PHILOSOPHIZING

Thought I would take an hour off to just write nonsense about points of view, what is “21st Century mathematics”. Please be indulgent. Some of the discussion that we have been having and that I have been having with the n-category community feels as if I perhaps went to sleep for 30 years and awoke to find the same debate going on now as before. Here is an entended excerpt from Infinite Loop Spaces, written by Frank Adams in the mid 1970’s, exactly verbatim, for your amusement:

“The differences of technique which one may observe between these authors [Boardman-Vogt and May] are perhaps correlated with differences of philosophy and outlook. I trust that I can show my sympathy for both sides. It seems that Boardman and Vogt feel that there really should be a theory of homotopy- invariant structures, and that someone should take the trouble to set this theory up properly for its own sake. For example, one sets up the theory of H-spaces so that if X is an H-space, then any space Y equivalent to X is also an H-space. Therefore one should set up the theory of A_n-spaces or E_∞-spaces to have the corresponding property: if X is a space with an A_n-structure or an E_∞-structure, and if $f : X \to Y$ is a homotopy equivalence, then there is one and (essentially only) one way to put an A_n-structure or an E_∞-structure on Y so that f becomes an equivalence of A_n-spaces or E_∞-spaces.

In order to carry out this program, it seemed reasonable to Boardman and Vogt to use PROP’s which are “free” in a certain sense; the precise sense need not delay us here. Boardman and Vogt construct PROP’s which are “free” in this sense. The construction involves combinatorial machinery (trees). Essentially this machinery corresponds to the “grammar” of “words” in the letters $M_2 : K_2 \times X^2 \to X$, $M_3 : K_3 \times X^3 \to X$, or similar letters for similar operations. It is this combinatorial machinery which gives the work of Boardman and Vogt much of its individual flavor.

By contrast, May feels (it seems) that the task of the theory is to prove the theorems as quickly as possible, so that we can all go back to our proper business which we enjoy so much, namely computing things which do have invariant meaning, such as homology operations. And to this end he makes adroit use of the following device. If you want to compare X and Y, don’t try to construct a map $f : X \to Y$ and don’t try to construct a map $g : Y \to X$; construct instead a new object Z which maps to both X and Y.”

Of course, as was his habit, Frank put things in starkly simple terms, but that passage does in essence frame the discussion we are now having about foundations of homotopical algebra. I do thoroughly understand the ideal position that we should always and in every case prove that everything is homotopy invariant, well-defined without choices or with a well-defined parametrizing object of choices. However, a recalcitrant part of me still feels that this idealism can slow things down terribly. Granted, 30 years ago I was in a hurry to do actual calculations and now I am not, but I still have a lot of sympathy with those who want to get to the calculations without having to slog through too much indigestible generality to get there. The proposal I just wrote was my way of trying to frame discourse in the area of n-category theory in such a way as to maximally encourage simultaneous attack from both philosophical points of view.

As I understand Kontsevich's work in progress, it takes up almost exactly where Boardman and Vogt left off 30 years ago:

Definition 1. A h-category C consists of a set $Ob(C)$ of objects, a space $Hom_C(E, F)$ in $Ob(Spaces)$ of morphisms for every pair of objects, and the action of the obvious $Ob(C)$-colored version of the BV-resolution of the operad of monoids.

Definition 2. An h-functor $F : C_1 \to C_2$ between two h-categories is given by a map $Ob(F) : Ob(C_1) \to Ob(C_2)$ of sets, maps of spaces $Hom_{C_1}(E, F) \to Hom_{C_2}(Ob(F)(E), Ob(F)(F))$ and higher homotopies governed by BV-resolution of the colored operad responsible for morphisms of monoids.

That is, it builds the BV machinery of trees into a basic definition of "weak category", my general umbrella name for the various (at this point competing) weakenings of the classical notion of category. Other versions and analogues of weak category include:

Segal category or Segal space (Segal, Dwyer and Kan, Rezk, Simpson)

A_∞-category (Stasheff, Fukaya, Fukaya-Seidel, Kontsevich, Keller)

[Kontsevich's new definitions work with the BV operad rather than Stasheff's]

[My version of higher category theory starts democratically, with any A_∞ operad]

Weak Kan complex = quasi-category (Boardman and Vogt, Joyal)

Simplicial category (obvious, but included here because Dwyer and Kan prove that this theory is essentially equivalent to the theory of Segal categories)

Simplicial localization, hammock construction (Dwyer and Kan)

These are part of or related to various programs for homotopy invariant homotopical algebra, some of which are:

Boardman and Vogt and direct sequels (especially Schwänzl and Vogt)

Heller (long series of papers: focus on homotopy level with minimization of role of point-set level: there is here a definition of cofiber that is unique and independent of choices, if I remember right)

Coherent homotopy theory (Vogt, Cordier, Cordell and Porter)

Rezk (A model for the homotopy theory of homotopy theory)

Joyal

Kontsevich

Coherent homotopy theory also takes up where BV left off, going back to a 1973 paper of Vogt. It gives a highly developed, but not well-known, theory involving diagrams in simplicial categories. Its basic definitions feel close in spirit to those of Kontsevich quoted above. Eventually we will have a coherent theory of coherent theories showing how to compare the various notions above. The germ of such a comparison is in place, and it doesn't look all that hard. In fact, I think a lot of it is implicit in the literature, except that nobody has read enough of the published literature to make the connections explicit (and of course several of the theories, such as Joyal's and Kontsevich's, are unpublished and embryonic).

Personally, I am ambivalent about preferences. I didn't like Boardman and Vogt aesthetically 30 years ago, and I have not yet been given a good reason to change my mind. This may be just that Boardman and Vogt didn't write for human beings [your terminology], but I think it goes deeper. The higher homotopies feel too much on the surface, making for a combinatorial heft that seems irrelevant to any kind of applications — we have had 30 years of applications after all, with no use of BV details — and makes the theory just too unpleasant for a general all-purpose tool fit for humans. I repeat that this was already my opinion 30 years ago. Now as then, it will take either a serious application or an aesthetically attractive reworking — which I think may well be possible — to convince me of my foolishness.

I don't know Heller's theory well, but I didn't like it when I reviewed it for Math Reviews: thought it a step backwards, too homotopical.

The other theories above are all simplicial, and I have long been ambivalent: simplicial theory is of course second nature to me, but I think we share something of a philosophical puzzlement as to why it should be quite so ubiquitous, as if there were no alternative. I like the ideal of starting out with any very good base category (simplicial sets, topological spaces, chain complexes over a commutative ring, maybe your idea of "spaces" that god has in mind, guessing that perhaps what he has in mind is an "idea" of spaces that can be expressed as axioms on the base category) and setting up a full higher categorical homotopy theory with everything done relative to that fixed base category. But as soon as one becomes technical, it seems that habit forces one to start thinking simplicially again: one reason is that one or another version of the bar construction is always relevant, and that really does seem to be intrinsically simplicial.

So far, this has been following up from BV 30 years ago. At nearly the same time, Quillen introduced model categories. It took me a long time, but I do now believe that this approach to homotopical algebra is exactly right for the May attitude of 30 years ago that Adams was describing. Voevodsky's proof of the Milnor conjecture and Simpson's recent work applying n-categories to non-Abelian Hodge theory are examples outside of algebraic topology of mathematics that I doubt could even be conceived without the model category approach. The only work in algebraic topology that is not purely foundational and that makes any use at all of any of the work mentioned above is a very recent preprint of Mike Mandell (so recent that I first read it last night). In it, he uses the Dwyer-Kan foundations (along with a lot more) to show to what extent integral cochains model homotopy theory.

To try to redevelop foundations without seamlessly incorporating the Quillen model approach is to throw the baby out with the bath. Said another way, it is like building a new (and for many or most applications slower) operating system that lacks backwards compatibility. Rezk's paper is part of building a good transition from model categories to the other theories above. He defines a notion of a "complete Segal space", which can be viewed as one generalization of a weak category. Ignoring set theoretical questions, any Quillen model category gives rise to a complete Segal space, and the complete Segal spaces are themselves the fibrant objects in a model structure on the category of all Segal spaces. There is a less high-powered paper that goes similarly from model categories to Heller's theory. I don't think we are too far away from a reasonably complete conceptual understanding. Rezk's paper is just one of many indications of the intimate relationship between model category theory and conceptual understanding of higher homotopy theory: they are inseparable.

Rezk's work is analogous to earlier work of Dwyer and Kan in which they considered a model category of simplicial categories. This is conceptually close to my model category of DG categories, and thus close to your paper. A key thing I don't yet understand, probably not because it is hard but because I just haven't had enough time to think about it seriously, is how to understand the homotopy category of a DG category, such as the triangulated category of a pretriangulated category, in terms of the homotopy category of the model category of DG categories. The commutation relation that you prove should be an example of that understanding. Still very fuzzy, I know.

Changing focus, I want to discuss the point you mentioned about the assymmetry of Quillen equivalence by means of worked examples. The point is well-known, and in practice it is a point that to my mind works in favor rather than against the utility of model categories. The examples come from the Memoir "Equivariant orthogonal spectra and S-modules" that Mandell and I wrote last year. The first concerns a situation where one encounter three model categories with left adjoints of Quillen equivalences $\mathcal{A} \to \mathcal{B}$ and $\mathcal{A} \to \mathcal{C}$, and one improves understanding by finding a left adjoint $\mathcal{B} \to \mathcal{C}$ of a Quillen equivalence so that $\mathcal{A} \to \mathcal{C}$ is a composite of such left adjoints:

"We assume that the reader is familiar with the notion of a Quillen equivalence of model categories ... This is the most structured kind of equivalence that ensures an adjoint equivalence of the associated homotopy categories. With Schwede and Shipley, we proved in MMSS that the category $\Sigma\mathcal{S}$ of symmetric spectra is Quillen equivalent to the category $\mathcal{IS}$ of orthogonal spectra. In Sch, Schwede proved that $\Sigma\mathcal{S}$ is also Quillen equivalent to the category $\mathcal{M}$ of S-modules. However, these comparisons do not give a satisfactory Quillen equivalence between the categories of orthogonal spectra and S-modules since the resulting functor $\mathcal{IS} \to \mathcal{M}$ is the composite of the right adjoint $\mathcal{IS} \to \Sigma\mathcal{S}$ and the left adjoint $\Sigma\mathcal{S} \to \mathcal{M}$ and therefore fails to preserve either q-cofibrations or q-fibrations.

We shall construct a Quillen equivalence between $\mathcal{IS}$ and $\mathcal{M}$ such that Schwede's left adjoint $\Sigma\mathcal{S} \to \mathcal{M}$ is the composite of the left adjoint $\Sigma\mathcal{S} \to \mathcal{IS}$ of MMSS and our new left adjoint $\mathcal{IS} \to \mathcal{M}$. This shows that orthogonal spectra are mathematically as well as philosophically intermediate between symmetric spectra and S-modules."

There is a deeper example in the same Memoir where no such "satisfactory" Quillen equivalence can exist. When one works equivariantly, with a compact Lie group G of equivariance, there are categories $G\mathcal{IS}$ and $G\mathcal{M}$ of orthogonal G-spectra and of S_G-modules (where S_G is the G-sphere spectrum). The category $G\mathcal{IS}$ has a model structure and the category $G\mathcal{M}$ has two model structures with the same weak equivalences, say $G\mathcal{M}$ and $G\mathcal{M}'$, where the former has fewer cofibrations. There are left adjoints of Quillen equivalences $G\mathcal{IS} \to G\mathcal{M}'$ and $G\mathcal{M} \to G\mathcal{M}'$ (the latter given just by the identity functor), but there is neither a left nor a right adjoint of a Quillen equivalence between $G\mathcal{IS}$ and $G\mathcal{M}$. To somebody who wants to prove theorems in equivariant stable homotopy theory, this is great. The categories $G\mathcal{IS}$ and $G\mathcal{M}$ are wildly different, and you can do things in each that simply cannot be done in the other. For example, $G\mathcal{IS}$ has a very nice point set geometric fixed point spectrum functor that only exists in crude up to homotopy form in $G\mathcal{M}$, while $G\mathcal{M}$ admits a theory of G-CW spectra, including a cellular approximation theorem, that is exactly like the classical nonequivariant theory of CW complexes. No such G-CW theory can exist in $G\mathcal{IS}$. The two Quillen equivalences guarantee that anything you can prove in one category can be reinterpreted as giving a result in the other category. A purist might ask for a more structured comparison between comparisons relating $G\mathcal{IS}$ to $G\mathcal{M}$, but in a case like this I truly do become recalcitrant. If 21st century mathematics insists that we try to think in terms of a homotopy invariant theory of comparisons every time we meet up with a situation like this, then I am not sure that it will be a century of progress.

When I was 10, I read a wonderful book on comparative religion, and came to the conclusion that all religions are equally false. While some will be better for some purposes than others, I think that all of the mathematical theories mentioned above are equally true, and I would like to see them compared. I am not in sympathy with a fundamentalist "this is the right way" and "that is the wrong way" to think about these foundations, either as to overall philosophy or as to implementation of a particular philosophy. I prefer to be cheerfully eclectic, just as Frank implied.