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A user's guide: Monoidal Bousfield localizations and algebras over operads

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3. Story of the development

Research in mathematics can be tumultuous. A mathematician is fundamentally a truth-seeker, but until the truth is found (s)he must hold conflicting possible truths in mind simultaneously (e.g. when deciding whether to look for a proof or a counterexample). This is especially true in graduate school, before one has properly developed the appropriate gut instinct for whether a statement is true or false, or for how difficult the statement will be to prove. Graduate school can also be difficult for other reasons: students don't know whether or not they'll be successful in completing the degree, whether or not they'll get a job afterwards, where they might end up living, or what other responsibilities life might send them. In addition, graduate school is a time to learn and obtain expert level knowledge in the discipline, but this can be difficult because in many cases there are too many references to possibly read and sometimes things are well-known but not written down anywhere.

In this section I will tell the story of my PhD thesis in the hope that it might help a future graduate student understand a bit of the process, especially the the challenges, doubts and questions most grad students face. This section also contains context for [**Whi14b**], information about how some of the ideas arose, and tips for making the most out of your time in graduate school.

3.1. Story of the problem.

I began working on [**Whi14b**] in August 2011, and it was not my first PhD project. During my third year in graduate school (2010-2011) my advisor Mark Hovey gave me my first project, to extend his work with Keir Lockridge and compute the homological dimension of the real (resp. complex) K-theory spectrum KO (resp. KU). I spent months learning the requisite background but found myself completely unable to make headway on the problem. Hovey and Lockridge already knew in 2009 that $2 \leq dim(KU) \leq 3$ and that $4 \leq dim(KO) \leq 5$, and that remains the state of knowledge today (here dim means global dimension as a ring spectrum).

As attempt after attempt failed, I became frustrated and devoted increasing amounts of time to my master's research in computer science with Danny Krizanc, which was just getting underway. I found that research much easier, and for about a month I began all my meetings with Mark by confessing I had nothing new to report about K-theory but had managed to prove some fact or other about navigation algorithms for autonomous agents moving on a graph. It amazes me that Mark let me get away with this, but I am very grateful he did. The side-project in computer science helped me rebuild my confidence and without my computer science degree I would not have the job I have today. In July of 2011 we decided to make one more concerted effort to resolve the dimension problem, and I made the following conjecture

Conjecture 3.1. If a localization $R \to R[v^{-1}]$ is sufficiently nice then it cannot reduce global dimension by more than 1.

This conjecture would resolve the question for both KO and KU at once, since the only way for them to differ by 1 would be if $dim(KU) = 3$ and $dim(KO) = 4$. This really felt like the right idea to me: it would be an elegant solution, it would involve formal arguments rather than spectral sequence computations, and it was borne out by examples in the setting of pure algebra (rather than the setting of ring spectra, sometimes referred to as "Brave New Algebra"). Sadly, after more than a month of working on this as hard as I could, I had nothing to show and we decided to find another thesis project. It must have been this experience that made Mark realize that I should be working on a problem featuring localization.

Over the next month I began to work out various facts about Hovey's new theory of Smith ideals of ring spectra, which had only just been defined and therefore seemed a perfect thesis topic to me (almost nothing was known and there was no chance of competition). However, after attending a conference in Germany in August, 2011 Mark returned to Wesleyan, called me into his office and excitedly told me he had found my thesis problem. His exact words were: "Something I thought always works turns out not to in an exciting new example. You are going to figure out why and to find conditions to make it work." The example is now Example 5.7 in [**Whi14b**] and Mark had learned it from a talk by Mike Hill. To summarize it:

Example 3.2. There is a stable localization of equivariant spectra which does not preserve commutative ring spectrum structure.

My paper [**Whi14b**] finds conditions on a model category and on a set of maps one wishes to invert (via Bousfield localization) so that this preservation does occur. Specializing to equivariant spectra, these conditions tell us which maps we can invert without losing commutativity, and demonstrate that the example is "maximally bad" in the sense that it destroys as much commutative structure as it possibly could while still being stable (here stable means with respect to the monoidal unit, not with respect to all representation spheres, and that's part of the problem). This was an excellent thesis problem: it had a concrete application at the end, related to one of the most exciting results in recent years (the Kervaire theorem of [**HHR15**]), and the solution allowed me to do some cool work in model categories of independent interest. For instance, it required me to work out when commutative monoids inherit a model structure (a problem that had been open for 15 years, since [**SS00**]) and then to work out when Bousfield localization respects monoidal structure (a kind of join of Hirschhorn's book [**Hir03**] on localization with the chapter of Hovey's book [**Hov99**] on monoidal model categories).

3.2. Story of the development of results. In the fall of 2011, I simultaneously read [**Hir03**], figured out conditions so that localization would preserve the pushout product axiom, and (with help from Mark) figured out sufficient conditions so that the localization would preserve monoid structure (preservation for commutative monoid structure only came later). Over the winter I used a new condition Mark came up with regarding something he called *homotopical cofibrations* to prove a result about preservation of the monoid axiom, though I felt the hypotheses were restrictively strong. I made pleasantly steady progress throughout the 2011-2012 year, with new results at almost every meeting with my advisor. Since he was Department Chair we met once every two weeks, sometimes with a formal meeting in between if he had time, or with hallway conversations about model categories whenever the opportunity arose.

During this year I organized the Wesleyan Topology Seminar and met many of the experts in the area. Everyone I invited responded favorably, and all who knew my advisor expressed their excitement at seeing him again. When they came I learned the pleasure of discussing research with experts from an array of backgrounds, and I also got to know my advisor better through dinners and conversations with the speakers; we began to become friends. Through these speakers I got new references to read, potential applications for my work, and their extremely valuable first impressions and flashes of insight on my research program. This experience led me to seek out experts whose papers I had read and engage them at conferences and through email. Those experiences in turn helped me hone my "elevator pitch" for my research: I learned the quickest way to describe my results, how to make the results sound interesting, which questions to expect, and which questions to ask so that I could move the research forward.

In addition to conducting the research in [**Whi14b**] and organizing the seminar, during this time I also wrote my master's thesis in computer science, and began in the spring to give talks in various seminars (organizing a seminar is also a great way to get invited to speak in other seminars). I found myself somewhat exhausted going into our spring break. I sent my master's committee my 80 page thesis and then took a two week vacation in the south of France to visit my then girlfriend. We decided to take a long weekend in Barcelona in the middle of this vacation.

I had recently read several papers by a well-known mathematician in Barcelona named Carles Casacuberta, who had done work related to localization and preservation of algebra structure years before. When I found this work in November of 2011 I worried that it might subsume my project, but Mark convinced me that they were different, for reasons which are now spelled out in [**Whi14b**] at several points. I wrote to Carles to ask if we could meet for coffee, figuring this would be another way to get feedback as with the Wesleyan Topology Seminar. He responded by inviting me to give a talk and then taking me out to a very fancy lunch with his postdoc (and former student) Javier Gutiérrez. The conversation went so well that Carles invited me to come back for the summer of 2013, and now both Carles and Javier are co-authors of mine (on different projects).

True to form, within a few minutes of the end of my talk, Carles's first instinct was spot on and gave me an idea which greatly influenced my research program. He remarked that my preservation result (Theorem 3.2 in [**Whi14b**]) was general enough to hold for colored operads, not just for associative and commutative monoids. I enthusiastically agreed even though at the time I had no idea what a colored operad was. It wasn't until a full year later that I really understood the story for operads (now Section 6.6 in [**Whi14c**]) and another year after that till I understood the version for colored operads (which has appeared in [**WY15**]).

In the spring of 2012 I focused on the situation for commutative monoids, and I discovered the commutative monoid axiom in May, just after defending my master's thesis. Marcy Robertson was our visitor that week and I distinctly remember Mark excitedly going down the hallway to check the new condition with her and see if it was likely to be satisfied in examples of interest. I spent the summer doing some extremely technical work related to proving that it suffices to check the commutative monoid axiom on generating (trivial) cofibrations (now appendix A of [**Whi14a**]), finding out when Bousfield localization preserves the commutative monoid axiom (now Section 6 of [**Whi14b**]), and working out the generalization to operads following Carles's suggestion. This was the majority of the hard work in the thesis, and was extra frustrating because (at least for the localization result) the only proof I could work out included hypotheses I felt sure were not necessary.

In addition, it turned out to be subtle to find the right condition so that categories of operad algebras inherit a model structure. Mark has no papers featuring operads, so we had to learn the field together. A visit by John Harper in the middle of the summer helped convince me I had the right condition, but it was difficult to write down a human-readable proof. Mark insisted on having such a proof, and I learned a great deal about writing as he rejected three versions before I produced one which he was happy with. This last proof and explanation is how I have presented the result ever since: if P is an operad in a monoidal model category $\mathcal{M}$ then in order to know that P-algebras inherit a model structure a cofibrancy price must be paid on either P or on $\mathcal{M}$. The most general form of this is now in [**WY15**] and it recovers all results of this sort (i.e. about inherited model structures on P-algebras) while also proving new ones about operads which are levelwise cofibrant, a situation where the cofibrancy price is paid partially by P and partially by $\mathcal{M}$.

3.3. The writing process. I planned in the fall of 2012 to apply for postdoctoral positions, write up the results from 2011-2012, give talks in various seminars, and work out examples of the theory I had developed, especially to the case of equivariant spectra so that I could understand Example 3.2 above. However, September of 2012 turned out to be one of the worst months of my life personally. My father was diagnosed with a dangerous form of cancer, my family's financial situation degraded rapidly for a different reason, an old injury in my shoulder returned and ended my ability to play volleyball (till then a passion equal to mathematics in my life), and a long term relationship ended. Suddenly I found myself needing to return to Chicago frequently, spending huge amounts of time dealing with financial matters, and doing my best to provide support for my family despite having no solid ground on which to stand.

Mark was extremely supportive during this time. I lost my ability to focus on mathematics, and our regular meetings often turned to personal matters. He helped me realize it is okay to have periods like this from time to time; perhaps they should even be expected. He also helped me find ways to get to Chicago and I will never stop being grateful for his support during this time. The only research I accomplished in the fall was extending my results from the context of model categories to semi-model categories. At the time this seemed trivial to both me and him, but it turned out to be important in my development as a mathematician. Semi-model categories have appeared in the majority of my papers, often in places where model structures do not exist. Although Mark invented them (in [**Hov98**]), he does not trust them much and made me carefully show him all steps of every result I claimed. In hindsight, it makes sense that I developed this extension at this time, since I had just read Markus Spitzweck's thesis [**Spi01**] the previous summer where semi-model categories were first explored in depth. From the point of view of this User's Guide, semi-model categories are the reason Section 8 of [**Whi14b**] is not central to the story, and the reader can still have preservation results without needing to digest the meaning of *homotopical cofibration* (now called h-cofibration following [**BB14**] who independently discovered several results in Section 8 in 2013). These maps have similar properties to the h-cofibrations in work of Peter May, but can be defined in any model category.

I spent winter break supporting my family, and by the start of the spring semester things had stabilized. I had a massive backlog of writing to do, and still needed to work out the application to equivariant spectra. I spent the spring writing the parts of the story I understood best (from 2011 mostly) and trying to learn enough about equivariant spectra to finish my thesis. I got many helpful references from Carolyn Yarnall and Kristen Mazur at a conference that spring, but the word on the street was that the definitive reference would be the appendix to [**HHR15**] which had not appeared yet. When I mentioned this to Mark he shared his own view of equivariant spectra via model categories and using that foundation I was able to work out the fact that all my model category axioms were satisfied by the positive stable model structure on equivariant orthogonal spectra. When I asked him the next fall for a reference I could cite we ended up writing [**HW13**] together to fill this gap in the literature, though that work has now been subsumed by [**HHR15**].

The appearance of [**HH13**] provided the last piece of the puzzle, as there were now numerous equivalent conditions a set of morphisms could satisfy, and these conditions implied preservation of commutative structure for G-spectra, at least for G a cyclic p-group and for a particular kind of localization. I proved that the conditions in my Theorem 6.5 implied one of the conditions in [**HH13**] and hence that my more general setting (G could be a compact Lie group, and I was inverting a set of maps instead of a single homotopy element) included as a special case the result required for the Kervaire paper, bringing my thesis problem full circle. From here it was also easy to see why Example 3.2 failed to satisfy these hypotheses, i.e. it is not a counterexample to my main theorem. I met Mike Hill at a conference in April and he kindly checked step by step the application of my general theorem to equivariant spectra. When he told me it all looked in order I knew my thesis was finished.

At this same conference I began a new project with Aaron Mazel-Gee and Markus Spitzweck in motivic homotopy theory. While this project never came to fruition, it was the starting point for the work I did that summer with Casacuberta in Barcelona. From April of 2013 till September of 2013 I tried to balance writing up the results in [**Whi14b**] with taking on new projects, giving talks in seminars, and preparing for the job market. I learned that it's always more fun to explore new math than to write up things I already understand, and for this reason I have to be very disciplined to actually write things up.

I spent May and June visiting Carles, and we proved the existence of a new localization in the context of motivic symmetric spectra, then began working out applications of this result. I also began a project on equivariant operads with Javier which we finally finished in the summer of 2015. In June I traveled to Nice (France), Nijmegen (Holland), and Lausanne (Switzerland) to give talks and discuss mathematics. My visit to Clemens Berger in Nice led to me strengthening [**Whi14b**] in two ways. First, he correctly suggested that the main result in Section 4 should be an "if and only if" statement. Second, he showed me a few things about h-cofibrations I didn't know and this led to cleaner statements and proofs in Section 8. During this visit I also had my first insight into the question of lifting left Bousfield localizations to categories of algebras, i.e. applying localization to commutative monoids (studying $L(CAlg(\mathcal{M}))$) rather than only looking at commutative monoids after localizing (studying $CAlg(L(\mathcal{M}))$). In the fall of 2013 I wrote a grant application to explore this problem with Michael Batanin and it ended up funding my trip to Australia in the summer of 2014.

I returned to the US in July to teach a short course in statistics, then returned to Barcelona in September using NSF funds for a conference on homotopy type theory. There Carles and I finished the first draft of our paper, though it took a long time until we could work that into a version to submit. All in all, the summer of 2013 was an extremely productive time for me and I found it very intellectually stimulating. I am grateful to Carles for hosting me and for making it possible. Once I returned from Barcelona in September, I spent October-January applying for jobs (120 cover letters takes a lot of time!), interviewing, giving talks in seminars, and writing [**HW13**] with Mark so that I had a framework to write [**Whi14b**].

I accepted my job with Denison in early February and then turned my various typed-up results, lecture notes from my talks, and hand-written notes from meetings with various mathematicians into [**Whi14a**] and [**Whi14b**] in March of 2014. I spent April writing my thesis and then spent May-August in Australia working with Michael Batanin. This was another intellectually stimulating time and we planted the seeds for at least three papers. Unfortunately, my Australia trip ended the same day that my job at Denison began and I learned first-hand that the first semester teaching takes up all of a new faculty member's time. In hindsight, I should have submitted my papers in March or April, because the publication process takes a long time and the delay in submitting did not improve the papers at all. My advice for graduate students is to send the paper to interested experts, spend two weeks making improvements based on their feedback (if any), put it on arXiv, and then submit it two weeks later unless you get more feedback. Although it seems like putting things on arXiv and submitting them are big commitments, it's always possible to make changes, minor changes are basically expected, and referees will make you change things anyway. So there is just no good reason to delay the process.

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