Extract from a letter from Alexander Grothendieck to Ronnie Brown, 09/06/83

Just one technical question relative to a fibering $E \to B$ and Loday's description of a crossed module made up with the π_1's of $E \times_B E$ and E. I feel a little silly I don't quite follow. Of course $E \times_B E$ and E make up together a category object in (Top), but why should the π_1-functor transform this into a category object in (Groups), while this functor, I guess, does not commute with the relevant fiber products? Thanks for rectifying my misconception with Kan-Dold-Puppe's theorem for cubical complexes. I must confess I never so far worked with cubical complexes at all, and don't even remember ever to have sat down to write down a formal definition of the category of "standard cubes" which should correspond to the category of standard (ordered) simplices, and played around some with it, for instance check that it is actually a "contractor" as I felt it should, because why should it behave any differently from Δ? Maybe I should do a little checking though, as it is the same "argument" of idleness which made me admit that of course the Kan-Dold-Puppe theorem couldn't fail to be true in the cubical case. Still the question of understanding the exact realm of validity of Kan-Dold-Puppe remains just as intriguing, and maybe even more so, as the need of introducing extra structure ("connections", as you call them in you letter) in the cubical case, gives the idea than an answer may turn out subtler than expected.

Excuse me, I overlooked in my first reading of your long letter the practical question of writing a letter of support for your research program proposals. I am not definite about wishing to keep out of it, if you have the feeling it may help, rather than make the referees more moody still! It is all too evident I am not an expert on homotopy theory, and the books I am bold enough to write now on foundational matters are very likely to be looked at as "rubbish" too by most experts, unless I show up with $\pi_{147}(S^{123}$ as a by-product (whereas it is for the least doubtful I will...). At the very least, you should give me some hints as to the kind of things I could reasonably say in a "formal note of support", besides how nice it would be to have a better understanding of the foundational matters. This makes me think by the way that (much to my surprise, I confess) I never got a line from Quillen in reply to my long letter from February. I guess since that time he should have gotten that letter, maybe you even gave him a copy time ago if I remember it right. As two letters for me in the Faculty mail got lost lately, it isn't wholly impossible that he did reply and I didn't get it. In case you should know something on this behalf, please tell me.

I realize somewhat belatedly that I should apologize for the mistaken impression I got, from a quick glance through the heap of reprints you sent me a year or so ago, and which I somewhat bluntly expressed in my first letter to you I believe - namely that you had little or no background in so-called "geometry". It would be more accurate, it seems, to say that your background and mine don't overlap too much. My own background has been somewhat moving for the last ten or twelve years, since I withdrew rather abruptly from the mathematical milieu. Thus my interest in the Teichmüller (or mapping class) group has developed mainly, in two steps, during the last two years and a half. It came quite as a surprise that you have come to some contact with these groups, too - and I would be quite interested to get a reference on this "amazing finite presentation" you are speaking of (and I can well imagine it must be tied up with the Mumford-Deligne compactification of the relevant modular multiplicity, whose π_1 is the group we are looking at). I was under the impression that to give an explicit presentation of the group, rather than of the groupoid, would be kind of inextricable, and it is surely an interesting fact it is not. Still, I am pretty sure for the "arithmetical" theory I am interested in, that one just cannot possibly dispense from working with groupoids, rather than just groups. A few times in your letter you stop to ask what of all you're saying would make sense with spaces replaced by topoi, and wondering if it would be a long way to do those things in the wider context. If you are just interested in homotopy types (more accurately, prohomotopy types) of topoi, it seems to me that Artin-Mazur have developed more or less all the machinery needed, in order for any result in semisimplicial homotopy theory, say, to carry over more or less automatically to topoi. This isn't really the most interesting thing they did, but rather what could be considered as the routine part of their work, which they develop by standard semisimplicial homotopy techniques. What they were really after was giving various "profinite" variants of homotopy types and a formalism of "profinite completion" of usual (pro)homotopy types, relevant when working with étale cohomology of schemes, and using this, stating and proving a few key theorems, a typical one being that for a proper and smooth morphism of schemes $[f]$ and taking profinite completions (of homotopy types) "prime to the residue characteristics", the theoretical "homotopy fiber" of the map $[f]$ can be identified with the (prohomotopy type of the) actual schematic geometric fibers of the map $[f]$. It turns out that the algebraic machinery reduces these statements to corresponding statements about cohomology with torsion coefficients (including non-commutative cohomology in dimension 1), which had all been proved in the SGA4 seminar by Artin and me.

I think within the next day I am going to read through your preprint "An introduction to simplicial T-complexes", as you suggested, maybe I'll write again if I have any questions. For the time being, I guess I'll stop. And thank you again very much for your patient help.

Very affectionately

Alexander