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

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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 reminiscient 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 $\mathbb{V}(BG)$ 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.

References

[Mal15] C. Malkiewich, *Coassembly and the K-theory of finite groups*, arXiv preprint arXiv:1503.06504 (2015).

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