Enchiridion: Mathematical User's Guides
`mathusersguides.com`
Volume 1 (October 2015)

A user's guide: Coassembly and the K-theory of finite groups

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Contents

1. Key insights and central organizing principles

1.1. Background. One of the main things we do as algebraic topologists is we take a gigantic object that has way too much data, such as a topological space with uncountably many points, and we distill that data down into a small, computable object like a finitely-generated abelian group. If we do our job well, the small invariant captures something essential about the big unruly space, and we can use that something to answer questions that would otherwise be out of reach.

Algebraic K-theory is a part of this larger story. It accepts as input any category C with a notion of weak equivalence and cofibration, and outputs a sequence of abelian groups $K_0(C)$, $K_1(C)$, ... In particular, we can take the K-theory of a ring, by feeding in the category of finitely-generated projective modules. Or, we can take the K-theory of a ring spectrum by feeding in the category of dualizable R-modules. It is also possible to take the K-theory of a topological ring such as $\mathbb{C}$ or $\mathbb{R}$. We can even define the K-theory of "spaces" by feeding in the category of retractive spaces over a fixed space X.

So, the algebraic K-theory machine boils the category C down to a manageable collection of abelian groups. These groups still contain essential information about C. The group $K_0(C)$ is just the free abelian group on the objects of C, but for any cofiber sequence $X \to Y \to Z$ we impose the relation $[Y] = [X] + [Z]$. So $K_0(C)$ remembers the information in C that adds over cofiber sequences, just like the Euler characteristic. The higher K-groups do not admit such a nice description, but they often contain obstructions for classical problems, such as recognizing families of finite cell complexes and constructing families of diffeomorphisms.

It's helpful to know that the groups $K_n(C)$ are actually the homotopy groups of a spectrum, or infinite loop space, $K(C)$. In particular, they form an extraordinary cohomology theory. In fact, when we take the *algebraic* K-theory spectrum of the complex numbers $\mathbb{C}$, the resulting cohomology theory $K(\mathbb{C})^0(X)$ agrees with the more familiar *topological* K-theory, $K^0(X)$. This explains why the term "K-theory" is used in both contexts.

1.2. The introduction and setup. These algebraic K-groups are very difficult to compute, so we sometimes just focus on their relationship to each other and to simpler groups. As we remark in the introduction to [**Mal15**], we get an *assembly map*

$$H_n(BG; K(R)) \xrightarrow{\alpha} K_n(R[G])$$

for any ring or ring spectrum R, and any topological group G. We seek cases where this map is injective, since that allows us to build nontrivial classes in $K_n(R[G])$ by first constructing them on the left-hand side.

Unfortunately, in homotopy theory we don't have a lot of methods for proving injectivity. The best we can do is describe α as a map of spectra

$$BG_+ \wedge K(R) \xrightarrow{\alpha} K(R[G])$$

and then produce some other map $K(R[G]) \to X$ so that the composite

$$BG_+ \wedge K(R) \xrightarrow{\alpha} K(R[G]) \longrightarrow X$$

is an equivalence of spectra. This is more than enough to conclude that our abelian-group version of the assembly map is injective.

In [**Mal15**] we investigate one such technique. Recall that $K(R[G])$ is the K-theory of the category of $R[G]$-modules that are dualizable over $R[G]$. If we instead take the $R[G]$ modules that are dualizable as R-modules, we get a different category. The K-theory of this new category may be called $G(R[G])$, the *Swan theory* of $R[G]$. It is the K-theory of *representations* of G in the category of R-modules. This functor has been carefully studied in discrete cases such as $R = \mathbb{Z}$ (e.g. [**HTW88**]), but when R is a ring spectrum such as $\mathbb{S}$, it is relatively unexplored.

Using Swan theory we are able to produce a sequence of maps

$$BG_+ \wedge K(R) \xrightarrow{\text{assembly}} K(R[G]) \xrightarrow{\text{Cartan}} G(R[G]) \xrightarrow{\text{coassembly}} F(BG_+, K(R))$$

where F stands for function spectrum. Essentially, the *coassembly* map at the end is a map from Swan theory into a kind of *cohomology* of BG. This is exciting because the two outside terms are far smaller and more computable than the two terms on the inside. In particular, one might expect that this composite has an explicit description. This is actually the main theorem of the paper:

THEOREM 1.1. *When G is a finite group, the above composite is homotopic to the equivariant norm map*

$$K(R)_{hG} \longrightarrow K(R)^{hG}$$

on the spectrum $K(R)$ with a trivial G-action.

This equivariant norm map happens to be an equivalence after certain kinds of localization, so we get a new context in which the assembly map splits. That's it for the motivation; we'll spend the rest of this section digging into the central ideas of the proof.

1.3. Key ideas of the proof. To understand where assembly and coassembly really come from, we have to re-interpret what it means to be a module over $R[G]$.

KEY IDEA 1.2. *Topological groups G correspond to connected spaces X under the identifications $X \simeq BG$ and $G \simeq \Omega X$. Modules over $R[G]$ correspond to bundles of R-modules over X. The underlying R-module is the fiber, and the G action is the monodromy.*

This idea lets us re-imagine K-theory and Swan theory of $R[G]$, for fixed R and varying G, as functors on spaces X. We use the notation $A(X;R)$ and $\forall(X;R)$ when we think this way. The assembly map then has a neat interpretation, which leads to the definition of the coassembly map (described this way in [**Wil00**] as well):

KEY IDEA 1.3. *When thinking of modules as bundles, assembly is a co-descent map, or a homotopy colimit problem map. It is a universal approximation by a homology theory in spectra. Therefore there is also a descent map, or homotopy limit problem map, or homotopy sheafification, called coassembly.*

Now we know what assembly and coassembly are. The Cartan map is just an inclusion of categories, since every dualizable $R[G]$-module is automatically dualizable over R. But how do we evaluate the composite of these things?

In Section 6 of [**Mal15**], we start from the universal properties of assembly and coassembly and produce a pair of maps that have explicit, combinatorial descriptions. The following key idea captures the result in a rough form.

Key Idea 1.4. *The assembly map takes each R-module M to $G_+ \wedge M$ or $\bigoplus^G M$. The coassembly map takes each $R[G]$-module N to its underlying R-module. So their composite takes each module M to a (twisted) G-fold sum of copies of M.*

This can't be a full description because we don't see what the two extra copies of BG do. In essence, they give a $G \times G^{\mathrm{op}}$-monodromy on that G-fold sum, permuting the terms of the sum using the left and right actions of G on itself. It is maybe not so obvious why this re-interpretation of assembly and coassembly is possible, but in the next section of the user's guide, we will attempt to give an intuitive explanation.

Next, we try to cut the work down as much as possible to the case of $R = \mathbb{S}$, and from there to the K-theory of finite sets, which is the sphere spectrum. This is a fairly natural plan of attack:

Key Idea 1.5. *Any natural construction on the K-theory of all ring spectra is likely to be determined by what it does to $K(\mathbb{S})$, and that in turn is often governed by what happens on $K(\text{finite sets})$.*

To make this maxim actually true, we set up our models for K-theory so that they are functors of spaces X, but still have the usual smash product pairings. So, in Section 4 of [**Mal15**], we build some Waldhausen categories of parametrized spectra. The construction of such things is generally believed to be possible, but they are rarely ever written up explicitly, perhaps out of fear that the treatment would be as long as in [**MS06**].

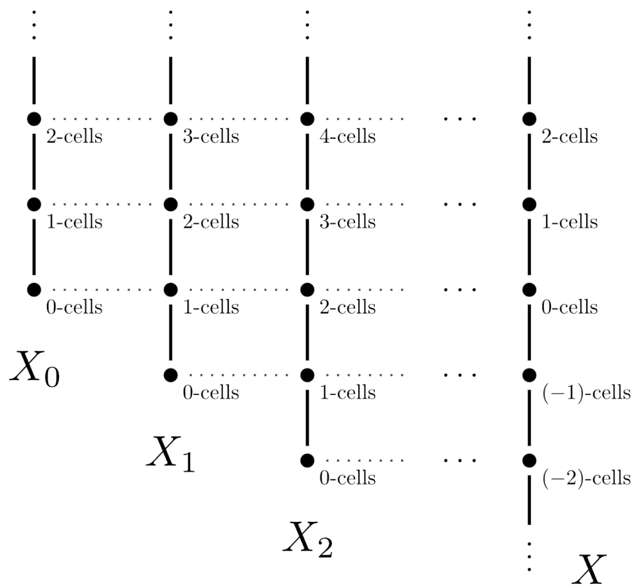
Finally, in Section 7 we show how to re-arrange some standard definitions of the transfer and norm maps so that they line up with the maps we saw in Idea 1.4 above. On the level of infinite loop spaces, we get a really nice and very classical picture of what a transfer is (compare with [**KP72**] and [**Ada78**]):

Key Idea 1.6. *Transfer maps are G-fold sums, where the individual terms of the sum may change their order as you move around the base. More specifically, if X is an E_∞ space, and we form a map $BG \times X \longrightarrow X$ by taking each point in X to a G-fold sum of copies of itself, with monodromy around BG that re-orders the terms of the sum, this gives a transfer map on the spectrum associated to X.*

Therefore the composite of assembly and coassembly is something that looks like a transfer. The bundle involved has base $BG \times BG$, and fiber G with $G \times G^{\mathrm{op}}$ acting by left and right multiplication, so its total space is BG again, which is not contractible. In truth, the map we have is a transfer from $BG \times BG$ up to BG, followed by a collapse of BG to a point. By a fun geometric argument with Pontryagin-Thom collapses, we can make this line up with the equivariant norm, and that's how the theorem is proven.

2. Metaphors and imagery

2.1. Spectra. It will be easiest if I begin with how to picture spectra. A spectrum is a sequence of based spaces X_0, X_1, X_2, ... with structure maps $\Sigma X_{n-1} \to X_n$. We'll assume that the spaces are cell complexes, and the maps are closed inclusions.



We arrange the spaces in a line as shown, and we think of X as their colimit. Their heights are staggered because each X_n is suspended before it is included into the next X_{n+1}.

We could say that X is made up of "elements," just like a set or an abelian group. An "element" of X is a point in the 0th space X_0, or a based loop $S^1 \to X_1$, or a based sphere $S^n \to X_n$ for any n. We can always increment n, but we only care about the behavior as $n \to \infty$. So an element of X is a collection of maps $S^n \to X_n$, for all sufficiently large n, that agree along the structure maps of X. Picture a sequence of spheres S^n inside the spaces X_n, each one casting a shadow S^{n+1} in X_{n+1} that lines up with the next sphere.

These elements can be added together. Thinking of them as maps out of spheres, we add them by pinching the sphere at the equator. The choice of pinch $S^n \to S^n \vee S^n$ is not unique, but it is more or less equivalent to choosing two points in $\mathbb{R}^n$, where n can grow as large as we like. So we get a contractible space of such addition maps. Therefore the addition is commutative up to homotopy in a very strong sense – to be more precise, the elements form an E_∞ space.

Of course, you can have elements of other degrees too. An element of degree $k \in \mathbb{Z}$ is a collection of maps $S^{n+k} \to X_n$ for varying n; we may imagine these elements living in the kth row of the diagram above. The kth homotopy group $\pi_k(X)$ can be elegantly described as the homotopy classes of degree k elements. A map of spectra is an equivalence when it induces isomorphisms on these homotopy groups.

With this language of "elements," the sphere spectrum $\mathbb{S}$ is just a free spectrum on one element of degree 0. Similarly, the n-sphere $\Sigma^n\mathbb{S} = \mathbb{S}^n$ is freely generated by a single element of degree n.

Since spectra are similar to abelian groups, it makes sense to talk about ring spectra and module spectra. A ring spectrum R has a multiplication, which takes two elements of degrees k and ℓ as input and returns an element of degree $k + \ell$ as output. It also has a unit, which is just a degree 0 element of R. Similarly, if M is a module over R, that means that an element of degree k in R and an element of degree ℓ in M give an element of degree $k + \ell$ in M, and the unit of R acts as the identity. (Of course, multiplication should be associative and distribute over addition, and indeed this happens up to a contractible set of choices.) Every ordinary ring R becomes a ring spectrum HR, which only has interesting elements in degree zero, given by the actual elements of R itself.

If we fix a ring spectrum R then it is easy to build some modules over it. I can take a finite number of copies of R, multiply them by discs, and glue them together along the boundaries of these discs, to make something like a cell complex. Then, if I want, I could cut down to some smaller module spectrum sitting inside the bigger one as a retract. Every module built this way is "perfect." This is the analogue of being a module that is finitely-generated and projective. Each cell in my complex plays the role of a generator or a relation, or sometimes a little bit of both. If I allowed infinitely many cells, then I could capture every R-module this way, up to equivalence.

Finally, if R is a ring spectrum and G is a topological group or monoid, there is a group ring spectrum $R[G]$, generated freely by R and by a degree 0 element for every point of G. Formally, the nth level of $R[G]$ is just $R_n \wedge G_+$.

As we mentioned in the first part of the user's guide, these group rings are closely connected to the study of parametrized spectra, or families of spectra that vary continuously over a base space B. Just picture a fiber bundle over B, each fiber of which is one of these spectra. The main idea is that spaces over B are essentially the same as spaces with an action of ΩB, so the category of parametrized spectra is essentially the same as the category of module spectra over the ring spectrum $\mathbb{S}[\Omega B]$.

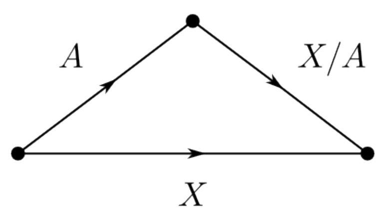
2.2. Algebraic K-theory. In [**Mal15**] we study the algebraic K-theory of a ring or ring spectrum R. This is a sequence of abelian groups $K_n(R)$, which are the homotopy groups of a spectrum $K(R)$. So we can think of $K_n(R)$ as the degree n "elements" of the spectrum $K(R)$, in the sense we discussed in the last section. Let's discuss what these elements look like.

We begin with the first space $K(R)_0$ in the spectrum $K(R)$. If R is a discrete ring, then $K(R)_0$ is the moduli space of all finitely-generated projective R-modules. A point in this space is an R-module, and a path between two points is an isomorphism of modules. Similarly, if R is a ring spectrum, then $K(R)_0$ is the moduli space of all perfect R-modules. A point is a perfect R-module, and a path between points is a weak equivalence of modules. Already, we can conclude that each perfect R-module M gives us some degree zero element $[M]$ of the spectrum $K(R)$.

When I move up to the space $K(R)_1$, each of these R-modules is now given by a based loop $S^1 \to K(R)_1$. Each isomorphism or weak equivalence of modules is now a homotopy of loops. But $K(R)_1$ is more than $\Sigma K(R)_0$, and the extra points do something neat. If I have a cofiber sequence of modules

$$A \longrightarrow X \longrightarrow X/A,$$

then each of the modules A, X, and X/A is a loop in $K(R)_1$. I add in a 2-simplex so that the composite of the loops for A and X/A is homotopic to the loop for X:



Then I add some higher-dimensional simplices in a similar way, to make the rest of $K(R)_1$. (If you want more detail, I'm thinking of Waldhausen's $\mathcal{S}_\cdot$-construction [**Wal85**].) As a result, in the spectrum $K(R)$, the elements $[A]$ and $[X/A]$ now sum to $[X]$, up to homotopy. This is *additivity*, the fundamental property of algebraic K-theory.

Additivity gives conceptual meaning to the addition of elements of $K(R)$: now $[A] + [B]$ is equivalent to $[A \vee B]$, and to any $[X]$ with a cofiber sequence $A \to X \to B$. This means that $K(R)$ is a certain kind of "group completion" of the space of R-modules.

I can repeat this process in a reasonable way to get $K(R)_2$ from $K(R)_1$, and so on. It turns out that after $K(R)_1$, nothing else interesting happens to our elements, so I can read off all the K-theory groups by taking the homotopy groups of the space $\Omega K(R)_1$.

There is one final important point. This $\mathcal{S}_\cdot$ construction on the category of perfect R-modules builds a tremendously large space. You could say this is the reason why K-theory groups are hard to compute. We don't have a nice, small model to calculate them, as we do in more basic problems in algebraic topology, like computing the homology of a torus.

2.3. Assembly and coassembly. Now that we know what $K(R)$ is, we'll try to understand how $K(R)$ is connected to $K(R[G])$ when G is a group. The relationship between them is the *assembly map*

$$BG_+ \wedge K(R) \longrightarrow K(R[G])$$

We picture the left-hand side as being much smaller than the right-hand side, though in truth it is still quite big.

There are two ways to understand the assembly map, and in the paper we explicitly prove that they give the same thing. The first and more classical perspective is this. Recall that a G-torsor is a left G-space that is isomorphic to G itself. We can interpret BG as the moduli space of all G-torsors, and $K(R)_0$ as the moduli space of R-modules. Given an R-module M and G-torsor $\widetilde{G}$, the tensor product

$$M \wedge \widetilde{G}_+ = \bigoplus^{\widetilde{G}} M$$

is a module over $R[G]$. This gives the assembly map $BG_+ \wedge K(R) \longrightarrow K(R[G])$ at spectrum level 0. It extends in a reasonable way to the higher levels of the spectrum as well. And if we want to think in terms of matrices, rather than modules and torsors, the assembly map sends a matrix $A \in GL_n(R)$ and an element $g \in G$ to a matrix in $GL_n(R[G])$ where every entry of A has been multiplied by g.

The second perspective is more homotopy-theoretic. Suppose F is a functor from unbased spaces to spectra. Each point of X gives a map $* \to X$. Apply F to get a map $F(*) \to F(X)$. When F is nice (either topological or a homotopy functor) then these maps $F(*) \to F(X)$ "assemble" together into a map

$$X_+ \wedge F(*) \xrightarrow{\alpha} F(X)$$

Why does this apply to $K(R[G])$? Remember that $R[G]$-modules are bundles of R-modules over BG. So I can think of $K(R[G])$ as a group-completed moduli space, the space of bundles of R-modules over BG, which came from perfect $R[G]$-modules. This extends to a functor on other spaces too: for each space X we take the bundles of R-modules over X. Given a map $X \to Y$, and a bundle E of modules over X, I can extend E to $\widetilde{E}$ over Y while keeping its total space weakly equivalent to that of E. This rule allows us to define an assembly map as above.

We can see why this gives the same assembly map. If M is an R-module, then for any point $b \in BG$, I can try to make a bundle whose total space is equivalent to just M sitting above the point b. The way to do this is to thicken the map $* \to BG$ to the bundle $EG \to BG$, then multiply by M to get the bundle $M \times EG$. If we let the point b vary, then EG itself does not change, but our choice of basepoint $* \in EG$ does vary. If b goes around a closed loop corresponding to $g \in G$, the basepoint of EG changes by multiplication by G. Imagine pulling $\bigoplus^G M$ around this loop in the bundle, and when we get back to the basepoint, we end up with a map

$$\bigoplus^{G} M \longrightarrow \bigoplus^{G} M$$

which multiplies on the left by G. So our bundle has fiber $\bigoplus^G M$ and monodromy given by left multiplication. It comes from the $R[G]$-module $\bigoplus^G M$, which is exactly the module we described in the classical version of the assembly map.

This entire discussion can be dualized, and the dual story is shorter. Any bundle over BG can be restricted to a smaller subspace, giving another bundle. This allows us to define a *coassembly map*

$$K(R[G]) \longrightarrow \mathrm{Map}_*(BG_+, K(R))$$

Each point in $K(R[G])$ is a bundle of modules E over BG. Given such a bundle, and a point $b \in BG$, we take the fiber E_b. That is the complete mental picture of the coassembly map.

Unfortunately we are lying a bit, because $K(R[G])$ isn't really a contravariant functor. The problem is with the finiteness conditions: the fiber E_b is certainly an R-module, but it may not be perfect. So this coassembly map isn't always defined.

If G happens to be a finite group, then E_b is finite, so coassembly is defined. More generally, coassembly is defined on those bundles of modules whose *fibers* are finite. So, we are studying $R[G]$-modules whose *underlying* R-module is finite. The K-theory of such things is the Swan theory $G^R(R[G])$. There is always a coassembly map

$$G^R(R[G]) \longrightarrow \mathrm{Map}_*(BG_+, K(R))$$

When G is finite, every finite $R[G]$-module is a finite R-module, so $K(R[G])$ includes into $G^R(R[G])$. This inclusion goes under the fancy name of the *Cartan map*.

2.4. The transfer and the norm. The main theorem of [**Mal15**] identifies the composite of the assembly and coassembly maps as a norm, so we end by painting a picture of transfer and norm maps.

Suppose we have two spectra X and Y, and a map $X \to Y$ whose fibers are finite sets. We would like to define a map $Y \to X$ going the other way. It would be nice if we could continuously choose, for each element of Y, some preimage in X. But this is usually impossible; most covering spaces do not admit a section. So instead, we take each element of Y to the sum of all its preimages in X. This eliminates all choices, and it is possible because the elements can be added together in a somewhat commutative way.

More specifically, suppose G is a finite group, X is a spectrum with a free left G-action (that is, free away from the basepoint), and $Y = X_G$ is the orbits. I "define" the transfer $Y \to X$ by taking each element of Y to the sum of its

G preimages in X. We have to be careful, because the sum of elements is only defined up to a choice of point in some contractible space. In particular, we have a rule for how to sum together $|G|$ different maps, each time I pick an embedding of G into $\mathbb{R}^\infty$. So for each element $y \in Y$, I should choose some embedding $p^{-1}(y) \to \mathbb{R}^\infty$ to define this sum, but I have to make these choices in a continuous way. In particular, if I pass around a loop based at y, the set of embedded points will move through $\mathbb{R}^\infty$and come back to itself, but each point x inside the set will travel to gx, for some fixed $g \in G$.

So my rule associates to each $y \in Y$ the G-fold sum of its preimages in X, but this rule has a twisting, or monodromy, as I rove around Y. It seems that I could just pick the rule once, and then change it by a G-action as I rove around Y. So let's pick a map of spectra

$$p : \mathbb{S} \longrightarrow \Sigma^\infty_+ G$$

It should be equivariant with respect to G acting on the right, and it should send the degree 0 generator on the left to the sum of the $|G|$ distinct degree 0 generators on the right. This isn't quite possible on the nose, but it becomes possible if we allow ourselves to replace $\Sigma^\infty_+ G$ up to equivalence. Anyway, this map p is called the *pretransfer*. Once I have the pretransfer, I use it on every point of Y, only changing it by multiplication by G as I rove around Y. In other words, I smash the pretransfer with the identity map of X, and divide out by the G-action:

$$Y \cong \mathbb{S} \wedge_G X \longrightarrow \Sigma^\infty_+ G \wedge_G X \cong X$$

The resulting map is the *equivariant transfer*.

If instead $X \to Y$ were a covering space with fiber $\underline{n} = \{1, \ldots, n\}$ (away from the basepoint), then I could express

$$X = \widetilde{X} \wedge_{\Sigma_n} \underline{n},$$

where $\widetilde{X}$ has a free Σ_n-action and $Y = \widetilde{X}_{\Sigma_n}$. Then I could do essentially the same thing as above, except that the pretransfer is a Σ_n-equivariant map $\mathbb{S} \to \Sigma^\infty_+ \underline{n}$. This recipe gives the classical transfer map.

Returning to the case where the fiber of $X \to Y$ is G, we observe that if we sum up all the points in a single orbit of X, that sum should really be fixed by the action of G. So the transfer map $Y \to X$ actually factors through the homotopy fixed points X^{hG}. The resulting map

$$X_{hG} \longrightarrow X^{hG}$$

is the *equivariant norm map*. It takes each orbit of X to the sum of the points in that orbit, regarded as a point of X which is fixed under the G-action.

Let's tie the pictures from the last four sections together, to see why our theorem should be true. If I apply the assembly and coassembly maps to $BG_+ \wedge K(R)$, the element $[M]$ is sent to $[\bigoplus^G M]$. But this agrees with the G-fold sum of the element $[M]$, by the additivity of K-theory. The transfer and norm maps are given by a similar sum, so we are led to guess that assembly and coassembly give some sort of transfer. This is more than just intuition; it is a rough outline of the proof!

3. Story of the development

The paper [**Mal15**] is part of a larger project to understand the behavior of the coassembly map for the K-theory of bundles and representations. This project started in the summer of 2012, when I attended the West Coast Algebraic Topology Summer School (WCATSS). I was impressed by the things we could actually say about $A(X)$, and consequently about diffeomorphisms of manifolds, and how Goodwillie calculus and linear approximations seemed to play such an essential role.

At the same time, I was also finishing a project on how Goodwillie calculus works for contravariant functors, and it seemed natural to ask what happened when you applied this kind of thinking to the contravariant analogue of Waldhausen's construction, $\forall(X)$. One gets a natural tower of "polynomial approximations" to $\forall(X)$, but it turns out to be degenerate. We get the coassembly map at level one, which is highly interesting, but after that, the higher-order approximations give nothing more than the coassembly map. This seemed suggestive, and I believed for a short time that the coassembly map was an isomorphism. I discussed these ideas with John Klein and Bruce Williams, and learned that this was not the case. Later on, I had a very fun week with my office mate Daniel Litt doing a rough computation for $\pi_0 \forall(S^1)$ and seeing how false this claim was.

There is still, however, a grain of truth: $\forall(X)$ takes the moduli space of fibrations over X with finite fibers, and applies the kind of "group completion" which splits cofiber sequences of such fibrations. But the moduli space itself is

$$\mathrm{Map}\left(X, \coprod_{[F]} B\mathrm{haut}(F) \right)$$

where $[F]$ ranges over weak equivalence classes of finite CW complexes F, and this is indeed excisive. Somehow, though group completion makes this space smaller and easier to understand overall, it breaks the property of excision.

In the fall of 2012 I discussed these ideas with my advisor Ralph Cohen, and we formulated a "dual Novikov conjecture" for $\forall$-theory. Our conjecture stated that the coassembly map is rationally split surjective when X is a finite CW complex that is also a $K(G,1)$. We also formulated a "strong dual Novikov" conjecture, which made the same claim for the functor $K(DX)$. As the names suggest, the strong conjecture implies the weak one.

I studied the strong form of this conjecture through the academic year 2012-2013. Over the course of the year, I learned the constructions of topological Hochschild homology (THH) and topological cyclic homology (TC). I solidified my understanding of G-spectra and of p-completion, both of which are essential for understanding TC computations. I even attended a course by Gunnar Carlsson on G-spectra and the Segal conjecture.

The process was very slow: long sessions of sitting at home, in an airport, at a friend's house in another city, early in the morning when they were asleep, trying to write down relations and conjectures, realizing that they were inconsistent, erasing them and starting again. I would try to piece together how genuine fixed points interact with Spanier-Whitehead duality, or how to relate the tom Dieck splitting to the restriction and Frobenius maps of THH. I would stare at incomprehensible papers, make laughably naïve guesses as to what was going on, prove the guesses were wrong, make slightly less wrong guesses, and continue.

Over time, my guesses became more and more correct, and my confidence improved. It was very gratifying, this feeling of tackling a very arcane subject, and sinking into it until you really start to "get it." Even better, by the end of the year, I produced an actual computation of $TC(DS^1)$. And I was shocked to find that our strong dual Novikov conjecture was false!

This computation gave the TC of a very small category of modules over $\mathbb{S}[\mathbb{Z}]$. In the summer of 2013, I began considering whether the computation could be expanded to the TC of a somewhat larger category, in order to get some evidence for or against the dual Novikov conjecture for $\forall(S^1)$. The most natural modules to consider are the $\mathbb{S}[\mathbb{Z}/n]$ for varying n, and they each come with an "assembly" map into the THH of the larger module category. I did some geometrically-flavored calculations of how coassembly worked for those modules, using parametrized spectra, and the results were surprisingly understandable compared to earlier calculations. In fact the maps that appeared were reminiscent of the maps of the Segal conjecture equivalence

$$\left(\bigvee_{(H)\leq G} \Sigma^\infty BWH_+\right)^\wedge_p \xrightarrow{\sim} F(BG_+,\mathbb{S})^\wedge_p$$

Excited by this connection, I switched my attention to coassembly for $\forall(BG)$ with G a finite group. I worked harder than usual, since I was applying for jobs that fall, but everything seemed to come together. By the end of the summer, I proved that the composite of some assembly maps with coassembly on THH did indeed give the equivalence of the Segal conjecture. As a consequence, the THH coassembly map for BG was split surjective after p-completion. So something like the dual Novikov conjecture was true $-$ but it was wildly different from our original claim. The final push happened while I was visiting Vanderbilt and my wife's family in August. I can clearly remember pacing through the balmy night air, putting together a geometric picture of what a transfer map really is. At one point I had a proof that it was a transfer, and I only had to calculate the monodromy; when the monodromy turned out to be correct, I was ecstatic.

However the project soon took a disappointing turn. I believed that I had proven a similar splitting for K-theory. But this was wrong, because the Segal conjecture does not apply to the non-finite spectrum $A(*)$. Even worse, the THH result did not even lift to the level of TC, because $F(BG, \mathbb{S})$ is not a cyclotomic spectrum. I became worried that the THH argument would say little to nothing about K theory, and this particular project stayed mostly inert for the rest of my time as a graduate student at Stanford. I did learn how to reinterpret my maps as the equivariant norm map, and I began to believe that the composite on K-theory was also a norm map.

In November of 2014, shortly after the start of my postdoc at UIUC, I had a very productive visit to Notre Dame, discussing many of these ideas with Bruce Williams and getting many more. I became inspired after a conversation with Mark Behrens, because what I had proven so far was enough to conclude that the assembly map splits after $K(n)$ localization. My postdoc mentor Randy McCarthy gave me the wonderful idea of lifting the argument to the level of finite sets, and that was the last conceptual hurdle.

It still took a few months to write the paper, for a few reasons. First, I realized that the definition of $\mathbb{V}(X)$ for spaces and for spectra do not agree, and I had a long, productive conversation about this with John Klein and Bruce Williams. Second, I found a new argument that would work at the level of K-theory and not THH, which was much cleaner. Third, I had been wanting to write about how to build good Waldhausen categories of parametrized spectra, and this seemed to be the right time to do it. Finally, I spent almost a month writing careful proofs that various kinds of transfers were the same, so that I could state the result with confidence. The paper was posted to the arXiv in March of 2015, and submitted for publication later that year.

The point, which I imagine every mathematician already knows, is that a lot of hidden work goes into most papers. Papers have a long history, with many moments of joy and heartbreak. Most of the promising thoughts, ideas, and calculations become dead ends. Sometimes even several months worth of work can suddenly become useless. But every once in a while, a stray thought or calculation will lead you in a new, completely unexpected direction. I suppose the best we can do as mathematicians is to keep an open mind, and let the winds and currents of mathematics take us wherever they go.

4. Colloquial summary

I'm going to focus on the subject of topology as a whole, before zooming in on the ideas of the paper [**Mal15**].

In topology, we study shapes. You already have some idea of what a shape is: squares, circles, triangles, silhouettes of dogs, etc. are all two-dimensional shapes. There are three-dimensional shapes too, like cubes, cylinders, and helixes. It's possible to study shapes in higher dimensions as well. You might think that the study of higher-dimensional shapes is really cool and mind-blowing, or you might suspect that it's silly and pointless. But in truth, these higher-dimensional shapes are not as hard to understand as you might think, and they're also pretty important. Though we only have three dimensions of space, any mathematical model that uses 4 coordinates is actually a system that lives in abstract, four-dimensional space. If I study cancer patients, and I measure ten characteristics of each patient, those patients become data points inside a 10-dimensional space. I can work with such shapes without blowing my mind, because each point in 10-dimensional space is just a list of 10 numbers, and that's not so bad. On the other hand, it can be pretty important to understand the shape that these points form. I might learn something new about cancer by studying it closely.

Topologists have lots of fancy techniques for studying and quantifying shapes. What do we do with these tools? You might imagine that we apply them to one shape at a time, as part of a quest to "understand all shapes." But this is not quite what we do, for two reasons.

First, there really are a tremendous number of different shapes out there, just as there are many flowers in a meadow. If you've never seen the flowers in some particular area before, it can be a lot of fun to examine a few of them very closely. But carefully observing one hundred or one thousand of them would be quite a bore. Instead, you would start thinking about how the flowers in this area are different from the flowers in that other meadow, or how they're different from last year. Similarly, when we learn a new tool in topology, it's a lot of fun to try it out on a few examples. But as we dig deeply into the subject, we don't simply keep applying the same tools to more and more examples. Instead, we focus on these larger-scale patterns.

Secondly, shapes turn out to be very complicated, and even our best tools aren't powerful enough to give complete answers to the simplest questions. So even the most basic shapes, like the sphere, are not completely understood. This is both frustrating and exciting, because sometimes after a tremendous amount of work you really can understand these examples better.

We also think about much more than just spheres. We think about some really tremendous, really huge shapes. Usually we build them by taking a bunch of triangles, and tetrahedra, and so on, and we describe some recipe that says how to glue all of these pieces together. This might seem like a strange thing to do. But the shapes we build this way are much easier to study, and in some sense, every shape out there looks an awful lot like one of the shapes that we can build out of triangles.

Sometimes the shape you want to understand was created for some ulterior purpose. There is a function, the Riemann zeta function, that contains astoundingly deep information about prime numbers. This is unexpected, because prime numbers are just whole numbers, you wouldn't expect them to be related to a smooth function. Similarly, there are some *shapes* that contain an unexpected amount of information about the primes. One of these shapes is called the "algebraic K-theory space of the integers." You only really need to know that this is indeed a shape, and it is quite hard to describe explicitly, but if you were able to understand all of its features, you would learn some difficult facts about prime numbers. The subject of *algebraic K-theory* builds lots of shapes like this. They contain really interesting information, but they are super hard to figure out.

In the paper [**Mal15**], we study something called the *K-theory assembly map*. You can think of it as a relationship between two of these shapes: one smaller, simpler shape that gets folded and deformed before it is stuffed into a larger, more mysterious shape. We are trying to understand something about this folding process. We would really like to show that, somehow, the small shape does not get destroyed beyond recognition as it is stuffed into the bigger shape. This is hard though. We don't have a small, simple picture of what's happening in the folding process. Instead, these shapes are formed by gluing together, say, thousands of triangles according to complex and arcane rules. So we can't really visualize directly what is happening to them. Instead, we have to pay attention to these gluing rules, and do some detective work to figure out how they behave as they get folded up. This is still hard though. The larger, second shape is much harder to quantify than the smaller, first shape.

However, we have a trick: we construct a third shape that will fit the second inside. Now the first shape is contained in the second, which is contained in the third, like a sequence of Russian dolls. Moreover, the first and last shapes are much easier to describe than the second. So our detective work gets a lot easier. We can show that, to some extent, that first shape was not destroyed beyond recognition, and so we understand these relationships a little bit better.

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