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1. Emily Riehl: Lifting properties and the small object argument

Thanks very much, I want to thank the organizers. I'm excited to start us off. Today I'm going to talk about the algebraic perspective on fibrations in homotopy theory. This is a talk in three cats, the first of which is the longest. I want to talk about the interplay between lifting properties and factorizations, maybe weak factorization systems or things that spring up naturally. I'll focus on particular examples, but this is all formal so it will be true in much more generality.

1.1. **Act I.**

Let's say that we have a goal to replace a map by a fibration, let's say I'm talking about compactly generated weak Hausdorff spaces, with Serre fibrations, a map is a fibration if it satisfies the lifting property

$$\begin{tikzcd}
D^n \arrow[r] \arrow[d] & X \arrow[d, "f"] \\
D^n \times I \arrow[r] \arrow[ur, dotted] & Y
\end{tikzcd}$$

So what we can do, let me draw this square again and push out.

$$\begin{tikzcd}
D^n \arrow[r] \arrow[d] & X \arrow[r, equal] \arrow[d, "Lf"] & X \arrow[d] \\
D^n \times I \arrow[r] \arrow[ur, dotted] & Ef \arrow[r, "Rf"] & Y
\end{tikzcd}$$

I need to solve all lifting properties, so I should do this over all possible lifting problems. Now there are more lifting properties in Ef so we haven't solved this problem.

Even though we've not made any progress, this data helps us detect fibrations. If f were a fibration, we'd have a lift in the right square, if we had a lift there, then we'd get a lift in the bigger rectangle. Then we've shown that a map is a fibration if and only if there is a lift for this single lifting problem:

$$\begin{tikzcd}
{} \arrow[r, equal] \arrow[d, "Lf"] & {} \arrow[d, "f"] \\
{} \arrow[r, "Rf"'] & {}
\end{tikzcd}$$

This is the same thing as realizing Rf as a retract and this is the same thing to say that f is an R-algebra for the functor R on the arrow category which takes a map to its right factor.

We can say, flipping around,

Definition 1.1. *A map is an L-coalgebra if and only if there is a lift*

$$\begin{tikzcd} X \arrow[r, "Lf"] \arrow[d, "f"'] & Ef \arrow[d, "Rf"] \\ Y \arrow[ur, dotted] \arrow[r, equal] & Y \end{tikzcd}$$

A map is an L-coalgebra implies it is a trivial cofibration. An L-coalgebra has the lifting property with respect to a R-algebra. Knowing that something is a coalgebra proves that it's a trivial cofibration.

I've made no progress toward my goal. I want to bring in another example, with the Hurewicz fibrations, they should satisfy the homotopy lifting property for maps from any space.

$$\begin{tikzcd} A \arrow[r] \arrow[d] & X \arrow[d, "f"] \\ A \times I \arrow[ur, dotted] \arrow[r] & Y \end{tikzcd}$$

We can't do the thing that we said before. We can't even do the thing that didn't work before. You can't attach all homotopies. I don't know who this is due to, there's a represented functor, a contravariant functor $Top^{op} \to Set$ which takes A to squares of the inclusion $A \to A \times I$ and Y, this is represented by a space Nf. A map to the diagram

$$\begin{tikzcd} & Y^I \arrow[d] \\ X \arrow[r] & Y \end{tikzcd}$$

corresponds to a map Nf. So the map $Nf \to Nf$ corresponds to a map and we get a diagram

$$\begin{tikzcd} Nf \arrow[r] \arrow[d] & X \arrow[r, equal] \arrow[d] & X \arrow[d] \\ Nf \times I \arrow[r] \arrow[rr, bend right] & Ef \arrow[r, "Rf"'] & Y \end{tikzcd}$$

and a lemma: a map is a Hurewicz fibration if and only if it's an algebra over this R functor. [fast proof].

Now is time we should make some progress toward the original problem. Whichever kind I mean, I've encoded these as algebras for a pointed endofunctor. What might we do? An idea: iterate these factorizations. I do want to differentiate between these two examples. Let's talk about the first one. Let's do this multiple times, factor, factor it again, countably infinitely many times, I've just done Quillen's small object argument, by compactness a disk that goes into the colimit lands in some finite stage, and so the Serre fibration we construct you might call $R^\omega f$. There is redundancy in this construction. We'll attach a homotopy every single time to a disk in X. I'll have more to say about that in just a second. For the Hurewicz fibration case this doen't actually work. We could apply our functor again and again. You might hope this glues in enough homotopies. There's actually a really interesting subtlety. We'll prove this factorization and get one that works in the other context at one stroke.

There's the notion of an (algebraically) free monad on a pointed endofunctor, and what I'll take as the definition, algebras for the monad the category is isomorphic to algebras for the pointed endofunctor over whatever (spaces in this case). So F algebras are fibrations. If I can produce the free monad, its algebras will be fibrations. How do we produce one?

Let's try and build F. Why am I talking about monads? We have this notion of free algebras. Anything that is F of something, that's an F-algebra and so that's a fibration. The first thing to think of, we have $1 \to R \to R^2 \to \cdots \to R^\omega = F$, and now we have a multiplication, maybe this will work, and this does work in some cases, for pointed endofunctors. This isn't great, I have two maps $R \to R^2$, if those two maps are the same, this is a well-pointed endofunctor, and in that case R^ω is the free monad. If they're different I have to do something a little more refined.

I want to point out the difference in our two examples. This'll show why the naive thing in the Hurewicz case doesn't work. For the Serre fibrations, we have these disks $D^n \to X$, and these gave us homotopies $D^n \times I \to Ef$, and ηR takes this to $D^n \times I \to Ef \to ERf$, we can also get $D^n \to X \to^{Lf} Ef$, and so we could get $D^n \times I \to ERf$, and this is $R\eta$.

For Hurewicz fibrations, $Ef = Nf \times I \cup X$, and $ERf = NRf \times I \cup Ef$, which is $(NRf \times I) \cup (Nf \times I) \cup Y$. We get a path in Y and a point in its fiber, a path can include in two places, two different embeddings, and we have a problem at ∞.

How do we solve this problem? The actual free monad function? We'll guess in step one that it's R, and our second guesss will be F_2, the coequalizer of these two maps $R \to R^2$. There's something more complicated at step three, and if the procsess converges, that's the free monad.

Theorem 1.1. *(Garner) If we're permitted the small object argument (SOA) then this process converges and F-algebras are isomorphic to R-algebras, and you get a functorial factorization $\xrightarrow{C}\xrightarrow{F}$ called an algebraic weak factorization system (awfs) (where the left factor is a comonad.*

This is a great thing! It solves the problem. The right factor is a fibration, you might worry that the right factor is not a cofibration anymore. This does, well, the left factor being a comonad is a coalgebra for itself, and coalgebras for the left factor, as I said, always lift against algebras for the right factor.

This solves the problem. Good. I meant to say, a lot of this story will be about producing better factorizations when you had something to start with. It's essential that you have something with lifting properties, that trivial cofibrations should be the things with lifting against the fibrations.

Richard Williamson first brought the issues with Hurweciz fibrations to my attention, and everything about that is joint work with Tobi Barthel who is here. For the Hurewicz case, we get a factorization into C followed by F, and we need to worry that C is not a cofibration.

Lemma 1.1. *(Garner)*
awfs $(\mathbb{C}, \mathbb{F})$ is a monad $\mathbb{F}$ together with a vertical composition law on $\mathbb{F}$-algebras.

Theorem 1.2. *(Barthel, R.)*
This works for the Hurewicz case.

We don't need to worry, without knowing any point set topology, this was a cofibration.

If we're only talking about point set, this is easy. You only need one factorization to get an h-model structure on a Top-bicomplete category that admits the small object argument. These are quite general examples.

[What about Cole?]

[Peter May: Cole made a mistake. Now you have the h-structure everywhere and so you have the m-structure everywhere which is where you really want to work.]

1.2. **Act II.** Often you have factorizations. We produced one that we needed and didn't have by other means. We may be able to make factorizations that we already have better. A corollary is that in any cofibrantly generated model category that permits the small object argument, there exists a cofibrant replacement comonad and a fibrant replacement monad, maybe we're talking about simplicial sets, you can get something that's a monad, and you can do even better, that's the point of Act II. I'll specialize to the case of a simplicial model category permitting the small object argument. If you prefer another enrichment, tensoring with base objects should be a left Quillen functor. That's two thirds of SM7. It's true in this setting but I don't need quite this much. The functor sending f to step 0 of the small object argument, the coproduct over disks, this is not a simplicial functor, not simplicially enriched. We've enriched the lifting properties, but not the factorizations at all. This is why. Our factorizations are already broken. There's something else we could try to do instead. The squares from j to f is just a set. But I can talk about the object $\underline{Sq}(j, f)$, which is the pullback of

$$\begin{tikzcd} & \underline{M}(dom\ j, dom\ f) \arrow[d] \\ \underline{M}(cod\ j, cod\ f) \arrow[r] & \underline{M}(dom\ j, cod\ f) \end{tikzcd}$$

So now if we use $\underline{Sq}$ we get a simplicial functor, and the underling set is correct.

Theorem 1.3. *In this context you get simplicially enriched factorizations and also fibrant and cofibrant replacement.*

This is true with the modification, if you ran Quillen's small object argument, you don't know that they factor where you want. If you run Garner's algebraic version, you do have control, and it's easy to see that the factorizations at the end are the right thing. I don't know what happens at the end, it's this complicated formula. The factorizations we see now, you take the coproduct over n of $\underline{Sq}(j, f) \otimes j$. If f is a fibration, you get a lift as in Act I. At this stage, Rf is not a fibration, but R-algebras are exactly fibrations. Because we can recognize the algebras as fibrations, we can use this free monad thing. It's also possible to check long-windedly, but the left factor lifts against the right factor so it's a cofibration.

1.3. **Act III.** I want to shift gears and talk about cofibrations. Fibrations are algebras for some monad. Every fibration is an algebra for some monad. I also have coalgebras for a comonad. Let me pretend I said trivial fibrations. It's not actually true that all cofibrations are coalgebras for the comonad. They are for the pointed endofunctor. Being a coalgebra for the comonad is a little more subtle, these are called cellular cofibrations. For example, taking the sphere inclusions, you get relative cell complexes. I guess the point of this digression is that if you want to pay special attention to the relative cell complexes, this makes it easy to do so. We have really good closure properties. Let me start by proving this. The comonad coalgebras are closed under pushout, composition, and the forgetful functor to the arrow category in Top creates all colimits. A coequalizer of cofibrations need not be a cofibration in general, but if everything is cellular and your maps preserve the structure, then the colimits are coalgebras and hence cellular cofibrations.

For a long time we have hypothesized (Richard Garner and I) that these coalgebras are exactly the cellular cofibrations.

Theorem 1.4. *T. Athorne. An algebraic relative cell complex is the same thing as a coalgebra over the comonad.*

A stratum is a space X together with a set of cells togethre with attaching maps. We can push this out, and X is the "boundary" of the stratum and $\underline{X}$, the pushout, is the body of the stratum.

What's an alegebraic notion of a relative cell complex? It's a connected sequence (the body of one is the boundary of the next) that is *proper*, meaning the attaching maps don't attach in previous strata.

There's a forgetful functor from Strata to Top which has a right adjoint which I will call L. I can look at all squares, all cells in the domain of X,

$$\begin{tikzcd} S^{n-1} \arrow[r] \arrow[d] & X \arrow[r, equal] \arrow[d, "Lf"] & X \arrow[d, "f"] \\ D^n \arrow[r] & Ef \arrow[r, "Rf"'] & Y \end{tikzcd}$$

How do we construct this? We could do an iterative process, this gives something that isn't proper. To construct the adjoint, you use the algebraic small object argument, and get the free monad construction again. This has a composition and so you get the comonad structure.

2. John Francis, factorization homology and low dimensional topology

I'd like to talk about knot homology. I'm easily influenced by the opinions of others. I'm MIT trained. No one would talk about knots but they'd talk about knot homologies. I'm kind of lazy. There are these papers. Choose a presentation, and then, well, I can't understand anything that starts with a presentation. The thing you did once you started with the presentation, I'd start reading something else. Let's say I'd want to understand this. So let's say that a knot K is something one dimensional sitting inside something three dimensional, and this is a closed submanifold. That, as far as I'm concerned, is what a knot is. A knot homology should be be valued in $\mathcal{C}$, it would take thde data of $K \subset M$, let's call the homology F, and it should go from these collections to $\mathcal{C}$. That seems pretty reasonable to me. This is now, hard for me to understand, I'd like to understand a simple case. There is one knot I have an understanding of, if we choose a judicious case in my mind for how this should behave, a special case, where K is empty. This might seem stupid, I'm revealing my ignorance. Let's try to understand this case where K is empty. I'll try to specify. This is joint with Ayala and Tanaka. Other parts are joint with Kevin Costello. Some of it is not joint. I'd like to understand what happens when K is empty. So now the question is, what is a homology theory for 3-manifolds supposed to be? If I were more familiar with what people do, I'd know the answer, but anything I say will not be related to 3. So anything I say will be about n-manifolds. What is a homology theory for n-manifolds supposed to be? It should be an assignment, once I've chosen where it's valued, it should take $Mfld_n \to \mathcal{C}$. What's an n-manifold. Let's decide. So n-manifolds, I see no reason to restrict myself. I like all manifolds, not just closed manifolds. At this moment I clearly don't know what I'm doing. Let's look at all manifolds. This is a category. The objects are n-dimensional manifolds. We could choose smooth or topological, without boundary. The boundary confuses me. Let's say that they're not too big. No surfaces of infinite genus. Let's say that they are the interior of a compact manifold.

I said it was like a category, it should be functorial with respect to maps. The first thought is to allow all continuous maps. I've been saying why not. If you allow all continuous maps, this category struggles to distinguish between categories which are homotopy equivalent but not homeomorphic. You don't want to allow all continuous maps because then M and M' which are homotopic but not homeomorphic aren't really distinguished. How could we get rid of this problem? Let's say we wanted to use all the maps we could without this problem, we could use just embeddings. This says it's properly functorial with respect to automorphisms, and any time, if one of the manifolds is not closed, you can embed it into something else, you want functoriality with respect to those as well. You'll still be able to distinguish those. The space of maps from M to N will be the space of embeddings.

Now we've said where we're coming from. What kind of a thing should $\mathcal{C}$ be? It should be of a comparable nature. We should have spaces of maps. $\mathcal{C}$ should be a topological category, or something that has a space of maps between objects. That seems like a good start. You don't just mean an assignment by homology, though. The global value of a homology should be determined by the local values. You should be able to understand the global data in terms of the local data. I'd talk negatively about it while you weren't listening if you tried that.

What's the simplest case of a gluing? The empty space, a disjoint union. If a homology theory is something where you can understand global values in terms of local values, the simplest thing to understand is this disjoint union. Here we have a couple of objects, when you have two objects you can replace them with one object. This will then be a symmetric monoidal category.

Definition 2.1. *The collection of* homology theories *for n-manifolds valued in $\mathcal{C}$, denoted $H(Mfld_n, \mathcal{C}^{\otimes})$ is a subset of the collection of symmetric monoidal topological functors from $Mfld_n^{\amalg}$ to $\mathcal{C}^{\otimes}$, and this should have a gluing, an excision, a monoidal version of excision.*

To say how this works, it's good to remind yourself how normal excision works. If you have X_0 sitting in X' and X'' via cofibrations, and X is the union, then $C_*(X)$ should be equivalent to $C_*(X') \oplus_{C_*(X_0)} C_*(X'')$, so the chains on a pushout are the chains on the pushout. By equivalent I mean quasiisomorphic.

Let's just copy this. We don't have cofibrations, any reasonably nice embedding will do. Given M which is the gluing of M' and M'' along $M_0^{n-1} \times \mathbb{R}$, I would like the value on M to be determined by the other values,

$$F(M) \cong F(M') \otimes_{F(M_0 \times \mathbb{R})} F(M'')$$

and this makes sense. I'll need the derived tensor product. This is a definition. $\mathcal{C}$ was a topological category so I can talk about homotopy equivalence there.

[How does the thing in the middle act?] Well $\mathbb{R}$ is an E_1-algebra in spaces.

Here's a definition. It's not clear that it's a good idea. I think it's a good idea. If you wanted to show it was a good idea, you might try to say that the things you got were interesting. Saying things are intersesting is kind of hard so I'll say what you get. If you were optimistic about describing these, you might take some motivation from Eilenberg-Steenrod. For spaces with values in chain complexes, these are functors from spaces to chain complexes satisfying excision. Since every space can be glued together from contractible spaces, a homology theory should be determined by their value on a point. The axiom says that this is an equivalence. You could work in spectra instead.

We could evaluate on a contractible space. Well, $\mathbb{R}^n$ is a lot like a point, you could evaluate there, that's an n-manifold. The generalization that you get, you get an object in $\mathcal{C}$ evaluating on $\mathbb{R}^n$. Well, $F(*)$ is acted on by the automorphisms of a point. Now we have an action of the automorphisms of $\mathbb{R}^n$, and this is equivalent to $O(n)$ in the smooth case. You don't have to think very long. It's easy to say. The structure, well, consider Euclidean spaces. Consider $Disk_n$ whose objects are finite disjoint unions of copies of $\mathbb{R}^n$ If you just remember the structure on $\mathbb{R}^n$. The structure that you see there, $Disk_n$ algebras in $\mathcal{C}$ is functors from $Disk_n$ with disjoint union to $\mathcal{C}$ with $\otimes$, this is like the Boardman-Vogt's E_n algebras, but with the extra data of an $O(n)$-action. If you choose an embedding, you get a map $A \otimes A \to A$. As you move the $\mathbb{R}^n$ around, you get a family of such structures, that's the structure of Boardman and Vogt and studied by May and others. The theorem is that this is an equivalence. How do you go back to homology theories on manifolds? The inverse is given by factorization homology, topological chiral homology of Jacob Lurie. I'll use the symbol $\int$ because you're summing local values, and you can construct it with a coend which is also denoted $\int$ by category theoriests. I'll put the names Lurie, Beilinson-Drinfel'd, Salvatore, Segal. If you can go back, you can do it in a unique way. There can't be more than one way. Is there a way at all, that's the question. Any formal construction, if it exists, it's the right one. You can take the left Kan extension, or the coend. Let E_M be the restriction of the Yoneda embedding. This sends the disjoint union of disks to embeddings. The factorization homology $\int_M A$ is $E_M \otimes_{Disk_n} A$.

We've lost track of the fact that I started with knots. I want to modify this story and add the one dimensional submanifold. This is a special case of something I've worked out with David Ayala and Hiro Tanaka. Let's put the knot, the link, back in.

Definition 2.2. *Let $Mfld_{n,k}$ be n-manifolds M with k-dimensional closed submanifolds with a trivialization of a tubular neighborhood, so a factorization, $K \to K \times \mathbb{R}^{n-k} \to M$. I want my morphisms to preserve trivializations. I want the same thing to happpen as you go out a little bit. These are the objects, and then the morphisms are embeddings preserving the triviality*

What should a homology theory be? The collection of homology theories should be the full collection of things that are symmetric monoidal topological functors to $\mathcal{C}$ that should satisfy excision. If I have (M, K), written as M, with K implicit. When I have $M'' \leftarrow M_0 \times \mathbb{R} \to M'$, where the cuts are transversal to K, then the value $F(M)$ is $F(M') \otimes_{F(M_0 \times \mathbb{R})} F(M'')$.

What structure is required? The basic idea was that you gave yourself values on the essential building blocks. If you can build spaces out of them, you know everything. You now could have a point on K. Here we have two essential building blocks.

Definition 2.3. *$Disk_{n,k}$ is a full subcategory of $Mfld_{n,k}$, whose objects are finite disjoint unions of $\mathbb{R}^n$ and $(\mathbb{R}^n, \mathbb{R}^n, \mathbb{R}^k)$. There is an embedding $(\mathbb{R}^n, 0) \to (\mathbb{R}^n, \mathbb{R}^k)$ but nothing going back.*

Definition 2.4. *$Disk_{n,k}$-algebras in $\mathcal{C}$ are functors from $Disk_{n,k}$ to $\mathcal{C}$, topological, monoidal.*

Theorem 2.1. *(Ayala,F.,Tanaka)*
$H(Mfld_{n,k}, \mathcal{C}) \to Disk_{n,k}$-algebras in $\mathcal{C}$ is an equivalence with inverse given by a Kan extension or a coend.

I want to look at a strange example when $\mathcal{C}$ is spaces with Cartesian product. We need an example of a $Disk_{n,k}$-algebra. There's a good one. If you give yourself a space X over $BO(n)$ and let's say these are fibrations, another space Y over $BO(k)$, with distinguished sections, and a map $Y \to X$. You have a map $\mathbb{R}^n \to BO(n)$ and a map $\mathbb{R}^k \to BO(k)$, and these could be any manifolds M^n and M^k, and you could consider lifts of these classifying maps over the fibers. Call this object $(\Omega^n X, \Omega^k Y)$. This is the compactly supported sections. I have a functor from $Mfld_{n,k}$ to spaces sending (M, K) to compactly supported sections $(M, K) \to (X, Y)$, they go to the canonical section outside a compact subset. Forget that if you like things closed. We can restrict that to $Disk_{n,k}$ and that retriction is $(\Omega^n X, \Omega^k Y)$. What homology theory do you get? If $X \to BO(n)$ is n-connected and $Y \to BO(k)$ is k-connected, then the factorization homology of $\Omega^n X, \Omega^k Y$ is the space of compactly supported sections (M, K) to (X, Y). I started with a definition of this thing, but this has a lot of content. This is glued together from configuration spaces of M and K glued together on X and Y. This has something really nontrivial in it. This is a generalization of the non-Abelian Poincaré duality of Salvatore and Lurie, which was a generalization of the work of James, MacDuff, Seigel, May, Boedigheimer, others, of the seventies and before.

What if you set $n = 3$ and $k = 1$. The mapping space is only sensitive to the homotopy type of the embedding. All embeddings of S^1 into $\mathbb{R}^3$ are homotopic, so this isn't going to impress any knot theorists.

To keep looking you might try to think about what $Disk_{n,k}$-algebras are. The $Disk_n$ algebras are very familiar, going back to the sixties. If you have, say, R a $Disk_{n,k}$-algebra in $\mathcal{C}$, this has several parts to it. Ther is the part that is just $\mathbb{R}^n$, that's an n-disk algebra, and the other is, well, these are determined by maps on $\mathbb{R}^k$, this is a k-disk algebra, and there's an action of the n-disk algebra on the k-disk algebra.

The proposition is that given these two things, how do you get an n, k algebra? Given A, a $Disk_3$-algebra, and B a $Disk_1$-algebra (associative) algebra, the structure of a $Disk_{n,k}$-algebra on (A, B) is an action, a map $HH_*(A) \to HH^*(B)$, I mean the complex or spectrum, which is a map of $Disk_2$-algebras. These are both $Disk_2$-algebras. Call the map ψ

Proposition 2.1. *(Ayala, F.,Tanaka) This trio of data is equivalent to $Disk_{3,1}$-algebras.*

You get a knot homology theory from this data, with a $Disk_3$ algebra, a $Disk_1$ algebra, and a map from the Hochschild homology of one to the Hochschild cohomology of the other.

We have one that we think is really interesting. Say that we're in characteristic zero. Take $\mathfrak{g}$ a Lie algebra with the invariant pairing $(\, , \,)$. There is a 3-disk algebra structure on $C_q^* \mathfrak{g}$, a deformation of the LIe algebra structure, this is A. Let B be the ground field. That should be the one-disk algebra part.

Construction (details in progress): Given a conjugacy class $\sigma \in \mathfrak{g}$ you can construct a map from the Hochschild homology of $HH_* C_q^*(\mathfrak{g})$ to the Hochschild cohomology of k, and so we think we can make that a $Disk_2$-algebra map, and that should be something interesting for knots.

[Is this related to Khovanov?]

You should have a map from the homology of long knots in a noncompact manifold to closed knots in a closed manifold. I ask the Khovanov homology of the empty knot and people seem to think that's a really dumb question, but then they have trouble answering it. I don't know, maybe it's too dumb. Oh, I'm being recorded. I take it back.

[Paul Goerss: I just tweeted it.]

No one retweet it. Well, I don't think, I don't know, I don't think Khovanov satisfies this. It would be cool if it did, because people seem to like that stuff.