Alexander Grothendieck's letter on Derivators

A. Grothendieck

02/04/1991

Translator's note

This page is a translation into English of the following:

Grothendieck, A. "Lettre d'Alexander Grothendieck sur les Dérivateurs, 02.04.91." Edited by Matthias Künzer. [PDF]

The translator (Tim Hosgood) takes full responsibility for any errors introduced, and claims no rights to any of the mathematical content herein.

Version: `f940d5d`

p. 2

Les Aumettes, 2nd of April 1991.

Dear Thomason,

Thank you for your letter, and forgive me for having taken so much time to reply. One reason for this is that, since for the past maybe two months I have been busy thinking about something that started as a small diversion, which I thought I would be able to sort out in a few days (a familiar sentence. . .), and I delayed this letter week after week. This thought is not really about homotopical algebra per se, but more about the foundations of category theory, and I have done a lot more than what I need right now [9, Chapter XVIII]. But up until now, I have been convinced that a homotopical algebra (or, in a wider vision, a "topological algebra") such as I envisage cannot be developed with all the breadth that it has without the aforementioned categorical foundations. It concerns a theory of (large) categories that I have been calling "*accessible*," and accessible subsets of these, completely rewriting the provisional theory that I present in SGA 4, §I.9 [2]. I have woven a tapestry of nearly two hundred pages on this seemingly trivial theme, and it would please me to present to you the outlines, if that would be of interest to you. There are also some intriguing problems that remain, that I feel are difficult, maybe even deep, and that could maybe (who knows) inspire you, or somebody else interested in the foundations of the tool that is category theory. But all this seems to me part of the domain of the tool, and I would prefer, in this letter, to speak about more central things. The main ideas were, for the most part, born over 25 years ago, and I see the enduring seedlings of them in my solitary reflections from the years '56 and '57, when I first had the need of categories of "coefficients" that were less prohibitively large than the interminable complexes of chains or of cochains, and the idea (after long periods of perplexity) of constructing such categories via passing to a category of fractions (a notion that had to be invented by considering concrete elements) by "inverting" the quasi-isomorphisms. The principal conceptual work that remained to be done — and that now seems to me as equally fascinating (as much as for its beauty as for its evident impact on the foundations of a cohomological algebra in the spirit of a theory of cohomological coefficients) as in the times of my first loves with cohomology — was to clarify the intrinsic structure of these categories. The fact that this work, that I had confided to Verdier around 1960, and that was meant to be the subject of his thesis [13], had still not yet been done, even in the case of ordinary and abelian derived categories, which nevertheless have ended up (by necessity) being used daily in both geometry and analysis, says a lot about the state of mentality in the mathematical community with regards to foundations. *p. 3*

This mood of contempt for essential foundational work (and, more generally, for anything that does not follow the fashion of the moment), I mentioned in my last letter, and I also come back to it many times in the pages of *Récoltes et Semailles* [8], and it is one thing (amongst many others) that quite is simply beyond me. Your response to my letter shows that you absolutely did not understand. It wasn't a letter to "complain" about this, or to say that it bothered me. But it was an impossible attempt to share a pain. I knew deep down that it was hopeless; for everybody shuns pain, that is, shuns knowledge (for there is no knowledge from the soul that is free from pain). A very rare attempt, possibly the only one in my life (at least, I can't remember another), and probably the last...

There are two directions of ideas, intimately linked, that I want to talk to you about, and that I have been especially keen to develop since the end of October (when I resumed mathematical reflection [9] for an indefinite period). They are already sketched out here and there (along with a number of other key ideas of topological algebra) in *Pursuing Stacks* [6, Section 69]. In this 1983 reflection, which has helped me a lot these days, I end up spreading myself rather thin by following lateral paths, rather than returning to the essential ideas of my initial point. As another useful source for anybody interested in these basic questions, I point to two or three letters to Larry Breen, which I thought that I would include in the published text of *Pursuing Stacks* [7]. On the one hand, I would like to talk to you about categories of models and the notion of "derivators" (replacing the defunct "derived categories" of Verdier, which are decidedly inadequate for our needs). On the other hand, I have a lot to say about Cat as a category of models for all kinds of "homotopy types." But this will probably be for another time (assuming that your interest survives reading this letter). So today it will be the category of models and the notion of a derivator.

1 The only essential structure of a category of models is the data of the "localiser" $W \subset \mathrm{Mor}(\mathscr{M})$

I also call a category $\mathscr{M}$ endowed with such a "localiser" (containing all isomorphisms, and containing the remaining arrow in the set $\{u, v, uv\}$ whenever it contains any two in that set) a "category of models." The essential homotopical constructions are independent of all supplementary structures, such as a set C of "cofibrations" or a set F of "fibrations," or even both together. Such supplementary structures are useful insofar as they allow us to make essential constructions more explicit, as well as establishing their existence. But they are no more essential for the intrinsic meaning of the operations (which they have actually tended to obscure up until now) than the more of less arbitrary choice of a basis for a module in linear algebra. As terminology, I will speak of "categories with cofibrations" (or "with fibrations"), or of "Quillen categories" (or "Quillen triples"), etc. whenever such supplementary structures appear.

By their richness of delicately intertwined structures, it is the *closed Quillen triples* (W, C, F) that are, to me, the most beautiful structure discovered so far of the "enriched" category of models. I had thought that I could do without them, but in the end I could not, and I believe that they will continue to be useful (if not absolutely essential). In the opposite direction, in terms of the economic use of means used to construct the essentials, it is the notion of a *category with cofibrations*, or *with fibrations*, by K.S. Brown [3] (with the quite obvious generalisation given by Anderson [1]) that appears the most beautiful to me. However, I did not manage to understand the justification for the system of axioms that you proposed to me in your first letter, which placed you somewhere halfway between Quillen [10] and Brown [3]. Your axioms[1] (seen as extending those of Quillen) seem to me to be prohibitively demanding, when compared to those of Brown–Anderson — apart from the fact, only, that you do not seem to require for the sets C and F to be stable under composition; but I hardly know of an example where this axiom would cause a problem. Please enlighten me if there is something that I am missing. *p. 4*

An example: if a structure of fibrations (in the sense of Brown–Anderson) satisfies the familiar condition of "properness" (which is the case for the structures considered first of all by Brown, where all the objects are fibrant over the final object), then we can replace this structure (W, F) with another (W, F_W) that is canonically associated with the localiser W, i.e. with the category of models in question, by taking F_W to be the set of arrows in $\mathscr{M}$ that are, what I call, *W-fibrations* $f: X \to Y$, i.e. those that are squarable and such that the change of base functor $Y' \mapsto X' = X \times_Y Y'$ from $\mathscr{M}/Y$ to $\mathscr{M}/X$ sends quasi-isomorphisms to quasi-isomorphisms. For every localiser, this gives a set of arrows that contains all isomorphisms, and that is stable under composition, change of base, and direct factors. Saying that (W, F_W) is a Brown structure is equivalent to saying that there exist "enough W-fibrations," by which I mean that every arrow u factors as $u = fi$, with $i \in W$ and $f \in F_W$. Dually, for categories with cofibrations (W, C), we can (in the proper case) replace this with some structure (W, C_W) canonically associated to the localiser, and by introducing the set of *W-cofibrations*. In this way, I tend to mostly think of a proper (and not "canonical") structure of fibrations (W, F) as a recipe or criterion for characterising *certain* W-fibrations, with which it often suffices to work, since there are "enough"of them. And yet, I have found in the case of Cat that working with W-fibrations (which is much less restrictive than the Quillen "fibrations" that you introduced) has been essential. And I have been persuaded that it should also be very useful in a category of models, such as $\Delta^\wedge$ (semi-simplicial sets), since, because everything is substantially less demanding than the notion of Kan fibrations, being a W-fibration already implies everything that I consider (whether right or wrong) as the essential cohomological and homotopical properties of Kan fibrations (which, to me, are *not* in the nature of extension-lifting properties of morphisms). This should allow us (by considering infinite "paths") to construct, in $\Delta^\wedge$, the analogue of the path spaces of Cartan–Serre, without having to replace the complex K by a Kan envelop beforehand. This is what I have verified anyway in the case of Cat (without having to take a detour via $\Delta^\wedge$), which is a very close neighbour [9, Chapter VII]. This is part of what I wanted to talk to you about next.

[1] *[Editor] The letter from Thomason to Grothendieck on the 3rd of January, 1991 [7]. See also [14,15].*

2 Prederivators, derivators

p. 5

When I said that the essential homotopical structures are already contained in the localiser W, I was thinking most notably of the exact sequences of fibrations and of cofibrations, which is a decisive test. I remain astonished that Quillen does not even mention this subject in his brilliant (and beautiful) work [10], and I presume that he succeeded (as many others did after him) in not seeing it. (To see it, he would have no doubt had to have not been blinded by the contempt *a priori* that he expresses for every research into foundations that went beyond what he had just done, with such success. . .) But the thing becomes evident in the derivators point of view.

The main idea of derivators

Bibliography

[1] D.W. Anderson. "Fibrations and geometric realizations." *Bull. Amer. Math. Soc.* **84** (1978), 765–788.

[2] M. Artin, A. Grothendieck, J.-L. Verdier. *Théorie des topos et cohomologie étale des schémas (SGA 4)*. Springer-Verlag, 1972. *Lecture Notes in Mathematics*. **269, 270, 305**.

[3] K.S. Brown. "Abstract homotopy theory and generalized sheaf cohomology." *Trans. Amer. Math. Soc.* **186** (1974), 419–458.

[4] D. Ferrand. *On the non additivity of the trace in derived categories*. 2005. http://arxiv.org/abs/math/0506589.

[5] A. Grothendieck. *Cohomologie ℓ-adique et fonctions L (SGA 5)*. Springer-Verlag, 1977. *Lecture Notes in Mathematics*. **589**.

[6] A. Grothendieck. *Pursuing stacks*. 1983.

[7] A. Grothendieck. *Pursuing stacks et correspondance*. 1983.

[8] A. Grothendieck. *Récoltes et semailles, témoignage sur un passé de mathématicien*. Montpellier, 1986.

[9] A. Grothendieck. *Les dérivateurs*. 1990. https://webusers.imj-prg.fr/%C2%A0georges.maltsiniotis/groth/Derivateurs.html.

[10] D.G. Quillen. *Homotopical algebra*. Springer-Verlag, 1979. *Lecture Notes in Mathematics*. **43**.

[11] O. Renaudin. "Plongement de certaines théories homotopiques de quillen dans les dérivateurs." *J. Pure Appl. Algebra*. **213** (2009), 1916–1935.

[12] J.-L. Verdier. *Catégories derivées. Quelques résultats (état 0)*. 1963.

[13] J.-L. Verdier. *Des catégories derivées des catégories abéliennes*. Soc. Math. France, 1996. **239**.

[14] C. Weibel. “The mathematical enterprises of robert thomason.” *Bull. Amer. Math. Soc.* **34** (1997), 1–13.

[15] C. Weibel. “Homotopy ends and thomason model categories.” *Sel. Math., New Ser.* **7** (2001), 533–564.