(\mathcal{M}, \mathcal{W}, \mathcal{C}, \mathcal{F})$ is accessible, that there is a right-induced structure on $\mathcal{N}$, a left-induced structure on $\mathcal{K}$, and we have some compatibility:

$$VU' = UV' \text{ and } FV = V'F',$$

then there exist right and left induced model structures on $\mathcal{P}$ and moreover these two are Quillen equivalent via the identity functor $\mathcal{P}_{right} \leftrightarrows \mathcal{P}_{left}$.

Note that they have the same weak equivalences. They are in the preimage of VU' or UV' but these composites are the same.

Sometimes they are the same and sometimes they are different.

I have thirteen minutes. I'll take audience requests. I can give some applications to how we create them in various situations. There are three things I can talk about in twelve minutes. I can talk about model structures on categories of diagrams. I can talk about examples related to chain complexes, and the third thing I could talk about is simplicial presheaves, finding a framework for homotopical Galois theory for motivic spaces and spectra.

[informal poll]

One should always listen to students. So I'll say something, a brief statement about diagram categories and then something about this last version.

So for any small category $\mathcal{D}$ and any accessible model category $(\mathcal{M}, \mathcal{W}, \mathcal{F}, \mathcal{C})$, we have adjunctions comparing $\mathcal{M}^{\mathcal{D}_0}$ and $\mathcal{M}^{\mathcal{D}}$, and we can think of ι^* as a left adjoint to right Kan extension or right adjoint to left Kan extension. These give you the injective and projective model structures. The acyclicity conditions are not hard to show in this case.

This is a well-known result for the projective structure for cofibrantly generated, it's due to Lurie for the injective case at least in certain cases, and we've pushed the boundaries of this in some cases.

So one more remark, if this $\mathcal{D}$ is what's called a *Reedy category*, then you could start in $\mathcal{M}$ as the discrete, and then put in the positive and negative structures as your $\mathcal{K}$ and $\mathcal{N}$, and then the induced structures on $\mathcal{P}$ coincide and are the Reedy structure.

Okay, presheaf categories. I said this was motivated by a desire to talk about motivic Galois theory. This part of the work is joint with Beaudry–Hess–Kędziorek–Merling–Stojanoska. This was great, some of us brought more experience with model categories and some of us with computations.

For $\mathcal{C}$ a small category, you want not the injective structure but some sort of left Bousfield localization of this injective structure on simplicial presheaves. The sort of question we had to answer was this. Since we were interested in Galois theory, we wanted both homotopy orbits and homotopy fixed points of certain actions. We fixed G a group, and got from the group a pair of adjunctions

$$\begin{tikzcd} \mathrm{sPre}(\mathcal{C})_{\mathrm{inj}} \arrow[r, "\mathrm{Triv}"] & G-\mathrm{sPre}(\mathcal{C})_{??} \arrow[l, bend right, "(-)_G"'] \arrow[l, bend left, "(-)^G"] \end{tikzcd}$$

and it turns out that you have two different model structures, one the proper one for homotopy orbits and the other appropriate for homotopy fixed points.

You homotopy orbits will be $(-)_{hG} = \mathcal{L}(-)_G$ and your homotopy fixed points is $(-)^{hG} = \mathbb{R}(-)^G$.

We get these model structures from the right and left induction. Actually, not from right and left induction from this, but another pair of adjunctions, you can also cross a presheaf with G, and you have a forgetful functor, using another adjunction

$U : G - \mathrm{sPre}(\mathcal{C}) \to \mathrm{sPre}(\mathcal{C})$ which has both adjoints $G \times -$ and $\mathrm{Hom}(G, -)$. These turn out to be the ones that give the things we want.

So if you've looked at the paper, you see that there are many other variations there, algebras and so on, and some of those use the square theorem.

6. Jae Choon Cha: Whitney towers in a rational homology 4-ball

Thank you to the organizers to arrange this very nice conference.

Usually the speaker says thanks for inviting me, but I'm from just across the hall. I appreciate the efforts of the organizers bringing the people from opposite side of the world, I'm glad that local people that you are visiting here and enjoying your stay here at Postech and in Pohang.

Today I'll talk about low dimensional topology, mostly in dimension 4. I'm thinking about disk embedding in a 4-manifold X. After the recent advances in dimension 3, this is the least understood question. The high dimensional approach uses disk embeddings, and the failure of this is what makes 4-manifolds hard. We have modern developments from gauge theory and Floer theory for smooth cases, and these are wonderful to detect difference, but it's hard to imagine that they will give us classifications. If we can get disk embeddings, then we can hope to get a classification, like Freedman. So understanding this failure precisely is more or less the same as classifying 4-manifolds. I'm thinking of a link L, a disjoint union of circles, in the boundary of X, and this is called *slice* in X if this link bounds a disjoint union of disks in X itself.

To avoid local issues we usually want to extend this to a tubular neighborhood, so regard this as $D^2 \times \{0\}$ and then we want to embed $D^2 \times D^2$ in this way. This is different in the smooth and topological case; one is automatic but the other is an extra condition.

Usually I'll think of $\partial X = S^3$, so for instance if $X = D^4$, then link slicing corresponds to a solution to "topological surgery." It means that the high dimensional surgery techniques work in dimension 4 if and only if a certain specific family of links is slice in some nice way. If you have a topological surgery technique, you can find embedded disks like this.

So I'd say that understanding these disks is at least morally equivalent to understand 4-manifolds.

The fundamental technology is tower constructions. What does that mean? Let me start with a Casson tower. What he did was, I have the boundary, and I have a link here, and we want to find a disjointly embedded disk, but if it's not possible, let's find immersed disks. Then why don't we find, for each double point, we get some other link, and then maybe find a secondary disk. So we can find a solution if we can find a second stage disk, and that might also be immersed, and you try to proceed like this, if my "yellow loop" is not nullhomotopic then that's important.

The really wonderful theorem of Freedman says that if you have a Casson tower of height 6 (the meaning of height must be clear) then this gives us an embedded disk bounded by the same boundary in X. This was the key technology for the classification of simply connected 4-manifolds in the topological category.

If you have height six, then you can raise the height as much as possible and get an infinite height thing, which is a *Casson handle*.

Another technique uses a *grope*, so if you have an embedded surface, not just a disk. If I can find a secondary disk along a cutting circle, then I could turn this surface into a disk. In general the second stage might be a surface and then you could proceed further. [pictures]

This is much more useful in dealing with non-simply connected cases, and the best, the strongest known theorem, for the current status let me mention some results

Theorem 6.1 (Cha–Powell)**.** *A Casson tower of height four is sufficient.*

Theorem 6.2. *A grope of height* 1.5 *with properly immersed caps gives embedded disks in general if* $\pi_1(X)$ *is "good".*

What does it mean? On one side height one and the other height zero. A cap is immersed at the top stage and they may intersect each other but not the body below. There's some question of whether all groups are "good" or not. But I won't talk about that.

So another thing is Whitney towers, and this is related to gropes and to some classical techniques. Let me draw some pictures. [Pictures]

So in a famous paper of Cochran–Orr–Teichner in 2003, they gave obstructions for a Whitney tower when they are symmetric. So Conant–Scheiderman–Teichner developed the asymmetric theory, where the higher stage can intersect the lower stage. I want to remark that thinking of Whitney towers is almost the same as thinking of gropes. [pictures]

So what I want to do is study links bounding asymmetric Whitney towers in rational homology 4-balls. I want to define the number of stages in an explicit way, because the shorter it is, the closer to embedded.

So let me define this precisely.

Definition 6.1. A *Whitney tower* in X bounded by $L \subset \partial X$ consists of immersed disks $T = D_1 \cup \cdots \cup D_m$ if $\cup \partial D_i = L$. These are *order zero* disks. Then if T is a Whitney tower and D is a Whitney disk pairing two intersections in T then $T \cup D$ is a Whitney tower.

Then order, an intersection point between D and D' has order $k + \ell$ if D and D' have order k and ℓ. A disk D which pairs two intersections p and q of order k has order $k + 1$.

A Whitney tower T has order n if all intersections of order less than n are paired off by disks in T. [pictures]

This notion of order measures how good of an approximation to a disk this is.

Let's fix for now a number of components of L, call it m, and let's define the following. $\mathbb{W}$ appeared a lot of times in the last lecture series. I'm using a different W, this is for Whitney. So $\bar{\mathbb{W}}$ has an ∞ which means not infinity but "twisted" and we define $\bar{\mathbb{W}}_n^\infty$ as the set of links in S^3 such that L bounds a *twisted* Whitney tower of order n in some X, a rational homology 4-ball.

In four dimensions, we have some *framing* that is very important. We get a framing of the disk uniquely and another nearby, induced, and if these are allowed to be incompatible then we call this twisted (otherwise they are framed). A tower is twisted if all disks of order less than $n/2$ are framed.

So this gives us a filtration. A slice link has a tower of arbitrary height so we get

$$\{\text{slice link}\} \subset \cdots \subset \bar{\mathbb{W}}_{n+1}^\infty \subset \bar{\mathbb{W}}_n^\infty \subset \cdots \subset \bar{\mathbb{W}}_0^\infty = \{\text{all links}\}.$$

I'd like to take successive quotients, but these are not groups. Now for L and L' in $\bar{\mathbb{W}}_n^\infty$, I can define $L \sim L'$ if $L \#_\beta - L'$ (where β is the choice of ribbon for the connect sum) is in $\bar{\mathbb{W}}^\infty_{n+1}$ for some β. Now I can define $\bar{W}^\infty_n$ as $\bar{\mathbb{W}}^\infty_n/\sim$ and can make the framed analogues $\bar{\mathcal{W}}_n$ and $\bar{W}_n$.

Now I can state a theorem.

Theorem 6.3. (1) *The set $\bar{W}^\infty_n$ is an Abelian group under connect sum, so part of this is that β was irrelevant.*

(2) *The group $\bar{W}^\infty_n$ is isomorphic to $\mathbb{Z}^{\mathcal{M}(m,n)}$ where $\mathcal{M}(m,n) = m\mathcal{R}(m,n+1) - \mathcal{R}(m,n+2)$, where $\mathcal{R}(m,n)$ is the rank of the degree n part of the free Lie algebra on m variables. A classical result of Witt says that this is $\frac{1}{n}\sum_{d|n}\phi(d)m^{\frac{n}{d}}$.*

Theorem 6.4. *$\bar{W}_n$ is an Abelian group under $\#_\beta$, and*

$$\bar{W}_n = \begin{cases} \mathbb{Z}^{\mathcal{M}(m,n)} \oplus \mathbb{Z}_2^{\mathbb{R}(m,\ell+1)} & n = 2\ell+1 \\ \mathbb{Z}^{\mathcal{M}(m,n)} & n \text{ even.} \end{cases}$$

So let me say something about the Milnor invariont. Let $\pi\pi_1(S^3 - L)$ and let π_k be $[\pi, \pi_{k-1}]$. Let F be the free group of rank m.

When λ_i (the longitude of L) is in π_{n+1}, then $\pi_{n+1}/\pi_{n+2} \cong F_{n+1}/F_{n+2} = L_{n+1}$. Now his invariant

$$\mu_n(L) = \sum_i X_i \otimes \lambda_i \in L_i \otimes L_{n+1}$$

and he wanted to understand if these things vanish, and this is the longitude in the free Lie algebra, and this is essentially what the Milnor invariant is, but it's the modern version.

So if $\mu_{n+2}(L) = 0$ then Milnor showed that $\lambda_i \in \pi_{n+2}$. This is the same as $\pi/\pi_{n+3} = F/F_{n+3}$. [ed: I think there are typos in this line but they were copied faithfully from the board.]

This is letting you improve your isomorphism along the next level of the lower central series.

So a question is, "which geometric property does μ_n detect?" So Igusa–Orr have deep work on the k-slice conjecture or theorem (since they proved it), which involves, still, the lower central series.

Theorem 6.5. *$\mu_{\le n}(L) = 0$ if and only if $L \in \bar{\mathbb{W}}^\infty_{n+1}$ i.e., $L = \partial T$ in a rational homology D^4, so T is twisted and order $n+1$.*

So one more further thing I can state, is, I discussed rational coefficients, what about other coefficients, what if $R \subset \mathbb{Q}$ is a subring containing $\frac{1}{2}$. Then L bounds a Whitney tower of order n in an R-coefficient homology D^4 if and only if L bounds a Whitney tower of the same order in a rational homology D^4. So we have a complete classification for such more general coefficients containing $\frac{1}{2}$.

So given that I stated those theorems, maybe I should discuss a little bit of the idea of the proof. Maybe I should discuss some trees. If I have a Whitney tower T, then I associate trees, uni-trivalent trees $t^\infty(T)$, I can have trivalent vertices and univalent vertices at the end. If you are familiar with quantum invariants or the Kontsevich integral, then these are like Jacobi diagrams, you have some boundary circles there, these are related to Milnor invariants, and these are extracted from [unintelligible], and in our context it can be described very explicitly. Define ∂D_i as the ith component of L, for such an order 0 disk, I get a root, and an i denoting the component number. If I have a Whitney disk, I always think of this picture. [picture]

So for this thing, t_D is defined to be the rooted tree which combines the two trees associated to the earlier stage of Whitney disk. This is the same as bracket arrangements for your Lie algebra.

From this the Milnor invariant is obtained. That in particular, if I have order n tower, then this tree gives us the Milnor invariant of order n, and [missed some argument about vanishing of up to order n].

So the algebraic side is coming from the tree, the free Abelian group generated by "order n trees" (those which are order n Whitney towers, modded out by equivalence relations IHX, AS, some others). This is an Abelian group, and in case of the standard four-space, earlier than my work, Conant–Schneiderman–Teichner extracted from these trees something like this. Start from the tree and W_n^∞ is the analog of $\bar{W}_n^\infty$ where we require B^4. Then there is a realization theorem, you can find a link realizing any tree, so you have a surjective map from our group to W_n^∞, and the Milnor invariants are obtained from tree information which give a factorization of the Milnor invariants through the tree information. They conjectured that the tree group was an isomorphism to W_n^∞, the *higher order Arf invariant* conjecture. We do not know the kernel. So in case of rational coefficients, now we have, this factorization, and this is the natural map $W_n^\infty \to \bar{W}_n^\infty$, and then I showed the key new thing, that the kernel of the map from the tree group to $L_1 \otimes L_n$ vanishes under the map to $\bar{W}_n^\infty$.

So that's brief and too fast, let me stop here.

7. April 27: Jessica Purcell: Uniqueness of plat diagrams

Thank you for the invitation to come. It has been great to renew some discussion with people in Australia and Korea. Everything today is joint with Moriah.

This is a broader topic. We'll start out with classical knot theory. The question that I'm interested in is how to classify knots. Today I will focus on one answer. Peter Guthrie-Tate was one of the first to classify knots, in the 1870s, he classified prime knots with up to seven crossings. Basically he looked for 4-valent graphs with over-under information, and he used moves that didn't have names at the time. Basically since Tate, knot theorists have been enumerating knots by crossing number.

There are some problems with enumerating by crossing number. First of all there is exponential growth when you increase crossing number by one. Another problem is that there's no natural way to organize diagrams. There's only one knot with one, three, and four, but then you have two choices for knots with five crossings. There's not a natural way to put these in a list.

It's also difficult to read other properties off of a list from crossing number. I said, Tate was classifying prime diagrams. If you start with four-valent graphs, it's hard to tell if one is prime. Are there essential meridinal annuli? Can it be reduced.

It's been about 150 years since Tate got started, a little less than that. We can only classify knots with up to 17 crossings, and only 16 crossings have been published. We're talking about millions of knots here.

This is one way to organize knots. This is fun. Ben is working on a project that does that now. But if we want to talk about knots more broadly, we might want to organize them differently. So in the 1950s, Schubert completely classified an (infinite) family of knots. These were two-bridge knots, which are also 4-plats. He actually classified links of this sort as well. This kind of knot has two bridges, a twist region, and then closes up. If you label the twist regions by the number of crossings, then you obtain a rational number, given by the continued fraction expansion, which completely determines a two-bridge knot and vice versa.

This is a very nice theorem. It also turns out that these two-bridge knots are very useful in saying things about knots. Algebraic and geometric, if you have a conjecture about knots, the first thing to do is throw the two-bridge knots at it, and that works. We know how to tell if these are hyperbolic, what the essential surfaces in the complements are, and so on.

So the idea of this talk is to follow Schubert and look at knots in terms of a plat diagram.

Let me say what that is. We'll start with the braid group on $2m-1$ generators, generated by $\sigma_1, \ldots, \sigma_{2m-1}$ where σ_1 is just a crossing in the first two strands, and so on. Then we'll look at a particular element $B = b_1 b_2 \cdots b_{n-1}$ which has the form

(1) If i is odd, then b_i is a product of towers of the even generators $b_i = \sigma_2^{a_{i,1}} \cdots a_{2m-2}^{a_{i,m-2}}$
(2) if i is even then b_i is a product of the odd generators in the same way.

[picture]

Then at the top and the bottom we connect the strands by "bridges". [picture]

Theorem 7.1 (Alexander)**.** *Every knot or link admits a plat diagram.*

The idea for this talk is, let's enumerate links by plat diagram. You pick integers for the coefficients.

The question is whether that's any better, and one question is how unique these diagrams are.

Let me give a few more terms. So m is the *width* of the diagram and n, the number of rows plus one, is the *length*. If the number of rows is even, then our last row will look like [picture], and your bridges look like this [picture]. Otherwise your last row looks like [picture] and your bridges look like [picture]. Those are the two options, and they are slightly different. The first is called a plat diagram and the other is called an *even plat*. I'll call everything a plat.

Note that a plat can be converted into an even plat and vice versa. [pictures]. So if you have lots of 0s and 1s, these are often not unique in obvious ways. That being the case, what we did was restrict to diagrams where the $a_{i,j}$ have absolute value at least 3.

Definition 7.1. A plat or even-plat is said to be c-highly twisted if $|a_{ij}| \geq c$ for all i and j, i.e., at least c crossings per twist region.

All right, so here is our theorem.

Theorem 7.2. *Let $\mathbb{K}$ be a knot or link with plat projections K' and K where*

- *K' has width $m' \geq 3$ and length $n' > 4m'(m'-2)$ and is 3-highly twisted,*
- *K has width m and is at least 1-highly twisted.*

Then $m \geq m'$ and if $m = m'$ then $K' = K$ up to obvious rotations.

A few comments.

- If you look at the restrictions here. The restriction should be $m' \geq 4$ for even plats, to be completely honest. I think I know how to fix that but I haven't written it down.

- The length requirement is an artifact of the way we prove this. It's probably not required, I'm not sure what the correct length bound is, maybe there is no restriction.
- The three-highly twisted, there's work of Wu where there is three-highly twisted things around the border, and we conjecture that 3's on the top, bottom, left, and right and then 1's in the interior suffice.

I want to make a few more comments about these types of plats. My collaborator had been looking at these from a topological and algebraic point of view and I'd been looking at them from a hyperbolic point of view.

Some nice properties of this form:

(1) there are known essential surfaces in the knot complement, due, for example, to Finkelstein–Moriah and Wu.
(2) you can read the rank (minimal number of generators) of the fundamental group off of the diagram. This is due to Lustig and Moriah.
(3) such links are hyperbolic; if c-highly twisted for $c \geq 6$ there are no exceptional fillings (Bachman–Schleimer; Futer–Purcell)
(4) There are bounds on volumes if $c \geq 7$ (Futer–Kalfagianni–Purcell).

So these are nice, this is much better than crossing number, lots of geometric information.

Now let me move to part two, which is about surfaces in plat diagrams.

Definition 7.2. An m-bridge sphere for $K \subset S^3$ is a 2-sphere in S^3 meeting K transversally in $2m$ points and cuts (S^3, K) into two trivial tangles (B_1, T_1) and (B_2, T_2), so B_i is a ball and T_i are m arcs that can be simultaneously isotoped to the boundary, fixing the boundary.

[picture]

So any horizontal line in a plat diagram gives you an m-bridge sphere. All such bridge spheres are isotopic. Schubert used this in the 1950s to define the bridge number of a knot.

Definition 7.3 (Schubert 1956)**.** The bridge number $\#b(K)$ is the minimal number m such that K has an m-bridge sphere.

Typically it's difficult to get the actual bridge number of a knot. It's not so hard to get an upper bound but it's hard to get the exact number.

We'll heavily use a theeorem of Johnson–Moriah and Tomova, which says that a 3-highly twisted plat with width m and $n > 4m(m-2)$ has bridge number m, with a unique m-bridge sphere up to isotopy.

In this plat diagram we have a 3-bridge sphere, and all the horizontal ones are isotopic to it. Maybe there's another one that is hiding here that is not the same kind. But this says the only ones there are horizontal.

Definition 7.4. A *horizontal bridge sphere* meets the plane of projection in a horizontal line.

That's one type of surface that we're going to be looking at. The other type of surface is vertical.

Definition 7.5. A *vertical 2-sphere* $S(\alpha_1, \ldots, \alpha_n)$ is constructed as follows. [picture]

You pick an arc running from the top to the bottom, with α_i twist regions to the left in row i. Each row has at least one twist region on either side of α. Then connect the ends in a closed curve through the unbounded region to get a closed curve and cap off. This is a sphere that meets the knot n times.

Theorem 7.3 (Finkelstein–Moriah; Wu, 80s–90s)**.** *Vertical 2-spheres are* essential *in odd plats.*

We have bridge spheres that are unique, the vertical spheres are essential in the odd case, and why they are useful, you can pick two that are exactly the same except they differ in one twist region. [missed something]

Proposition 7.1. *Suppose K' and K are two plat projections of the same link with the same conditions as before, $m \geq 3$, the length $n > 4m(m-2)$ for K' and K' is 3-highly twisted, while K is 1-highly twisted. Then there exists an isotopy $\phi : (S^3, K') \to (S^3, K)$ such that*

(1) *horizontal bridge spheres will go to horizontal bridge spheres.*
(2) *vertical two-spheres map to vertical two-spheres.*

Corollary 7.1. *"Isolating spheres" bounding tangles map to isolating spheres under such an isotopy. Then you have an isolating sphere that will be adjacent to another isolating sphere, and they agree and patch up together, so at the most what you could have done was rotated.*

Then the big theorem follows. I'll stop there.

8. Sang-hyun Kim: Diffeomorphism groups of critical regularity

Thank you very much for the invitation. It is my pleasure to visit IBS and this city. I've been coming here every year in the third week of April. In every year I talked about the answer to a question I raised the year before. This is about diffeomorphism groups of one-manifolds. So I want to think about $M = I$ or $M = \S^1 = \mathbb{R}/\mathbb{Z}$. My main object as a geometric group theorist is $\mathrm{Diff}_+^k(M)$, the C^k diffeomorphism group. This is the collection of C^k diffeomorphisms, these are C^k maps such that f' is positive and f is bijective. In the circle this might not be globally injective.

So this is a group. You have Diff^0 which is Homeo, and Diff^1, Diff^2, and so on, and you can talk about the intersection which we call Diff^∞. If you have a real number τ between 0 and 1, then you can define a norm, if $f : M \to \mathbb{R}$, you can define $[f]_\tau$ as $\sup_{x \neq y} \frac{|fx - fy|}{|x-y|^2}$. If τ is 1 it's Lipzchitz.

So this gives a further stratification, you can define $\mathrm{Diff}_+^{k+\tau} M$ which is $f \in \mathrm{Diff}_+^k M$ such that $[f^{(k)}]_\tau < \infty$. So then you have this very refined stratification, and you can do even more. If $\alpha : [0,\infty) \to [0,\infty)$ is a concave homeomorphism then you can define $\mathrm{Diff}^{k,\alpha}$ which uses the norm $\sup \frac{|fx-fy|}{\alpha(x-y)}$. But I'll just use this quadratic version.

So my theme is the question, which finitely generated groups arise as subgroups of $\mathrm{Diff}_+^r(M)$ for $r \in [1,\infty]$. [Some motivation that I missed] Especially people are interested in manifold groups.

You have Diff^ω, the analytic ones, inside Diff^∞, and then inside that $PSL_2(\mathbb{R})$.

A very concrete question: let $k \in \mathbb{N}$. Does there exist a finitely generated group $G_k \leq \mathrm{Diff}_+^k$ so that G fails to embed into $\mathrm{Diff}_+^{k+1} M$? Or ask this for a countable simple group.

Let me talk a little bit about the history. One of the most surprising results about this diffeomorphism group is the Mather–Thurston theorem, from the 70s.

Theorem 8.1. *The group* $\mathrm{Diff}^r_c(\mathbb{R}^n)$ *is simple for all real* $r \in [1,\infty]$ *except for* $n+1$.

The $k = 0$ case is known, also due to Thurston in 1974: Whenevr you have a nontrivial finitely generated subgroup of $\mathrm{Diff}^1 I$ then $H^1(H,\mathbb{Z}) \neq 0$ so $H \twoheadrightarrow \mathbb{Z}$. So such a thing cannot be a perfect group (trivial Abelianization).

So for example, take $G = \langle a,b,c | a^2 = b^3 = c^7 = abc\rangle$, this surjects onto the orbifold fundamental group of the $(2,3,7)$-hyperbolic orbifold. Since this is in $PSL_2\mathbb{R}$, we can lift it to the cover, and this is a subgroup of the cover. This is subgroup of the homeomorphisms of the real line. So this group G acts faithfully on the real line, and embeds into $\mathrm{Homeo}(I) \cong \mathrm{Homeo}(\mathbb{R})$. But by computation, one can check that $G/[G,G] = 1$ so $H^1(G;\mathbb{Z}) = 1$. So G does not embed into Diff^1. This is Thurston's observation.

That's an example of a topological but not C^1-smooth action.

There is an example by Calegari in 2006, which is yes for $k = 1$ and the circle. So the $k = 1$ case, we're happy.

The next question is $k = 2$, also by Thurston, the Plante–Thurston theorem, about 1976, which is that nilpotent subgroups of $\mathrm{Diff}^2(M)$ is Abelian. So in particular, the Heisenberg group $\begin{pmatrix} 1 & \mathbb{Z} & \mathbb{Z} \\ 0 & 1 & \mathbb{Z} \\ 0 & 0 & 1 \end{pmatrix}$ does not embed in Diff^2. But Farb–Franks in 2003 showed that every finitely generated torsion free nilpotent group embeds into $\mathrm{Diff}^1_+ M$. So the Heisenberg group is an example. A C^2-faithful group is not possible.

So you con replace things in Farb–Franks with residually nilpotent, so you can do right angled Artin groups, pure braid groups, and so on.

So our question reduces to $k \geq 3$. I'd like to talk about our results. Let me introduce some notation. If $r \geq 1$, then we define $\mathcal{G}^r(M)$ as the set of scountable subgroups of $\mathrm{Diff}^r_+(M)$ up to isomorphism.

So we know that $\mathcal{G}^0(M) \smallsetminus \mathcal{G}^1(M)$ and $\mathcal{G}^1(M) \smallsetminus \mathcal{G}^2(M)$ contain finitely generated groups.

The theorem is as follows

Theorem 8.2 (K.–Koberda)**.** *For all real numbers* $r \in [1,\infty)$ *each of the sets*

$$\mathcal{G}^r(M) \smallsetminus \bigcup_{s>r} \mathcal{G}^s(M)$$

and

$$\bigcap_{s<r} \mathcal{G}^s(M) \smallsetminus \mathcal{G}^r(M)$$

contain finitely generated groups and also simple groups.

Let me make some remark regarding these conditions. You might wonder why the interval $[0,1]$ is missing, there is a result of Deroin–Nanas–Rivas that for all G countable and contained in $\mathrm{Homeo}(M)$, we have G embeds into $\mathrm{Diff}^{\mathrm{Lip}}_+(M)$, so these all go to almost 1-smoothable.

You also might want to consider the difference between $\mathrm{Diff}^r I$ and $\mathrm{Diff}^r_c \mathbb{R}$ for $r \geq 1$. In the former you don't need to be tangent to the identity near the endpoints. In the latter you have this C^r tangency at the endpoints. There is an embedding from the latter to the former. What is interesting, the other direction has a monomorphism, there's a trick by Müller and independently by Tsuboi, which says that $\operatorname{Diff}_+^r I$ embeds into $\operatorname{Diff}_c^r \mathbb{R}$, you mollify the endpoints to make it flat. You think of a very flat map, that looks like $e^{-\frac{1}{x}}$ near 0, nad you map g to $\varphi^{-2} g \varphi^2$ and then this makes everything flat near the endpoints. You have to show that it has the desired regularity.

Corollary 8.1. *For every real $r \geq 1$, and every closed oriented 3-manifold M with $H_2(M, \mathbb{Z}) \neq 0$ admits a transversally oriented codimension 1 C^r-foliation which is not isomorphic to a $\bigcup_{s>r} C^s$-foliation.*

This is a very direct consequence if you are familiar with foliation theory. We have some subgroup $G \hookrightarrow \operatorname{Diff}_+^r I$ but doesn't go into any infinitesimally better diffeomorphism group. Then you get some map of $\pi_1 \Sigma_g$ onto the free group F_g^{free} and then from there to our diffeomerphism group. This determines a foliation on $\Sigma_g \times I$, and φ is not conjugate into the infinitesimally better group. This is a much weaker conclusion than the main theorem because the main theorem doesn't use conjugacy.

The second corollary is more group theoretic. You can find the critical regularity of a group, $\operatorname{Crit} \operatorname{Reg}_M G$ which is defined as

$$\sup\{r \in [1, \infty) : G \hookrightarrow \operatorname{Diff}_+^r M\}$$

Corollary 8.2. *The set of critical regularities exhausts $\{-\infty\}$ and $[1, \infty]$.*

There are outstanding questions.

1. (well-known) Does there exist a finitely generated simple subgroup of the C^0 group $\operatorname{Homeo} I$? This exists for the circle, it's called Thompson's group.
2. Does there exist a group which embeds into $\operatorname{Diff}_+^k I$ for all k but it's never C^∞ realizable.
3. This should be quite hard. Does there exist a group G_k contained in $\operatorname{Diff}_+^{k+\mathrm{Lip}} M$ such that G_k cannot be realized as $\operatorname{Diff}_+^{k+1} M$. You cannot distinguish Lipschitz and this +1 smoothability. There are famous examples from real analysis. I don't know how to group theoretically distinguish these.
4. How about $\operatorname{Diff}^r M$ and $\operatorname{Diff}^s M$ for all smooth manifolds?

My argument, many arguments are specific to one dimension.

So let's see. I'd like to talk a little bit about the proof. The first ingredient is centralizer theory. This is well-studied by dynamicists, [unintelligible], or even Smale, or [unintelligible]. The things that we need is a disjointness condition. If you have two commuting C^2 diffeomorphisms f and g, let me talk about the interval for brevity, and $U \in \pi_0 \tilde{f}$ and $V \in \pi_0 \tilde{g}$, here $\tilde{f}$ is the complement of the fixed points of f. Then they are equal or disjoint. This is a feature of C^2-diffeomorphisms. This is not true for C^1. There are Pixton–Tsuboi examples. [pictures].

This is a *non-commutativity certificate.* If the support intersects but does not coincide then you get noncommutativity.

I'd like to attack the case of regularity C^1. Then there's a similar version to that theorem, due to Bonatti–Monteverde–Navas–Rivas. Consider the group $BS(1,2) = \langle a, e | aea^{-1} = e^2 \rangle$. If $\langle c \rangle \times BS(1,2) \hookrightarrow \operatorname{Diff}_+^1 I$, then $\tilde{c} \cap \tilde{e} = \varnothing$. The idea is that this BS group can be affinely realized, $a : x \mapsto 2x$ and $: x \mapsto x + 1$. Then the idea is that the C^1 action should look like the affine action. Suppose the contrary of the conclusion, and you have your support $\tilde{c}$ and $\tilde{e}$, then the BS group acts on these intervals like the affine action. [pictures].

This is a very brief explanation of the idea. The second idea is about covering distance. The difficulty for us to prove this theorem is the s part. You can just concretely construct a group. And you want to show that an arbitrary map to Diff^s is not injective. There are so few structures that we can employ. In particular, suppose you have a generator v, it might have lots of support in the intervals. So $\tilde{\psi}_v$ might have lots of accumulation points. We want a connection between topology and group theory. We can measure some kind of energy. We consider an open set $\mathcal{U}$ which is the union of the connected components $\pi_0\tilde{\psi}_v$. Then you have some kind of distance defined topologically. Say $d_\psi(x,y)$ for $x<y$ in the interval, is the minimum number of intervals to cover them: it's the infimum ℓ such that $[x,y]\subset u_1\cup\cdots\cup U_\ell$ with $U_i\in\mathcal{U}$.

The key lemma which is simple to prove is the following.

Lemma 8.1. (1) *If $x<y$ and ℓ is the covering distance $d_\psi(x,y)$, then there is a word in G such that*

$$||w||_{\mathrm{syl}} = \inf\{k|w = v_1^{p_1}\cdots v_k^{p_k}\}$$

is precisely ℓ and $\psi_w(x)>y$.

(2) *For any word in G with syllable length less than ℓ, then we have $\psi_W(x)<y$.*

My time is almost over, I'll just briefly mention about the last link. We have a group theoretic condition related to the toplogical condition. What is the relation between topology and geometry and regularity, analysis? We have this connection:

$$\begin{tikzcd}
\text{group theory} \arrow[d, squiggly, "\text{covering distance}"] \\
\text{topology} \arrow[d, squiggly] \\
\text{geometry} \arrow[d, squiggly, "\text{smoother is slower}"] \\
\text{analysis}
\end{tikzcd}$$

So if f has regularity $s=k+\epsilon$ and we want to measure for $x\in J_i$, the difference $\frac{|fx-x|}{|J_i|}$, but

$$|fx-x| \le \underbrace{\int\cdots\int}_{k} |f^{(k)}(t)-\mathrm{id}^{(k)}(t)| \le |J_i|^k[f^{(k)}]_\epsilon |J_i|^\epsilon$$

by Hölder continuity.

9. Dominic Verity: Synthetic ∞-category theory and ∞-cosmology (Part III)

[note: the first two talks were slide talks]

Thank you very much and thanks again to the organizers. I promised I would say something about stable ∞-categories. Maybe I'll just get on with the next topic instead. Have a look in the book if you want to see it.

So what I want to do today is to talk about monads and monadicity in this framework. As I move along we'll see some of the reasons that will be interesting. I'll tell you a story about 2-category theory that will turn into a story about ∞-category theory. I'll use K for, I'll just assume it's a 2-category, and I'll suppose I have an adjunction inside K. This consists of a pair of objects A and B, a pair of arrows $u : A \to B$ and $f : B \to A$, and a pair of 2-cells ϵ and η that look like the counit and unit that satisfy the triangle equalities.

I want to understand what the generic 2-category that contains an adjunction looks like. If we look at Categories for the Working Mathematician, we think there's a category which contains a generic monoid, Δ_+, and any other monoid in any other monoidal category, I can find a functor, unique, which takes my special monoid in Δ_+ to that monoid.

So I'll find a special 2-category Adj, so that for any other adjunction in K there is a unique 2-functor carrying Adj to that adjunction in K.

It was shown, this category Adj was constructed by Street and Schanuel some time in the 1980s. They give a simple description. I have to tell you the objects and the hom categories. It will have an object for each end of the adjunction, $-$ and $+$. We know that given an adjunction, and this going back again to Categories for a Working Mathematician, then you get a monad at one end whose underlying functor is the composite in one direction and a comonad at the other end. These are just monoids for the composition, so we can make a good guess at the hom categories at $-$ and $+$. So $\mathrm{Hom}(+,+)$ is Δ_+, so finite ordinals and order preserving maps, and $\mathrm{Hom}(-,-)$ is Δ_+^{op}. Then there's a question about what's going between $+$ and $-$, and one thing that irritated me is that they kind of dream up what goes between them. They don't quite prove it's the right thing. It turns out that the categories going between $-$ and $+$ are $\mathrm{Hom}(-,+) = \Delta_\perp$ and $\mathrm{Hom}(+,-) = \Delta_\top$ both of which are subcategories of Δ, and $\Delta_\perp$ is the subcategory of maps that preserve minimal elements. So 0 maps to 0. Then $\Delta_\top$ preserves maximal elements. Then they describe composition. The composition on $+$ is a nice thing, ordinal $\star$, the dual at the $-$, and we won't worry about what is going on with $\Delta_\perp$ or $\Delta_\top$. There's a special adjunction sitting there. There's a pair of objects, which are $-$ and $+$. Then we have the arrows $[0]$ and $[0]$ between them, and there are units and counits, and you can prove that there is a unique 2-functor from this into K which carries this special adjunction to the one I thought of in K. So you can interpret a lot of the theory of monads and adjunctions in terms of this category.

So is it possible to do something like this in ∞-cosmoi? The first thing we might ask is, is there a simplicially enriched category that contains a generic adjunction. If I have an adjunction in a quasi-categoric-enriched category, then I want to extend that to a simplicial functor out of this simplicial category. There are lots of ideas of how you might construct this kind of thing. It turns out to be really easy to construct this thing.

The first thing to notice is that this thing can be made simplicially enriched just by applying the nerve to the hom categories. In fact it's quasi-categorically enriched, since the hom spaces are already nerves of categories.

When I want to think of it as a simplicial category, this presentation I've given isn't so useful. I want some kind of direct description of the structure here.

I know it has two objects $-$ and $+$. I have a simplicial set of maps from $-$ to $+$ and from $+$ to $-$ and I need to tell you what the simplices look like. I told you about objects and arrows but I didn't give any higher structure. Over here I need to give you more details about the simplices.

The way to understand the simplices from $-$ to $+$ or $+$ to $-$, and an n-simplex, we might call it an n-arrow, the clue to me for understanding this is to go back to the string diagrams we drew in the first lecture [picture] and it turns out that we can describe the simplices, we might be given an example where we're composing together a bunch of ϵs and ηs and we can interpret this by writing down levels, and it turns out that we can draw the simplices or n-arrows in terms of this kind of diagram.

[pictures]

It turns out that there is a theorem mirroring the theorem of Schanuel and Street

Theorem 9.1. *Suppose that we have an adjunction $f : B \to A : u$ in an ∞-cosmos K. Then I can construct a simplicial functor $\mathbb{A}$ from* Adj *into K which carries the generic adjunction to the chosen adjunction in K.*

The proof is, well, I have these pictures. Since this is about quasi-categories—I've hidden a couple of important points. The information that I gave before, we presented adjunctions in the homotopy 2-category so the unit and counit were homotopy classes, and I have to make a somewhat careful choice. The result says that I can extend as long as I'm willing to throw away either the unit or the counit and replace it with something equivalent.

The second thing I should say, in the two-category case, there was a unique 2-functor. We have something similar, but now there's a *contractible space* of such things, rather than a unique one.

[pictures]

So how might we use this? What we've shown here, we've taken a kind of 2-dimensional structure and used a universal property to complete this up to a homotopy coherent structure, we have homotopies going through every dimension. What we might call a simplicial functor out of Adj into K where K is an ∞-cosmos, a natural thing to call this is a *homotopy-coherent adjunction*.

Once we have homotopy coherent adjunctions, we can say we knew classically that we goet a monad at one end and a comonad at the other end and the whole ponit of this picture was to make this easy to see. If you're given a homotopy coherent adjunction, complete it up to something called, something from $\mathbf{A} \to K$, now we can extract the monad. I have two ends of my category Adj. The plus thing has, well, let's let Mnd be the full simplicial subcategory on $+$. I could call the full simplicial subcategory on the object $-$ by the name Co Mnd if I wanted to.

If I want to do something with a monad induced by an adjunction, I precompose by the inclusion of Mnd into Adj. I will call this thing from Mnd to K a monad. You might ask what these kinds of thengs look like.

This gives a strict monoid map of simplicial monoids from $(\Delta_+, *, [-1])$ to $(\mathrm{Fun}_K(B, B), \circ, \mathrm{id}_B)$. This might seem strict, this is a rectification theorem. These monads are the strict thing.

I have to show you that this is the right thing to do. How do we go about doing that? What we do is think about the kinds of things we might have done with monads when we first met them.

So I think about whether I can create an adjunction from a monad, we know we get a monad from an adjunction, and of course there are two extremally different answers. A monad is a functor from Mnd to K, and the Eilenberg–Moore object is a limit construction. To understand it, we should understand the cones for that limit notion. There's a category A with a functor T on it, and if I had cones that look like

$$\begin{tikzcd} X \arrow[r, "x"] \arrow[dr] & A \arrow[d, "T"] \\ & A \end{tikzcd}$$

and I ask for a two-cell there. So I have something that looks like $Tx \rightrightarrows x$, and in a 2-category the correct notion is to take the lax limit of the diagram from Mnd to K. The basic properties follow from that.

So we might ask if we can do a similar sort of thing in the ∞ version. We have the diagram

$$\begin{tikzcd} \mathrm{Mnd} \arrow[r] \arrow[dr, "\mathbb{T}"] & \mathrm{Adj} \arrow[d, dotted] \\ & K \end{tikzcd}$$

and we ask if we have a right Kan extension there. So because this is a computad, you turn out to be able to form this thing. You need a "weight" and this is "projectively cofibrant" and in particular cellular. The construction process we have is [unintelligible]so I can form this Kan extension, and I call this $\mathbb{A}^{\mathbb{T}}$. So from a monad, by Kan extension, I have an adjunction, which is a good thing.

Here I started with a monad and extended to an adjunction. But what happened if I started with an adjunction in the first place? Then I'd have two triangles of the same basic form. Then since this is the Kan extension, I get a map $\mathbb{A}$ to $\mathbb{A}^{\mathbb{T}}$, and maybe this is a simplicially enriched natural transformation. Also, when I restrict back to Mnd this becomes an identity.

Now we have two adjunctions, one between A and B, and one between $\mathrm{Alg}_{\mathbb{T}}(B)$ and B, and I have a comparison k which commutes with the functors going up, the functors going down, and with the unit and counit.

This is the same as in the classical case.

An important question, if I'm given a monad, I have this Eilenberg–Moore category, and I also have the Kleisli category, which is a lax colimit, and maybe there's a comparison between these things. There may be a lot of other adjunctions between. How do I tell if an adjunction is monadic?

When we dig into the characterization, there are connections to important things in other parts of mathematics. Beck pointed out that there are questions about connections between monadicity and descent or effective descent. To be a 2-category theorist, two-dimensional monads are an important part of coherence. We can do similar things in $(\infty, 2)$-category. Beck gives some conditions having to do with this functor having a left adjoint. He observes that alegbras for a monad is a colimit of free algebras. A group is a coequalizer of two free groups. Beck worked these things out.

To get this thing to have a left adjoint I'll need ceration left limits. In this context, you need to be a little more wily because coequalizers won't be enough, but we can use the kind of colimit notions we talked about before to touch what it means to be the geometric realization of a [unintelligible]object. Maybe I'll conclude the talk by drawing this for you. The kind of things you find in Beck monadicity, he'll draw diagrams like this, for reflexive coequalizers, or at least special ones, those which when I map to B by the forgetful functor is a slightly more special case, it's a split coequalizer downstairs.

In fact what we have is a lot more structure upstairs, we have not just splittings at the first two levels but at all dimensions. The amazing thing about this is that you can pretty much, once you've understood a bit of formal structure of Adj, it's easy to mimic the popular proof of monadicity. It asks you for colimits of a shape which give you an adjunction, and then we ask for something else to get this to be fully faithful, and then if u is conservative, then we get that this is an equivalence.

10. Benjamin Burton: The computational complexity of the HOMFLY-PT polynomial

Thanks, so, we were talking in the lift yesterday night. I say I do abstract nonsense at home, but here I feel like a proper applied guy. I write software, look at computational complexity, and so on. Can I thank the Koreans here for supplying actual blackboards. I love it. Let me talk about tables of knots. Jessica was talking about better, brighter ways of tabulating knots, but I'm talking about the old stale way, and I think Stavros has done up to 17, if you email him. I'm working on the 18-crossing tables. There's two parts to building a table. You need every knot to appear and you need no duplicates. They're two different problems, quite hard.

Making sure everything there, that's a combinatorial enumeration problem. To give a timeline, from November, in around a few weeks, the enumeration was done. There are very fancy algorithms behind it. If you do it right, the computation takes a few weeks. What do you have at this point? roughly 58 million knots, and you have to tell them apart. So then you have from then until April, finding the right invariants, and computing them, computing them on an enormous scale. If there's an invariant that takes you a day to compute, you can't do it here. Most of these 58 million knots seem to be distinct. So what kind of invariants can you use. For the census, I've been using things that range from easy to slow to crazy slow. For easy, I mean things like the Jones and HOMFLFY-PT polynomials. I don't actually need Jones. You can run it over 58-million knots, but it's exponential. Topologists should not listen to computer scientists when they say something is intractable because everything is.

So for slow, I mean things like algebraic invariants of the fundamental group, finite presentations, finite subgroups, things that are stronger as the index increases, and the bulk of the time between November and April is computing subgroups of index five. I can't possibly do index six, which still leaves you with more things to distinguish. For crazy slow I mean genus, crosscaps, you're running a super-exponential thing, Jae Choon Cha and Livingston have knotinfo, which has 70 invariants or so for knots up to 12 crossings, but there are only thousands of these. I want to focus here on the easy case.

So having said that, you do the enumeration, and then you do a breadth search via Reidemeister moves. So the motivation then is computing HOMFLY-PT polynomials, I computed these in November, this result is from December. Can I assume that people have heard of HOMFLY-PT? It's a two-variable polynomial invariant, that's all you need to know for now. This is $\#P$-hard to compute. What do I mean? You have NP-complete, which means that unless you can solve Hamiltonian circuits and university timetabling, you can't do these in polynomial time. Make a bet that something NP-complete has no polynomial algorithm. So NP is about yes or no problems. A problem is NP hard if it is at least as hard as NP-complete things. The $\#P$ problems are counting problems so they are sometimes much harder. They are at least as hard as NP things.

The best algorithm is exponential, this is $O(c^n)$, and I'll talk about the Kauffman skein-template which is 2^n times polynomial in n. For the 58 million knots, let me plug Regina, a piece of software, and if you get it from the working repository you'll get knots, and it has Kauffman's algorithm with some careful optimization. All 58-million knots, you take about 60 hours in total. It mean it's doing around 269 knots for second. So this is a 2^n algorithm that is taking milliseconds for the largest knots in the census.

So this is not about practicality. This is about understanding the computational complexity of these knot polynomials. There are some big open questions. If I ask you if a knot is the unknot, it's unknown if this is polynomial time. The best algorithm is exponential. This is what computer scientists and topologists are getting together and thinking about.

Of the 58 million, the HOMFLY-PT makes about half of these unique. So it's a good first pass.

So I want to talk about complexity. One of the theorems, let me give you the spoilers now. Kauffman's algorithim is exponential, and what you can prove is the following.

(1) It is fixed parameter tractable in tree width. I'll explain what this means. But if you measure, if I draw the knot, I get a 4-valent graph in the plane. I can measure the tree width of this graph, and it's polynomial times a function of the tree width. All the hardness is encapsulated in the tree width. If I give you an infinite family of knots with tree width bounded then I can give you a polynomial time algorithm.
(2) You can do better than exponential. In fact it is $e^{O(\sqrt{n}\log n)}$. This is bigger than polynomial but less than exponential. This is a one line corollary, but it's the first subexponential algorithm.

What I find particularly interesting is that the second result is a one-line corollary. So build this thing using the measurement of the graph. The algorithm is tailored to the tree structure of the graph. But then you bound the tree width and this just falls out. So you use parameterized complexity and use it to get a complexity result.

Any questions before I start defining the things. I think—

So parameterized complexity. This came out of work by Downey and Fellows in the 80s and 90s. In traditional computer science you measure the complexity as a function of n, whatever the input size is. What they do is not just measure it in terms of n, but do a more refined version. The simplex method for linear programming is what everyone uses, even though it's exponential. It's average polynomial time, generic polynomial time, smooth polynomial time, so this is an example of an algorithm that is fast in practice but traditional complexity doesn't see that.

Let me give you a toy problem which is vertex cover. The input is a graph. What you need to do is find a cover, which is a selection of vertices so that every edge meets one of the vertices that you chose. Every edge touches a yellow vertex [pictures]. The input is a graph and a number k. The output is a yes or no problem. Does there exist a cover of size less than or equal to k. There is an easy algorithm of size 2^n, in fact $\binom{n}{k}$, but let me give you something better. So traditional complexity says this is 2^n times some polynomial in n.

But this time we'll measure the complexity not just in terms of n but also in terms of k. Choose an edge that does not touch any vertices. So choose either the left or the right. Now choose another uncovered edge. At each node of this search tree we get one of the vertices to include. This does not need to go beyond depth k, because then we've chosen too many vertices. You make different choices on different sides of the search tree.

So you follow this search tree to depth k and either find a solution or you don't. So if there is one, you find it in the search tree. It has size 2^k. So what is the running time? It's 2^k times however long it takes you to find the next edge, which is like n or n^2. So this is a function of k times polynomial. So if I want a fixed cover size, then I can let the graphs get as large as I want but it's polynomial. If k is small then it's fast and if k is fixed then it's quadratic.

What does it mean to be fixed parameter tractable? It means $O(f(k)\text{poly}(n))$.

This lived in theory in graph theory, factorials, exponentials, towers of exponentials in k. This vertex cover is the poster child for a case where it actually works.

But people in other fields have started to use these algorithms, and they have started to become tractable. In actual software in biology they've used these. For me, more interesting, in topology, to compute the Turaev–Viro invariants, they have a traditional backtracking and also a fixed parameter thing, and the surprising thing, we implemented it and found out that it's faster, sometimes orders of magnitude faster. If you want to find a [unintelligible]structure, there's a couple of people here who know what that is, but it's faster. I don't know yet about the HOMFLY-PT. Maybe backtracking is fast enough for 18 crossings but not for 45.

So that's parameterized complexity. The next thing is tree width. Any questions about parameterized complexity. This also comes from graph theory. I give you a graph and you want to measure how much soju you have to drink to make it look like a tree. A complete graph has tree width $n-1$. A graph made out of cycles joined together in a tree-like fashion is tree width 2.

The idea is that I give you a graph, and what you want to do is group the vertices into *bags*. YOu want to group them together in clumps so that the bags are hooked together like a tree. [pictures]

The tree width, you arrange things so that the maximum bag size is as small as possible:

$$\text{tw} = \min \max |\text{bag}| - 1$$

You choose the bags, you choose the tree, and you want to make it as small as possible. This is a property of a graph. It's hard to compute, but we'll come back to that. This will be our parameter.

I first encountered tree width because it turns out, well [anecdote about boats in Darwin], Dehn fillings give you long chains of tetrahedra, and these tree-like structures are quite natural.

There is a theorem, Lipton–Tarjan that have a theorem on planar graphs, which says that the tree width of a planar graph is $O(\sqrt{n})$, and for a knot this is the 4-valent graph. If you can find the tree width, it's guaranteed to be small. There's this Haken picture of an unknot, I put this into Regina by hand, and it has tree width 12 but 141 crossings. Regina with greedy heuristics found a decomposition with bag size 13. If you have a knot where the tree width is large, you can move things around and try to make it smaller. You can just try again.

Okay, so any questions about tree width?

Let me give you the basic idea behind this algorithm. First of all, let me show you why the Jones polynomial is fixed parameter tractable in tree width, this is around 2005, Makowsky. He also showed the Tutte polynomial is fixed parameter tractable in tree width. You can compute this with the Kauffman bracket, you undo the crossing in one of two ways, for the first direction you get the bracket polynomial of a smaller knot and multiply them by something. Also if there's an unknot you peel it off and multiply by some factor. You build a tree of resolutions, undoing each crossing in turn and at the bottom you get a collection of unknots and work your way back up the tree. How do you make this fixed point tractable?

So what's the idea, we think each bag, we have these bags that make up the knot. If I give you a knot, it's the projection. Every crossing is a vertex, every arc is an edge. So work one end of the tree to another. Look at the stuff that happens in here. In the intersection, there are some crossings. All the crossings that appear in your bag alone you will never see again. What it means is that all the vertices that you see to the left in this picture, you aggregate and forget. If there are B vertices in the bag, you compute a partial polynomial inside the bag. You don't know how to connect these up to the rest of the world. You don't know what's going to happen. For every crossing in the bag, you look at all the ways they travel out and return. For each different way, you compute a subtree. [pictures] You have something like $2^b(2b)!$ from partial resolutions and pairings. This is n times something in the tree width, which controls the size of the bags b.

For HOMFLY-PT you try to play the same game, but the skein relation is not so nice. It's a polynomial in α and z. If you have some knot with three different resolutions, you zoom in and change in three ways, and the skein relation relates these [pictures]. So this is enough information to compute, but in the Jones polynomial, at every stage the knot gets smaller. If you resolve K_+, one gets smaller but the other one has fewer crossings. So Kaufmann's algorithm, you use this to switch crossings that gets you closer to unknots and unlinks. You know that any knot can be turned into the unknot by changing crossings.

So Kauffman says, traverse the knot and at each stage arrange it to go over, and then when you come back you go under. We're not reducing exponentially, so you don't get the neat aggregation. Because Kauffman wants you to follow from start to finish, you need to know whether things are the first or the second time you've seen it. You don't know what order you'll visit in. So actually the bulk of the work is moving around this so that you can work this out. If you need the starting points it starts involving n. You want the starting ponit for the traversals to determine an ordering on the strands of the knot which tells you where to start for subtraversals, so that you don't need a superpolynomial function of n here.

That's how it's working. Are there any questions before I say a couple more things? Maybe I'll just give some references. If you want to read about this, it's in SoCG 2018 (pronounced "sausage"), this is for computer scientists, the preprint is on the arXiv. What i want to show you is that the algorithms can be tractable. This is the Turaev–Viro, this is looking at hundreds of thousands of manifolds. This is a log log plot of the running time. [pictures]