

CORRESPONDANCE

ALEXANDRE GROTHENDIECK – RONALD BROWN

Éditée par M. Künzer

(avec la collaboration de R. Brown et G. Maltsiniotis)

Note des l'éditeurs

Cette correspondance, éditée par M. Künzer, avec la collaboration de R. Brown et G. Maltsiniotis, fera partie d'une publication en deux volumes de la Société Mathématique de France, à paraître dans la collection *Documents Mathématiques*, consacrée à la "Poursuite des champs" d'Alexandre Grothendieck. Le premier volume [79], édité par G. Maltsiniotis, comportera les cinq premiers chapitres du tapuscrit de Grothendieck, et le second [80], édité par M. Künzer, G. Maltsiniotis et B. Toën, sera consacré aux deux derniers chapitres, ainsi qu'à la correspondance de Grothendieck avec R. Brown, T. Porter, H.-J. Baues, A. Joyal, et R. Thomason, autour des sujets traitées dans la « Poursuite ».

Les notes de bas de page indiquées par "N. Éd" sont dues aux éditeurs, ainsi que les références bibliographiques et les index. La correspondance est en anglais, mais le « métalangage » de l'édition est le français. Les rares passages supprimés sont indiqués par "[...]".

toposes. Have you or any of your students ideas on this? I am happy for the research proposal to be shown to anyone who may be interested.

With all best wishes,

Yours cordially,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 15.02.1983

Les Aumettes, 15.2.1983

Dear Ronnie,

Please excuse this very belated answer to your last letter and greetings, and (I am afraid) to one or two other letters of yours. One reason to my poor answering is that I feel somewhat “out of the game”, and that I am keen at not getting caught in any big technical machinery – the machine-building time is over for me now, and I want to be careful not to do more than occasionally throwing a very casual glance at the machine-building others pursue, and possibly making a comment or two, without really getting involved. Another reason lately was a pending letter to Illusie on “integration” of homotopy types, of which I was considering sending you a copy. Finally I got to write that letter about three weeks ago, but then it appeared the way it was written (with various misunderstandings of mine gradually clearing up while writing) wasn't too suited really to be of use to you. As a result of having finally written down somewhat vague intuitions, my ideas have clarified sufficiently that I feel able to write you a letter on this topic, in case you should feel interested, which you may find not too confused. The main point for you (still vague in my mind though, because apparently the basic definitions of “fundamental n -groupoid” of a semisimplicial set (say) is still lacking, as well as the notion of $(n+1)$ - \varinjlim of n -groupoids) would be what I still view as the most natural ultimate statement of a generalized “higher” van Kampen theorem: for any “direct system” of semisimplicial sets (or topological spaces, or topoi) $(X_i)_{i \in I}$, the n -fundamental groupoid $\Pi_n(X)$ of $X = \int_I X_i$ is canonically n -equivalent to $(n+1)$ - $\varinjlim_I \Pi_n(X_i)$. Here \int_I is the symbol for “integration” of semisimplicial sets or topoi over an arbitrary indexing category I , and n is any natural integer. In the cases $n = 0$ and $n = 1$ (where precise definitions of Π_n and of $(n+1)$ - \varinjlim are available), I view the statement as more or less tautological when Π_1 is interpreted in terms of classification of étale coverings. In cases when \int_I

coincides (up to homotopism) with \varinjlim_I (a priori there is a map

$$\int_I X_i \longrightarrow \varinjlim_I X_i ,$$

which in some interesting cases is a homotopism...), it boils down essentially to a reformulation, in terms of fundamental groupoids, of “descent theory” for étale coverings (which was one of the key techniques I used for the study of the fundamental groups of schemes, notably in SGA 1 [78]). I suspect that once the basic definitions are in place, the “higher” van Kampen theorem will be just as tautological.

From one comment (n° 5) to your program submitted for support, it seems that the notion of n -groupoid or ∞ -groupoid you are working with is too narrow to encompass n -homotopy types and homotopy types, the reason being I guess that you are probably insisting on *strict* associativity of various compositions, which does not hold of course for the so-called “ ∞ -groupoid” which I suggested to associate to an arbitrary space or semisimplicial set, in my (first?) letter to Larry Breen, in order to get a dictionary

n -truncated homotopy types \longleftrightarrow n -groupoids and n -equivalences of such ,

which apparently is still lacking in your setup. That such an equivalence should exist, is of course one main content of that letter of mine, and I doubt that a comprehensive higher van Kampen theorem can be developed without this dictionary being well understood. There was a misunderstanding in my mind before, as I believed that your machinery, with lots of equivalences between a bunch of categories with barbarous names, included that dictionary I had been contemplating. If this indeed is *not* so, this would seem to me the most urgent and most fundamental gap to bridge. As is the case so often, the main work to be done is in the very first place conceptual, and the technicalities are just the indispensable “hardware”, so to say, to give a mathematical existence status to the necessary concepts.

I hope your heavy reference to the program outlined in those letters to Larry Breen will work for you and not against you – which is not so clear beforehand! I noticed that the dispositions of many mathematicians (including former friends) with respect to my person are quite ambivalent, due to the unorthodox and puzzling way I left mathematics (or, at any rate, the mathematical social microcosm). In any case, I wish you good success with your application! By the way, I found one or two misleading statements about what I was supposed to have in mind with that program – for instance I must confess (and disappoint you!) that I never had any feeling or intuition concerning the Riemann conjecture; this has been part of my (numerous) basic gaps in my overall grasp and vision of mathematics, and presumably, it will remain so, as mathematical interests have been fading into the background. Also in your translation of my letter, which I glanced through, I found some mistranslations – for instance (p. 5) it wasn’t at all along Serre’s ways and style to suggest “ambitious theories” on local duality and the like, rather I was pondering heuristically in this

direction, with minimal technical background (there was no étale cohomology yet) – none of this has been worked out since, as far as I know, with the exception of some related work of Contou-Carrère on generalized local and global Jacobians, in the relative case with relative dimension 1 [53]. (Things come out quite beautifully indeed...) However, I am struck a lot less by the few mistranslations here and there, than by the fact that you were at all interested to, and managed to, decipher those letters and make some sense of them, at least draw some inspiration or encouragement – and, in the stride, to present a translation which makes sense. To finish this letter, please accept my heartiest (although belated!) wishes for a very happy and successful year 1983.

Yours very cordially

Alexander

P.S. The idea comes to my mind that the French mathematician Bénabou, in his thesis (?), developed a very topological approach to n -categories. I couldn't say whether his n -categories are strictly associative on all levels, nor if there is any hint in his work about an actual dictionary between n -homotopy types and n -groupoids. His thesis should be more than fifteen years ago – I don't know the reference (just happened to be there when he had his soutenance) nor the present whereabouts of Bénabou. His name though should be in the list of members of the Société Mathématique de France. His main accent, if I remember well, was not on topology though, but on coherence properties for \otimes and Hom type internal functors in a category.

Lettre de Ronald Brown à Alexandre Grothendieck, 07.03.1983

7th March, 1983

Dear Alexander,

It was a great pleasure to receive your letter of 15/2, and also the copy of your long letter to Quillen, which arrived this morning. I thank you particularly for your exemplary patience with my mis-statements, and for your good wishes for 1983.

I was entirely aware of the limitations of crossed complexes, but they are not entirely abelian, as they do satisfactorily include the fundamental group and its actions. They seem to give a technical advance on the use of chain complexes of modules over a group (or groupoid), and indeed the precise relationship between this latter idea and that of crossed complex (and so of ∞ -groupoid) is the subject of a paper that Philip

Higgins and I are writing. I have just found some applications to second homotopy groups and to the second homology of groups which it seems more difficult to obtain by other means, if at all. For this and other reasons, it does seem reasonable to regard the work done so far as the first step in constructing a non-commutative homological algebra, and a step with a lot of precise detail in which to suggest analogies and possibilities for what might happen in the further stages.

I have also been clear from your first letters that you have been interested in non-strict gadgets, and this presents an interesting contrast in philosophy and approach. One striking point which interests me is that Loday's gadgets [99], n -cat-groups, are strict gadgets, and yet *they do capture truncated homotopy types*. Indeed, the definition is of an amazing simplicity: it is simply an $(n + 1)$ -fold groupoid in which one structure is a group structure, so that it can also be described as an n -fold groupoid internal to the category of groups, that is, as some form of group with additional structure. So we have an extraordinarily clean kind of algebraic structure for modelling homotopy types, and the implications of this might be rather enormous. The 1-cat-groups are just the same as crossed modules, which you describe in your third letter to Larry Breen, and the 2-cat-groups have been described by Guin-Waléry and Loday as what they call a "crossed square" [83].

This consists of a diagram

$$\begin{array}{ccc} L & \longrightarrow & M \\ \downarrow & & \downarrow \\ N & \longrightarrow & P \end{array},$$

together with actions of P on L , M , N , making all the maps in the above diagram, and also $L \longrightarrow P$, crossed modules, and the diagram giving morphisms of crossed modules, together with a function $h : M \times N \longrightarrow L$ satisfying various properties analogous to those of commutators, and which are written in detail in Loday's J. of Pure and Appl. Alg. 24 (1982), pp. 179–202, paper [99]. A student here, Graham Ellis, has succeeded in characterising crossed cubes [67]. All of these structures are *strict*, and this is very useful from the point of view of computation, since solutions to universal problems in this context, and so descriptions of the results of a van Kampen theorem, are given in terms of generators and relations for groups.

This whole problem of computation is one that worried me at various stages and various levels of the development of the previous theory. Initially, it seemed a good idea to get rid of base points in homotopy theory, and go overboard for the use of groupoids, in particular the fundamental groupoid $\pi_1 X$ of a space X . However, to compute an explicit example, one usually wants to consider the fundamental groupoid $\pi_1(X, X_0)$, where X_0 is a subset of X . Usually, X will be given as a union of open sets, and X_0 will be taken as one point in each path component of the intersections of the various sets. Thus it gradually came to seem that the fundamental groupoid should

be regarded not as a functor of X , but as a functor of the pair (X, X_0) , particularly as in many situations, such as simplicial sets, the set X_0 is given as part of the situation, for example as 0-skeleton.

Similar problems occur in higher dimensions. For a homotopy theorist, like myself, it is reasonable to replace a space X by its singular complex SX . It is known that SX is a Kan simplicial set, but very little else seems to have been written down on its algebraic structure. There seem to be lots of ways of gluing the model simplices together, and so of obtaining various higher dimensional compositions to make the singular complex into some kind of ∞ -groupoid, but with all the axioms being up to coherence, and the rules which obtain being effectively derived from the convex, linear structure on a simplex. In one sense I was taking a naive and simple-minded approach, namely that such a gadget is too complicated, take the horrible thing away! Your letters, and particularly the last one to Quillen, seem to show the sort of hold one might be able to get on this kind of structure. But there is still the general question: how can such a formulation lead to computations?

The second point is that I was trying to follow out one particular idea, namely a kind of suggestion for a proof of a higher dimensional van Kampen theorem, in which the problem was to find an algebraic gadget in which a particular geometric idea could find expression, and this was that one should be allowed to talk about the boundary of a cube being the composition of its faces, and that one should also be able to perform cancellations. This meant one needed two basic lemmas, first of all some form of the homotopy addition lemma, and secondly some form of *strict* groupoid. So in some sense your point about lack of overall vision, with motivation coming from wide areas of geometry, could be put even more starkly, in that the whole theory, the heap of reprints and preprints, has really been developed in response to one, or possibly two, geometric ideas! So I would only claim credit here for a certain bulldog determination, not to give up an idea until the last amount of juice has been extracted. In some respects, I have been influenced by my impression of my supervisor, Henry Whitehead, and the way in which he followed through ideas, even if they seemed unfashionable. In the process, he managed to invent new theories (like simple homotopy types) well in advance of his time, and this is something I would certainly like to try and do, at least as far as necessary in response to a geometric problem. A further motivation here is one of taste, namely that I do not really like the present basic expositions of homotopy theory. I believe that the present Brown-Higgins theory of crossed complexes (and all the other “barbarously named” gadgets) does allow for an exposition of homotopy theory and singular homology up to and about the homotopy addition lemma and relative Hurewicz theorem, in a way in which the geometry is much better modelled by the algebra than in previous expositions. In spite of the limitations of these gadgets, some new results are obtained using them.

When I started on this theory (circa 1966), I was under the impression that it could lead directly to new results on absolute homotopy groups, and this was the direction which I tried to make things work. Unfortunately, I got myself hopelessly confused, and could not get definitions to go directly, at least with a view to a proof of a van Kampen theorem. It was only with the use of relative gadgets in working with Philip Higgins (1974) that things dropped into place, and the basic lines of the proof were very easy to write down. Similarly, for higher dimensions, the gadgets derived from filtered spaces seem to make a lot of sense, and to give a theory which is an advance on chain complexes. Now Loday's methods, and the joint work in which we have been involved [39, 40, 41], shows that good gadgets can be defined associated with n -cube diagrams of spaces, and that the gadgets, the n -cat-groups, are strict gadgets. I should say that the proof even for $n = 2$ has not yet been fully written down, although its main lines are clear, and for higher dimensions considerable reformulation will probably be needed. The advantage of having strict gadgets, if one can find them, is that they should lead to rather explicit computations in specific cases, and this indeed we have found even in the case $n = 2$. Such methods also bear a formal resemblance to some unpublished methods of Michael Barratt in computing homotopy groups, which use a spectral sequence whose construction involves n -adic homotopy groups, *i.e.* groups derived from n -cube diagrams.

16/3/83

So what I have done is to borrow your overall plan for a non-commutative homological algebra, involving some kind of multiple groupoid gadget, applied though to a gadget different to that which you had in mind. These n -cat-groups have at least some of the formal properties that seem required and which you envisaged.

This may at some stage throw light on the original programme: from your proposed gadgets one might be able to extract, in specific structured situations, n -cat-groups, gadgets which are available, and seem likely to satisfy a van Kampen theorem. So a lot of work is clearly needed to see how far your programme can be carried out in these terms. I have the feeling that the results Loday and I are getting are working towards expressions for “interactions” between sets of relations for a given set of generators of a group G . We obtain for example a new description of H_3G as the kernel of a commutator map $R \overset{F}{\wedge} F \longrightarrow R$, when

$$1 \longrightarrow R \longrightarrow F \longrightarrow G \longrightarrow 1$$

is exact, F is free and $R \overset{F}{\wedge} F$ is a “non-abelian exterior product”, a group given by generators $(r, f) \in R \times F$ and a fair number of relations which I will write out if you are interested. My expectation is that these are just glimpses of a new territory.

One teasing aspect is that the subject seems to be working out under the general labels you suggested, but not in the same precise form. In particular, we have nothing

analogous to a Picard groupoid, and certainly not a Picard n -groupoid. I do not know what to make of that.

The general problem of putting extra structure on the category **Top** of topological spaces so that **Top**(X, Y) has some structure of “coherent ∞ -groupoid” is being considered by my colleague Tim Porter (Bangor) in joint work with J.-M. Cordier (Amiens) [54], and I have shown your letters to Porter. They have come across some technical problems, for example a combinatorial difficulty in dimension 7 in realising a plausible definition of the nerve of an ∞ -category. Since they are pursuing this line strongly, I have left it to them, in order that I could concentrate on 2-cat-groups and crossed squares, which are immediately giving new kinds of results in homotopy theory and group theory. A further aim is to look at the generalisations of these methods to other algebraic systems, with a view to eventual geometric applications.

It will be interesting to hear of Quillen’s reactions to your letter. He is working with Loday on aspects of Connes homology for Lie algebras [100]. I will send him a copy of this letter.

I have some fears that this letter may be too long and rambling. But it reflects my delight at finding from you such a sympathy to the spirit of what I have been trying to do, and your encouragement to look at the methods in a wider context. I was down in the dumps last May, and your letters were something of a lifeline, particularly the way your intuitive feelings about “integration of homotopy types” requiring some form of multiple groupoid confirmed much of my own impressions.

So all this may yet startle the world (or at least a small part of the mathematical social structure). On the other hand it may not!

A striking application in a geometric problem would be helpful. But too much concentration on such immediate ends might deflect attention from the actual needs of the particular mathematical development, and so delay future applications. The new factor in the story has been Jean-Louis Loday’s confidence from my visit in Strasbourg in November 1981 that there is a van Kampen theorem for n -cat-groups, coupled with his theorem that n -cat-groups model truncated homotopy types. There is surely a lot of work to see clearly the implications of this.

However, it might be too easy to take too lowly a view of the previous work leading to “crossed cohomological algebra”. This mildly non-abelian theory is familiar to only a few. Henry Whitehead’s basic 1949 paper “Combinatorial homotopy II” [137] is understood by hardly any homotopy theorists (in spite of the fact that “Combinatorial homotopy I” [136] is a fundamental paper), and the work there can now be made available in a wider context.

It will be interesting to see what my colleagues make of my research proposals!

With very best wishes,

Ronnie

Lettre d’Alexandre Grothendieck à Ronald Brown, 12.04.1983

Les Aumettes, 12.4.1983

Dear Ronnie,

It was nice to hear from you again. Mathematically speaking, this time I didn’t have as strongly the feeling that most of what you said was passing above my head – presumably because I had started thinking after all about foundational matters in homotopy theory. Since my letter to Quillen (I didn’t get any answer yet, by the way), I finally started on a systematic reflection, which very quickly has diverged from stacks to an attempt to come to an understanding about manifold ways by which the homotopy category (Hot) can be obtained as a “localization” $W^{-1}M$ of some category M (of algebraic structures of some kind or other, say) with respect to a notion of “weak equivalence” W . I have the feeling I am coming to a good hold upon this question now, very different by the way from Quillen’s approach, whose aim was rather to get *away* from (Hot) and get more general kinds of categories where homotopy constructions make a sense, whereas mine was to come to a better understanding of Her Majesty (Hot) herself, and for the time being, of the way of approaching Her. Presumably this will turn out to be useful for the general study of stacks which I’d had in mind, but for the time being stacks are kind of forgotten, and I have a lot of fun getting at the “modelizing story”. (The pairs $(M, W \subset \text{Fl}(M))$ giving rise to a localization equivalent to (Hot) I call “modelizers”, as the word “model category” was already taken by Quillen, and his approach presumably will enter into play, too, at a later stage.) For the time being I’ve written up nearly 200 pages of informal reflections, typewritten and in English this time, as it grew out of the correspondence with you and Quillen. I now envision to publish those notes as they are – once the first bunch is finished, which may take still one month or two presumably. The idea occurred to me that, if you are interested enough, it would be a good thing if you read through those notes before they’re published and submitted your comments – the point is that if anybody, you or one or two among your friends working on homotopy theory should be able to make sense out of those notes. Thus, if here and there, or throughout, there should occur serious difficulties in communication, namely that the point or meaning appear obscure, this would show the need of adding explanatory notes to help the reader (if any!) and possibly even myself to understand

what it is all about. One difficulty for you, for instance, may be in the circumstance that rather explicitly, I used the notion of a topos and of a map of topoi, as an ideal means and guide to put geometrical and topological significance into purely algebraic situations. This may be a little hard for one who is not fairly familiar with these notions. Technically speaking, I do not really make any use of the notion of a topos it seems to me, but the whole technical setup would be kind of meaningless to me personally, if there wasn't the unifying intuition of the relevant topoi constantly behind the technicalities. These, by the way, go through amazingly smoothly, without really anything like a computation anywhere till now, and no reference whatever to semi-simplicial techniques so far. Thus it turns out (exaggerating just slightly) that for "nearly any" small category A , the category A^\wedge of presheaves on A (*i.e.* contravariant functors from A to (Sets)) is a modelizer – thus the category of standard simplices or standard cubes, and corresponding simplicial or cubical computations, are kind of ruled out from the outset, because these conventional categories are not considered any better than any other "test category" A for describing and investigating homotopy types. (A "test category" of course is one such that A^\wedge , with the natural notion of weak equivalence, is a modelizer – in a slightly stronger sense than the one I said before.)

What you write about Loday's n -Cat-groups [99] makes sense for me and is quite interesting indeed. When you say they capture truncated homotopy types, I guess you mean "pointed 0-connected (truncated) homotopy types". This qualification seems to me an important one – while they are presumably quite adequate for dealing with a number of situations, it is kind of clear to me they are *not* for a "passe partout" description of homotopy types – both the choice of a base point, and the 0-connectedness assumption, however innocuous they may seem at first sight, seem to me of a very essential nature. To make an analogy, it would be just impossible to work at ease with algebraic varieties, say, if sticking from the outset (as had been customary for a long time) to varieties which are supposed to be connected. Fixing one point, in this respect (which wouldn't have occurred in the context of algebraic geometry) looks still worse, as far as limiting elbow-freedom goes! Also, expressing a pointed 0-connected homotopy type in terms of a group object mimicking the loop space (which isn't a group object strictly speaking), or conversely, interpreting the group object in terms of a pointed "classifying space", is a very inspiring magic indeed – what makes it so inspiring is that it relates objects which are definitively of a very different nature – let's say, "spaces" and "spaces with group law". The magic shouldn't make us forget though in the end that the objects thus related *are* of different nature, and cannot be confused without causing serious trouble.

The intriguing thing about these group objects, which I didn't really understand as yet, is that whereas loop-type objects are definitively *not* strictly associative (whether working with semi-simplicial complexes, or with n -groupoids or whatever), one can

all the same get away with strict group objects. It reminds me strongly of two related observations; one by Giraud [74], that any fibered category over a base category B is fiber-equivalent to a strict one, corresponding to an actual *functor* $B \rightarrow (\text{Cat})$ (with strict associativities); the other by Mme Sinh [120], stating that any group-like category is “equivalent” (in the relevant) sense of this word in the context of Gr-categories to a strict one. In both cases the proof is so simple-minded that presumably, as soon as there will be a suitable language for expressing the “higher” analogs, involving n -categories (or ∞ -categories, which I like better now), the corresponding statement will come out just as simply.

The mere fact that n -Cat-groups do modelize (truncated, pointed, 0-connected) homotopy types isn’t really too surprising, nor exciting by itself. If it were only to get models related rather closely to the intuitions going with n -categories and n -groupoids, we could get away with a lot simpler structures still. Thus, instead of using the test categories Δ or \square of standard simplices or standard cubes, you could use a still simpler one, corresponding to “bi-gons” rather than to triangles or to squares, and which I call the “hemispherical” category \mathbf{O} of “standard hemispheres” of all dimensions, having just two face operators in each dimension (the “positive” and the “negative” hemisphere) and one degeneracy. The corresponding “hemispherical complexes” can be viewed as “ ∞ -categories without any composition laws” whatever – just the target and the source maps, and the degeneracies, corresponding to identities. These suffice for modeling homotopy types,⁽¹¹⁾ with the notion of weak equivalence valid in any category of the type A^\wedge (in any topos, as a matter of fact). Computation of homology and cohomology in terms of these complexes should be simpler still than in the semisimplicial or cubical game, because there are still less face operators – and accordingly, I’m pretty sure there should be a still simpler version of the Dold-Puppe theorem in this case – “simpler” at least as far as computations go when it comes to specific cases. One interesting question here which I did not clear up yet, is, whether weak equivalence for a map of hemispherical complexes can be explicitly tested in terms of the source and target maps, just the same way as if we had actual ∞ -groupoids or ∞ -Gr-stacks (never mind whether associativities are strict or not), when the homotopy groups can be computed directly in terms of these extra structures. When you write down the condition that you get isomorphisms for these, it turns out though that the condition makes sense in terms of the source-and-target structure alone, without having to use the composition laws at all (nor even degeneracies). This is a strange fact, which should be understood.

The question you raise “how can such a formulation lead to computations” doesn’t bother me in the least! Throughout my whole life as a mathematician, the possibility

⁽¹¹⁾ N. Éd. Cela n’est pas vrai.

of making explicit, elegant computations has always come out by itself, as a by-product of a thorough conceptual understanding of what was going on. Thus I never bothered about whether what would come out would be suitable for this or that, but just tried to understand – and it always turned out that understanding was all that mattered. I remember, when I first came into contact with abelian groups and complexes of groups and singular theory and all that, in the late forties, in Cartan's seminar and his courses at the ENS – the size of those singular complexes looked just completely crazy to me, how could anything reasonable possibly come out of such monsters. The same with injective resolutions of course. Finally, I realized that size doesn't matter in the least – what counts is a firm hold on the formal properties of the objects one gets, and develop a corresponding intuition of what is likely to happen in such or such situation. The same with étale cohomology – I myself wondered for a while how one possibly could ever get at anything like a single explicit cohomology group, having at hands just such a general nonsense definition via sites, injective resolutions and all that. Finally, it took a few days of intensive reflection to discover the two main formal extra properties which were lacking in the conventional formalism, and proving them, in terms of which the computation of cohomology for all standard varieties such as grassmanians, affine spaces, abelian varieties and the like, would go through just as smoothly as if working with the same things over the complex numbers, namely with true honest topological spaces. The funny thing now is that chaps like you are using the singular complex of a space as if it had been lying in their cradle already – but you raise your arms in the air and ask mercy when it comes to having a closer look at something which has the bad taste to be insistently around all the time in all kind of situations, including alas the singular semisimplicial one, without being duly authorized of doing so by the relevant textbooks. (I hope you don't mind my teasing you a bit!)

It is a surprise each time when getting a letter from you that you could make any sense of my previous one, or at any rate that you appreciated getting it and found some kind of stimulation in it. This brings me back to my suggestion of your pre-reading those notes of mine, which begin with my letter to Daniel Quillen (I guess he won't object my publicising it, I'll ask him anyhow), and which I guess should be a lot easier for you than my letters to Larry Breen, say. These notes are not intended to be dug through like a textbook or monograph, say, but to read as loftily, so to say, as they were written – therefore the reading may not be as demanding as the relatively impressive number of pages might suggest. So if you are interested and want to try at least, just tell me and I'll send you at once whatever will be written down then –

and send you the rest as I am writing it down, namely getting it straight myself. At any rate, I'll appreciate hearing from you again, and having your comments to mine!

Very cordially yours

Alexander

P.S. It struck me you wrote there wasn't any place for n -Picard categories in your panoply. This however should be for you the simplest thing in the world. Such an animal can always be described in terms of just an ordinary n -truncated chain complex of abelian groups

$$\cdots \longrightarrow 0 \longrightarrow 0 \longrightarrow L_n \longrightarrow L_{n-1} \longrightarrow \cdots \longrightarrow L_1 \longrightarrow L_0 \longrightarrow 0 ,$$

i -objects are just elements of L_i , an $(i+1)$ -arrow between two such, x and y , being essentially the same as an element h in L_{i+1} satisfying

$$d_{i+1}h = y - x ,$$

with evident composition (which is *strict*, in this setup). Arrows between arrows *etc.* are defined accordingly. This at least is what the familiar case $n = 1$ suggests, *i.e.* the case of just ordinary Picard categories. While I am writing, it strikes me though that there is an inbuilt inaccuracy in the way I just formulated things – if we want that arrows should determine their source and target, in the case $n = 1$ already, we should take as 1-objects, namely “maps” or “arrows”, not (elements of) the set L_1 , but $L_1 \times L_0$ instead, by considering that (u_1, u_0) stands for the “map” u_1 from the “source” u_0 to the target object $u_0 + du_1$ (N.B. Subscripts denote dimensions of objects). Iterating this description, we see that contrarily to what I rashly suggested, i -objects in the Picard n -category we are after are elements in

$$C_i = L_i \times L_{i-1} \times \cdots \times L_0 ,$$

where intuitively for me, the first component u_i is really *the* i -arrow I was thinking of at the start, whereas the other components are there for determining the iterated source objects. Quite explicitly, the source and target maps from C_i to C_{i-1} are given by

$$\begin{aligned} s_i(u_i, u_{i-1}, \dots, u_0) &= (u_{i-1}, \dots, u_0) \\ t_i(u_i, u_{i-1}, \dots, u_0) &= (u_{i-1} + du_i, u_{i-2}, \dots, u_0) , \end{aligned} \quad (12)$$

one readily checks indeed $s_{i-1}s_i = s_{i-1}t_i$, $t_{i-1}s_i = t_{i-1}t_i$. As for the degeneracy δ_i , it is given obviously by

$$\delta_i(u_i, u_{i-1}, \dots, u_0) = (0, u_i, u_{i-1}, \dots, u_0) .$$

(12) “Truncation”, *i.e.* “forgetting the first component”.

Without having looked for it, I just wrote down one-way the Dold-Puppe functor, from chain complexes to hemispherical abelian complexes; we read from this the way how to define the functor in the opposite direction, namely

$$L_i = \text{Ker}(s_i : C_i \longrightarrow C_{i-1}) .$$

Here I have been describing Picard n -categories in terms of abelian group objects in the category of hemispherical complexes (which has been so far the more familiar version for me). But of course, the emphasis is not quite the same when speaking of “Picard- n -categories” (or Picard- ∞ -categories, which is simpler after all than sticking to truncations all the time), or of chain complexes (or their hemispherical interpretation, which surely amounts just to abelian groups in the category of (strict) ∞ -groupoids). Thus, what one is really thinking of when speaking of “maps” between Picard n -categories, are maps in the *derived category* of the category of complexes – more accurately, we should not distinguish between the chain-homomorphisms which define the same map in the derived category (e.g. when they are homotopic) any more than between two isomorphic functors between two categories. Another point to keep in mind is that in the geometrical contexts where Picard categories and n -categories of all kind occur quite frequently (cf. examples in my letter to Larry Breen), these are practically never *strict*; but in the commutative set up, just as in the non-commutative, it seems to turn out that you always can “represent” your object by an “equivalent” strict one (where strictness here refers both to associativity and unity, *and* to commutativity).

Of course, the abelian case has comparatively little charm in the setup where I have been stating it – namely just over the one-point or “final” topos. For instance, as the category of abelian groups is of cohomological dimension 1, up to non-canonical isomorphism every chain complex can be viewed as one with zero differential operator – besides the homology groups, there are no homological invariants, the first to consider would be in the $\text{Ext}^2(H_i, H_{i+1})$ ’s, which are zero! The situation is a lot richer when looking at “Picard- n -stacks” over a topos – and it is through such global situations that they actually entered into the picture, as the name of course suggests. Another way of de-trivializing the situation, which is equally suggested by a number of geometric situations (especially in the theory of deformations of all kind of algebro-geometric structures, as studied extensively in Illusie’s thesis for instance), is by looking at n -Picard categories which, besides the additive structure, have also a ring k operating on it (where again the module structure need not be strict, I guess... poor us!). These now can surely be interpreted in terms of chain complexes of k -modules, viewed again essentially as objects of a derived category.

All this for the time being is pure heuristics (except the Dold-Puppe story, which is explicit and simple-minded enough), because as yet the language for giving precise meaning to the intuitions and vague statements is still lacking. But it has become

rather clear to me by now, or rather ever since my letter to Daniel Quillen, that the language can be developed, without getting stuck in messiness – and I guess I'll spend a little while having fun in trying to get that carriage off the mud!

A last word of comment about hemispherical complexes. Surely hemispherical group objects (not abelian ones now) should modelize pointed 0-connected homotopy types – presumably Loday has hit upon these a while ago, and I would be interested to know what *his* point is, or your collective guiding principle if any, to be interested in the highly more sophisticated ∞ -Cat-group structure. After all, the main appeal of using ∞ -groupoids as models, in terms of pure homotopy theory (*i.e.* without geometrical motivations in terms of non-commutative cohomological algebra), is that they allow for a direct description of homotopy groups, isn't it? But when dealing with group objects as models, isn't it true equally that you get the homotopy groups just as in the abelian case, namely taking $L_i = \text{Kern } s_i$ to get a (non-commutative) chain complex, and take its naive homology groups? But perhaps the point is that, just as in the commutative case, the composition law of maps in the ∞ -groupoid is determined *automatically* in terms of the group law – or more explicitly, that the forgetful functor (forgetting composition of arrows) from ∞ -groupoids (or even from ∞ -categories) to hemispherical complexes, induces an equivalence between the corresponding categories of group objects? Maybe this was kind of understood in between the lines in your letter, after all – and the structure of an ∞ -Cat-group is a lot simpler still than I thought from your letter it was.

Lettre de Ronald Brown à Alexandre Grothendieck, 21.04.1983

21th April, 1983

Dear Alexander,

This is just a quick note to say how pleased Tim and I would be to receive a copy of your notes on homotopy theory to see what comments we might find to make. If there is a chance of your making another copy, another person who would feel privileged to receive it is Dr. Jean-Marc Cordier. Dr. Porter has the address – I will fill it in later.

I am amazed at the way in which you come out with ideas in a short space of time to which we had gently been edging over a period of months or years. The hemispherical complexes we had termed *globular complexes*, and I do not know if group objects in this category model homotopy types. I think though that you are quite right about the Dold-Puppe-theorem, although this is not a point that had occurred to me earlier. But without writing down the details, it seems to me that this follows

from the equivalence between ∞ -groupoids and crossed complexes, proof by Brown-Higgins [30], because in a globular abelian group you can define the structure of an ∞ -groupoid by defining $a +_i b$ to be $a - s_i d_i a + b$, and I think one can check from this that one obtains the required equivalence. A similar idea for cubical complexes with connection is sketched at the end of my preprint on “An introduction to simplicial T-complexes” [21]. For cubical complexes, one really does have to have the connection, but it is rather irritating that a lot of the theory of cubical complexes with connection (for example realisations, group objects, *etc.*) have not yet been worked out.

I met Quillen at the British Mathematical Colloquium at Aberdeen earlier this month, and at first we were a little at cross-purposes because, since he is in Oxford this year, he had not yet received your letter. However, we quickly sorted things out, and I showed him all the correspondence. So he now also has copies of your letters to Breen, and I have also taken the liberty of sending him a copy of your latest letter. He promised to write to you, and also to get his secretary at M.I.T. to send on your letter.

Jean-Louis Loday has a very clear idea for a proof of van Kampen for n -cat-groups, reducing it to one statement about n -simplicial spaces [41]. We have some nice applications of the theorem for $n = 2$, but higher dimensional applications are at present pretty conjectural as there is a lot of algebra to be understood.

With very best wishes,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 02.05.1983

Les Aumettes, 2.5.1983

Dear Ronnie,

I am very glad that you and Tim Porter are going to read those notes of mine – or trying to read at any rate. So I'm going to send you the first bunch this week or the next, and look forward to any comments, about readability particularly. For the last three weeks, I haven't gone on writing the notes, as what was going to follow next is presumably so smooth that I went out for some scratchwork on getting an idea about things more obscure still, particularly about understanding the basic structure of (possibly non-commutative) “derived categories”, and the internal homotopy-flavored properties of the “basic modelizer” (Cat). Finally I got involved with getting an overall view of cohomology (= homotopy) properties of maps in (Cat), namely of

functors between “small” categories, modelled largely on work done long time ago about étale cohomology properties of maps of schemes. I am not quite through yet, but hope to resume work on the notes next week.

Don’t be amazed at my supposed efficiency in digging out the right kind of notions – I have been just following, rather let myself be pulled ahead, by that very strong thread (roughly: understand non-commutative cohomology of topoi!) which I kept trying to sell for about ten or twenty years now, without anyone ready to “buy” it, namely, to do the work. So finally I got mad and decided to work out at least an outline by myself.

Yours very cordially

Alexander

P.S. I got a letter from Loday with two reprints, one about his n -cat-groups [99]. Maybe I’ll find the suitable moment to look up what he did, and why! – It was O.K. of course to show my letters to Quillen – or to anyone you feel like including making copies.

Lettre de Ronald Brown à Alexandre Grothendieck, 07.05.1983

7/May 1983

Dear Alexander,

My last letter was written rather hurriedly in the impact of a new term with a lot to do, and it has just occurred to me that I should explain more about my mention of J.-M. Cordier at Amiens; also I was not clear if you have the secretarial assistance to produce copies easily, and I became worried about the prospect of you spending your time on mundane chores.

Jean-Marc Cordier was a student of Ehresmann, but realised he should broaden his studies away from pure category theory. Madame Ehresmann suggested the area of shape theory and collaboration with Tim Porter, who was then at Cork. Their collaboration has proceeded happily, and has led to a strong interest in the categorical foundations of homotopy theory, as leading both to interesting mathematics and a good way of presenting shape theory and the important area of *homotopy limits*. These are used by a number of writers (Bousfield-Kan [15], Vogt [128], Gray [76], and others), but the expositions leave a lot to be desired. The presentation which Porter and Cordier are working up into a set of notes is based on ideas of coherence, although I have not seen details of the latest version [54, 55]. The general range of

ideas seemed to have strong relations to the ideas you have outlined, and I know he would be both conscientious and clear in commenting on such a manuscript as you propose, and indeed would be delighted to have the opportunity.

However, I really don't want to presume too much – if you would be happy for him to have a copy, I would be pleased to copy it here, but I would understand if you wanted it more restricted for the moment.

Jean-Louis Loday was with me for five days in April, and things are looking promising on that front. He has an amazing outline proof of van Kampen for n -cat-groups [41], but there is one point *so far* about which I am unhappy.

I feel the theorem is “obviously true” (!), mainly because the corollaries we have found for 2-cat-groups are so clear, precise, and fit with so many other topics in which both Jean-Louis and I together (*i.e.* collectively) have an interest. On the other hand, I can quite see many topologists suggesting that the idea of computing homotopy types by a van Kampen type theorem is clearly absurd – how are you going to prove $\pi_6(S^2) \simeq \mathbf{Z}/12\mathbf{Z}$ by regarding S^2 as $E_+^2 \cup E_-^2$ with intersection S^1 ?

Of course, this baffles me as well. But I am in entire agreement with you (if that is not too boring a statement!) that the formal properties of the proposed gadgets should in principle lead to such calculations. This *may* be too optimistic, but on the evidence so far one can reasonably expect that *new* results will be produced, and in some cases one will get information not easily obtainable by other means.

I think I ought to explain some of the details of what we have in mind, otherwise it won't be clear why n -cat-groups have turned out to be such an excellent gadget. There are likely to be rather a lot of other nice gadgets. It would be interesting to know if there are some fundamental reasons why these particular gadgets seem, as far as our present limited vision goes, to be able to reach further than some other possibilities.

A fundamental idea is the equivalence between group objects in (Cat) and crossed modules, and of course a group object in (Cat) is also a Cat-object in (Group). In fact, the following data are equivalent (to repeat some well known ground):

- (i) A category object in (Group).
- (ii) A groupoid object in (Group).
- (iii) A pair of groups and homomorphisms

$$G \begin{array}{c} \xrightarrow{s} \\ \xrightarrow{b} \end{array} P \xrightarrow{i} G \qquad \begin{array}{l} s = \text{source} \\ b = \text{but} \end{array}$$

such that $si = bi = 1_P$ and $[\text{Ker } s, \text{Ker } b] = 1$.

- (iv) A simplicial group whose Moore complex is of length 1.

- (v) A crossed module, *i.e.* a group homomorphism $M \xrightarrow{\mu} P$ and an action of P on M (on the left) such that

$$\mu({}^p m) = p(\mu m)p^{-1}, \quad mm_1m^{-1} = {}^{\mu m}m_1, \quad m, m_1 \in M, \quad p \in P.$$

The details of this are given in Loday’s paper [JPAA 1982] [99], and most of it has been well known for some time.

Topologically, crossed modules arise because if $F \rightarrow E \rightarrow B$ is a fibration of pointed spaces, then $\pi_1 F \rightarrow \pi_1 E$ can be given the structure of crossed module (conjugating loops in F by loops in E). (This was observed by Quillen (I believe) extending an observation of Henry Whitehead, that $\pi_2(X, Y) \rightarrow \pi_1 Y$ can be given the structure of crossed module [137].) I think this fact on fibrations has not yet appeared in any textbook on algebraic topology or homotopy theory; nor indeed has a proof of Whitehead’s theorem that $\pi_2(X \cup \{e_\lambda^2\}, X)$ is a free crossed $\pi_1 X$ -module on the 2-cells e_λ^2 ; it was this result that was both evidence for, and a test case of, the Brown-Higgins theorem on pushouts of crossed modules [29, 32] (clearly, Whitehead’s result should be proved by verifying the universal property; on the other hand, a 2-dimensional van Kampen theorem ought to recover Whitehead’s theorem).

This raises an interesting point in relation to your programme.

In dimension 1, we do have the correspondence

$$\text{groups } G \quad \sim \quad \text{pointed aspherical connected spaces } K(G, 1)$$

and we have a

$$\begin{array}{ccc} \text{van Kampen} & \sim & \text{gluing} \quad (\text{integrating}) \\ \text{theorem} & & \text{homotopy types.} \end{array}$$

As you say, the better version uses groupoids instead of groups – this was my entrée into examining the use of groupoids in mathematics. (I have been attempting to develop groupoid techniques on the grounds that one should examine how and why they are useful. But in 1981 a Malaysian ex-research-student of mine told a famous visiting U.K. mathematician he was working on groupoids, only to be informed “Groupoids are rubbish”. This left me the tasks of patiently rebuilding the chap’s confidence, and of writing to the famous visitor objecting to his sabotage, and of pointing out to him that the work of Ehresmann, Grothendieck, Mackey, Connes, *etc.* who have used the notion. Incidentally, the reason Mackey uses the notion is interesting and may not be familiar to you, so I take a little space to explain it.)

G. Mackey has a strong interest in representations of locally compact groups, and in ergodic theory. Let the group G act on the set S . If the action is transitive, then it is equivalent to the action of G on the set of cosets $G/G(s_0)$, where $s_0 \in S$, and $G(s_0)$ is the stability group of the action at s_0 . That is

transitive action \sim subgroup.

Mackey decided to develop a correspondence

ergodic actions \sim “virtual” subgroup.

His theory went through various stages, and eventually came to the following formulation. (“Ergodic theory and virtual groups”, Math. Ann. 166 (1967), 187–207 [105].)

From the action of G on S , construct the groupoid $G \ltimes S$ (my notation) with object set S and an arrow $(g, s) : s \rightarrow s^g$ for all $s \in S$, $g \in G$, and composition

$$(g, s)(h, s^g) = (gh, s).$$

(This construction is of course well known to you.) If we wish to say the action is ergodic, then S must have a Borel and measure structure, so it is expected that G should be also a Borel group, and the action should be Borel. Then $G \ltimes S$ becomes a Borel groupoid, with some measure notion. Mackey’s idea is to define a “virtual group” (= “ergodic groupoid”) to be an equivalence class of such groupoids, two being equivalent if they are equivalent in the usual normal sense for groupoids, except that the morphisms and natural equivalences must be Borel functions, and also that they are required to be defined only *almost everywhere*. (The above may not be right in detail but I hope gives the general flavour.) This theory has been developed by Mackey and his school in many papers which are heavily measure theoretic, but have strongly influenced the work of Connes. The reason seems to be that given a suitable measure structure which is left invariant on a groupoid G , one can define a convolution algebra $C(G)$, and this becomes a non-commutative C^* -algebra. Thus one has an intriguing link with the idea that groupoids are essential for investigating *non-commutative* aspects of many traditional ideas.

At first sight, this sounds an extraordinary idea. But one finds amazing analogies.

What is a group object in (group)? An abelian group.

What is a groupoid object in (group)? A crossed module (a non-commutative gadget).

What is a *group* object in the category of commutative algebras (not necessarily with 1)? An algebra with zero multiplication.

What is a *groupoid* object in the category of commutative algebras? A “crossed module of algebras”, *i.e.* a morphism $\mu : M \rightarrow P$ of commutative algebras (not necessarily with identity) such that M is also a P -module, μ is a morphism of P -modules, and also the multiplication in M satisfies

$$mm' = \mu(m)m' \quad \forall m, m' \in M.$$

Thus a crossed module in this context should be regarded as an externalisation of the notion of ideal. Also, if $\mu = 0$, then M is simply a P -module, and is in fact an abelian group object in the category of crossed P -modules.

(Writing this letter may be very useful to me. I had agreed to write a survey article on “Groupoids in mathematics” for the Bulletin of the Australian Mathematical Society since a friend of mine is an editor, and the more I wrote, the more boring it became. I am now inclined to stick my neck out and write a frantically speculative article about *groupoids and non-commutative aspects of mathematics*, somewhat in the vein of this letter, and emphasizing the theme of structured groupoids as a key aspect of the mathematics of the 1990’s. I guess it may be presumptuous of me to attempt to do this, but I can’t think of many who have kept themselves informed of various strands of work going on, and so can attempt to draw them together, even if my working knowledge of the analysis side is very superficial.)

Similarly, one finds *groupoids in* (Lie algebras) are interesting gadgets, again forms of crossed modules. Such is a morphism $\mu : M \rightarrow P$ of Lie algebras, such that P acts on M , μ preserves the action, and also $[m, m'] = \mu(m)m'$ for all $m, m' \in M$. (One needs also that the action $P \times M \rightarrow M$ is bilinear and via derivation, *i.e.* $[p, p']m = p(p'm) - p'(pm)$ and $p[m, m'] = [pm, m'] + [m, pm']$.)

Kassel and Loday have used these gadgets recently in relation to Connes homology (an article in the last issue of the Annales de l’Institut Fourier [93]).

But let me get back to my theorem of p. 34. In dimension 2, we have the correspondence

$$\begin{array}{ccc} \text{crossed modules} & \sim & \begin{array}{l} \text{pointed, connected} \\ \text{homotopy types with} \\ \pi_i X = 0 \text{ for } i > 2 \end{array} \end{array}$$

and we have a

$$\begin{array}{ccc} \text{van Kampen type the-} & \sim & \text{integrating homotopy} \\ \text{orem (due to Brown-} & & \text{types.} \\ \text{Higgins [29, 32])} & & \end{array}$$

If one doesn’t like the connectedness condition, then one moves to “crossed modules over groupoids” defined by Brown-Higgins in “The algebra of cubes” [32].

The point towards which I am moving is to ask whether the grand scheme you have been evolving for applications in algebraic geometry, can be worked out in detail at this level, where the algebra and homotopy theory can be claimed with some justification to be well understood?

9/5/83

On second thoughts, the words “well-understood” are overoptimistic. But at least there is a recognisable theory in which the Hilbert programme of “syzygies among syzygies”, which leads to free resolutions of modules, is replaced by the notion of “identities among relations”, where the crossed modules, and particularly the free

crossed modules over free groups, play a key role. This is spelled out in some detail in Brown-Huebschmann “Identities among relations” [35], but the main point is that just as in specifying a relation between generators, one has to work in a free group, so in specifying an identity among relations, one is saying when a *specified* product

$$c = (r_1^{\varepsilon_1})^{u_1} \cdots (r_n^{\varepsilon_n})^{u_n}, \quad \varepsilon_i = \pm 1, r_i \in R, u_i \in FX,$$

(arising from a presentation $P = (X, R)$ of a group G) has $c = 1$, and this can only be expressed by considering the free group H on $R \times FX$ with elements of $R \times FX$ written ${}^u p$ instead of (p, u) say, so that H is the free FX -operator group on R . This gives a *precrossed* FX -module $\theta : H \rightarrow FX$, ${}^u p \mapsto uru^{-1}$ (*i.e.* the second relation for a crossed module is not satisfied). Factorising by the Peiffer relation

$$hkh^{-1} \theta^h(k^{-1}), \quad h, k \in H,$$

gives a crossed module $\partial : C \rightarrow FX$, whose kernel is the G -module of identities among relations.

This procedure seems reasonable for any algebraic system, and Tim Porter has found that the corresponding theory does give a natural expression for ideas long familiar in commutative algebra (A-sequence, Koszul complexes, *etc.*). So the idea that

$$\text{“groupoid objects} \sim \text{crossed modules”}$$

has a payoff for commutative algebras.

Where does the theory go next? Here one comes to Jean-Louis Loday’s remarkable set of ideas, which in relation to group theory and homotopy theory are completely clear.

We have: a Cat^1 -group is a category (or groupoid) object in (Group). The next stage is to consider a category object in (Cat^1 -group), giving (Cat^2 -group). So by induction, one gets (Cat^n -group). (We have modified the terminology because n -categories are really special cases of n -fold categories, and it is the latter that are required here.)

Alternatively, a Cat^n -group is a group G with a family of subgroups P_i , $i \in \{1, \dots, n\}$, and morphisms

$$G \begin{array}{c} \xrightarrow{s_i} \\ \xleftarrow{b_i} \end{array} P_i$$

such that

- (i) $s_i|P_i = b_i|P_i = 1_{P_i}$,
- (ii) $[\text{Ker } s_i, \text{Ker } b_i] = 1$,
- (iii) $s_i s_j = s_j s_i$, $s_i b_j = b_j s_i$, $b_i b_j = b_j b_i$ for $i \neq j$.

So we have n compatible category structures on G .

The Cat^2 -groups are equivalent to *crossed squares*. These consist of a square

$$\begin{array}{ccc} L & \xrightarrow{\lambda'} & N \\ \lambda \downarrow & & \downarrow \nu \\ M & \xrightarrow{\mu} & P \end{array}$$

of morphisms of groups, together with an action of P on each of L , M , N and a function $h : M \times N \rightarrow P$ satisfying the following conditions, in which M acts on N , L via μ and N acts on M , L via ν .

- (i) μ , ν and $\kappa = \mu\lambda = \nu\lambda'$ are crossed P -modules, and λ , λ' are P -maps (whereas λ , λ' are crossed modules),
- (ii) $\lambda h(m, n) = m {}^n m^{-1}$,
 $\lambda' h(m, n) = {}^m n n^{-1}$,
- (iii) $h(mm', n) = {}^m h(m', n) h(m, n)$,
 $h(m, nn') = h(m, n) {}^n h(m, n')$,
- (iv) $h(\lambda l, n) = l {}^n l^{-1}$,
 $h(m, \lambda' l) = {}^m l l^{-1}$,
- (v) $h({}^p m, {}^p n) = {}^p h(m, n)$

for all $m, m' \in M$, $n, n' \in N$, $l \in L$, $p \in P$.

The formal reason for this equivalence is that $L \rightarrow N$ is in some sense a “crossed module over the crossed module $M \rightarrow P$ ”. Hence $M \rtimes P$ operates on $L \rtimes N$, and so we can form

$$G = (L \rtimes N) \rtimes (M \rtimes P),$$

or, more symmetrically,

$$G = \left(\begin{array}{cc} L & M \\ M & P \end{array} \right)_{\rtimes}.$$

In effect, G becomes a group with two “compatible” semi-direct product descriptions. The real novelty in the axioms for a crossed square is the function h , which is directly related to the fact that $\pi_3(S^2) = \mathbf{Z}$ (by van Kampen for Cat^2 -groups).

Given two crossed modules $M \xrightarrow{\mu} P \xleftarrow{\nu} N$, we can form a “universal crossed square”

$$\begin{array}{ccc} L & \longrightarrow & N \\ \downarrow & & \downarrow \nu \\ M & \xrightarrow{\mu} & P, \end{array}$$

and then we call L the “non-abelian tensor product” and write it $L = M \overset{P}{\otimes} N$. It has generators $m \otimes n$, where $m \in M$, $n \in N$, and relations

$$\begin{aligned} mm' \otimes n &= {}^m(m' \otimes n)(m \otimes n) \\ m \otimes nn' &= (m \otimes n) {}^n(m \otimes n') \\ m {}^n m^{-1} \otimes n' &= (m \otimes n) {}^{n'}(m \otimes n)^{-1} \\ m' \otimes {}^m n n^{-1} &= {}^{m'}(m \otimes n)(m \otimes n)^{-1}, \end{aligned}$$

where ${}^x(m \otimes n) = {}^x m \otimes {}^x n$. If P acts trivially on M and N , or if $\mu = 0$ and $\nu = 0$, then

$$M \overset{P}{\otimes} N = M \otimes_{\mathbf{Z}} N,$$

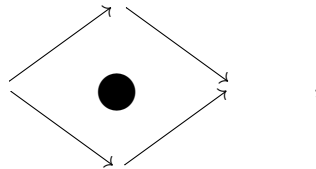
the usual tensor product of abelian groups. But in general, $M \overset{P}{\otimes} N$ is non-abelian. For example, if M, N are normal subgroups of P , then

$$\lambda : M \overset{P}{\otimes} N \longrightarrow M$$

has image $[M, N]$. So we are truly in a non-abelian situation. Some of our applications of the putative van Kampen theorem for Cat^2 -groups involve this tensor product.

I think it is about time I stopped because tomorrow I go to our University of Wales Pure Maths Colloquium at Gregynog Hall in mid-Wales. This is a private colloquium for the pure mathematicians in this university, but we have two outside speakers giving two lectures each, and one lecture from someone at each of Bangor, Aberystwyth, Cardiff and Swansea. I am giving a talk on “A survey of groupoid methods in mathematics”, a grandiose title under which I have now decided to indulge in speculation, in order to explain why I have for some years been investigating groupoid methods, to the dismay (so I believe) of some of my colleagues in topology in the U.K.

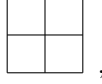
(In spite of this dismay, Mackey’s school in the USA have been developing ergodic groupoids, holonomy groupoids, *etc.* In some ways, the holonomy groupoid, regarded as the non-commuting of parallel transport round a square



again conveys a non-commutative association.)

I would also like to believe that as groupoids arise naturally in term of flows, which are themselves related to ordinary differential equations, so multiple groupoids and multiple compositions could prove themselves related to partial differential equations.

(Atiyah once asked himself as a student: what happens if time is 2-dimensional? He started to consider compositions of squares



but apparently never got very far, at least he told me nothing more.)

How wise is it to indulge in such speculation? This hides two questions: privately, or publicly? In truth, I rather enjoy rambling on in this way. But on an occasion in 1976 when I rambled (at a British Mathematical Colloquium splinter group) about T-complexes, which had only just been invented by Keith Dakin [56, 57], and explained what they were supposed to be for, I got some odd looks, so perhaps I should then have explained how the van Kampen theorem for crossed modules gave explicit calculations of presentations of second relative homotopy groups.

Next week I give lectures at Athens, Xanthi and Thessaloniki – I am really looking forward to that. The impetus for the visit came from a general topologist at Xanthi who is interested in function spaces. I’m hoping he may become interested in the development of practical applications of a “topological topos”, *i.e.* a topos found by Peter Johnstone [90] which includes the usual category of sequential topological spaces. I suspect this could have important implications for analysis, which at present concentrates on functions with a given domain rather than with varying domain. This again is another speculative indulgence, since I don’t have any theorems in mind. What is true is that talking to Mac Lane in 1972 about topoi led to a very nice topology for giving an exponential law for the category of spaces over B , and this has useful applications in homotopy theory. But this is restricted to the case B is T_0 , so that the fibres of $p : E \rightarrow B$ are closed, and for some areas of mathematics such as foliations it seems necessary to topologise the set of arbitrary subsets of a space.

Here is an example. Let \mathbf{Z} = integers, \mathbf{R} = reals, and for $t \in \mathbf{R}$, let

$$\begin{aligned} f_t : \mathbf{Z} \times \mathbf{Z} &\longrightarrow \mathbf{R} \\ (m, n) &\mapsto m + nt. \end{aligned}$$

Then the range of f_t is dense or discrete according as t is irrational or rational. Now on any reasonable topology on $\mathbf{R}^{\mathbf{Z} \times \mathbf{Z}}$, the function $\mathbf{R} \rightarrow \mathbf{R}^{\mathbf{Z} \times \mathbf{Z}}$, $t \mapsto f_t$, is continuous (its adjoint $\mathbf{R} \times \mathbf{Z} \times \mathbf{Z} \rightarrow \mathbf{R}$ is $(t, m, n) \mapsto m + nt$). In a topos, the function $X^Y \rightarrow \mathcal{P}(X)$, $f \mapsto \text{range of } f$, is a morphism. So if we had a topological topos, then we have a curious “continuous family” of subsets of \mathbf{R} . Another good example is the flow⁽¹³⁾ on a torus of angle λ , considered as a function of λ . This again should be continuous, even \mathcal{C}^∞ . But how should this be defined? I must conclude this

⁽¹³⁾ But considered just as a subset of \mathbf{T}^2 , not as a function $\mathbf{R} \rightarrow \mathbf{R}^2$.

letter. Let me return to the non-abelian theme with a point I had originally intended to make, even if in vague terms.

One of your letters to Breen concludes with questions about resolutions. What then is a non-abelian resolution? There seems to be a range of possible answers, and the one that fits most easily into the present ethos of algebraic topology is to consider a simplicial group, possibly free in each dimension. Now Loday's ideas [99] seem to point to a different method, namely a cubical resolution. *I.e.* to resolve X , first choose a free K_1 and a map $K_1 \rightarrow X$ with kernel K_2 . Now resolve the map $K_2 \rightarrow K_1$ to give a square

$$\begin{array}{ccc} K'_2 & \longrightarrow & K_2 \\ \downarrow & & \downarrow \\ K'_1 & \longrightarrow & K_1 . \end{array}$$

Now resolve the square to give a cube, *etc.* This seems to be behind the construction of a Cat^n -group from a space X . But at present I don't guarantee the above in detail. What is clear is that this range of ideas, *i.e.* Cat^n -algebras, gives a new range of algebraic objects. We know what are Cat^3 -groups, Cat^2 -(commutative algebras), Cat^2 -(Lie algebras), but the appropriate techniques for this kind of homological algebra are a long way from being developed. It is only the Cat^1 -groups or Cat^1 -algebras where the relevant notions have a long history (Gerstenhaber, *etc.*)

I was entertained by your teasing – perhaps you will get worried if you find it results in a long, closely written, rambling reply!

13/5

Once again I've had my programmes turned down by S.E.R.C., but the marks are going up, and the number of referees who very much like the proposals has only gone up. Apparently, there was a clear split – either totally for or totally against the proposal, and this as a general reaction I have found to what I have been doing in groupoids for some years. I try to present a balanced view: if groupoids occur and are useful, they should be used. So I find the “Groupoids are rubbish” school of thought rather curious. It reminds me of the famous doggerel about a Master of Balliol College, Oxford.

“First come I; my name is Jowett.
There's no knowledge but I know it.
I am the Master of this College.
What I don't know, isn't knowledge.”

However, the proposals will be sent again, and again, and again, each time with more solid foundation and less clearly speculative. I think also some of our big

names in the U.K. have very black and white views about what is and what is not mathematics, so much the worse for them! If someone does not see that a van Kampen theorem for crossed modules is a new type of result, then there is not much I can do for them.

Where your overall attitude seems to me so encouraging, is that it suggests that if you find a faint, smudged footprint which does not seem to fit with any known animal, then it is a good idea to investigate further. The history of science surely confirms this. What was a surprise to me in my own experience with this area was how long it took, and how much help I needed, to follow up the few clues I had.

The scent is now getting very much stronger, I could probably do with a much sharper machete, but the hunt is rather fun.

Tim Porter and I look forward to seeing your manuscript.

Sincerely yours,

Ronnie

Lettre d’Alexandre Grothendieck à Ronald Brown, 10.05.1983

Les Aumettes, 10.5.1983

Dear Ronnie,

I guess that when you get this letter, the first bunch of notes I gave for xeroxing will have reached you. Any comments you and Tim Porter care to make will be welcome – whether on substance, presentation, style, linguistic mistakes, etc. There are just the first few pages of part III missing, which I will send when part III will be written down.

At present just a silly technical question, due to my ignorance of the standard facts of homotopy theory. Namely, let $X \supset Y$ be a pair (of semisimplicial sets, say), assume X/Y (deduced by contracting Y to a point) is aspheric, i.e. $X/Y \rightarrow e$ a weak equivalence – does it follow that $Y \hookrightarrow X$ is a weak equivalence, and if so, why (in terms of standard exact sequences or the like, say)? The dual statement, for a Serre or Kan fibering $X \rightarrow Y$ with aspheric fibers being a weak equivalence, is clear to me, in terms of the exact sequence of homotopy groups, or (if using the cohomological description of weak equivalences) in terms of the Leray spectral sequence for the fibering.

In a rather different direction, not really tied with my present reflection: it occurred to me a while ago that the profinite completion $\widehat{\mathbf{Z}}$ of \mathbf{Z} , viewed as a multiplicative

monoid, operates on the profinite completions of homotopy groups of spheres $\pi_i(S_n)$, and hence on those homotopy groups which are finite. (This occurred to me first through the action of the maximal subgroup $\widehat{\mathbf{Z}}^*$, interpreted as the Galois group of the maximal cyclotomic extension of \mathbf{Q} .) A simple way of describing it, is by noting that the degree map yields an operation of $\widehat{\mathbf{Z}}$ on the profinite homotopy type of any sphere S_n . I wonder if this operation has been noticed and investigated by the homotopy theorists.

Yours very cordially

Alexander

Lettre d'Alexandre Grothendieck à Ronald Brown, 23.05.1983

Montpellier, 23/5/83

Dear Ronnie,

I'm sending you the four pages of the "Stacks pursuit" which were not included with the bunch sent to you lately – it finally took a little longer than expected here to have it sent.

On page 44 I assert as a well-known fact that in (Hot) finite limits exist. This of course is a mistake, which I carried along for a little while. As a matter of fact, the so-called "integration" of homotopy types is just a substitute for the lacking direct limit in (Hot) – in the notes there will be a very smooth treatment of this basic operation, in due course. It makes sense for any small indexing category I , for a direct system $(X_i)_{i \in I}$ with values in practically any modelizer M – but the one ideally suited for expressing $\int X_i$ is (Cat). The main property is that for a map of direct systems

$$(X_i) \longrightarrow (X'_i)$$

with some indexing category I , if this is componentwise a weak equivalence $X_i \rightarrow X'_i$, then the induced $\int X_i \rightarrow \int X'_i$ is a weak equivalence.

I believe that the dual operation $\prod X_i$, replacing inverse limits, should make a sense, too, at least for finite indexing categories – possibly we'll even have to assume I ordered, i.e. a family $(X_i)_{i \in I}$ is just a commutative diagram in M of type I . However, the construction is surely a lot more delicate. I carried in through only in case of $I = \bullet \begin{smallmatrix} \nearrow \\ \searrow \end{smallmatrix} \bullet$, namely getting the substitute for fiber products in (Hot), including notably the well-known operation of taking the "homotopy fiber" of a map $X \rightarrow Y$, with Y pointed.

The only type of finite limits apparently which actually exist in (Hot), are sums and products (including infinite ones). I tried to check that filtering countable direct limits exist in (Hot), which brings us to the situation of a sequence

$$X_0 \hookrightarrow X_1 \hookrightarrow X_2 \hookrightarrow \cdots \hookrightarrow X_n \hookrightarrow \cdots$$

of inclusions of semisimplicial sets, say, and to check that for any Kan complex Y , the natural map

$$\overline{\mathrm{Hom}}(X_\infty, Y) \longrightarrow \varprojlim \overline{\mathrm{Hom}}(X_i, Y)$$

is bijective, where $X_\infty = \varinjlim X_i$, and $\overline{\mathrm{Hom}}$ denotes homotopy classes of maps. The map turns out to be surjective, but I suspect it is not always injective. Do you have a counterexample? I finally doubt filtering countable direct limits exist in (Hot).

As during the vacations I will not be at the University (where otherwise I am only once a week anyhow), it would be more convenient to write to my personal address:

A. Grothendieck, Les Aumettes,
84570 Mormoiron, France.

With my best wishes

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 25.05.1983

25/5/1983

Dear Alexander,

This is to acknowledge your two letters and the receipt of the 191 pages of notes. Tim and I are really looking forward to seeing what is in them – we are making extra copies so that we can read and comment independently. A lot of Tim's stuff on coherent prohomotopical algebra [111, 112] is relevant to these matters, and he will be sending some offprints and writing separately about this. He is a quick and imaginative person with an excellent range of knowledge in homotopy theory, commutative algebra and categorical methods – so particularly well qualified for this task, which links very well his and my separate programmes.

I want to make two simple mathematical points in answer to some of your questions before getting on with clearing my desk.

- (i) Quillen's $+$ -construction gives, for a connected CW-space and perfect normal subgroup π of $\pi_1 Y$, a map $i : Y \longrightarrow Y^+$ such that i induces an isomorphism in homology (in fact, in cohomology with all local abelian coefficients) and such

that $i_* : \pi_1 Y \rightarrow \pi_1 Y^+$ is epic with kernel π . By using mapping cylinders, we may assume i a cofibration. Then Y^+/Y has trivial π_1 (by van Kampen) and trivial homology (by the long exact homology sequence). So Y^+/Y is contractible, but $i : Y \rightarrow Y^+$ is not a weak equivalence if $\pi \neq 1$.

- (ii) There are CW-complexes X, Y and maps $f : X \rightarrow Y$ such that f is not null-homotopic, but $f|X^n$ is null-homotopic for any $n \geq 0$. (Such maps are called *phantom maps*.) Here one can take Y to be a sphere. The examples come from an exact sequence of sets

$$(*) \quad 0 \rightarrow \varprojlim^1 \overline{\text{Hom}}(SX^n, Y) \rightarrow \overline{\text{Hom}}(X_\infty, Y) \rightarrow \varprojlim \overline{\text{Hom}}(X^n, Y) \rightarrow 0,$$

where \varprojlim^1 is the first derived functor of \varprojlim . One reference is B. Gray, "Spaces of the same n -type, for all n ", *Topology* 5 (1966), 241–243 [75]; more recently, W. Meier and R. Strebel, "Homotopy groups of acyclic spaces", *Quart. J. Math.* (2) 32 (1981), 81–95 [107]; or better W. Meier, CRAS Paris 281 (1975), 787–789 [106], who gets an exact sequence like $(*)$ for $X = \bigcup X_\alpha$ a direct system of finite subcomplexes, Y a rational H-space of finite type.

I will leave your question about $\widehat{\mathbf{Z}}$ -operations on profinite completions of homotopy groups to Tim, who also has lots of useful things to say on homotopy limits and colimits, as he is writing an exposition of this area.

With very best wishes, and with great interest in your notes,

Ronnie

Lettre d'Alexandre Grothendieck à Ronald Brown, 14.06.1983

Montpellier, June 14, 1983

Dear Ronnie,

Please excuse the belated answer to your long and interesting letter of last month, and the shorter one acknowledging receipt of the first bunch of notes. It's nice you have been taking the trouble to have the notes copied and sent to colleagues whom you think may be interested – and still nicer to have such a lot of patience for explaining things so painstakingly to an outsider and mere "passant" like me. It was a relief to get rid of the misconception I had about Y/Z contractible implying $Y \rightarrow Z$ weak equivalence. Also, my reflection of the last three months bring me closer to an ability for appreciating some of your comments – for instance what you say in your last (shorter) letter about homotopy groups of "pushouts" of $K(\pi, 1)$ -spaces. This really does give a strong a posteriori motivation for the introduction of non-abelian

tensor products of groups. The one or two instances before when you mentioned this operation, I just wasn't ready yet to make much sense of it.

What you write about various equivalent formulations of the notions of “crossed module” has been kind of familiar to me, via category theory however much more than via homotopy theory. My point of view was rather that I was interested in so called Gr(oup-like)-categories, as a commonly met-with non-commutative variant of Picard categories, and the relevant notion here was again rather *equivalence* of categories (respecting the product operation up to given compatibility isomorphisms) rather than isomorphism. As in the “Picard”-case, it turns out that such an object is equivalent to a strict one, namely a group object in \mathbf{Cat} . (I wasn't aware those objects had been introduced by H. Whitehead a long time ago.) But I was interested in those gadgets rather as objects of a “derived category”, regarding as “essentially the same” two such truncated complexes, if they were related by a homomorphism inducing an isomorphism on π_0 and π_1 . This has been the heuristic way for me to visualise (in terms of a concept clear to me) 2-truncated 0-connected pointed homotopy types (with π_1 and π_2 relabelled π_0 and π_1 , as customary when passing to a loop space). I confess that, while from the beginning of our correspondence you have been referring repeatedly to so-called “crossed modules”, there has been a kind of psychological block in my head against these, till your recent letter when you took the trouble to be really specific about concepts. The main reason for this block, I believe, was that the terminology “crossed module”, suggesting that the structure is concerned with “module” of some kind or other, seems terribly inadequate and misleading. I am sure, even if I had been a student and close friend of H. Whitehead, I wouldn't have followed him using such a name, for such a nice object!

Maybe some day you'll tell me what this Brown-Higgins theorem “on pushouts of crossed modules” [29, 32] is, and possibly even the Brown-Loday theorem of van Kampen type [41], giving rise to the nice relations in your last letter, about pushouts of $K(\pi, 1)$'s. But before trying your patience again, I'll have to check if it isn't all in one of your older letters, and I have been a very bad reader indeed!

To the list of mathematicians who thought it worth their while to ponder on groupoids, you could add Quillen and Illusie in the late sixties. Quillen developed a nice notion of “formal groupoids”, which may be viewed as a unifying concept for differential calculus of infinite order (for a scheme over another, say) and for formal groups (over an arbitrary ring). He gave a structure theorem in characteristic 0, corresponding to the “smooth” case, which for formal groups reduces to the Lie-type statement that the formal group is determined by its Lie algebra, or equivalently, by its de Rham complex. I don't know if he ever published notes on this – but Illusie took up the topic in one chapter of his thesis [87], introducing divided powers in the relevant augmentation ideal, which allows to rid oneself of characteristic zero assumptions.

Your idea of writing a “frantically speculative” article on groupoids seems to me a very good one. It is the kind of thing which has traditionally been lacking in mathematics since the very beginnings, I feel, which is one big drawback in comparison to all other sciences, as far as I know. Of course, no creative mathematician can afford *not* to “speculate”, namely to do more or less daring guesswork as an indispensable source of inspiration. The trouble is that, in obedience to a stern tradition, almost nothing of this appears in writing, and preciously little even in oral communication. The point is that the disrepute of “speculation” or “dream” is such, that even as a strictly private (not to say secret!) activity, it has a tendency to vegetate – much like the desire and drive of love and sex, in too repressive an environment. Despite the “repression”, in the one or two years before I unexpectedly was led to withdraw from the mathematical milieu and to stop publishing, it was more or less clear to me that, besides going on pushing ahead with foundational work in SGA and EGA, I was going to write a wholly science-fiction kind of book on “*motives*”, which was then the most fascinating and mysterious mathematical being I had come to meet so far. As my interests and my emphasis have somewhat shifted since, I doubt I am ever going to write this book – still less anyone else is going to, presumably. But whatever I am going to write in mathematics, I believe a major part of it will be “speculation” or “fiction”, going hand in hand with painstaking, down-to-earth work to get hold of the right kind of notions and structures, to work out comprehensive pictures of still misty landscapes. The notes I am writing up lately are in this spirit, but in this case the landscape isn’t so remote really, and the feeling is rather that, as for the specific program I have been out for is concerned, getting everything straight and clear shouldn’t mean more than a few years work at most for someone who really feels like doing it, maybe less. But of course surprises are bound to turn up on one’s way, and while starting with a few threads in hand, after a while they may have multiplied and become such a bunch that you cannot possibly grasp them all, let alone follow.

As for predicting whether the “ground scheme” (as you call it teasingly) I have been pursuing in homological algebra is going to have any payoff in the kind of situation you or Loday or Higgins are familiar with, or for computing $\pi_6(S^2)$ and the like, I am wholly unable to do so. Of course I would be pleased if there was a payoff of that kind, and not too surprised – but my motivation is in an entirely different direction. If I was younger and more unconditionally devoted to mathematical work, my present reflections could have provided an excellent opportunity to become familiar with some of the main features of the more down-to-earth, hard-stuff-type homotopy theory such as $\pi_i(S^n)$, cohomology operations and the kind of things you have been doing. But I am rather in a hurry to finish writing up those notes, and come back to the action of $\text{Gal}(\overline{\mathbf{Q}}/\mathbf{Q})$ upon the tower of profinite Teichmüller groupoids and the like – which is at present where my main interest lies (in mathematics). It is closely connected with

motives of course, but for the time being I decide to ignore the motives, and to come to a thorough understanding of the manifold structure of the Teichmüller tower itself.

What you wrote on pages 40 and 41 of your letter on a so-called “topological topos” (of Peter Johnstone), an “exponential law for the category of spaces over B ” and what not, was wholly incomprehensible to me. If at any time you feel like being more specific, I’d be interested. I have the feeling, generally speaking, that the notion of a topos is a lot better suited for geometrical use than the notion of a topological space (which has been designed for the use of analysts rather than for geometry), but somehow it never became familiar to geometers, including my former students who seem to have forgotten all about it. For the use of geometry, topological spaces are either a lot too weak a structure, with vastly too many maps and automorphisms – or just not general enough for embodying topological intuition wherever “topology” does enter into play. In this latter respect topoi so far (possibly enriched by a sheaf of rings) seem to me to have met all requirements. Also, they are ideally suited for formulating universal problems and get “classifying topoi” for most structures met with so far in mathematics. For instance, there is, for every integer $n \geq 0$, a topos \mathcal{V}_n which is locally a topological variety of dimension n (I call this a (topological) *multiplicity*), and which can be viewed as *the* “universal” n -variety (more accurately, the universal n -multiplicity). The homotopy and (co)homology invariants of this topos, and of the differentiable *etc.* variants, are, I feel, extremely interesting, really basic invariants – but as far as I know, they have never been investigated. Thus the cohomology ring of \mathcal{V}_n with coefficient in a ring k say, can be viewed as the ring of “characteristic classes” for varieties of dimension n (topological, or differentiable, *etc.*) with coefficients in k . Presumably, a few things are known about characteristic classes, but is it a generally understood fact that they can be viewed in a natural way as cohomology classes of a suitable homotopy type? The construction of \mathcal{V}_n is moreover extremely simple, and can be rephrased in manifold striking ways...

Still a question (if you got time to answer): why is the result on π_2 of pushouts of $K(\pi, 1)$ -spaces you talk about in your last letter a generalisation of Hopf’s formula for $H_2(\text{group})$ ($= 0$, if I remember rightly?); and what makes you write that the latter is “one of the foundation stones of homological algebra”? Maybe you are thinking of Hopf’s structure theorem for characteristic 0 Hopf algebras, which, however, is a lot more general result, except for restriction to characteristic 0?

I am afraid this letter got nearly as long as yours, and with a lot less substance, so I better stop! With best wishes for nice vacations

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 22.06.1983

22/6/1983

Dear Alexander,

It is always a great pleasure to receive your letters as there is so much in your attitudes and advice to which I respond. This makes it especially enjoyable to explain some of the matters in which I have been involved.

On a topological topos.

Topologies on the space $\mathcal{C}(Y, X)$ of continuous functions $Y \longrightarrow X$ were considered by many writers, and one of the intuitive topologies was that of uniform convergence on compact subsets, which has as sub-base the sets

$$W(C, U) = \{f \in \mathcal{C}(Y, X) \mid f(C) \subset U\}$$

for C compact in Y and U open in X . This is also called the compact-open topology, and was studied by Fox [69], Arens-Dugundji [2], Jackson [88], and others. Particular interest was in the exponential law: give conditions under which the *exponential function*

$$\begin{aligned} e &: \mathcal{C}(Z \times Y, X) \rightarrow \mathcal{C}(Z, \mathcal{C}(Y, X)) \\ f &\mapsto (z \mapsto (y \mapsto f(z, y))) \end{aligned}$$

is well defined and a bijection (even a homeomorphism). For example, Fox proves it a bijection if Y is locally compact and Hausdorff. Jackson proves it a homeomorphism into if Z and Y are Hausdorff.

I was writing a thesis in 1961 on the algebraic topology of function spaces under the supervision of Michael Barratt (very strong on hard homotopy theory, a non-publisher, of marvellous insights, and tremendous to talk to, [...]) after the death of my earlier supervisor Henry Whitehead, who made an enormous contribution to topology and algebra (CW-complexes, simple homotopy theory, crossed modules, automorphisms of free groups, PL-topology, ...). He had wide interests and no trace of snobbishness. He once silenced (temporarily) a bright young spark by saying: "It is the snobbishness of the young to suppose a theorem is trivial because the proof is trivial."

My thesis was full of exponential laws in various categories (simplicial sets, chain complexes, simplicial abelian groups, ...), and it became obvious that the exponential law depended on the "function object" and the "product". So I tried the weak product $Z \times_W Y$ of topological spaces, where $Z \times_W Y = k(Z \times Y)$, make $Z \times Y$ into a k -space by giving it the finest topology with respect to all inclusions of compact subspaces. To my surprise, if one k -ified everything, one obtained an exponential law for all Hausdorff k -spaces. I also found an exponential law $\mathcal{C}(Z \times_S Y, X) \simeq \mathcal{C}(Z, \mathcal{C}(Y, X))$, where $Z \times_S Y$

has the final topology with respect to inclusions of the subspaces $\{z\} \times Y$, $Z \times B$ for all $z \in Z$ and all compact subsets B of Y . This led to a paper “Ten topologies for $X \times Y$ ” [17] and another, “Function spaces and product topologies” [18]. The idea of using Hausdorff k -spaces was popularised by Steenrod as a “convenient category of topological spaces”, in 1967 [123]. It has since been found that the Hausdorff assumption can be dropped by using $k(X)$, which means take the final topology with respect to all maps of compact Hausdorff spaces $C \rightarrow X$ (one can show a *set* of such maps is sufficient to define $k(X)$). So one *can* work in \mathbf{Top} (the usual topological category), but for algebraic topology, $k\text{-}\mathbf{Top}$ is more useful, since the exponential law in $k\text{-}\mathbf{Top}$ implies, for example, that if f, g are quotient maps, so also is $f \times g$ (the categorical product in $k\text{-}\mathbf{Top}$). In modern parlance, $k\text{-}\mathbf{Top}$ is a cartesian closed category. (Please excuse me if all this is wellknown to you.)

If B is a topological space, one can form the category \mathbf{Top}_B of spaces over B , which has of course a product, the *fibre product* or *pullback*

$$\begin{array}{ccc} & X \times_B Y & \\ \swarrow & \downarrow & \searrow \\ X & & Y \\ \searrow & \downarrow & \swarrow \\ & B & \end{array}$$

Does \mathbf{Top}_B have an exponential law? An obscure (in various senses) paper of R. Thom (Louvain, 1956 [126]) suggests it does, but he never really defined the topology on the appropriate function space. A student of mine, Peter Booth, took up the question and produced a topology, and later he and I tackled it from a more conceptual viewpoint.

We require for any

$$\begin{array}{ccc} Y & & X \\ & \searrow q & \swarrow p \\ & B & \end{array}$$

a space $(Y \times X) \xrightarrow{(q, p)} B$ over B and an exponential law

$$\mathbf{Top}_B(Z \times_B Y, X) \simeq \mathbf{Top}_B(Z, (Y \times X))$$

(for suitable Z, Y, X in the usual topological category, and more generally, in a convenient category). From a set-theoretic point of view, it is easy to see that $(Y \times X)$ should be the disjoint union of $\mathbf{Top}(Y_b, X_b)$ for all $b \in B$, where $Y_b = q^{-1}(b)$ and $X_b = p^{-1}(b)$. What does it mean for a map $f : Y_b \rightarrow X_b$ to be “near” a map $f' : Y_{b'} \rightarrow X_{b'}$? Our first idea was that such an f is a “partial map” $f : X \rightarrow Y$ (*i.e.* $D_f \subset Y$) and that the compact-open topology extends to partial maps since $f(C) = f(C \cap D_f)$ by definition. Unfortunately, I found the proofs rather difficult.

Then I met Mac Lane, who told me of the Lawvere-Tierney theory of *elementary topoi* [96, 97, 127] (cartesian closed categories with finite limits and colimits and a subobject classifier). One of the main results was that these coincided (I think) with topoi as defined by you in terms of sheaves.⁽¹⁴⁾ For me, a useful elementary fact was representability of partial maps in a topos. So I tried to do the same for spaces.

What is easy to do for spaces is representability for partial maps with *closed* domain. For any X , let $X^\wedge = X \cup \{\omega\}$, where $\omega \notin X$, with C closed in X^\wedge if and only if $C = X^\wedge$ or C is closed in X . (So X^\wedge is usually non-Hausdorff.) We now have a bijection

$$\begin{array}{ccc} \text{PC}(Y, X) & \xrightarrow{\sim} & \text{Top}(Y, X^\wedge) \\ f & \mapsto & \hat{f}, \end{array}$$

where $\text{PC}(Y, X)$ = partial maps $Y \rightarrow X$ with closed domain, and

$$\hat{f}(y) = \begin{cases} f(y) & \text{if } y \in D_f \\ \omega & \text{if } y \notin D_f \end{cases}.$$

This bijection is a homeomorphism if both sides have the compact-open topology. Now

$$\begin{aligned} \text{PC}(Z \times Y, X) &= \text{Top}(Z \times Y, X^\wedge) \\ &= \text{Top}(Z, \text{Top}(Y, X^\wedge)) && \text{for suitable } Y \\ &= \text{Top}(Z, \text{PC}(Y, X)), \end{aligned}$$

which is the exponential law for partial maps.

Now we go back to Top_B . The elements of $(Y \rightarrowtail X)$, if $Y \rightarrow B$ and $X \rightarrow B$ are partial maps, with a closed domain if B is T_0 , so let us suppose B is Hausdorff, for safety, and so that $Y \times_B X$ is a closed subset of $Y \times X$. So we give $(Y \rightarrowtail X)$ the initial topology with respect to the two maps

$$\begin{array}{c} (Y \rightarrowtail X) \longrightarrow \text{PC}(Y, X) \\ \downarrow \\ B \end{array}$$

and, lo and behold, we have a nice topology on $(Y \rightarrowtail X)$, giving the right kind of laws.

What one has found is that $\mathbf{k}\text{-Top}$ is not a topos, but a kind of “pseudo”-topos – not all subobjects are classifiable.

Also there is a curious contrast. You are interested (as I understand it) in a topos as a generalisation of a space, the classical example being sheaves on a given space, or more generally, on a site. In the above, we are using the Lawvere-Tierney approach of topoi as models of the category of sets, or instead, looking for topos-like models of the category of topological spaces. Peter Johnstone has constructed a possible model [90], using a mixture of ideas from sequential spaces (spaces whose topology

⁽¹⁴⁾ N. Éd. Cela n’est pas vrai, il faut supposer de plus que la catégorie soit cocomplète.

is defined by sequences) and from topos theory (as in P. Johnstone’s book “Topos Theory”[89], which I have hardly looked at, for the usual reason of time).

What seems clear is that the function space $(Y \rightarrow X) \rightarrow B$ and various other modifications of this (often easily defined using the space X^\wedge) will play an increasing rôle in algebraic topology, and the Newfoundland group (Booth, Heath, Piccinini) are happily working away on this.

What I further believe is that if one can “improve” the notion of a topological space so that one gets representability for all partial maps, and so in particular a sub-object classifier for all subspaces of a space, then one should have a useful tool in wider areas of mathematics, when the basic notions of, say, differentiability are thoroughly worked out. This could be very useful in foliation theory, where interesting leaves are neither closed nor open.

Pushouts of $K(\pi, 1)$ ’s.

A famous result of Henry Whitehead is that if the CW-complex X is the union of connected CW-complexes X_1, X_2 with $X_0 = X_1 \cap X_2$, X_1, X_2 all $K(\pi, 1)$ ’s, then X is a $K(\pi, 1)$ if $\pi_1 X_0 \rightarrow \pi_1 X_1$ and $\pi_1 X_0 \rightarrow \pi_1 X_2$ are *injective*. The proof uses the van Kampen theorem to describe $\pi_1 X$; some combinatorial group theory to prove that $\pi_1 X_1 \rightarrow \pi_1 X$ and $\pi_1 X_2 \rightarrow \pi_1 X$ are injective; and then universal covers and the Mayer-Vietoris theorem to prove that $H_*(\tilde{X})$ is trivial (whence \tilde{X} is contractible).

This leaves open the question of describing the homotopy type (or just homotopy groups) of X if the injectivity assumption on $\pi_1 X_0 \rightarrow \pi_1 X_1$ and $\pi_1 X_0 \rightarrow \pi_1 X_2$ is dropped. The Brown-Higgins theorem [27] gives information when these two maps are *surjective*; the conclusion is that

$$\pi_2(X) \simeq \frac{M \cap N}{[M, N]}, \quad [M, N] = \frac{\text{subgroup generated by commutators}}{mnm^{-1}n^{-1}, m \in M, n \in N},$$

where M, N are the kernels of $\pi_1 X_0 \rightarrow \pi_1 X_1$ and $\pi_1 X_0 \rightarrow \pi_1 X_2$, respectively,

The theorem from which this description follows is, in its most general form, as follows. First, one needs the notion of “crossed module over groupoid”, which in one description is a morphism

$$\begin{array}{ccc} C_2 & \xrightarrow{\delta} & C_1 \\ \downarrow & & \downarrow \\ C_0 & \xrightarrow[1]{=} & C_0 \end{array}$$

of groupoids with objects C_0 and over 1_{C_0} such that C_2 is a family $C_2(p)$, $p \in C_0$, as follows. First, one needs the notion of “crossed module over groupoid”, which in one description is a morphism

The usual rules are to hold

$$\begin{aligned} \delta(a^x) &= x^{-1}\delta(a)x \\ a^{-1}a_1a &= a_1^{\delta a} \quad \begin{array}{l} a, a_1 \in C_2(p) \\ x \in C_1(p, q) \end{array} \end{aligned}$$

If $\underline{X} = (X_2, X_1, X_0)$ is a triple of spaces, then the crossed module (over a groupoid) $\pi \underline{X}$ has

$$\begin{aligned} C_1 &= \pi_1(X_1, X_0) = \text{homotopy classes rel } \dot{I} \text{ of} \\ &\quad \text{maps } (I, \dot{I}) \longrightarrow (X_1, X_0) \\ C_2(p) &= \pi_2(X_2, X_1, p) \quad p \in X_0 \end{aligned}$$

with the usual boundary and operation given by change of base point.

Let $\mathcal{U} = \{U^\lambda\}_{\lambda \in \Lambda}$ be an open cover of X_2 . For each U^λ , let \underline{U}^λ be the triple $(U^\lambda, U^\lambda \cap X_1, U^\lambda \cap X_0)$. We say that the triple \underline{X} is connected (Brown-Higgins use the term *homotopy full* for the filtered space notion) if $\pi_0 X_0 \rightarrow \pi_0 X_1, \pi_0 X_0 \rightarrow \pi_0 X_2$ are surjective and each triple (X_2, X_1, p) ($p \in X_0$) is 1-connected (*i.e.* the homotopy fibre at p of $X_1 \rightarrow X_2$ is 0-connected, or each path $(I, 0, 1) \rightarrow (X_2, X_1, p)$ is deformable into X_1 , or $\pi_1(X_1, p) \rightarrow \pi_1(X_2, p)$ is surjective). For $\nu = (\lambda_1, \dots, \lambda_n) \in \Lambda^n$ let $U^\nu = U^{\lambda_1} \cap \dots \cap U^{\lambda_n}$ and $U_i^\nu = U^\nu \cap X_i$, so we have a triple \underline{U}^ν . Form the diagram of crossed modules over groupoids

$$(*) \quad \bigsqcup_{\nu \in \Lambda^2} \pi \underline{U}^\nu \xrightleftharpoons[b]{a} \bigsqcup_{\lambda \in \Lambda} \pi \underline{U}^\lambda \xrightarrow{c} \pi \underline{X}$$

where if $\nu = (\lambda, \mu)$ then a, b are induced by the inclusions $U^\nu \rightarrow U^\lambda, U^\nu \rightarrow U^\mu$, and c is induced by $U^\lambda \rightarrow X_2$, and $\bigsqcup =$ disjoint union $=$ coproduct in (crossed modules over groupoids).

Theorem. *If all triples \underline{U}^ν for $\nu \in \Lambda^2$ are connected, then $(*)$ is a coequalizer diagram of crossed modules over groupoids. \square*

I don't know any good application of this general case, but the proof is no more difficult (really) than the case $X_0 = \{p\}$ and $\Lambda = \{1, 2\}$ when we deduce a pushout

$$\begin{array}{ccc} \pi \underline{U}^{(1,2)} & \longrightarrow & \pi \underline{U}^1 \\ \downarrow & & \downarrow \\ \pi \underline{U}^2 & \longrightarrow & \pi \underline{X} \end{array}$$

of crossed modules (over groups) assuming $\underline{U}^1, \underline{U}^2, \underline{U}^{(1,2)}$ are connected. Now the *proof* goes via “double groupoids with connection” and a construction $\rho \underline{X}$. The reason is that $(\pi \underline{X})_2$ consists of homotopy classes of maps of a square

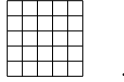
$$\begin{array}{|c|} \hline \cdots \\ \hline X_2 \\ \hline \end{array} \quad p \quad \text{with addition} \quad \begin{array}{|c|c|} \hline \cdots & \cdots \\ \hline & \\ \hline \end{array}$$

while $(\rho\underline{X})_2$ consists of homotopy classes of maps

$$\begin{array}{ccc} p & X_1 & p \\ X_1 & \begin{array}{|c|} \hline X_2 \\ \hline \end{array} & X_1 \\ p & X_1 & p \end{array} \quad (I^2, \dot{I}^2, \ddot{I}^2) \longrightarrow (X_2, X_1, X_0)$$

homotopy rel \ddot{I}^2

which is more symmetric and more suitable for subdivision



That is, in $\rho\underline{X}$ you can form multiple compositions $g = [g_{ij}]$. This is one of the key ideas which makes the proof work. The equivalence between crossed modules over groupoids and double groupoids takes $\pi\underline{X}$ to $\rho\underline{X}$, so you can work with either. (See my exposition “Higher dimensional group theory” [20].)

To go back to pushouts of $K(\pi, 1)$ ’s: Unfortunately, I have changed my notation! The X_0, X_1, X_2 on page 52 should now be $U^0 = U^{(1,2)}, U^1, U^2$. Assuming our triple \underline{X} is (X, U^0, p) , and that U^0, U^1, U^2 are $K(\pi, 1)$ ’s and assuming the appropriate connectivity, we get $\pi\underline{X}$ is the *coproduct* $\pi U^1 \circ \pi U^2$ in the category of crossed $\pi_1 U^0$ -modules. An analysis of this coproduct (see latest batch of offprints, particularly 83.5) gives $\pi_2 X = (M \cap N)/[M, N]$ as stated before.

So the question is whether there are any morals to be drawn from the fact that this scheme works in homotopy theory? Crossed modules describe pointed homotopy types of spaces with $\pi_i = 0$ for $i > 2$. The double groupoid gadgets are not hard to work with, since the pictures are easy to draw. Can any of the above homotopy theory be done for topoi?

29/6/83

Your new set of notes (191–258) arrived on Monday and I have sent copies to Higgins, Loday, Kamps, Cordier, Porter.

I can’t resist taking up the criticism of “crossed module” as a term. J. H. C. Whitehead was thinking of the description of $M \xrightarrow{\mu} P$ with P acting on M , *etc.*, as a generalisation of P -module (the case $\mu = 0$), and also thinking of the rule $a^{-1}a_1^{-1}aa_1^{\mu a} = 1$ as equating a “crossed commutator” to 1. The term was introduced in a paper of his in 1946 [135, Eqn. (1.1)] and developed in “Combinatorial homotopy II” [137]. But

the idea goes back to his remarkable series of papers written just before the war (1938–40) [132, 133, 134]⁽¹⁵⁾ and which laid the foundation of so much work in the 1960's. His deepest theorem on crossed modules is that the boundary $\pi_2(X, Y) \xrightarrow{\partial} \pi_1 Y$ is a crossed module such that (and here is the crunch) if $X = Y \cup \{e_\lambda^2\}$ (add a family of 2-cells) then $\pi_2(X, Y)$ is the free crossed π_1 -module on the (characteristic maps of) the 2-cells e_λ^2 . The second proof published was by Brown-Higgins in 1978 [27], since it is an immediate application of the 2-dimensional van Kampen theorem described above, *i.e.* free crossed modules are special cases of pushouts of crossed modules. Whitehead's exposition (spread over three papers 1941–1949 [133], [135], [137]) is difficult to follow, and I got a rewrite of it published [19]. The ideas are now coming into vogue in combinatorial group theory. This whole area of 2-dimensional complexes is very hard. People have been struggling for years over Whitehead's conjecture (or question): is a subcomplex of a 2-dimensional $K(\pi, 1)$ necessarily a $K(\pi', 1)$?, and haven't got very far. The question can be translated into an algebraic question on free crossed modules, but this does not help very much.

Oh yes, I was going to explain the relevance of the above result on $\pi_2 X$ to Hopf's formula for $H_2 G$ (G a group). Consider again the pushout diagram

$$\begin{array}{ccc} K(P, 1) & \xrightarrow{i} & K(Q, 1) \\ j \downarrow & & \downarrow \\ K(R, 1) & \longrightarrow & X. \end{array}$$

The Mayer-Vietoris sequence gives of course

$$\cdots \longrightarrow H_n(P) \longrightarrow H_n(Q) \oplus H_n(R) \longrightarrow H_n(X) \longrightarrow H_{n-1}(P) \longrightarrow \cdots$$

The problem is to identify $H_n(X)$ in terms of invariants of P , Q and R . (There are several papers on 8- or 9-term exact sequences which don't see the problem this way around.) If i_* , j_* are injective, $X = K(Q *_P R, 1)$, and we are O.K. Suppose i_* , j_* are surjective, and their kernels M , N together generate P . Then $\pi_1 X = 0$, and so $H_2(X) = \pi_2(X) = (M \cap N)/[M, N]$. This gives us

$$H_2(P) \longrightarrow H_2(Q) \oplus H_2(R) \longrightarrow \frac{M \cap N}{[M, N]} \longrightarrow H_1(P) \longrightarrow H_1(Q) \oplus H_1(R) \longrightarrow 0$$

(a new exact sequence!). If $M = P$, we have $Q = 0$ and so an exact sequence

$$H_2(P) \longrightarrow H_2(R) \longrightarrow \frac{N}{[P, N]} \longrightarrow H_1(P) \longrightarrow H_1(R) \longrightarrow 0$$

⁽¹⁵⁾ N. Éd. Par exemple dans [133], dans la note en bas de la page 422, on trouve une des équations définissant un module croisé.

(an exact sequence of Stallings [122]). If P is free, then $H_2(P) = 0$ and

$$\begin{aligned} H_2(R) &= \text{Ker} \left(\frac{N}{[P, N]} \longrightarrow \frac{P}{[P, P]} \right) \\ &= \frac{N \cap [P, P]}{[P, N]}, \end{aligned}$$

which is Hopf’s formula. In a similar spirit, the van Kampen theorem for Cat^2 -groups implies a formula

$$H_3 R = \text{Ker} (N \overset{P}{\wedge} P \longrightarrow N),$$

where $N \overset{P}{\wedge} P$ is a “non-abelian exterior product”. Unfortunately, the proof of this theorem is hitting a number of snags – it seems to be of a higher order of difficulty to the Brown-Higgins stuff, basically because we cannot find gadgets which will nicely allow the same scheme of proof (using subdivisions) [41]. A further difficulty has arisen with Loday’s proof of his modelling of $(n+1)$ -homotopy types by Cat^n -groups – I can’t follow his basic lemma (3.5 of his JPAA paper [99]) constructing an n -cube-of-fibrations from a space. It seems to be more subtle than is indicated there, even at the level of groups.

For more on “crossed modules”, see my exposition with Huebschmann on “Identities among relations” [35]. The point there made is that “chains of syzygies” à la Hilbert are about presentations of *modules*. If you want analogous ideas for presentations of *groups*, you are led inexorably to crossed modules (as were Peiffer [110] and Reidemeister [117], independently of J. H. C. Whitehead), of groups. For presentations of commutative algebras you need crossed modules – of commutative algebras! *I.e.* Cat^1 -objects in (commutative algebras). Tim Porter is pursuing this analogy strongly, which has so far escaped notice as a fundamental idea. Similar ideas hold for Lie algebras, and have been used by Loday [93, Déf. A.1].

Another remarkable fact is that if $F \longrightarrow E \longrightarrow B$ is a based fibration, then $\pi_1 F \longrightarrow \pi_1 E$ can be given the structure of crossed module. Loday has another nice description. Form the fibre product

$$\begin{array}{ccc} E \times_B E & \longrightarrow & E \\ \downarrow & & \downarrow \\ E & \longrightarrow & B. \end{array}$$

Then the two projections to E and the diagonal $E \longrightarrow E \times_B E$ induce the structure maps $\pi_1(E \times_B E) \overset{\rightrightarrows}{\longrightarrow} \pi_1 E$ of a Cat^1 -group. Isn’t that nice?! This construction is the foundation of J.-L. Loday’s work on Cat^n -groups, which must surely be correct, even if there is (to me) at present a hole in the proof. That is the notion of *cubical*

resolution is not yet properly worked out. (I can't prove his key lemma 3.5 of his JPAA paper [99].)⁽¹⁶⁾

Your remarks in your notes on Kan complexes prompt me to suggest you might like to glance again at my notes "An introduction to simplicial T-complexes" [21]. The idea of "thin filler" has enormous attractions for me, and Philip and I used it crucially in our proofs of the general van Kampen theorem for crossed complexes.

Let me say here that the result on page 54 and on page 55 giving

$$\pi_2 X = \frac{M \cap N}{[M, N]}$$

is not (so far as I know) provable by other methods (although the special case X_0, X_1, X_2 are 2-dimensional and X_0 is the common 1-skeleton of X_1 and X_2 , is in the literature). The fact that I thought of it only in February 1983 (9 years after Philip Higgins visited Bangor and we started playing around with pushouts of crossed modules) suggests there should be a lot more to do. I haven't submitted the preprint for publication, as it was written hurriedly, and needs maturing to get the emphasis right. *E.g.* it does not bring out clearly enough the problem of pushouts of $K(\pi, 1)$'s, which further discussions with Jean-Louis Loday have clarified.

The question of generalising cubes, simplices, globes (what is wrong with rhombic dodecahedra, anyway?) has been taken seriously by my student David Jones, who came up to Bangor from his father's sheep farm the other day to collect the final typed chapters of his thesis. I think he has done a beautiful job – for example, the question of degeneracy maps on the models is taken seriously. The concepts are non-trivial, as you can see by trying to describe all "degeneracy maps" from our old friend the rhombic dodecahedron to a square. The chief disadvantage of his thesis is that he has (on my suggestion) taken the fundamental problem as that of generalising simplicial T-complexes, which to anyone not associated to Bangor ideas must seem outré, or worse. What one would like to see tackled is the equivalence of homotopy categories, and this needs a careful analysis of the Gabriel-Zisman proof

$$\text{Hot}(\text{Kan}) \sim \text{Hot}(\text{CW}) .$$

So we are a very long way from convincing the world that poly-sets are the best thing since sliced bread. Nevermind – the T-complex problem has proved a marvellous test-bed of techniques, and I like to believe that the flexibility of the poly-approach will allow for new links and methods for tackling topological and combinatorial problems, getting away from the rigidity of simplices or cubes.

One minor point on the Dold-Puppe theorem (strictly, this was discovered independently by Dold [61] and Kan [92], although Kan had the nicer formulation of the

⁽¹⁶⁾ N. Éd. Cette difficulté a été résolue par Steiner dans [124]. Voir aussi [43].

functor

$$\begin{array}{ccc} (\text{chain-complexes}) & \rightarrow & (\text{simplicial abelian groups}) \\ \text{as } C & \mapsto & (\Delta^n \mapsto \text{Hom}_{(\text{chain-complexes})}(C_N(\Delta^n), C)) , \end{array}$$

Puppe got involved later [62]): the “simple” cubical theorem is not true; one has to introduce the extra structure of “connections” which come from the maps

$$\begin{array}{ccc} \gamma_i : I^{n+1} & \rightarrow & I^n \\ (t_1, \dots, t_n) & \mapsto & (t_1, \dots, \max(t_i, t_{i+1}), \dots, t_n) . \end{array}$$

So we get chain complexes are equivalent to “cubical abelian groups with connections”. I guess a direct proof should not be too hard, but this is in fact a consequence of the equivalence between crossed complexes and ω -groupoids (see the last page of “An introduction to simplicial T-complexes” [21]).

Once again my research proposal has got turned down⁽¹⁷⁾. Apparently, opinions of referees ranged from wild enthusiasm to comments of “speculative” or “rubbish”. It is all very curious. It also slows up the pace, as the group at Bangor is very small (me, Tim, one research student Graham Ellis, ...) and could do with a broader range of expertise. The reason for the doubts of referees are, I guess, simply a disbelief that groupoids and van Kampen theorems can really lead to things algebraic topologists actually want to know.

I shall continue to submit proposals (it just takes time) partly out of irritation, and also to have my speculations on record. I hope it is not improper to ask, but would you be willing to give a formal note of support which could be sent in with the next proposal? Or perhaps to write separately to S.E.R.C.? It has been suggested that supporting letters with the proposal *might* help to sway the doubters. But I could quite understand you might wish to keep out of it.

Other things of course occupy time, so please excuse the lack so far of a detailed commentary on your notes (which in any case are also right up Tim Porter’s street). I have things to do as Head of Department, and have lately been fighting a battle on a fundamental point of principle concerned with academic standards versus the Welsh language – this is a very long story! Six of our children are at home now, although in fact they take good care of themselves.

Also I have gone a bit mad lately on preparing for publication joint work with a research student Steve Humphries on “Orbits under symplectic transvections” [36], [37]. [...] I have found the work very rewarding, and an interesting change from homotopy theory. However, Steve’s work stems from an interest in the mapping class group M_g of an orientable surface of genus g . He found in 1977 a minimal set of “twist generators” [86] which have since been used by Wajnryb to give an amazing finite presentation of M_g [129], and which is related to work of Mumford... (so I am

⁽¹⁷⁾ I’ve said this before.

told by Joan Birman)! I expect that 1983 will see two students (Steve and David) getting their Ph.D.'s, somewhat belatedly, but both very nice pieces of work, in entirely different areas. Steve's area I had to learn from scratch when his previous supervisor died in a mountaineering accident in 1978, and I have learnt most of it from trying to understand his proofs.

30/6/83

I ought to say something about the Brown-Loday set of ideas.

As said in a previous letter, my aim for van Kampen has been to find a setting in which an idea of a proof would turn into a proof of a theorem (a "proof" in search of a theorem). I am not sure how unusual such a method is, and it is not one I would have chosen, given a choice. For a long time I was trying to make this work in an *absolute* setting, until work with P. J. Higgins led first to triples $X_0 \subset X_1 \subset X_2$ and the associated homotopy double groupoid, and then to filtered spaces

$$X_0 \subset X_1 \subset \cdots \subset X_n \subset \cdots$$

and the associated ω -groupoid or crossed complex. However, the disadvantage of crossed complexes is that they do not model all homotopy types. Nonetheless, the "idea of proof" does find a complete expression in this setting, so it may be that to proceed further, a new set of ideas is needed.

Loday's idea [99] is that convenient generalisations of crossed modules (or Cat^1 -groups) are obtained from n -cubical diagrams of spaces, particularly n -cubical diagrams of fibrations. This process seems clear. As an example, one associates to a square of pointed inclusions

$$\underline{X} : \begin{array}{ccc} C & \hookrightarrow & B \\ \downarrow & & \downarrow \\ A & \hookrightarrow & X \end{array}$$

a Cat^2 -group $\pi \underline{X}$ (its *fundamental* Cat^2 -group) consisting of homotopy classes of maps $I^3 \xrightarrow{f} X$ such that $f(\partial_1^\varepsilon I^3) \subset A$, $f(\partial_2^\varepsilon I^3) \subset B$, $f(\partial_3^\varepsilon I^3) \subset *$, $\varepsilon \in \{0, 1\}$, and f maps all edges to $*$. Then $\pi \underline{X}$ obtains groupoid structures in directions 1 and 2, and a group structure in direction 3, satisfying the obvious interchange laws. The proof that $+_1$ and $+_2$ are defined, is not entirely trivial.

The square \underline{X} is *connected* if all of the spaces C , A , B , X and all homotopy fibres F_α ($\alpha \in \{0, \dots, 4\}$) are connected.

$$\begin{array}{ccccc}
 F_2 & \longrightarrow & F_3 & \longrightarrow & F_4 \\
 \downarrow & & \downarrow & & \downarrow \\
 F_1 & \longrightarrow & C & \longrightarrow & B \\
 \downarrow & & \downarrow & & \downarrow \\
 F_0 & \longrightarrow & A & \hookrightarrow & X .
 \end{array}$$

The aim is to obtain and prove a coequaliser diagram

$$(*) \quad \bigsqcup_{\nu \in \Lambda^2} \pi \underline{U}^\nu \rightrightarrows \bigsqcup_{\lambda \in \Lambda} \pi \underline{U}^\lambda \longrightarrow \pi \underline{X}$$

of Cat^2 -groups, assuming all finite intersections \underline{U}^ν , $\nu \in \Lambda^n$, are connected squares. (What relation does this have to “descent”?)

Suppose we assume Loday’s theorem that Cat^2 -groups describe pointed 3-types ($\pi_i = 0$ for $i > 3$). Then the diagram $(*)$ gives a kind of “integration of 3-types”, where the extra structure on X of square allows for more control of the description of the way X is obtained by glueing subspaces together. Put in another way, to describe homotopy information in dimension 3, we need to know how X is put together in dimensions 0, 1, 2 as well as in dimension 3. The complicated cross-dimensional homotopy interrelations imply that dimensions cannot be isolated so much as in done in homology. These interrelations get more and more complicated as dimension goes up (there appear Whitehead products, higher Whitehead products, Toda brackets and all sorts of strange beasts). This must be allowed as background to any doubts of referees that a generalised van Kampen theorem could allow for real higher dimensional computations. (But of course, if structure was never pursued because the full implications could not be seen, then nothing, pretty well, would get done, as you have strongly pointed out several times.) For me, the clean description of the structure of Cat^n -group ($= \text{Cat}^1$ -(Cat^{n-1} -group)) suggests there is a large amount of work to be done to illuminate at least some of the implications. A further point is that a number of results in homotopy theory have reasonable formulations only in the simply-connected case; or when some abelian group or module is to be described. This restriction becomes irksome (or absurd) in many parts of low-dimensional topology, algebraic K-theory, and group homology.

I have an impression that, as crossed modules ($= \text{Cat}^1$ -groups) are relevant to describing identities among relations, so Cat^2 -groups are relevant to describing “deeper” forms of interactions among relations. If so, then they should give information on problems at present out of reach, such as Whitehead’s conjecture. What might be an easier line of obtaining progress is to consider, say, Cat^2 -(commutative algebras),

where it should be much easier to obtain precise computational results. But the information available is sparse (a bit like group homology pre-Eilenberg-Mac Lane). We are considering, for example, what should be a “cubical resolution” of a group. This has become a bit clearer.

This starts off as follows. Let $\mathcal{P} = (X, R)$ present the group G . Form the free group FX on X and the free crossed FX -module CR on the relators R (cf. Brown-Huebschmann [35]) giving a sequence

$$CR \xrightarrow{\partial} FX \longrightarrow G$$

exact at FX and with $\text{Ker } \partial = \pi(\mathcal{P})$, the G -module of identities among relations.

Now CR itself fits in an exact sequence

$$1 \longrightarrow P \longrightarrow FY \longrightarrow CR \longrightarrow 1,$$

where $Y = FX \times R$, and P is the “Peiffer group” (*ibid*). One can obtain a diagram

$$(*) \quad \begin{array}{ccccc} FY \otimes^{\Phi} FY & \xrightarrow{i'} & FY & \xrightarrow{\theta} & CR \\ \downarrow g' & & \downarrow g & & \downarrow \partial \\ FY & \xrightarrow{i} & \Phi & \xrightarrow{s} & FX \\ \downarrow \theta & & \downarrow b & & \downarrow p \\ CR & \xrightarrow{\partial} & FX & \xrightarrow{p} & G. \end{array}$$

Here Φ is the semi-direct product $FX \ltimes FY$, where FX operates on FY by extending the action of FX on the generators $((u, r)^v = (uv, r), u, v \in FX, r \in R)$. However, the composite $\partial\theta$ is not a crossed module, but only a pre-crossed module (the crossed commutator relation does not hold). Let $s, b : FX \ltimes FY \longrightarrow FX$ be the maps $(x, u) \longrightarrow u, (x, u) \mapsto x(\partial\theta u)$. Then $FX \ltimes FY$ is a pre-Cat¹-group (we don’t have $[\text{Ker } s, \text{Ker } b] = 1$).

Because FY is free, we can define $g : FY \longrightarrow \Phi$ on the generators by $[y] \mapsto (\partial\theta y, [y]^{-1})$. Then $\text{Im } g = \text{Ker } b$. Let $i : [y] \mapsto (1, y)$. Then $\text{Im } i = \text{Ker } s$.

The diagram $(*)$ looks like the first stage of a “resolution”, with the top left hand corner square being a crossed square.

However, this gives the impression of being *ad hoc*. Further, it is not so clear how to proceed in detail to resolve the crossed square into a “crossed cube”. Finally, for theoretical purposes, the Catⁿ-group language is preferable, and there should be a formal description of a “Catⁿ-resolution” of a group G .

It seems a lot more conceptual experimentation is needed. I hope I can get more things straight before I see Loday next session!

One final not so mathematical point I would like to bring up. The idea of a working symposium on “non-abelian cohomological methods” has been mooted, since

possible ramifications need discussion (*e.g.* Alain Connes non-commutative de Rham-theory [51]). Would you be interested? There is question of support. One possible source would be NATO. Would you object to that? I hope you don't mind the question, but one does have to think a long way ahead to plan support (maybe also the USA Nat. Sci. Foundation would help). Loday has suggested Marseille-Luminy as a suitable place, but there are other possibilities. At the moment, this is just a suggestion. But one would ask to come other people like Giraud, Illusie, Connes, Higgins, Loday, Kassel, Quillen, Mac Lane, *etc.* What do you think?

I still haven't answered your question about "Hopf's formula as a foundation stone of homological algebra". My understanding from reading, say, Mac Lane and other historical accounts, is that Hopf's formula drew attention to the need to describe $H_i(K(G, 1))$ in algebraic terms. This was solved by Eilenberg-Mac Lane [64, 65]. Expositions by Cartan and Serre (or at least in the Cartan seminar) led to the notion of free resolution of \mathbf{Z} by $\mathbf{Z}G$ -modules. This was generalised to the projective resolution of R -modules by Cartan-Eilenberg [44]. In fact, Mac Lane says in a paper on "origins of the cohomology of groups" [103]:

Hopf's 1942 paper [85] was the starting point for the cohomology and homology of groups ... indirectly the starting point for several other developments ... resolutions ... homological algebra.

Mac Lane's paper is rather nice and I enclose a copy.

Let me finish with one point: theory versus computation. I accept Loday's attitude that Cat^1 -groups are better theoretically than crossed modules. However, the pushout theorem is best (I believe) proved using the equivalent "double groupoid with connections". For computation of groups by generators and relations, crossed modules seem more explicit and easier to work with.

Similarly, Cat^2 -groups are equivalent to crossed squares, the latter being easier to compute with, the former having theoretical advantages. Thus our deduction of $\pi_3(S^2) = \mathbf{Z}$ from van Kampen for Cat^2 -groups in fact goes via crossed squares and the \otimes_P -construction. One difficulty may be that we don't have an analogue of "triple groupoids with connections" and my attempts to write out a proof for Cat^2 -groups by direct subdivision arguments have led to complicated pictures of cubes and tubes winding like mad, but not much else. We need a dreamy multi-simplicial proof!

The easiest stated application of the theorem is as follows. Suppose given a diagram of pointed spaces and maps

$$\begin{array}{ccccc}
 & F & \longrightarrow & Z & \longrightarrow & Z' \\
 & \downarrow & & \downarrow & & \downarrow \\
 (\dagger) & Y & \longrightarrow & C & \longrightarrow & B \\
 & \downarrow & & \downarrow & & \downarrow \\
 & Y' & \longrightarrow & A & \longrightarrow & X
 \end{array}$$

such that $*$ is a homotopy pushout, all the rows and columns are fibrations, and all spaces are connected. *Then*

$$\pi_1 F \simeq \pi_1 Y \overset{\pi_1 C}{\otimes} \pi_1 Z ,$$

i.e.

$$\begin{array}{ccc}
 \pi_1 F & \longrightarrow & \pi_1 Z \\
 \downarrow & & \downarrow \\
 \pi_1 Y & \longrightarrow & \pi_1 C
 \end{array}$$

is a crossed square which is universal for crossed squares

$$\begin{array}{ccc}
 L & \longrightarrow & \pi_1 Z \\
 \downarrow & & \downarrow \\
 \pi_1 Y & \longrightarrow & \pi_1 C .
 \end{array}$$

The various exact homotopy sequences of the fibration give detailed information. For example if the middle

$$\begin{array}{ccccc}
 & & \bullet & & \\
 & & \downarrow & & \\
 \bullet & \longrightarrow & \bullet & \longrightarrow & \bullet \\
 & & \downarrow & & \\
 & & \bullet & &
 \end{array}$$

of the diagram (\dagger) is

$$\begin{array}{ccccc}
 & & K(M, 1) & & \\
 & & \downarrow & & \\
 K(N, 1) & \longrightarrow & K(P, 1) & \longrightarrow & K(R, 1) , \\
 & & \downarrow & & \\
 & & K(Q, 1) & &
 \end{array}$$

we get exact sequences

$$\left. \begin{array}{ccccccccc} \pi_2 Z & \longrightarrow & \pi_2 Z' & \longrightarrow & \pi_1 F & \longrightarrow & M & \longrightarrow & \pi_1 Z' & \longrightarrow & 1 \\ 0 & \longrightarrow & \pi_2 X & \longrightarrow & \pi_1 Z' & \longrightarrow & \pi_1 B & \longrightarrow & \pi_1 X & \longrightarrow & 1 \end{array} \right\},$$

$\pi_3 X \simeq \pi_2 Z'$, *i.e.*

$$\begin{aligned} 0 &\longrightarrow \pi_3 X \longrightarrow N \overset{P}{\otimes} M \longrightarrow M \longrightarrow M/[N, M] \longrightarrow 1, \\ 0 &\longrightarrow \pi_2 X \longrightarrow M/[N, M] \longrightarrow P/N \longrightarrow \pi_1 X \longrightarrow 1, \end{aligned}$$

giving complete descriptions in algebraic terms of $\pi_2 X$ and $\pi_3 X$. (The description of $\pi_2 X$ also follows from Brown-Higgins, as said before.) From this information on $\pi_3 X$ and $\pi_2 X$, one can deduce information on $H_3 X$ and $H_2 X$, particularly if $\pi_1 X = 1$.

Having said all this, and granted pushouts of crossed modules resp. Cat^1 -groups are well-founded, how much mileage is there in the “fundamental Cat^1 -group of a map of topoi”, and the associated “integration theorem”?

Yours affectionately,

Ronnie

Lettre de Ronald Brown à Alexandre Grothendieck, 15.08.1983

15th August, 1983

Dear Alexander,

Thank you very much for the latest section of your notes, which I am duplicating and sending out.

I will not be able to say very much in return in the next few weeks, what with a formal popular lecture to give to the British Association for the Advancement of Science next week, on “How Algebra gets into Knots”, and in preparing two papers with Steve Humphries on Symplectic Geometry [36, 37], as well as writing up my talk to the Conference of Categorical Topology at Toledo, Ohio [23], and some continuing work on my article on groupoids. Reading your notes, and feeling the contact through them, is also a help taking my mind off the sadness of the loss of a beloved son in a climbing accident quite recently: for his final hours I was called back from Toledo.

It did strike me that one portion of your notes is heading towards the idea of a polyhedral set, using models more general than globes, simplices or cubes. As I explained to some extent in my previous letter, a lot of the ground work in this has been done by David Jones, and I am hoping to receive the final copy of his thesis [91] fairly soon. The typed version was all but finished in July. I will certainly send

you a copy of it when it is available. It is definitively not a complete account of all that should be done in this area, and in particular the topics of realisations, and of homotopy categories, have not been touched upon. But at least we have a model category with both a geometric and a combinatorial description, and so a whole vast range of subcategories of this model category which can be considered for particular purposes. My hope is that this is the beginning of work in this area, and I will certainly be intrigued to see if you have a reaction to the material when I get it to you in the end.

I expect to be writing again in September.

Yours very cordially,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 06.09.1983

6.9.1983

Dear Ronnie,

I took the occasion at last of a break, in my marathon ponderings, through the events of life, and before plunging into those notes again, for reading more attentively than I had done, your long, lively and substantial letter of June. Even now, I should confess, your letter would have deserved a more competent reader, or more accurately, one more eager than I am to acquaint himself with some of the harder (or subtler?) conceptual technicalities of current work on foundational matters of homotopy theory. Still, I feel that your help, as well as your encouragement, and Tim Porter's, too, have been extremely valuable in various ways, while apparently I am just going on spinning my thread in my own corner and not taking too much advantage of the ideas you both are so patiently trying to get through to me. On a different level, the mere fact of hearing from you that you take interest in those bulky rambling notes of mine and sometimes have pleasure in reading in them, is quite an encouragement by itself, and you need not excuse yourself for not finding the time so far for commenting on them. It would be quite useful though if you could give me some overall comments maybe till the end of this year, namely before I give the manuscript of the first volume to the editor (it seems there are going to be two rather bulky volumes, if I carry through that journey to the end). It looks so that the "modelizing story" alone will take about eight to nine hundred typewritten pages, namely more than what can reasonably be squeezed into a single volume; therefore I am thinking of gathering in one volume the six first chapters, which center more closely around the

general question of finding models for ordinary homotopy types, whereas chapter seven (on derivators) is concerned with “models” for broader kinds of “homotopy type” notions. As the endless (and wholly unforeseen) chapter 5 on abelianization seems to be nearing completion, this would leave me with writing mainly chapter 6, plus introduction, appendix with letters to Breen (to retype for the printer), terminological index, footnotes, *etc.* – something I should hopefully be through with within a few months from now. Whatever comments will reach me in the meanwhile will of course be very welcome. – Now to some comments to your letter – I am afraid they are going to be rather superficial and not at all “à la hauteur”!

If I got it right, you’ll say that in a given category T there is “an exponential law” (devil knows why you call it this way!) when T is stable under cartesian product (or possibly endowed with a more general type of inner “product”?), and stable moreover under the corresponding “function-object” formation, which in my seminars was consistently denoted by $\underline{\mathrm{Hom}}_T(X, Y)$ or simply $\underline{\mathrm{Hom}}(X, Y)$, representing the functor

$$Z \mapsto \mathrm{Hom}_T(X \times Z, Y) \simeq \mathrm{Hom}_{T/Z}(X_Z, Y_Z),$$

where $X_Z = X \times Z$ is viewed as an object in T/Z . From my point of view, $\underline{\mathrm{Hom}}_T(X, Y)$ is defined for any two objects X, Y in any category T , but in general it is an object of T^\wedge , *i.e.* a presheaf (of sets) on T – when it is representable, we identify it as usual with a representative object in T . Your interest, as I understand it, was in cases when the products $X \times Z$ as well as $\underline{\mathrm{Hom}}(X, Y)$ “are in T ”, for any objects X, Y, Z in T – and more particularly, to find interesting cases of categories T made up with topological spaces, and suitable variants of the notion of a topological space, so as to ensure stability under these operations, and possibly stable also even under “wider” operations still, such as $\underline{\mathrm{Hompart}}(X, Y)$ (taking the “partial function object”), which a priori again is just an object of T^\wedge , which, however, you would like to be representable. I never had to work so far with this variant of the $\underline{\mathrm{Hom}}$ -object which you got involved with, but with quite a number of other variants currently met with in algebraic geometry, one being $\underline{\mathrm{Hom}}_S(X, Y)$, when X and Y are objects of T “over” the object S , *i.e.* objects in the category T/S , and the object $\underline{\mathrm{Hom}}_S(X, Y)$ can be interpreted as just the usual $\underline{\mathrm{Hom}}_{T/S}$ with respect to the ambient category T/S rather than T itself. Of course, whenever such objects $\underline{\mathrm{Hom}}$ and variants “exist” in a given category, this is quite a useful feature – I doubt very much though that such requirements will prove to be the decisive leading thread, to find one’s way towards *the* notion of a “space” still lacking, as the suitable medium for expressing and stimulating geometrical topology. But here of course I am prejudiced, as I think I do have such a leading thread (in a different direction from topoi, which is the most suitable expression for just certain kind of purposes...), showing one’s way towards so-called “tame topology”...

The main technical difference between Lawvere’s topoi [96, 97, 127] and mine [4, exp. IV] is that I am insisting on stability under infinite limits, while he

is not and replaces this requirement by a requirement of existence of the “Lawvere object” (as I called it in my notes), representing the sub-objects functor, besides of course stability under finite limits. My approach to categories in general, and to topoi more particularly, was strongly influenced from the very start, just as Lawvere’s, by the idea of performing in general categories all operations one is accustomed to in the category of sets (or abelian groups in the “abelian” case, *etc.*), and with “the same” formal properties. These operations include infinite limits of course, and it was natural for me (as my interest in sites, and later in topoi, came from the need of broadening the realm of topological intuition and cohomological techniques going with it) to include these, whereas it was natural for Lawvere, interested in logics, to focus attention on finite operations rather.

I still didn’t get the “crunch”, I’m afraid, of the main ideas around crossed modules and their main variants, non-commutative syzygies, a 2-dimensional van Kampen theorem, and, more specifically, pushouts of $K(\pi, 1)$ ’s – maybe I’m just not going to, unless at some time some really strong interaction appears with the thread I am now following. Somewhere in my program there surely *is* an ultrageneral van Kampen theorem for n -truncated homotopy types in “van Kampen” type situations (as a matter of fact, in the general situation for “integrating” homotopy types) – when I get to it, I’ll have to see if it does readily give the Brown-Higgins result as just a particular case, as a key test I would think of whether the formulation I’ve in mind is the right one, as I hope it is. Still, I do not feel like cutting short this unending digression I am involved in now with the modelizing story (even though I presumably could well dispense with almost all of it, if it were just as a means for working more at ease when it comes to working with ∞ -Gr-stacks as models for homotopy types). Therefore, it is likely that I am not going to come back to stacks before the beginning of next year. I thought in the beginning, my reflection on foundational matters of homotopy theory would take a month or two, now it appears that one year is a more likely estimate (six months of which have passed already...).

Just one technical question relative to a fibering $E \rightarrow B$ 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 your 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 $\pi_{147}(S_{23})$ 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 [60] 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 [5] 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 and taking profinite completions (of homotopy types) "prime to the residue characteristics", the theoretical "homotopy fiber" of the map can be identified with the (prohomotopy type of the) actual schematic geometric fibers of the map. 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 SGA 4 seminar by Artin and me [4].

I think within the next day I am going to read through your preprint "An introduction to simplicial T-complexes" [21], 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

Lettre d'Alexandre Grothendieck à Ronald Brown, 08.09.1983

8.9.1983

Dear Ronnie,

I hope you don't mind my typing, I got back to it through typing those notes, for mathematical correspondence maybe it is convenient because of easier reading, and it is quicker, too. Also, it is convenient sometimes to keep a copy – but please tell me if you prefer the handwriting.

Yesterday I had a more careful look than before on your introductory preprint on T-complexes, and also on your Oct. 1980 application to SERC. This set of six

algebraic categories which are non-trivially equivalent does look intriguing indeed, I know just one other example from algebraic geometry, with about a dozen equivalent categories over any given ground scheme S (say), one of them being “forms” over S of the standard projective line \mathbf{P}_S^1 over S . And also six or seven remarkable interpretations or descriptions of the group $\mathrm{GL}(2, \mathbf{Z})$, counting only as one (not twelve!) the interpretation via automorphisms of $\mathbf{P}_{\mathbf{Z}}^1$ or one of the other equivalent structures. This group $\mathrm{GL}(2, \mathbf{Z})$, or $\mathrm{SL}(2, \mathbf{Z})$, or better still its “universal covering” $\widetilde{\mathrm{GL}}(2, \mathbf{Z})$, is the main building block for screwing together the Teichmüller groupoids – but I am diverging, sorry! Of all those equivalent categories you are so fond of, the only one which elicits response from my own experience is ∞ -groupoids – but *strict* ones, and I haven’t got yet a precise feeling of what exactly is implied by this strictness, what exactly it is meaning. A strong hint comes of course from the fact that the simply connected homotopy types which can be represented by such models are merely products of Eilenberg-Mac Lane spaces, or equivalently, their canonical map towards their abelianization is an isomorphism of homotopy types. This means that, apart from actions of fundamental groups, the whole theory is essentially “abelian”, it represents at any rate one diversified (via six equivalent categories!) way of looking upon abelian objects, or maybe abelian objects with a groupoid acting on it. This, as you know, kind of tempered my enthusiasm or faith in these gadgets as candidates for “the” objects I had in mind for the last ten or fifteen years. Still, I would be glad to get a clear idea of what exactly the *geometric* (or topological) meaning of these gadgets is – for instance, exactly what extra structure *on a homotopy type*, represented by a semisimplicial set as model, is implied by a T-structure on the latter; for instance, in the 1-connected case, is it just no more, no less than decomposition of the homotopy type as a product of its Eilenberg-Mac Lane factors? The way your preprint is written, there is not the slightest hint, it seems to me, that the existence of a T-structure on a semisimplicial complex isn’t automatic, that it is indeed a highly restrictive condition on the corresponding homotopy type, besides being an extra structure whose geometrical meaning is remaining obscure. As a matter of fact, I was indeed misled a year ago by a quick glance through some of the material you sent me, and I would have been again this time, if by the end of my reading I hadn’t told myself what I am now telling you, and which of course I know only because I once got it from a casual footnote of yours!

The existence of this bunch of equivalent algebraic categories suggests of course that there should be a corresponding bunch of non-trivially equivalent geometric gadgets interpreting these. Thus T-structures would correspond maybe to homotopy types endowed with some extra structure as suggested above, and similarly one would expect an interpretation for crossed complexes. Your preprint suggests that such an interpretation has something to do with filtered spaces, a very interesting kind of

object indeed – but there is merely a description of a functor going from these remarkable objects to crossed complexes, period. This is still vague to my taste, what I would like of course again is the statement of an equivalence of categories, between one which is of geometric description, and another which is algebraic. Only once I got the hint of such kind of precise relationship, I am ready to get interested in the algebraic gadgets, with the conviction that a strong interplay between algebraic and geometric intuitions is going to take place. Maybe here the answer is to take the localization of the category of filtered spaces with respect to the set of arrows made invertible by the functor to crossed complexes. Then there comes this intriguing fact that to any filtered space, via the associated crossed complex hence a T-complex, there is another space canonically associated to it, with moreover a T-structure on it (whatever this will turn out to mean), and the converse being almost true (namely becoming true when working in a suitable localization, say, of the category of filtered spaces...). To say it differently, to me that diagram of six sophisticated algebraic categories and equivalences in between will start becoming exciting when it is becoming clear that this is just the intuitively sophisticated (while technically adequate) translation of a number of precise, possibly quite unexpected, relationships between intuitively appealing geometric kinds of structures. There *are* of course a lot of geometric motivations behind the algebraic constructions, and in your preprint you try to suggest in informal language what these motivations are – as you did a number of times, too, in your painstaking letters to me. Still, with me it just doesn't get through, because on the one hand, it doesn't directly appeal to anything from my own very limited experience with algebraic interpretations of shape and form, but also (it seems to me) because there is a lack of precise statements, of the type I've been alluding to, which would be strong motivations for taking an interest and get the feeling that one can understand and handle these objects right away, without having to spend again (as you and some others did) ten years or so to get the right feeling and develop ability for handling these monsters at ease!

All this of course it not meant as arguments against getting involved with these gadgets, rather I am getting intrigued and start wondering whether maybe you haven't hidden in your sleeves "the" statements which will tell me right away what they are all about...

Looking forward to your comments to mine, Ronnie, very affectionately yours

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 15.09.1983

15th September, 1983

Dear Alexander,

It is a pleasure to get any letter from you, in whatever form you find convenient. In return, I hope you don't mind that this letter is dictated, but I do find that with one of these pocket dictating machines, and particularly also now that we have a word processor in the Department, that I can get through a lot more correspondence and say a lot more of what I want to say (with of course the help of Susan, our charming and skillful secretary!).

I would like particularly to thank you for your letter in response to the news of the death of my son. We have indeed had many letters, and they have been a great source of spiritual strength and comfort to us, bringing also a realisation of the sorrows of others (as they have appeared from some letters), and also makes me realize how widely and deeply people feel along with the sermon of our great English poet, John Donne: "No man is an island, entire of itself, every man is a piece of the continent, a part of the main; ... any man's death diminishes me, because I am involved in mankind. And therefore never send to know for whom the bell tolls. It tolls for thee."⁽¹⁸⁾ We shall keep these letters for many a year, but I think that yours will always be a particular treasure for us, as it so well expresses a depth of feeling and the complexities and mysteries of our relationship to death.

This brings me to your last two letters of 6/9 and 8/9.

The reason why I fell in to talking about the "exponential law" is that this law for sets is simply a generalisation of the ordinary law for positive integers $(l^m)^n = l^{mn}$. But I would not fight for the name. I am going to a Category Theory conference at Oberwolfach next week, and I hope to discuss these questions on partial maps with some experts there, particularly Peter Johnstone, who has written a book on topos theory.

With regard to non-commutative syzygies, there is a discussion in my paper with Huebschmann on "Identities Among Relations" [35], for the case of presentation of groups. I believe that the same sort of idea has been developed by A. Fröhlich in some long papers on non-abelian homological algebra in the Proceedings of the London Mathematical Society 1961–1962 [70, 71, 72], but these papers are rather difficult to read.

The basic idea though is that if you have a presentation $(X; R)$ of a group G , then you obtain a short exact sequence $1 \rightarrow N \rightarrow F \rightarrow G \rightarrow 1$, where F

⁽¹⁸⁾ N. Éd. Citation de John Donne, *Meditation XVII*.

is the free group on the set of generators X , and N is the normal subgroup of F generated as a normal subgroup by the set of relators R . In trying to express relations among relations you need to bring in the operations of F into account. This leads to the consideration of the free group H on the set $R \times F$ with operation of F on H determined by the rule $(r, u)^v = (r, uv)$, $r \in R$, $u, v \in F$. There is also a map $\theta : H \rightarrow F$, $(r, u) \mapsto u^{-1}ru$. However, some identities among relations are always present whatever the form of R , and so one factors out by the Peiffer group, which is the normal subgroup of H generated by the basic Peiffer elements

$$(s, 1)^{-1}(r, 1)(s, 1)(r^{-1}, s), \quad r, s \in R.$$

If we set $C = H/P$, we obtain a crossed module $\partial : C \rightarrow F$ whose kernel is the G -module of *identities among relations*. Geometrically, the kernel of ∂ is $\pi_2(K)$, where K is the geometric realisation of the presentation, namely it has a 1-cell for each generator and a 2-cell for each relator, with attaching map determined by the relator. This kind of result was originally proved by Whitehead [133, 135, 137], and is exposed in my paper with Huebschmann [35].

The above algebraic form of procedure works for other categories of algebras, and I think leads naturally to the notion of crossed complex or crossed resolution in various algebraic contexts. For example, a commutative algebra will be presented as a quotient of a free algebra F by an ideal N . It seems natural to consider generators of N as an ideal rather than just as an F -module. Going through a similar sort of process to the above leads to a “crossed module in the category of commutative algebras” whose kernel is naturally considered as “identities among relations”. This seems to be implicit in what Fröhlich [70, 71, 72] and Lue [101, 102] (a student of Fröhlich) have done, but they don’t write it out quite in the above form. To obtain a crossed resolution of the group G , you then splice in to the crossed module a projective resolution of the module of identities, in the usual sense.

I have some fear that the above falls so far short of a complete exposition, as simply to be confusing. Still, I hope that something of the idea comes over.

I confess that one of the reasons why I did not put in the answers to the questions you raised about homotopy types of T-complexes, is that at the time I was unaware of the fairly simple answer to the question. I am sure you are correct that a re-write of that article ought to make points like this clear, and I am hoping to do so in a rather brief article for the conference on Categorical Topology at Toledo, Ohio [23], which I attended half of in August. There seemed to be a reception for the talk that here were some ideas which were quite new (although for me, they are not so new, but perhaps I am presenting them better).

Maybe a fair viewpoint on the status of all this stuff is that it constitutes a rewrite of elementary homotopy theory up to and around the relative Hurewicz theorem and the homotopy addition lemma. All this is done in about seventy pages of quite complete

and technically detailed proofs in the two JPAA papers of Brown-Higgins [32, 29]. On the other hand, if you look at a standard exposition by, say, Spanier [121], or G. W. Whitehead [131], all sorts of previous machinery has to be set up, including singular homology theory, and even then the proof is fairly messy.

So the claim is that the appropriate algebra for the homotopy addition lemma is one or other of the six or so algebraic categories which are mentioned in my research proposals. This homotopy addition lemma, which says intuitively that the boundary of a simplex or a cube is the appropriate “composite” of its faces, is intuitively of a fundamental nature. But the expression using crossed complexes, *i.e.* relative homotopy groups, leads to some curious formulae which are not at all easy to understand. The reason for the lack of clarity is that they are a “folding” of a simplex or of a cube. I am afraid this is not very clear. But let me put it that in the lowest dimension, given a simplex of the following form

$$\begin{array}{ccc} & 2 & \\ c \nearrow & & \nwarrow b \\ 0 & \xrightarrow{a} & 1 \end{array} \sigma$$

then we normally think of the boundary of σ as simply $c^{-1}ab$ (if you choose 2 as a base point and choose an appropriate orientation). But that is the folded expression, when you release 0 and 1 and allow them to vary. The correct expression for the boundary of σ is simply the diagram given above.

Now what is the boundary of a 3-dimensional simplex? There is a formula which I always forget and have to work out by drawing a diagram as follows,

$$\begin{array}{ccc} & 3 & \\ d \nearrow & \textcircled{c} & \nwarrow e \\ & 2 & \\ \textcircled{f} \nearrow & & \nwarrow b \\ 0 & \xrightarrow{a} & 1 \end{array}$$

in which f corresponds to the face $(0, 1, 3)$, and then working out that the following composition is trivial.

$$e^{-1}bf f^{-1}b^{-1}a^{-1}cf f^{-1}c^{-1}dd^{-1}ae$$

This is the formula

$$\partial\sigma = (\partial_0\sigma)((\partial_3\sigma)^{-1})^f(\partial_1\sigma)^{-1}(\partial_2\sigma) .$$

This is actually a formula for the boundary

$$\partial : \pi_3(\Delta^3, \Delta^{3,2}, 3) \longrightarrow \pi_2(\Delta^{3,2}, \Delta^{3,1}, 3) ,$$

where $\Delta^{n,r}$ denotes the r -skeleton of Δ^n , and σ is a generator of the first group.

But to go back to the properties of the various functors considered. We have the following functors.

$$\begin{array}{llll} \pi & : & (\text{filtered spaces}) & \longrightarrow (\text{crossed complexes}) \\ \lambda & : & (\text{crossed complexes}) & \longrightarrow (\text{T-complexes}) \\ U = \text{forget} & : & (\text{T-complexes}) & \longrightarrow (\text{simplicial sets}) \\ |\cdot| = \text{realisation} & : & (\text{simplicial sets}) & \longrightarrow (\text{CW-spaces}) \end{array}$$

We write N for the composite $U\lambda$ (capital N is for nerve), and B for the composite of the realisation with N .

If X is a filtered space, which is *connected* in the sense that each based pair (X_r, X_n, x_0) , $x_0 \in X_0$, is n -connected for $r > n$ and also $\pi_0 X_n \longrightarrow \pi_0 X_r$ is surjective for $n = 0$ and bijective for $r > n \geq 1$, then there is a fibration $X \xrightarrow{p} B\pi X$ if X is a CW-space. Then the homotopy exact sequence of this fibration is Whitehead's exact sequence

$$\cdots \longrightarrow \Gamma_n \tilde{X} \longrightarrow \pi_n X \xrightarrow{\omega} H_n X \longrightarrow \Gamma_{n-1} \tilde{X} \longrightarrow \cdots$$

where ω is the Hurewicz map, and where \tilde{X} is the universal covering of X . This gives conditions for p to be a homotopy equivalence, which is indeed a very restricted class of spaces. But that is no surprise, because what we have is a rewrite of the first chunk of homotopy theory.

So now I have to explain why I should want to rewrite the beginnings of homotopy theory in this way. I suppose one's first answer could be that I just prefer it this way, as it seems so much nicer, and if other people don't like it, well, that's too bad. However, this would not be a totally satisfactory position.

The next defence is that these methods do give new theorems on homotopy classification, which in fact ought to be standard, and are at the same elementary level, but in fact they are not standard. For this, see my preprint on "Non-abelian cohomology and the homotopy classification of maps" [22], and also the M. Sc. thesis of Graham Ellis [66, 68]. You would think that the theorem which gives circumstances under which for CW-spaces X, Y , there is a bijection

$$[X, Y] \simeq [C(\tilde{X}), C(\tilde{Y})],$$

where the left hand side is homotopy classes of maps of spaces, and the right hand side is homotopy classes of maps of the cellular chains of the universal covers, with the operations of the fundamental groups, such a theorem should be a standard piece of elementary homotopy theory. It generalises the Hopf classification theorem, and in particular examples lead to some nice calculations with twisted coefficients (as in Graham Ellis' thesis). Now there is probably another proof of this theorem, but the published proof is in "Combinatorial Homotopy II" [137], and that proof uses a subtle analysis of the relationship between crossed complexes (which he calls homotopy systems) and chain complexes with operators. I think very few people have

had an understanding of the theorems that Whitehead proved, and little inclination to see what these “homotopy systems” were actually about. But Whitehead says at the start of this paper, translated into our language, that chain complexes with operators seem better adapted to handle calculations, while crossed complexes seem better adapted to handle realisation questions. So the two things are not the same, and you lose information in going from crossed complexes to chain complexes with operators.

Before going on to explain something of what this translation is, I ought to answer a question of where one might go from here. The trouble is that this is very much work in progress. I am not entirely convinced by Loday’s proof that all spaces with finitely many non-zero homotopy groups can be represented as the classifying space of what he calls an n -cat-group [99]. However, I am pretty sure that the theorem is true. What I would like to imagine is that there are a series of convenient models adapted to different purposes and for particular classes of spaces. For example, I would imagine that *double crossed complexes* would model another tranche of spaces, and have convenient algebraic properties. For example, they ought surely to include spaces with three non-trivial homotopy groups. However, if one is to pursue such imaginings (ravings?) then one has to get a very good hold of the first stage.

This brings me back to the relationship between crossed complexes and chain complexes with operators. The precise situation is that there is a pair of functors

$$\Delta : \left(\begin{array}{c} \text{crossed} \\ \text{complexes} \end{array} \right) \begin{array}{c} \longrightarrow \\ \longleftarrow \end{array} \left(\begin{array}{c} \text{chain complexes with} \\ \text{groupoid operators} \end{array} \right) : \xi .$$

such that Δ is a left adjoint to ξ . So for C a crossed complex, there is a map $C \longrightarrow \xi \Delta C$. But the induced map on classifying spaces is not a homotopy equivalence in general (the difference is on π_2). It *is* a homotopy equivalence if C is free in each dimension.

The functors are interesting. (This stuff is being written up by Higgins-Brown, but it seems to get pushed aside by other seemingly more urgent matters.) First, there is a functor

$$i : (\text{chain complexes}) \longrightarrow (\text{crossed complexes}) ,$$

where $(iL)_0 = L_0$; $(iL)_1 = L_0 \times L_1$ with the groupoid structure with initial and final maps $(x, a) \mapsto x$, $(x, a) \mapsto x + \partial a$; $(iL)_n = L_0 \times L_n$ ($n \geq 2$) considered as a family of abelian groups indexed by L_0 . The structures are the “obvious” ones. (The groupoid part appears in your SLN on “Fibred categories” [77], as was pointed out to me by Tim.)

Lettre d'Alexandre Grothendieck à Ronald Brown, 21.09.1983

21.9.1983

Dear Ronnie,

Here is another bunch of notes. On page 426, there is a somewhat personal comment concerning (among others) your own person. In case you should feel it improper for being included in the planned book, please tell me so and I'll take it out from the final typescript.

Thanks for the manuscript of David Jones' thesis [91], which I just got. I look forward to looking it through!

Yours affectionately

Alexander

Lettre d'Alexandre Grothendieck à Ronald Brown, 28.09.1983

28.9.1983

Dear Ronnie,

It was nice to get a letter again from you, after various preprint and reprint material, including David Jones' work on poly-T-complexes [91]. Again I had only a very superficial reading of your letter for the time being, and no comments therefore. Just one comment about my previous letter, where I am making a stupid misstatement, to the effect that the property for a simply connected space to be homotopic to a product of Eilenberg-Mac Lane spaces can be expressed by the canonical map to its abelianization being a homotopy equivalence – a property which apparently is practically *never* satisfied (except for contractible spaces only?).

Yesterday I looked through David Jones' notes – to my surprise I didn't find there any of what I expected from your comments. The two things namely I was out for are (1) for a “model category” M in Γ , made up with “polycells”, and an object X in M^\wedge , may the homology and cohomology of the geometric realization of X be computed in terms of the obvious face operators (using the signatures ε_i of prop. I 5.1), and (2) does the geometric realization functor $M^\wedge \rightarrow (\text{Spaces})$ and its right adjoint Sing_M establish an equivalence of a localization $S^{-1}M^\wedge$ of M^\wedge with the homotopy category, and (similarly) does the corresponding statement hold for the functors i_M , j_M between M^\wedge and (Cat) , *i.e.* is M^\wedge a weak test category? Failing to prove such a statement, it seems to me that the name of a “model category” given to M is

misleading (as one would think of course that the objects of M “model” homotopy types, in a precise sense). The main thing I learned from the notes was about the “shellability” condition, as a handy purely combinatorial condition insuring that certain cone complexes are topological (even combinatorial) spheres or balls. Also, the idea of “marking” a cone complex, or equivalently an ordered set (*i.e.* choosing for each element in it one among its predecessors), as one way for eliminating un-wished-for automorphisms, looks interesting – but to my taste Jones’ work is not yet wholly conclusive for showing that this approach is a fruitful one, namely does give rise to a large bunch of actual model categories. I should confess I didn’t really look through chapter IV on degeneracies, because it was about clear that I wouldn’t find there either what I am interested in at present, by way of polyhedral cells taking the place of simplices and the like to do homotopy theory. It turns out that the main emphasis of the whole work is on “thin” structures, more specifically to get another (infinite) bunch of categories (of M - T -complexes) equivalent to the bunch of merely five which were already around – and in the process the question of modelizing homotopy types (via localized categories $S^{-1}M^\wedge$) seems entirely forgotten!

I also had a quick glance at the reprint of S. P. Humphries [86] and preprint of Wajnryb [129], on the Teichmüller groups, which I denote by $T_{g,n}$ (g is the genus, n the number of “holes”), while $M_{g,n}$ denotes for me the corresponding modular variety (a “multiplicity”, more accurately), whose fundamental group is $T_{g,n}$. The presentation you told me about looks extremely simple indeed, and I’ll surely have to come back upon it when I am going to take up my ponderings on the Teichmüller tower. I noticed for the time being that Wajnryb’s presentation is stated for $n = 0$ only, with no mention of a generalization to general n . Do you know if there is such a generalization indeed? Of course, when $g = 0$, the groups $T_{0,n}$ are just braid groups, and there is no mystery of how to get presentations of these. My point though is that the standard presentation are of no use for the arithmetical and geometrical study I have in mind, as they are not adapted for displaying the manifold relationships between these groups (rather, groupoids now!) $T_{0,n}$ for variable n , closely related to the natural stratification of the compactified modular multiplicities $M_{0,n}^\wedge$ [60]. The first (indeed crucial) case which I have not yet fully worked out, and which involves a lot of beautiful geometry (notably with the pythagorean regular polyhedra), is the case $T_{0,5}$ (of modular dimension 2). It seems that a full description, from my point of view, of the “tower” of groupoids $T_{0,n}$ is in no way any less subtle than the similar tower for general genus g (just the proof that the description is accurate may turn out harder). Roughly speaking, the main generating building block for the complete tower of all Teichmüller groupoids is $T_{0,4}$ (of modular dimension 1), whereas the relations among generators should spring from the relations in $T_{0,5}$ – in much the same way as the rank one group $SL(2)$ is the main generating building block for all semisimple algebraic groups, whereas the relations all come from relations in the rank

two groups. What I was saying in the Teichmüller case is somewhat oversimplified (whereas in the Lie case it is not), but still it does give the general idea of the kind of description I have in mind.

Yours very affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 07.10.1983

Bangor

Dear Alexander,

I would like to thank you very much for the latest batch of notes, and letter of 21/9/83.

I certainly do not feel that what you say on p. 426 is improper, and indeed have a strong impression of your deep sense of courtesy to expect that to be likely. I am also enchanted by your sense of mutual sympathy. This is so much shared that it almost gets that a week without a letter from A. G. is hardly a week, and that a letter makes my day. Then arises the problem of my dropping everything to reply in a proper spirit, and so getting further behind. Also to answer the mathematical points takes up thought, so that the more everyday details don't get put down – only those events which suddenly envelop ones life like a monsoon rain, and one can only hope that the floods from this leave a deposit of silt from which new life will grow.

All these matters are very strange. There is no question in my mind of complaint at the loss, because we are not in a position to complain. One has only to pick up a paper to realise that loss in countless different forms is an everyday occurrence. Also in our lives we have realized clearly the lack of guarantees in our lives or in those we love, with the severe mental handicap of our third child, Adrian, possibly as a result of measles and mumps at the age of 16 months. At this time he is leading a reasonable but supervised life in a hospital 7 miles from Bangor which is able to give him the settled life he needs. Of course, many would say “what a tragedy, what a waste” (though such a unsubtle comment would not come from you). Certainly, it is not something one would wish on any parent. But Adrian has brought amazing richness into our lives, through our involvement with other parents and with professionals, so that we have had contact with a range of wonderful people whom we otherwise would not have met. Indeed, I even addressed an International Conference on Autism on the need for parent guidance (although Adrian's problem is not precisely autism). So in the end this boy with very limited capacity (apparently) has, through other people, had

an extraordinary influence. What I have learned through necessity of some aspects of psychology has also influenced my attitudes towards teaching and the training of research students (and possible training of myself), based on the psychological truism that people behave much more alike at the limits of their abilities. In this way one should appreciate a student struggling with a concept which to us is very familiar. To find a parallel in our/my behaviour with that of students, one has to put oneself at the limit of one's own understanding – which is usually not too difficult.

But perhaps one of the clearest lessons one learns from dealing with retarded people is that one learns from success, and that the more success the better. In fact, you need training to tolerate failure, based on persistence succeeding. The problem is to arrange for success. A method of proved value is to arrange so many props and hints that success is inevitable, and then gradually remove the props. With such processes of error free learning, amazing successes can/may be achieved (although the process is not certain). The academic profession has many who do not realise that in any teaching process, there are two variables, the teacher (and his method), and the taught, and lack of success (*e.g.* if the taught learns nothing) is a comment on the combination of teacher and taught. While it is not true that one can teach anybody anything, it does seem to be true that everyone can learn something – a key limitation being time. It is also true that it is very difficult to predict a reaction to a training programme. I have found myself over the years amazed and delighted by the creativity of my research students.

3/10/83

All this is a digression, except that it maybe suggests that sense of strangeness and wonder in life, which to me comes through your letters and writing, and to which I very much responded.

To come back to p. 426: your phrase “dialogue de sourds” seems to me much too harsh, at least on you! The amazing thing about your approach to non-abelian cohomology has been, to me, that you seemed to be imagining the existence of gadgets which had many formal similarities to gadgets which I was considering for entirely different reasons. To me this was an enormous encouragement to continue working confidently and attempting to pursue these various ideas. But many aspects of our approaches differed greatly. You start with a broad fund of examples in algebra and algebraic geometry, and in the latter subject I am woefully ignorant. In homotopy theory I have studied a few aspects deeply, but seem to have got involved since writing my book “Elements of modern topology” in testing out some new ideas and expositions to see how they worked. In particular, analysing one geometric idea closely led to a suggestion for higher dimensional van Kampen theorems, and a search for algebraic models which, by sufficiently modelling the geometry, would give expression to such a theorem. Your deep experience of algebraic geometry suggests also the need

for an analogous theorem. But in order to “integrate homotopy types”, one needs some algebraic models, and your instinct is to go for “lax” models, while mine is to seek “strict” ones. The difficulty with my programme is that it has yet to prove itself in the more complicated cases. My recent visit to Strasbourg for a weekend’s work with Jean-Louis convinces me that his ideas of a proof for van Kampen for n -cat-groups (*i.e.* Cat^n -groups, *i.e.* n -fold groupoids internal to (groups)) are correct, but the implications of this are unclear. Fortunately, Graham Ellis, a research student at Bangor, has made excellent progress at understanding Cat^n -groups [66, 68] by looking at generalisations of the equivalences

$$\begin{aligned}\text{Cat}^1\text{-groups} &\sim \text{crossed modules} \\ \text{Cat}^2\text{-groups} &\sim \text{crossed squares,}\end{aligned}$$

and made precise (I think) Jean-Louis’ intuition that crossed squares are crossed modules internal to the category of crossed modules. This gives an inductive notion of crossed n -cube as crossed module internal to crossed $(n - 1)$ -cubes, and so an equivalence

$$\text{Cat}^n\text{-groups} \sim \text{crossed } n\text{-cubes (of groups).}$$

This algebra seems marvellously rich, as it needs to be if Cat^n -groups model truncated pointed homotopy types (as Loday claims [99], and it must surely be true).

There is a serious point about “lax” structures, because all these structures (*i.e.* the fundamental Cat^n -group of an n -cube of maps, something which I won’t try and define in detail here) come from the known structures of the unit interval I and its products I^n . Some years ago (1967 or so) I set my student Phil Heath on the lines of thinking: the unit interval I is a cogroupoid up to homotopy, where “homotopy” is defined using the unit interval I – a nice example of taking in one’s own washing. There is a rather large literature on n -fold iterated loop spaces, and ways of characterising their algebraic structure (up to homotopy, of course) with “recognition principles”, and many applications. I’ve never been very attracted to this area, because of my own limitations; dislike of bandwagons (really, I would like to run my own bandwagon); irritation with the necessity of finding base points; and desire to develop my own ideas. After all, the space of maps X^{I^n} is an iterated something, but what? Such a characterisation, involving all the X^{I^n} , $n \geq 0$, *i.e.* the cubical singular complex KX , would surely be some kind of *lax* ∞ -groupoid, not a popular structure. But the key point was that my model of a proof of the van Kampen theorem involved strict groupoid structures. This may be because of too narrow a view of what a van Kampen theorem should be. Indeed, Dieter Puppe (Heidelberg) has recently pointed out [114, Lem. 2.3] that one of the basic facts about the (simplicial) singular complex SX should be phrased as: if \mathcal{F} is a family of subsets of X whose interiors cover X , and $S_{\mathcal{F}}X$ denotes the singular \mathcal{F} -complex, of simplices $\sigma : \Delta^n \rightarrow X$ such that

$\text{Im } \sigma \subset \text{some set of } \mathcal{F}$, then the inclusion

$$|S_{\mathcal{F}}X| \longrightarrow |SX|$$

is a homotopy equivalence. This is almost (?) a van Kampen theorem. But where is the algebra?

I have thought of trying to characterise SX as a “weak T-complex”. It has “thin” elements, intuitively those which factor through some map

$$\begin{array}{ccc} \Delta^n & \xrightarrow{\quad} & X \\ & \searrow r & \nearrow \\ & L & \end{array},$$

where L is a proper subcomplex of Δ^n and r is a retraction, but I am not sure if this is the right definition, nor what are the axioms for such a weak T-structure. So in the end, I go back to strict structures, which can be extracted from the presence of additional structure on X , in the first place a filtration, later an n -cube of subspaces. In the latter case, there are a lot of details to make precise. In the filtration case (due to the clarity of thought of Philip Higgins) the whole theory makes a marvellous, satisfactory and elaborate structure (but then I’m prejudiced) with all the details fitting excellently together. The fact that it is a limited theory (as you constantly urge to me point out), together with possible future successes for Cat^n -groups, may give it mainly a key rôle in exposition of elementary homotopy theory. Considering the number of people who understand this theory for filtered spaces, suggests that such expositions are a long way off.

Also, I have to admit that people (*e.g.* me) tend to learn things when they can see a need to do so. The people one wants to take notice are usually very busy with their own plans and ideas, and require some good indication that the effort of learning particular new ideas is worthwhile. This reminds me of your question about Quillen, which I discussed briefly with Jean-Louis, who is working with Quillen on “cyclic homology” [100], a recent development from work of Alain Connes [51]. It seems Quillen has received your letter, but the impression of Jean-Louis was that Quillen is now much more interested in concrete, even formulistic, type problems, than he was 15 years ago. For example, many now are interested in “polylogarithms” $\sum_n \frac{x^n}{n^k}$. Properties of this (so Jean-Louis claims) are related to the homology of Lie algebras of matrices, so Quillen is not so much interested in foundational questions. Also I get the impression Quillen writes only when he has some clear mathematical point to make. He told me last April in Aberdeen that he would certainly write to thank you, that he would wait till his secretary produced the letter...

There is an approach to the homotopy theory of topoi which I have discussed briefly with Tim and of which I would like to ask your opinion, or if it strikes a chord.

The usual Čech theory of spaces involves approximating a space X by an inverse system $\{K_\lambda\}$ of polyhedra, where λ runs over the category $\text{Cov}(X)$ of open covers of X . One then computes H_*K_λ and $\varprojlim H_*K_\lambda$. Alternatively, one can compute C_*K_λ (the chains of K_λ), $\varprojlim C_*K_\lambda$ and $H_*\varprojlim C_*K_\lambda$. Alternatively, as Tim suggests, one can replace \varprojlim by holim (since chain complexes have homotopy notions) and obtain (so I understand from Tim) Steenrod-Sitnikov homology.

A further possibility, which has not been investigated at all, is to form πK_λ , the homotopy crossed complex of K_λ filtered by its skeletons, and form $\text{holim } \pi K_\lambda$. This crossed complex has a fundamental group, and homology groups. The latter correspond to the homology of the universal cover. In particular, H_2 of this crossed complex is an analogue of π_2 . To get more complicated invariants, one would replace crossed complexes by Cat^n -groups. This involves replacing the polyhedron K_λ , the nerve of a cover λ , by a “multi-polyhedron”. I am not sure of the precise definition, but it seems to me that such clearly exist. The realisation of a multi-polyhedron is multifiltered, and so gives rise to a Cat^n -group. Now do the above process, and one obtains a “Čech Cat^n -model” of X .

If this makes sense, then it might be possible to repeat the above process for topoi, *i.e.* for X a topos. Isn't it true that even π_2 of a topos has no clear definition? Even the above filtered method suggests a possible approach.

It has been pointed out to me by Peter Johnstone that if X is a topos, then K_λ is not a set polyhedron in the usual sense, but is internal to the topos. I don't really understand this. But it still seems reasonable to construct πK_λ (internal to the topos, using generators and relations if necessary) and from this a $\pi_2(\text{topos})$ internal to the topos. Does any of this make sense to you?

I am still edging away from answering directly your main question in your letter of 21.9.83. Should the personal matters on p. 426 be included. My reaction is somewhat mixed, but with no impression of “proper” or “improper”. Your manuscript seems like a log, or diary, of a prospect or on a voyage or journey, hunting for gold, but also delighted with the scenery, and involved with the journey for its own sake. The readers will fall into at least two classes. One such class are those who want to exploit the mineral wealth (though “exploit” is an often misused word) and want to know mainly where the gold is, being too busy to worry overmuch about the trials of the voyage. (Am I labouring an obvious point?) Others will be fascinated by the story of the journey, if they have time to read it. For such readers, the personal element is important and fascinating. Another class of readers has just occurred to me: those who would like to set out on a voyage of discovery, but have no idea what it would involve, and what it would mean, in personal terms. My eldest daughter Tania and her boyfriend have set out round the world on a motorcycle (with a sidecar and hangglider, both now discarded in Pakistan). They read what accounts they could

get hold of on such journeys to get some idea of the equipment needed and of the likely hazards.

I am reminded also of the nineteenth century physicist who looked through Faraday's notebook, and decided that if Faraday thought it worth investigating if magnetism affected polarized light (it didn't for Faraday), then the experiment was worth trying again. Faraday had this deep sense of interconnections between the forces of nature, so it seems to me the analogy is fair.

All this is a roundabout way of leaving it to you to decide what personal element you would like to be in your volume; but I also feel that mathematically, our correspondence suffers from our not meeting, so that it is quite difficult to be sure of the level at which you would wish me to write, and which would communicate the main point without being overtechnical. Your *feel* for what you want from homotopy should be paid more attention to, than any so-called "illiteracy" in the subject. If Tim or I find we can clarify some technical points, we are only too delighted to be able to help. In any case, I am personally attracted to the idea of starting from a totally "wrong" point of view and modifying it in the light of experience till it becomes correct and novel – most of my ideas are totally wrong, anyway, but one can try, like the Red Queen in "Alice through the looking glass", to believe five impossible things before breakfast, it is only a matter of practice.

On looking at your paragraph on p. 426 again, it strikes me that I have been thinking of my own experiences and not worrying as I should about your thoughts of your granddaughter Ella, and your sense that the word "accident" is inappropriate, at least as I judge from what you say. I think I sent you a copy of the order of funeral service for Gabriel – in case not, I enclose another, because I am struck again by the quotations, particularly the one from Michel Quoist, the book opening at the page to show my wife those words. I think they are very *hard*, in several senses. But they suggest, what it seems we both feel, that there are levels of life of which we are only dimly aware, and in which action and reaction (what in India is called karma) mean something different to what is visible before our eyes. One forgets how easy it is to depart this life: while swimming in a public bath, I looked down once, and saw a girl of nine or so lying flat on the bottom; I screamed at the attendants, one of whom dived in, dragged her out and saved her. But to know more of how all this works, is way beyond us, like the Greeks trying to imagine atoms. It is a cliché, I know, but I do believe that "the world is not only stronger than you imagine, but stronger than you can imagine", and it is a delight in such a fact which surely shines through your work.

Another quotation is from “Mister God, this is Anna”, which you may like in relation to your granddaughter. I was particularly struck by the phrase: “When I die, I shall do it myself”. I shall send you a copy of this book when it arrives.

5/10/83

This letter is going on too long, and not too clear, but I have sufficient trust that you will forgive any maundering element. Actually, it is remarkable that this correspondence has got so far, without either of us able to envisage a facial reaction or tone of voice – it does reflect your command of the English language and way of writing so that one catches a tone.

I would be delighted if you felt that I was helpful in stimulating your musings on non-abelian cohomology, and if you said so, but on second thoughts maybe this manuscript is not the best place to mention Gabriel. I know too many people with severe problems (particularly those with handicapped children) and many who have suffered similar losses, to feel one is in this respect more special than anyone else is special.

This brings me to your letter of 28/9 received today.

I entirely agree that David Jones’ thesis [91] does not solve all the problems one would like. Incidentally, this thesis is not meant to be comparable to a Doctorat d’État since the minimal time for submission of a Ph.D. would usually lead to a candidate of age 24 – David’s is 3 years over this kind of time. For the last three years he has been on his family’s sheep farm in Mid-Wales, seeing me occasionally. I don’t think the equivalence of homotopy categories is as hard as what he has done (it is surely true), but it is of more direct relevance to matters of general interest in homotopy theory, than the “thin” structures are at present. What is more embarrassing is that the simplicial T-complex structure is available only in Ashley’s thesis. That can’t be such bad stuff because Lawvere and Duskin were moving along similar lines (they were dealing with “hypergroupoids”), but they never got as far as T-complexes. Jack and I agreed at Oberwolfach to try and do something about writing up the equivalence (simplicial T-complexes) \sim (crossed complexes). However, my own mood has changed, and I would want to spend more time on the Cat^n -groups, where the algebra is very intricate and fascinating, with apparent/clear relations with higher commutator theory in groups, relations which need to be made explicit and exploited. It is not clear that polyhedral ideas are going to help at all here, so I think the only thing is to send it around, get David to solve the obvious immediate problems and see what other people’s reactions are.

I am fascinated by your cryptic indications on “Teichmüller groupoids”. Would you mind sending me any of your notes and see if any of us could follow up some of your ideas here? In particular, I would really like to see how the groupoids come in.

7/10/83

It is ridiculous not to post this letter.

Yours affectionately,

Ronnie

Lettre de Ronald Brown à Alexandre Grothendieck, 27.11.1983 ⁽¹⁹⁾

U.C.N.W.
27 Nov 1983

Dear Alexander,

First, many thanks for the latest batch of notes up to p. 550, which I am sending out to various people. I very much enjoyed the story of the birth of your grandson.

Mathematically, things have been hectic and so I have not yet had further time to go in detail over your manuscript. I intend to get down to this in the next four weeks.

At the end of October, Jean Pradines from Toulouse came to Bangor with British Council support for a brief visit. I do not know if you know him – he is of course in close contact with Molino, because of their close interest in differential topology. Jean has since the mid 1960’s been advocating and developing the use of groupoids in differential topology, and in 1966 he published a short C. R. note on the holonomy and monodromy groupoids [113]. The holonomy groupoid is a kind of obstruction to extending a differential structure on a neighbourhood of the identities of a groupoid, so as to obtain a differential groupoid. There appears to be some relation with your work on étendues. The interest of our discussion is to increase my knowledge of differential ideas, and swap groupoid techniques, but principally to try and extend notions of holonomy to *double holonomy*, a conjectural notion presumably involved when dealing with two compatible equivalence relations or other structures. We have not got far as yet.

Jean-Louis Loday came over from Strasbourg on November 14, so I was then working hard trying to sort out my ideas on pushout n -cubes, for deducing a general Hurewicz theorem from the van Kampen theorem for Cat^n -groups [41]. We talked for

⁽¹⁹⁾ N. Éd. Il n’est pas certain que cette lettre ait été envoyée.

two and a half days, and went over the proof of the van Kampen theorem in detail. His draft is now being revised, but it really does seem a marvellous multisimplicial proof. The basic idea is that a Cat^1 -group is a diagram of groups and morphisms

$$G \begin{array}{c} \xrightarrow{s, b} \\ \xleftarrow{i} \end{array} N \quad \text{with } si = 1_N = bi \text{ (so that } N \text{ can be considered as a subgroup of } G),$$

and such that $[\text{Ker } s, \text{Ker } b] = 1$. This condition is equivalent to s, b being the initial and final maps of a category (groupoid) structure on G which is compatible with the group structure. So a Cat^1 -group is just a category object internal to (groups). If the condition $[\text{Ker } s, \text{Ker } b] = 1$ is dropped, then we obtain a *pre-Cat*¹-group. We call G the “big group” of the Cat^1 -group.

Colimits of Cat^1 -groups $\varinjlim (G_\lambda, N_\lambda)$ are easily calculated. Form the colimit as a diagram of groups obtaining $G' \begin{array}{c} \xrightarrow{s', b'} \\ \xleftarrow{\quad} \end{array} N'$, a *pre-Cat*¹-group. Then factor G' by $[\text{Ker } s', \text{Ker } b']$ to obtain the colimit as a Cat^1 -group. It is this algebraic method which is modelled by the proof of the van Kampen theorem, first for Cat^1 -groups, and then for Cat^n -groups (by induction). I am sending separately a first draft of the proof, and examples of some calculations.

There is one problem about Cat^n -groups, namely that Loday’s “proof” in his JPAA paper [99], Lemma 3.5, that $\pi_i X = 0$ for $i > n + 1$ implies $X \simeq BG$ for a Cat^n -group, is not correct. ⁽²⁰⁾ So at the moment we do not *know* that Cat^n -groups model all truncated homotopy types. I suspect that what is needed are some developments involving multiple singular complexes (following a suggestion of Tim Porter), but nothing definite has appeared as yet. I am also sending separately an expository paper submitted for the Toledo Conference on Categorical Topology [23] (which I had to leave hurriedly to say farewell to Gabriel). I have endeavoured to take account the points you have made earlier on the paper “An introduction to simplicial T-complexes” [21]. I am also encouraging David Jones to look hard at the realisation problem (once his sheep become less troublesome!). It occurs to me that there are also questions about realisations of multisimplicial singular complexes.

The fact that the van Kampen theorem for Cat^n -groups involves in its proof entirely different ideas from the earlier proofs for crossed complexes [29, 32] does raise questions about the status (or eventual status) of these earlier results. For the moment it seems best to have various accounts, particularly as I suspect that very few people understand the earlier results and their implications, such as they are. In my Toledo article, I have taken the line that the methods give a new *exposition* of basic homotopy theory up to the relative Hurewicz theorem, and without having to discuss singular homology theory.

⁽²⁰⁾ N. Éd. Cette preuve a été complétée par Steiner dans [124]. Voir aussi [43].

Lettre d’Alexandre Grothendieck à Ronald Brown, 07.12.1983

Les Aumettes, 7.12.1983

Dear Ronnie,

Thank you very much for your warm and thoughtful letter, written more than two months ago – and please excuse the long delay for answering. Those two months up to today have been rather dense with events from life, and at some times, too, I have been intensely busy with mathematical reflections (still a strong passion of mine it would seem). A number of times though I have thought about this or that in your letter, lying on my working desk for all that time, as a reminder of a pleasant thing ahead, namely replying.

What you write about the loss of your son Gabriel, and about Adrian – about the hidden blessings in events which strike us as misfortune, is very much in keeping with what I have been learning for the last twelve or thirteen years. Whatever has occurred to me since early childhood, however grim (and worse sometimes), once it has been accepted, digested, assimilated, becomes a blessing, a source of quietness and joy. I should add, however, that I am far from through with learning this one lesson, about the hidden blessing. Again and again, it happens that the first reflex, when faced with events unexpected and unwelcome, is refusal. This reflex is strongly rooted, and all conditionings throughout my whole life have acted towards strengthening it. Most part of my life, it seems to me, there would safely occur something within myself going beyond this automatism of rejection of most of the fruits of life. That now something has come into being (or maybe rather, has surfaced, while it had remained repressed before . . .) which does go beyond, has been a deep change in my life.

Rejection of death is part of this strong-rooted mechanism of rejection. It seems to me that there is no such rejection of death any more within me. That is why I would not feel in unison with Donne, whom you cited in one of your letters – but maybe it was a misunderstanding of mine, when I felt that in the words you were citing, death was being resented like a calamity – that by the death of someone, the world was deprived, “diminished” (if I recollect it right). For many years, the reminder of my own death (through the death, say, of someone else) comes to me as a secret joy, not as a sadness, still less fear. However, until last year, I never became consciously aware of this simple fact, nor did I pause, for some minutes or hours, to ponder about its significance.

There is a reverse face of that knowledge about the hidden blessing, surely you have experienced it as I did, many times – it is about the hidden aspects of what first strikes me as a blessing: everything in the long last is straightening out thank Gods, conflict of long standing is about to resolve itself, say, something maybe I had been longing

for is coming true, you are full of joy and thankfulness — and maybe the very same day, or months later, never mind which, there comes “the other side”, sometimes very brutally indeed, which then may well be taken as the hidden *curse* in the apparent blessing! It is the up-and-down of things in life, which sometimes is rocking my boat pretty strongly. I became aware of this movement for the first time very strikingly, one day I remember well, it was December 23, 1979 (easy to remember, just one day before Christmas!). I daresay God took the trouble to show me this movement to me that afternoon, during maybe one full hour or two, I wouldn't say really, through the hide-and-show game of the sun and the mist, with the scenery drastically changing under my eyes within minutes from gloomy darkness (when you would doubt there was such a thing as a sun in the world!) to brilliant blue sky with the sun shining right into my eyes, and reversely — with millionfold intermediate sceneries flowing one into the other all around me you wouldn't say how, and it was enough to turn your head a bit to see a still different world around you, a world however just as fluctuant as the one behind you. It was a grandiose show, and a humorous one, too — so much so that with all the amazement I was feeling, just not believing what was going on nonstop under my eyes a number of times, I couldn't help laughing aloud, with the meaning of all this (as an incredible kind of humorous parody of what had been going on in my own life, and all the trouble and tenseness it used to cause me!) well in my mind.

The next day I got the unexpected visit of my oldest son Serge — and the same evening occurred the first dialogue we had, in his life and in mine. While I am now writing you about all this, it occurs to me that the unbelievable “show” of the day before could well be taken also as a paradigm of what the relation between Serge and me has been since that memorable day. The death of my granddaughter Ella (who is his daughter, too), with the manifold aura of events and forces which have surrounded this death as well as her life (which has not been a happy one), appears as part of that movement — and so are the sadness or the sorrow, or the lack of either, which now and then the thought of her life and her death will cause in me, who during her life was frustrated of the joy of a simple loving relationship with my granddaughter.

Ronnie, I see this letter has been getting a long one, while I haven't started answering yet anything except barely to the first page of your very long and substantial letter. Maybe I better stop now, and come back to your letter one of the next days, maybe I'll get even to answering some of your mathematical comments! In case I should write later than I expect, let me already send you now my very warm greetings, to you, your wife and your children, for Christmas and for the New Year!

Very affectionately yours

Alexander

Lettre d’Alexandre Grothendieck à Ronald Brown, 08.12.1983

Les Aumettes, 8.12.1983

Dear Ronnie,

There I am again – just read again through your long letter of September, with a view of going on answering it. It strikes me that your letter is so rich with manifold personal comments on your own experience of life, and on your reactions to what appears to you from mine, that an adequate answer is next to impossible. Much of what you say or suggest gets an immediate response from my own experience of things, for instance about the capacity of anyone for learning – I would add even: for learning also a lot more essential matters than merely theoretical stuff or mental or practical know-how. For me, the capacity for learning, in the genuine, non-academic sense of the word, is one major aspect of the creative capacity, or to say it differently: learning is one major part of any creative act or process, and learning by itself is a creative act or process. Seen in this light, it is by no means “time” which appears as a limitation – it is *not* by “lack of time” that through most of our lives, not to say through all of our lives, we fail to be creative, and from young age to death stubbornly go on just repeating the same kind of clichés, never learning from what comes back to us (just as stubbornly!) through our actions.

I didn’t really know at all whether or not you would feel it improper that a mention should be made in my notes of such a strongly personal matter as the loss of your son, occurring about the same time as Ella’s death. This feeling of uncertainty and caution was present at the very moment when I wrote the few lines echoing these two events – and it was clear that I would have to ask you about how you felt about it. At any rate, I was confident you would not object to my having included those lines in a preliminary draft of very limited circulation. (As a matter of fact, the “circulation” is practically limited to the copies you make at Bangor and circulate as you think adequate, with the only exception of one copy for Larry Breen . . .) Your own feelings about the matter have evolved during the writing of your last letter, and I am glad your were quite outspoken about that change. I’ll make the necessary adjustments before giving the bunch of notes to the printer. As for my indebtedness to you in various ways in the writing up of those notes, it will be my pleasure to acknowledge it at the proper place, namely in the introduction of the first volume.

Now to some of your mathematical comments. There is no essential difference between the Čech process, applied either to a true honest topological space, or to a topos. In both cases, one gets an inverse system (on a filtering indexing ordered set, or indexing category, never mind, it amounts to the same) (K_λ) of semisimplicial-sets-up-to-homotopy – but *not* of semisimplicial sets, and therefore the suggestion to take

$\varprojlim C_*(K_\lambda)$ (rather than $\varprojlim H_*(K_\lambda)$ or $\varprojlim C_*(K_\lambda)$) seems to me nonsense. Instinct tells me (without having worked out anything) that $\varprojlim C_*(K_\lambda)$ (or its homology) is the better choice, not $\varprojlim H_*(K_\lambda)$. Another possibility of course would be to take $\varprojlim(K_\lambda)$, and then C_* of it – I don't know if anybody looked upon how the two compare. Surely, in many "reasonable" cases, they should amount to the same. If interested in homotopy groups, one may either take $\varprojlim \pi_i(K_\lambda)$, the more evident and technically less sophisticated choice, or $\pi_i(\varprojlim(K_\lambda))$, presumably a better one. My experience with π_1 in algebraic geometry suggests, by the way, that there may be a third choice which in some respect is still a better one, namely keep the inverse system $(\pi_i(K_\lambda))_\lambda$ as a "progroup" (call this the *ith prohomotopy group*), and refrain to pass to the limit, by which you'll lose information contained in the pro-object. If I remember it right, that's what Artin-Mazur have been doing [5]. (I was a little floppy here with basepoints, which of course should be made clear with care.) Thus, not only does the π_2 and higher π_i of a topos have a clear definition, via a suitable Čech formalism, but there are a few possible choices of such definitions which are closely related, and presumably in many cases will give the same result. This has been about clear at least for twenty years now. What you mumble about the K_λ being "polyhedra internal to the topos" is surely a misunderstanding, either in your mind or in Peter Johnstone's. It may arise from confusion between two kind of situations and constructions. One, to which I have been just referring, is the "absolute" one, starting with a topological space or a topos, one is interested in constructing "absolute" homology, cohomology or homotopy invariants, and the Čech type approach is one possible approach (whereas the singular approach is another one, of great algebraic simplicity, but not suited for all purposes). The other situation is a "relative" one, when the space or topos S we start with is being viewed as the "base" for topologically flavoured objects, such as (fiber-)spaces over S , which at any rate are interpreted intuitively as such fiber-spaces or "families of spaces" X_s , parametrized by the "points" of the space or topos S . The *relative* homotopy, cohomology and homotopy invariants of such an object over S are then sheaves of sets or groups (or modules) over S , whose fibers, roughly, describe the corresponding invariants for the (hypothetical) fibers X_s . Thus they may be viewed as groups, *etc.* "internal to the topos". On the other hand, one convenient way for describing such "relative" objects for S is by just taking semisimplicial sheaves of sets on S , and passing to a corresponding derived category (which was carried out in some detail in Illusie's thesis [87]). When S is a one-point space, we just get the derived category of ordinary semisimplicial sets, namely the ordinary homotopy category. In the general case, if X_* is any semisimplicial sheaf on S endowed with a section σ ("base point"), we get invariants $\pi_i(X_*, \sigma)$, which for $i \geq 1$ are sheaves of groups on S , with π_1 operating on the higher ones, which are abelian, *etc.* These sheaves of groups should not be confused with the absolute invariants $\pi_i(X_*, s)$ which are just ordinary groups, provided we got a base point on X_* (for instance $s = \sigma(s_0)$, where

s_0 is a point of S , in which case we should get a canonical mapping

$$\pi_i(X, \sigma)_{s_0} \longrightarrow \pi_i(X, \sigma(s_0)) ,$$

where the first member is the fiber of π_i at the point s_0 .)

To pass to a similar situation, for fixed S (a space, say), consider variable spaces X over S . When X is fixed and endowed with a section σ , it shouldn't be hard to define homotopy sheaves on S , $\pi_i(X/S, \sigma)$ – already when S is reduced to a point, we have the choice between two definitions, the singular one (surely the more popular one) and the Čech one. In view of generalization to the similar situation with topoi, let's rather take the Čech approach. When S is again a general topological space, this will induce us to devise a Čech-type formalism of X *relative to* S , when X is being approached by an inverse system of semisimplicial *sheaves* on S , each being viewed as defined up to homotopy. If Peter Johnstone had something sound in mind when telling you about “internal polyhedra”, it must be something very much along these lines, I believe. At any rate, I hope you'll get rid of the feeling that all this is something very mysterious and beyond plain mathematical “bon sens”.

I am very pleased that the little I wrote about the Teichmüller groupoids is appealing to you, and I would be delighted to send you notes on this, if there were any. But for the time being there are a bunch of scratch notes from two years ago and some newer ones from this year, which nobody except myself could possibly make sense of. There is a seminar going on on the matter for nearly two months, which has been getting lively lately, with two really interested participants besides me. (Unfortunately, nobody for the time being is taking any notes, not any clean enough to be readable by someone who was not present.) I am barely spending a few hours a week pondering about the subject, in connection with the seminar – still there must be some underground work going on alongside, I feel that the situation is ripening rather quickly, while I keep devoting my main energy input to the reflections on homotopy.

To come to a more practical matter – the publisher of “Pursuing Stacks”, and presumably of a wider series “Réflexions Mathématiques”, has asked me to write a short text of presentation in English of the planned series, as well as of the part now in preparation. I am sending you one copy of what I wrote up – if it is not a nuisance to you and you find a moment for it, could you tell me if there are any linguistic serious shortcomings which may obscure the meaning. Larry Breen already read it, and he was so kind to send it back with a bunch of suggested corrections⁽²²⁾ – but as a number of these seem to me stylistic rather than linguistic and to change the meaning or at any rate the nuance I was having in mind, I decided it was a lesser evil keeping some awkwardness in expressing myself in a foreign language, rather than

⁽²²⁾ I am joining on this copy.

saying things differently and in a style which isn't mine. So please tell me at least if the text as I wrote it is at all readable and makes a good sense to you.

Yours very affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 12.12.1983

12th December, 1983

Dear Alexander,

I must apologise for not writing for some time, and for not acknowledging receipt of pages 500–550 of your notes. In truth, I also wonder if some of the comments made in my last letter were unhelpful, carried away by the analogy of the moment.

There are various things which have kept me since September in a tizzy. At the Oberwolfach meeting, I gave two talks, one on a survey of groupoid methods, and another on nonabelian methods in homological algebra. The second was an evening talk, and came about because I met there Bénabou for the second time, and suggested we could have a chat, whereat he suggested I give an extra talk. This was arranged, but being as it was an extra, I felt I had to produce my bottle of duty free whisky for the audience. At this talk I mentioned Cat^n -groups only as a line for further work. However, the talk was useful to me for getting into mind the style in which I wanted to write my account for the Toledo proceedings [23] (and of which you now have a copy). The main points I was making about groupoids were the wide variety of subjects in mathematics in which they occur, from ring theory (with the work of Brandt on orders [16]) to differential topology, C^* -algebras and algebraic geometry, *etc.* What I was trying to suggest is that the extension from groups to groupoids is likely to become a standard part of at least postgraduate work.

After Oberwolfach, I spent the weekend with Jean-Louis Loday, and we had three days going over his outline proof of the van Kampen theorem for Cat^n -groups [41]. Back in Bangor I got down to writing up the Toledo proceedings, and was also involved in getting towards finalising the work with Steve Humphries on Orbits of Symplectic Transvections [36, 37].

At the beginning of November, Jean Pradines from Toulouse came up for a week (under British Council support), and we discussed again our various projects on double holonomy, using multiple groupoids. It is clear that this is still a very speculative programme, but it would be very strange if the use of double groupoids was not able to make a significant advance in those areas of differential topology in which

groupoids have already been found to be a significant tool. The main fashionable area here is certainly that of foliations, in which the equivalence relation defined by the leaves of a foliation gives rise by Pradines' method of going from a "differential piece of a groupoid" to the holonomy groupoid (although Pradines' work on this back in 1966 [113] is not widely known), and also Mackey's theory of ergodic groupoids [105] has lead with Connes' work to an index theorem for foliations [52], generalising considerably that of Atiyah-Singer [9]. However, that is an area in which I am very much a layman. What may be interesting to you, however, is that Connes' work at present is on non-commutative differential topology, in particular the non-commutative de Rham complex [51]. Notes on this work are available from the IHES.

Meanwhile, Jean-Louis had sent me a very clear draft of his proof, in preparation for his visit on November 14. I just managed to get my Toledo paper off, then tried to sort out for Loday's visit how the general van Kampen theorem for Cat^n -groups would imply a general Hurewicz theorem for triad groups, and more generally, n -ad groups. Finally, all this turned out to be quite simple, provided one used systematically pushout n -cubes and also homotopy pushout n -cubes. The cubical situation here seemed to work very well, because apparently of the relation to finite intersections of a family of sets.

When Jean-Louis came, I was totally convinced by the arguments, and a number of points were clarified, including the inductive nature of the proof, so that the $(n - 1)$ -dimensional theorem is used in the proof of the n -dimensional theorem.

The proof uses heavily multi-simplicial ideas, and so is quite different from the style of proof advocated by Brown-Higgins. It thus gives a new proof of the Brown-Higgins theorem for crossed modules [27]. I suspect that this kind of proof will be more appealing to a lot of people, and also it is not really all that long, although it does take for granted a lot of facts on multisimplicial spaces, including the spectral sequence of a bisimplicial space.

There are still a number of technicalities to resolve in order to obtain all the applications which we see should easily fall out. For example, the precise details of the general Blakers-Massey excision theorem [12] have not yet been put down, although it is clear that it follows by passing from a particular truncated bi-filtered space to an $(m + n)$ -cube.

There seems to be an enormous amount of algebraic work which clearly needs to be done here, and I get the impression that we have uncovered a fairly large lode.

Graham Ellis, a research student in his third year here, has got very well with the algebraic side of this programme, in particular in sorting out the relation between Cat^n -groups and crossed n -cubes [66, 68]. At the moment he is finding very interesting results, partly in collaboration with Tim Porter, relating the low-dimensional ideas in this area to other work in low-dimensional homology and Baer invariants.

In all this, your programme is very much as a backdrop to the thoughts of Tim and me. We talk about it often. I should say that copies of your manuscript have gone out to an assortment of people, rather randomly in terms of people whom I knew well and who would seem to be interested. Also, some copies have been reduplicated, and so landed up with other people.

18th December 1983

[...]

I think what I learned from Henry Whitehead was a catholic taste, an interest in seeking out algebra for modelling geometry, and a willingness (stubbornness?) to chew over an idea until all its juices had been extracted. I remember a student of Eldon Dyer said that he and Eldon had tried six weeks to get a homotopy invariant involving maps of squares, and so obtaining a double groupoid structure. It took me seven years to obtain the very simple answer. I am not sure if this is a good recommendation or not!

[...]

I received your letter dated 7/12 after that dated 8/12 – this explains some of the form of this letter. I agree with you about John Donne. I hope you like the story sent separately of Anna and Flynn.

I have the possibility of a grant from the British Council to visit Toulouse for discussion with Pradines sometime in the new year. I have to put in a definitive proposal. How would you like me also to visit Montpellier? This is all for short visit(s), as I don't want to be away too long. If this sounds feasible, a formal letter of invitation would be useful for extracting money from the British Council. I might even try SERC, who are more generous for short visits. As they say in the films, on can't go on not meeting like this!

I am of course continuing working with Jean-Louis, and he has some money for me when I visit Strasbourg, as Professeur Associé pour un mois (making a series of visits up to September 1984).

Lettre d'Alexandre Grothendieck à Ronald Brown, 08.01.1984

Les Aumettes 8.1.1984

Dear Ronnie,

Thanks a lot for your letters, preprints, Christmas gift, *etc.* – so many things came within a week or two, and then lately from Tim Porter, too, that I am quite