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

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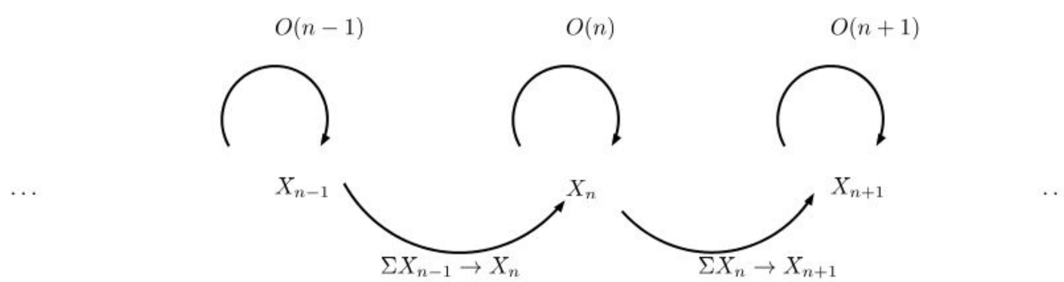
2. Metaphors and imagery

Most of my work is phrased in the language of model categories, settings that allow abstract homotopy theory to be applied to numerous other fields. When I think of model categories, I don't think of the definition. Instead, I see a suite of commutative diagrams telling me exactly what I know I can do with a model structure. These diagrams seem beautiful and well structured. I have confidence that if I can just fit the pieces into the right places then they will commute (i.e. the various ways of moving through the diagram will agree) and prove what I need them to prove. Some of these diagrams are simplistic, e.g. triangles showing me I can factor maps. Others are complicated, e.g. towers to compute simplicial and cosimplicial resolutions (necessarily with many arrows between any two vertices in the tower), or large dimensional hypercubes which encode different possible orderings on letters in a word (these arise naturally when studying monoids). Simultaneously, I think of a suite of examples and counterexamples that warn me about which properties do not come for free, about times I've been surprised by the axioms (e.g. the model category of Graphs and its weak factorization systems), and about manifestations of model categories in radically different fields.

2.1. Examples of model categories. The example I most often think about is that of topological spaces. When I inhabit this example I am immediately aware of safe and nice things I can do, such as CW approximation, mapping cylinders, finding liftings via sections to covering maps and properties of monomorphisms, and monoidal properties given by Cartesian product and the compact-open topology on hom spaces. I tend to picture spaces as things I can draw, such as manifolds, but I also picture my favorite counterexamples as weakenings of the various manifold axioms. I like to think about the long line (which has trivial homotopy groups but is not contractible), the Sierpinski space (which demonstrates that Top cannot be locally presentable), the Sorgenfrey line, finite topological spaces, a pushout of an inclusion of compactly generated weak Hausdorff space which is not even injective, etc. My love of this subject began in point-set topology with Munkres's *Topology* and for this reason I believe counterexamples are a fundamental and beautiful part of the subject. That said, I do at times zoom in to nice subcategories such as CW complexes, Δ-generated spaces, and compactly generated spaces. When doing so I keep track of certain spaces I care about, mostly compact Lie groups and orthogonal groups $O(n)$.

My next most favorite examples come from algebra. I think of the projective model structure on Ch(R), and I have a mental switch which I can throw telling me whether we are thinking of bounded chain complexes (where everything is nice) or unbounded chain complexes where things can be tricky. When thinking about monoidal properties I have another switch which makes sure I am either working over a field of characteristic zero or proceeding with extreme caution. In this example I often switch from thinking about maps to thinking about objects: namely their fibers or cofibers. This shift makes things much easier, as I can simply ask if an object is acyclic rather than trying to use the functoriality of homology to study $H_*(f)$ for some map f. Similarly, the example of the stable module category is a great place to test conjectures about model categories because all objects are both fibrant and cofibrant, the category is stable so I can always reduce to studying objects (which are simply modules here), and yet I know many interesting examples that I can use to disprove conjectures. Example 4.1 in [**Whi14b**] is computed in this setting and as far as I know this is the first non-trivial example of a model category with a monoidal product which fails to satisfy the Pushout Product Axiom or the Monoid Axiom.

After that I like to think of spectra. I envision them just like chain complexes, i.e. infinite chains with some strands connecting adjacent ones. It is very easy to shift viewpoint to orthogonal spectra by simply allowing an action of $O(n)$ on the n^{th} space X_n, and I view this action simply as an arrow from X_n to itself.



An Orthogonal Spectrum

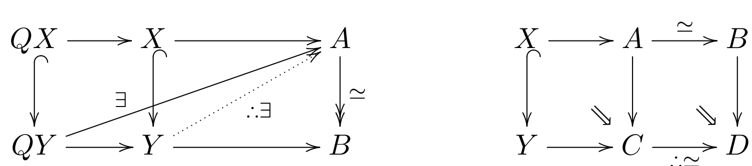
I have similar ways of thinking about equivariant spectra, now with actions of $G \times O(n)$, which I discuss in Subsection 2.3 of this document. Thinking of symmetric spectra is more difficult because they are sequences of simplicial sets rather than topological spaces. I have never felt as comfortable with simplicial sets as with other model categories I study, partially because there are no good ways to visualize them (especially the face and degeneracy maps), so I always find myself thinking of simplicial complexes instead, i.e. of triangles, tetrahedra, and higher dimensional analogues. I know the various properties of simplicial sets and symmetric spectra, and can use them to make formal arguments. I rarely move inside to make point-set level arguments unless it is a lemma about smallness or presentability, which I understand by analogy to the category of sets. In Section 7 of [**Whi14b**] I needed to make arguments at the level of simplices and it was difficult. In the end I relied on properties of the monoidal product, proving something holds for all simplicial sets K rather than just for K of the form $\partial\Delta[n]$ or $\Delta[n]$.

Other examples I often consider include simplicial presheaves, diagram categories with projective/injective/Reedy model structures, W-spaces, motivic symmetric spectra, and graphs. I think of the first three as categories of functors, effectively never using that W-spaces can be thought of as sequences because that would require fixing a skeleton for the category of finite CW complexes. I think of simplicial presheaves as diagrams where I know many formal properties are satisfied, but I never use simplex level arguments. I think of motivic symmetric spectra just like symmetric spectra, but at each level n I see a shadow grading (so that the spectrum looks like a 2-dimensional lattice if viewed correctly). This tells me I can think of motivic symmetric spectra either as the stabilization of motivic spaces with respect to the functor $- \wedge \mathbb{P}^1$ or as the stabilization with respect to $- \wedge S^1_{alg}$ of the stabilization with respect to $- \wedge S^1_{top}$ of motivic spaces. Thinking by analogy to symmetric spectra lets me work in the motivic setting, but always cautiously to make sure my lack of background in algebraic geometry will not lead me to false conclusions. Lastly, when I think of the category of graphs many bits of mathematics I enjoy pop into my mind, e.g. Markov chains, electrical flows on graphs, algorithms to create spanning trees, etc. I need to shut these images down because to study the model category of graphs requires thinking about graph homomorphisms and zeta series on graphs, which I understand less about. The model category of graphs is surprising, and is best kept as an interesting example to study carefully on its own in some future work. For now it is too dimly lit and poorly explored to feel comfortable.

2.2. Properties of model categories. When I think generally, rather than in the context of specific examples, things become much easier for me. The model categorical tools I most often use feel comfortable and smooth. For instance, when I use cofibrant replacement I envision fattening up an object X to its cofibrant replacement QX. I often view this as spreading X out, e.g. shifting how I am viewing it so that instead of seeing a grid in 2 dimensions I see a 3 dimensional grid where the extra dimension was hidden from my previous perspective. I often think of X as living inside of QX, though known counterexamples warn me not to use that intuition when writing down proofs. Instead, thinking this way makes me comfortable and confident, and this in turn increases the number of attempts I am willing to make on a problem in a single sitting without giving up.

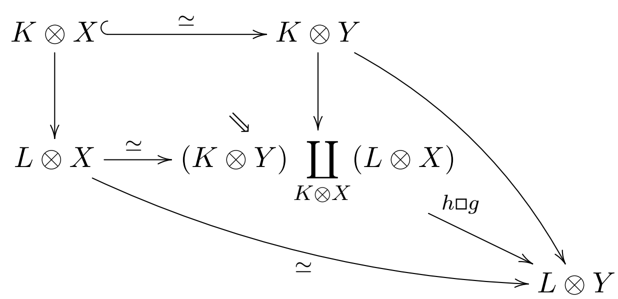
While fibrant replacement is formally dual to cofibrant replacement, it feels less natural to me. Instead of feeling smooth and comprehensible, I see a twist somewhere in the middle and it casts everything thereafter into doubt. I envision possible jagged edges arcing out of this twist in the medium of my understanding, and this prevent me from feeling fully confident in the use of fibrant replacement. Instead of feeling white or silver like cofibrant replacement it feels darker and a bit shadowy. The main reason for this is that I always work in cofibrantly generated settings, and this means I have a collection of cells that let me build cofibrant replacement, e.g. CW approximation in the category Top of topological spaces, or projective resolution in a category Ch(R) of chain complexes. Injective resolution never felt as natural to me, and I learned about Top before simplicial sets (sSet) so I tend to like cofibrant replacement more. This preference has strongly affected my work, as in my thesis I focused on the "cofibrant" way to build model structures on algebras over a monad rather than the "fibrant path object" method. The benefit is that my work was able to apply to examples which had not previously been studied due to the fact that my theorems require different hypotheses (at times easier to satisfy) than theorems obtained via the fibrant approach.

When I think of properties a model category can have or fail to have, I again most often think of what these properties are good for rather than their literal meaning. For example, a model category $\mathcal{M}$ can be left proper, and this has a definition regarding the behavior of weak equivalences along pushouts by cofibrations. Rezk has given equivalent definitions in terms of behavior of functors on over/under categories. However, I think of left properness in terms of the two pictures below. The one on the left reminds me of Proposition 13.2.1 of [**Hir03**], which says that in a left proper model category it's sufficient to test lifting against cofibrations between cofibrant objects. The one on the right reminds me of [**BB14**]'s proof that $\mathcal{M}$ is left proper if and only if all cofibrations are h-cofibrations. I have at times found this formulation easier to work with.

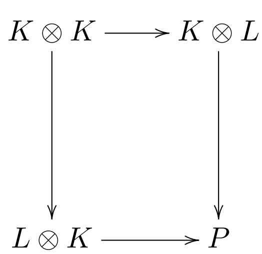


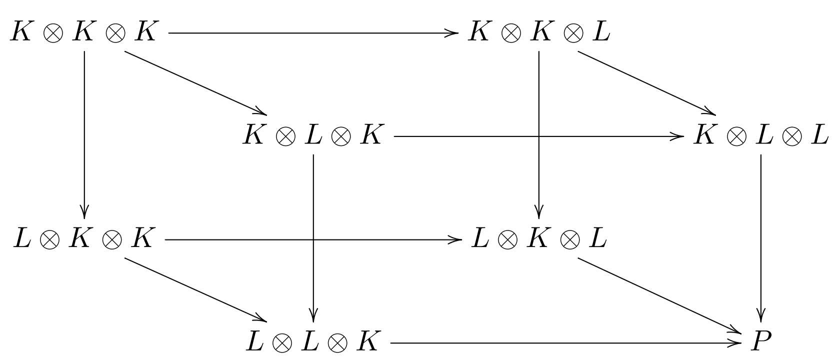
When I learn a new fact about model categories I must find the right diagram to encode the fact; until I "see" it presented in my own way I cannot understand why the fact is true. Next, in order to remember the new fact I must fit it into my memory alongside all the other diagrams. I visualize this process as analogous to the way a computer writes to disk memory. My mind skims through all the facts I know and determines which are similar to this one. It then creates mental web strands to connect this new fact with the other similar facts and fits the new diagram into its place at the barycenter of the related facts. I use this mental web frequently when searching my mind for workable proofs, and I share this imagery of "latching new knowledge onto existing knowledge" with my students in every class I teach. I do a similar searching process when I prove something, to determine where precisely in the literature this new fact fits.

Let me give an example: when I started working with the pushout product axiom, which tells when a monoidal structure and a model structure are compatible, I frequently drew the picture below. It appears in the proof of Proposition 4.12 in [**Whi14b**], and is the key step to proving the main result of Section 4, characterizing monoidal Bousfield localizations.



Another picture which often occurred was that of an n-dimensional punctured hypercube, e.g. the following for $n = 2$ and $n = 3$:





In both cases P is the pushout of the rest of the cube (called the 'punctured cube') and it maps to $L^{\otimes n} := L \otimes L \cdots L \otimes L$ because all vertices in the cube do so. When studying commutative monoids one must also take into account the Σ_n action on the cube, which can equivalently be thought of as permuting the letters in the words which appear as vertices in the cube. These cubes appear all over Section 6, 7 of [**Whi14b**] and in the Appendix to the companion paper [**Whi14a**]. One limitation of my mental imagery is that it's difficult to distinguish when a map (viewed as an edge in some diagram) respects the Σ_n action and when it does not. This made working out the mathematics in Section 6 extremely difficult for me, and as a result I decided to simply convert the problem of finding a Σ_n-equivariant lift to the problem of finding any lift at all in a related diagram that I could understand better.

2.3. Equivariant spectra. In Sections 5 and 7, I apply the main results of the paper to several examples. I recover classical results about spaces, spectra, and chain complexes. The main new results are about G-equivariant spectra, where G is a compact Lie group. In order to have a good monoidal product I work in the context of G-equivariant orthogonal spectra. I picture this setting much like the picture of orthogonal spectra from 2.1 above, since to me a G-spectrum is just an orthogonal spectrum on which G acts, i.e. an $\mathbb{N}$-graded sequence such that for all n, $G \times O(n)$ acts on X_n. I am aware that in every dimension n there are also spaces X_V related to X_n but with a twisted G-action inherited from the G-action on the n-dimensional representation V. I view a G-spectrum as a chain with shadow versions of X_n clustered in level n for all n. I also see restriction and transfer maps between the shadow versions at different levels, just like I see the suspension map data as floating "between" different levels. The structure maps are now a bit more complicated, because the maps $S^V \wedge X_W \to X_{V \otimes W}$ must be $G \ltimes (O(V) \times O(W))$-equivariant. However, this is easy to remember because G acts by conjugation on maps, so I simply view it enveloping the orthogonal group actions.

In order to properly study commutativity for G-spectra, one must work with smash products indexed by G-sets. I often restrict attention to finite cyclic groups G or symmetric groups so that I can see the action of G on the smash product. I then write down proofs in the maximal generality possible and check that they hold for compact Lie groups G. In order to even define equivariant homotopy one needs the notion of H fixed points for a subgroup H of G. Here I again think of G permuting points and it is easy in this light to see the points which are not permuted. I view subgroups via their permutation actions, and these are usually color-coded so that I can see which points they move and which they do not. At times I've had to work with the whole lattice of subgroups of G, or with families of subgroups of G (i.e. sets of subgroups closed under conjugation and passage to subgroup). I again use color-coding for this (e.g. H is usually red, K is blue, and subgroups of K are different shades of blue), and it makes it easy to keep track of the various family model structures (where you vary the weak equivalences according to which homotopy groups are seen to be isomorphisms by the family) by their colors.

I picture the lattice of subgroups as another diagram, whose edges tell me whether one family is contained in another. When I draw this lattice I often draw it as a tower due to space constraints. One of the main results of this paper is that there are localizations which destroy some, but not all, of the commutative structure (equivalently, of the multiplicative norms). The structure that is not destroyed can be viewed as a change of family, so I often draw maps between these towers. This story is made rigorous using a collection of model structures on the category of G-operads (one for each feasible choice of families $\mathscr{F}_n$ of subgroups of $G \times \Sigma_n$) in ongoing joint work with Javier Gutiérrez.

Related to my picture of the lattice of family model structures is the lattice of universe model structures, where you vary the weak equivalences based on which G-representations are allowed (these model structures are discussed in [**HW13**], among other places). I have often wondered about the relationship between these family model structures and these universe model structures. I envision a two dimensional grid of model structures on the category of ($\mathbb{N}$-indexed) G-spectra with some possible folding I have not yet understood that will tell me when a universe model structure is Quillen equivalent to a family model structure (i.e. encode the same homotopy theory). There is a natural way to relate universes and families via determining where isotropy subgroups embed, but I have not yet proven that this correspondence yields Quillen equivalences.

2.4. Bousfield localization. When I think of localization, it is as a large arrow between categories satisfying a universal property. This arrow is a functor, and so specializes to small arrows for any objects in the category. Each such arrow takes an object to the closest object which is local, and by construction each such arrow will be a local equivalence. Thus, I think of the target of the arrow as a shade of the first, partially transparent, which is as much of the original as can be "seen" by the localization (i.e. by the maps that are being inverted).

The paper [**Whi14b**] is about monoidal localization, so I always assume the model category $\mathcal{M}$ has a monoidal product that respects the homotopy, i.e. descends to a monoidal product on the homotopy category $Ho(\mathcal{M})$. This means that for any objects X and Y, if I choose weakly equivalent objects X' and Y' from the respective fuzzy red clouds I see around X and Y then I will end up with a homotopy equivalence between $X \otimes Y$ and $X' \otimes Y'$. I want the same to be true after localization, i.e. in $L_C(\mathcal{M})$ where C is the set of maps I am inverting. This $L_C(\mathcal{M})$ has more weak equivalences, so the clouds of equivalent objects have gotten bigger. I provide general conditions which guarantee that $L_C(\mathcal{M})$ is again a monoidal model category, and then consider these conditions in my examples of interest.

In spaces and bounded chain complexes the conditions are always satisfied. In spectra one needs to know that the localization is *stable*, i.e. commutes with suspension. I visualize this as a map from a chain X to its shade $L_C(X)$ (another chain) and I am simply asking that if I shift my window on X to the right then shift it down to $L_C(X)$ then this will be the same as first going down to $L_C(X)$ then shifting to the right. I even hear the sound of gears locking together when I visualize these shifts. For equivariant spectra the conditions are more subtle. In the end it comes down to the functor Sym (which takes an object X to the free commutative monoid on X) respecting local equivalences. If only some equivalences are respected (e.g. those in a family) then failure can occur. So I need my localization to respect all families, i.e. all colors, which is equivalent to asking it to respect the family of all subgroups.

2.5. Infinity categories. This paper does not deal with ∞-categories, but I often receive questions about them when I speak about this material and I want to share the imagery I associate with them. Currently, ∞-categories (in particular, quasicategories) are very popular among young researchers and some want to replace model categories with them. The upside would be that many statements of interest would be easier to prove, e.g. determining when a localization is monoidal, or when one has a good homotopy theory on a category of algebras. The downside is that one loses the ability to do many of the nice computational constructions one can with a model category, e.g. (co)fibrant replacement, (co)simplicial resolutions, computing homotopy (co)limits, finding explicit lifts, etc. People often ask why I don't work in the easier setting. The simplest answer is that in all my projects so far, ∞-categories were insufficient to encode the items I needed to study. In [**Whi14b**] the problem is that ∞-categories cannot discuss strict commutativity, but rather only E_∞-algebra structure. As the examples in Section 5 and 7 show, this is often the wrong concept to study. In my papers with Michael Batanin, ∞-categories cannot encode the notion of a weak n-category with no higher cells. Instead, they can only talk about (∞, n)-categories with a contractible choice of higher cells. In my work with Javier Gutiérrez, the concept of cofibrant replacement is crucial to constructing the operads we wish to study. More generally than all of these examples, I have yet to find a problem that I cannot solve using model categories but could solve using ∞-categories.

I understand why some want to research ∞-categories, especially those with a strong background in simplicial sets. It's a context where you are virtually guaranteed of success as long as you can keep up with all that has been written by others on the subject. However, I prefer to continue to work in the setting of model categories, where things feel more hands-on to me, where I understand and use all the tools, and where I already have familiarity translating general results into specific examples. In addition, I believe working with model categories makes my work more applicable, since any ∞-category theorist can easily translate my results into their setting (whereas translating the other way is often difficult and not always possible) but for the majority, who understand model categories but not yet ∞-categories, my results are already in a language they can use. Lastly, I like to work in fields where there are actual counterexamples! This was part of what brought me into topology in the first place and discarding model categories would be letting go of some of my favorite counterexamples.

A few weeks ago I was exploring an art museum and I didn't have enough time to see everything. I love art museums and have learned over the years that art which is carefully detailed appeals to me most strongly. In particular, landscapes and large scale paintings of human endeavor always catch my eye. If I am pressed for time I usually skip the impressionist gallery, but on this day the entire special exhibit was on impressionists. The museum had pieces from all the best masters, and I could finally understand why this form of art appealed to so many. The paintings I saw perfectly captured the light at various parts of the day, but completely blurred out the actual objects of study. I realized my taste for model categories and away from ∞-categories is the same as my taste in art. I like to see the details, the precise and intricate point-set level constructions, and the clever ways of fitting known diagrams together to give new facts. I am not satisfied by just "the essence" of the object or its characterization by a universal property, though I do see the artistic value in that approach.

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