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A user's guide: Relative Thom spectra via operadic Kan extensions

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3. Story of the development

Before we dive in to the story of this particular paper, it's necessary to give a bit of background. When I got to graduate school in 2010, I only knew that I liked category theory and that Grothendieck seemed cool. After my first year I asked Jack Morava to be my advisor, primarily because, like Grothendieck, he seemed cool. What I didn't know at the time was that Jack, with his visionary ideas about the structure of the stable homotopy category, was effectively the father of what came to be called *chromatic homotopy theory*. The original motivation for my thesis, and thus [**Bea17**], came entirely from this chromatic realm.

Chromatic homotopy theory amounts to the realization that the stable homotopy groups of spheres, and subsequently the category of finite CW complexes, can be *stratified* into layers that look like the height stratification on the moduli stack of formal groups. These strata are the "colors" that give chromatic homotopy theory its name. Ravenel had made several conjectures about this structure in [**Rav84**], the most famous being the so-called Nilpotence, Periodicity and Thick Subcategory conjectures. These were later proven in [**DHS88**] and [**HS98**].

The only thing the casual reader needs to know about these conjectures, which are now theorems, is that they: (i) tell us that if X is a finite cell complex and $\alpha \in \pi_*(X)$ then α is nilpotent if and only if $MU_*(\alpha)$ is trivial, and (ii) the homotopy category of finite cell complexes is stratified into so-called "thick" subcategories that directly mirror the structure of the moduli stack of formal group laws. It's this last statement that always struck me as somewhat miraculous. We have two things that are by all appearances completely unrelated: finite cell complexes and group structures on formal schemes (which come up in number theory). The Thick Subcategory Theorem however tells us that there is a deep (and still mysterious!) link between them. Understanding this connection is the true motivation for [**Bea17**].

As for the paper that I wrote, its genesis depends on the fact that I had a thesis advisor that more or less let me do whatever I wanted. This was both a blessing and a curse. It allowed me to avoid doing any of the really unpleasant homological algebra that is the bread and butter of algebraic topology but it also meant that after about 4 years in graduate school and 3 failed thesis projects, I was in a bit of a bind (one reason being that I wasn't very good at hard homological algebra...). I knew that I wanted to try to say something about stable homotopy theory and its relationship to algebraic geometry via the Nilpotence Theorem of [**DHS88**], but I didn't know how.

Moderately frantic conversations with my advisor and my de facto co-advisor Andrew Salch, led me to the following question: what is it about the Nilpotence Theorem that is essentially algebro-geometric or category theoretic? In other words, given an arbitrary symmetric monoidal ∞-category $\mathcal{C}$, how many of Ravenel's conjectures can we even state, much less prove, in $\mathcal{C}$? Going back to Ravenel's so-called "Orange Book" [**Rav92**], I found that the proofs of the Nilpotence and Periodicity conjectures relied heavily on a certain sequence of spectra, called $X(n)$ by Ravenel, whose colimit was the complex cobordism spectrum MU. In particular, Devinatz et. al. showed that for any ring spectrum R, if an element $\alpha \in R_*$ is trivial in $X(n)_*(R)$, then it is nilpotent in $X(n-1)_*(R)$. From this they proved the Nilpotence Theorem, which is central to proving the Periodicity Theorem and the Thick Subcategory Theorem.

My quest then became to understand what was really happening between $X(n)$ and $X(n+1)$. Crucial to the story here is the fact that a map of ring spectra $X(n) \to R$ determines a formal group law structure on R_* *modulo degree n terms*. Thus they interpolate between the unit map $\mathbb{S} \to R$ and a complex orientation $MU \to R$. Andrew Salch pointed out that Lazard, in proving that the Lazard ring is polynomial [**Laz75**], had passed from the classifying object for n-truncated formal group laws to $n+1$-truncated formal group laws by doing a sort of deformation theory. Jack Morava pointed out to me that this might be a Galois theoretic or descent theoretic type of construction, and suggested I look into [**Hes10**] and [**Rog08**].

I hypothesized that the unit maps $\mathbb{S} \to X(n)$ were so-called Hopf-Galois extensions like MU (as Rognes had noticed in [**Rog08**]). It's pretty clear that this is true, since the salient feature of being a Hopf-Galois extension is admitting an equivalence $X(n) \wedge X(n) \simeq X(n) \wedge Z$ for some "spectral bialgebra" Z. Since $X(n)$ is a Thom spectrum this is pretty clearly true, with $Z = \Omega SU(n)$. However, there was a problem, because Rognes had worked only with $\mathbb{E}_\infty$-ring spectra and the $X(n)$ were only known to be $\mathbb{E}_2$. It seemed like a shot in the dark, but rather than try to recover everything Rognes had done, but for $\mathbb{E}_n$-ring spectra, I went to Google and typed in "Hopf-Galois extensions of associative ring spectra." To my indescribable delight, I found that Fridolin Roth, a student of Birgit Richter, had written his thesis [**Rot09**] on exactly this topic!

It followed immediately from [**Rot09**] that the maps $\mathbb{S} \to X(n)$ were Hopf-Galois extensions of (at least $\mathbb{E}_1$) ring spectra. A very natural question to ask, then, is whether or not each of the intermediate maps $X(n-1) \to X(n)$ are also some kind of Hopf-Galois extensions. This turns out to be true, and the Hopf-algebras controlling the extensions are $\mathbb{S}[\Omega S^{2n+1}]$. This is proven in an unpublished paper I wrote. I was able to prove this by simply calculating the homology of everything in sight, e.g. $X(n) \wedge_{X(n-1)} X(n)$, $X(n) \wedge \mathbb{S}[\Omega S^{2n+1}]$, and the maps in between them. One of the reasons that it's unpublished is that the proof felt unsatisfactory and like it missed some very natural structure. In particular, the equivalence $X(n) \wedge_{X(n-1)} X(n) \simeq X(n) \wedge \mathbb{S}[\Omega S^{2n+1}]$ looks a lot like the generators of $H_*(X(n-1);\mathbb{Z})$ are "canceling out" corresponding generators in $H_*(X(n);\mathbb{Z})$ and the only thing left is the top dimensional class. Ultimately, [**Bea17**] was an attempt to see this structure on the level of the spectra themselves, rather than just calculationally.

In my attempts to prove something like the main theorem, I spent a lot of time looking at [**Mah79**] and [**ABG$^+$14**], trying to understand the Thom diagonal as a sort of "shear map," similar to the classical group isomorphism $G \times_H G \cong G \times G/H$ for $H \lhd G$. Eventually I realized that I would get this map for free by simply producing $X(n)$ as a Thom spectrum *over* $X(n-1)$. In other words, I needed to produce a map (preferably of $\mathbb{E}_1$-spaces) $\Omega S^{2n+1} \to BGL_1(X(n-1))$ such that the colimit of the composition $\Omega S^{2n+1} \to BGL_1(X(n-1)) \to LMod_{X(n-1)}$ is equivalent to $X(n)$. Because the theory of [**ABG$^+$14**] produces a Thom isomorphism and Thom diagonal for Thom spectra over *any* ring spectrum, this would give me the structure I wanted.

Rereading (parts of) [**ABG$^+$14**], I started to really understand the way in which they were describing Thom spectra as *quotients*. It finally clicked that we can think of $X(n-1)$ as $\mathbb{S}/\Omega^2 SU(n-1)$, and $X(n)$ as $\mathbb{S}/\Omega^2 SU(n)$ and that the fibration $\Omega^2 SU(n) \to \Omega^2 SU(n) \to \Omega^2 S^{2n+1}$ is telling us that we should be able to go from $X(n-1)$ to $X(n)$ by just quotienting by *more*. I spent some time trying to think about *iterated homotopy quotients* in the form of bar constructions before I asked about this construction on MathOverflow. And just as I suspected, someone had indeed thought about this before. In particular, Qiaochu Yuan pointed out to me that these quotients can be described as Kan extensions and that iterated quotients, i.e. $(\mathbb{S}/H)/(G/H)$ for a group G with a subgroup H, can be described as iterated Kan extensions [**Yua15**]. I had to use Lurie's *operadic* Kan extensions to retain the $\mathbb{E}_n$-monoidal structure, but from that point on it was mostly just working out technical quasicategorical details!

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