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 "[...]".

Lettre de Ronald Brown à Alexandre Grothendieck, 22.02.1982

22nd February, 1982.

Professor A. Grothendieck,
Department of Mathematics,
Université de Languedoc,
MONTPELLIER,
FRANCE,

Dear Professor Grothendieck,

I have been told by Jack Duskin that you have a long standing interest in the area of multiple categories and groupoids, and I am therefore sending by separate mail some preprints and offprints on this area. I am also writing because I am hoping to attend a conference on topology organized by the Universities of Lille and Nice, June 1–5, 1982, at Marseille, and therefore would have the opportunity to visit your University if you could see that this would be possible and convenient for you.

I should say that I do not much understand the background in algebraic geometry, but it does seem to me that the new methods in homotopy theory suggested in the papers I am sending should have implications in areas other than algebraic topology. I would certainly be very interested if there were an opportunity for discussing these matters with you.

I look forward to hearing from you.

With respect,

Yours sincerely,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 04.03.1982

Montpellier March 4th, 82

Dear Ronnie Brown,

Thanks a lot for your letter and reprints. I somewhat lost contact with the technicalities of homological algebra, but glancing through your reprints did recall me about my own ponderings on foundational matters, which resulted in an extensive program of a synthesis of homological and homotopical algebra, n-categories and n-groupoids

and stacks of such on topoi, and non-commutative homological algebra on topoi. It seems to me that such synthesis is still lacking today. Your reprints seem to indicate that some relevant notions (such as ∞-groupoids) have been developed indeed, but I got the impression of a lack of a sweeping perspective. Your lack of familiarity with the notion of a topos, and with standard situations of algebraic geometry, is surely a serious handicap. I thought you may be interested though in reading a copy of an extensive letter I wrote in 1975 to Larry Breen, where I have been sketching a program – unfortunately I did not find a copy of this letter in my papers. I just wrote him to ask for a copy, and will be glad to send you one, if you are indeed interested (and if Larry does find that letter).

Very sincerely yours

Alexander Grothendieck

Lettre de Ronald Brown à Alexandre Grothendieck, 11.03.1982

11th March, 1982.

Dear Alexander Grothendieck,

Thank you very much for your letter of March 4, and your interest. I would indeed be interested to see your letter of 1975 to Larry Breen, with whom I have had some interchange of offprints. A student of his, Conduché, is interested in related ideas, and will be in Bangor for the British Mathematical Colloquium this month.

I was interested in your impression of a lack of sweeping perspective in the offprint sent. The whole emphasis of the programme has been on giving reality to a possible new tool in algebraic topology, rather than in developing the technical tools to make the machine work, and in verifying it is appropriate to some concrete problems. In view of the widespread influence of algebraic topology on other areas of mathematics, it has always been my hope that these new methods, when developed, would have fairly wide ramifications. It is now clear that the methods do give a new and useful approach to obstruction theory and non-abelian cohomology in problems in homotopy theory, and for example one result is a rather easy calculation giving the homotopy classification of maps from a surface to the projective plane, a problem which has been found awkward by traditional methods. A further point is that these methods capture only a slice of homotopy theory, and I at present have a project with Jean-Louis Loday to generalise these methods to his theory of n-cat-groups [**99**], for applications both to homotopy theory and algebraic K-theory. This again looks quite a difficult task, both technically and conceptually, and my impression is that it is in this area that

my time is best spent, at the moment, rather than in attempting to generalise the methods to topoi and algebraic geometry. However, a colleague here, Tim Porter, and I have discussed extensively the relationship between these ideas and those in cofibred categories and cotangent complexes, of yourself [**77**] and of Illusie [**87**]. In particular, the analogue of crossed complexes for commutative algebras, rather than groups, looks like having interesting possibilities.

It thus seems reasonable for me to advertise the present methods as a reformulation using homotopical algebra or non-commutative homological algebra in such a way that it should reasonably generalise to wider situations; it would also be useful to ferret out related notions from work such as yours, in order to indicate possible new techniques and directions for the present programme.

Maybe also the preprints so far do not give an idea of an underlying motivation of all this work, which is to find an algebraic inverse to the process of subdivision. It is clear to me that this is a significant question in algebraic topology and in combinatorial group theory. It would be useful to know if it had wider significance.

With great respect,

Yours sincerely,

<div align="center">R. Brown</div>

P.S. It would be useful to know if you think a visit would be worthwhile and convenient, as my travel grant application for the Marseille Conference June 1–5 on homotopical algebra asked for money to cover a proposed meeting with you, and then on to Toulouse for discussions with Pradines. I have to give confirmation or not of this, as the closing date for the application was March 1.

Lettre d'Alexandre Grothendieck à Ronald Brown, 25.03.1982

<div align="right">Montpellier, 25.3.1982</div>

Dear Ronnie Brown,

Thanks a lot for your letter and your explanations on your motivations, which I got yesterday at the same time as an extensive letter from Larry Breen, together with copies of three letters on a kind of working programme I outlined in 1975. Glancing through all this, I realize I rather phased out of these kind of questions of foundational character in homology and homotopy language, and it seems to me that a meeting with you therefore wouldn't make too much sense. If you are interested in meeting with a mathematician with substantial know-how in homotopy and homology theory

(which I am lacking in), and broad geometrical background, to enlarge your vision on the scope and significance of the algebraic formalisms you are interested in, I would strongly suggest to meet with Larry. [1] This letter I just got shows he is very much "in" indeed still in all these questions, while my own interest, in mathematics as a whole even, has become somewhat sporadic, and all the more reluctant to let myself be drawn again into building up heavy machinery! By separate mail, I am sending you a copy of my first extensive letter to Larry, and if it makes any sense to you, I'll be glad to send you the two others too – just let me know.

Very cordially yours

<div align="center">Alexander Grothendieck</div>

P.S. Larry thinks your ignorance of topoi *etc.* isn't a big drawback, and that generalisation from a point as a base to the general case shouldn't make a problem. Surely he is right in technical terms...

Lettre de Ronald Brown à Alexandre Grothendieck, 15.04.1982

<div align="right">15th April, 1982.</div>

Dear Alexander Grothendieck,

I was very pleased indeed to receive your encouraging letter of 25 March, and later a copy of your letter to Breen dated 5/2/1975.

I will be writing as soon as possible to Larry Breen to see what is the best method of proceeding.

I was really delighted with your letter to Breen. It is amazing that there should be so many points of contact of philosophy, since we have come at the subject from such different lines of enquiry.

Part of my background motivation is the idea that early writing on homology is trying to think of a cycle as some sort of composite of all the little bits out of which the cycle is made. Later, it was found convenient to define such a composite as a formal sum, and this inevitably leads to an abelian homology theory. But such a method could be looked on as a technical gimmick, a way of getting some sort of sensible theory. The real problem is to define the "actual composite" of all the little bits, and this must inevitably lead to the idea of finding an algebraic inverse to the notion of subdivision. Such an idea has led Philip Higgins and me to notions of crossed complex,

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[1] meeting with Jean Giraud would equally make sense, it seems to me...

ω-groupoid, ∞-groupoid, T-complex, and a study of their interrelationships [**26, 27, 28, 32, 29, 31, 30, 33**]. In fact, the remarkable thing is that all these categories are equivalent, and so give different views of the same geometric, or algebraic, object. The replacement of chain complex by crossed complex is then the key step in passing from an abelian to a non-abelian homological algebra. There are still many of the details of this passage to be worked out. In particular, the abelianisation functor will clearly play a key role.

An obvious question is: how much of the theory envisaged in pages 5-10 of your letter to Breen can be said to be worked out?

There is now a satisfactory theory of ∞-groupoids[(2)], and this has the more computational form of a crossed complex. If one is doing non-abelian cohomology, it is sensible to ask what the coefficients should be. It seems simplest to say that they could be any ∞-groupoid, or, equivalently, any crossed complex. If X is a geometric object, and C is a crossed complex, then the cohomology $H^0(X; C)$ should be defined as the set of homotopy classes $[\pi \underline{X}, C]$, where $\underline{X}$ is some additional geometric structure associated with X, and $\pi \underline{X}$ gives some crossed complex economically associated with $\underline{X}$. In the topological example, X is a CW-complex, $\underline{X}$ is the skeletal filtration, and $\pi \underline{X}$ is the homotopy crossed complex, a structure first discussed by Blakers [**11**] and later by J. H. C. Whitehead in his paper "Combinatorial homotopy II" [**137**]. It would be fascinating to have other examples of this kind of method.

You ask on p. 9 for a nerve of an ∞-groupoid, C. The equivalence between ∞-groupoids and simplicial T-complexes immediately gives such an idea, namely that the nerve of C is the underlying simplicial set of the associated simplicial T-complex.

Rather than give the details of all this in this letter, I enclose an account of the background to simplicial T-complexes which is to appear in an issue of Esquisses Math. [**21**] with the theses of Dakin [**56, 57**] and Ashley [**6, 7, 8**] who have developed this notion. Also enclosed is an account of a programme developed by Tim Porter and myself which was submitted in 1980 to the U.K. Science Research Council, but turned down. Of course, differential ideas are a bit out of my line of country, and recently I submitted a programme for collaboration with Loday of Strasbourg for development of multiple groupoid ideas in homotopy theory, and two weeks ago heard that this was turned down. You can imagine then how heartwarming it was to get your encouraging letter, and the first page of your letter to Breen also for me had a message, namely the necessity of trying to elucidate the basic conceptual ideas in an area of mathematics, however strange and elusive they seem. What was particularly interesting to me about the programme with Loday was its involvement with n-tuple category objects in the category of groups. This is an idea of Loday's which gives a more general

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[(2)] But this is the strict form – the correct notion of "∞-groupoid-up-to-homotopy" proves elusive, though some ideas are floating around.

algebra than that of n-groupoids, these latter being now well-understood in terms of the various equivalences of categories. Loday has a really good grasp of these more subtle invariants, and we hope that a combination of his methods with those of Philip Higgins and mine will produce some new methods in homotopy theory and homological algebra.

In conclusion then, what you have sent me makes a great deal of sense, and I would be very grateful indeed for copies of the remaining letters. My colleague Tim Porter has an excellent grasp of the algebra necessary to follow the applications which you have in mind, and copies of the material enclosed here will also go to Breen.

With cordial greetings,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 05.05.1982

Les Aumettes le 5.5.1982

Dear Ronnie Brown,

It has been a surprise to me that my letter to Larry Breen did make sense to you, and I am glad of course it did. Please excuse my delay in answering your cordial letter of April 15th, which I got only two days ago, as I have been sick for a few weeks. I hope, however, to be able to make this week photocopies of my two subsequent letters to Larry, as you say you are interested – they may be still harder though to make sense for someone who is lacking the proper background in algebraic geometry and on topoi – the latter being extremely handy objects for expressing certain important aspects of topological intuition – namely, roughly, those centering around the notion of "localization" and passage from local to global information. In this context, direct limits of topoi make always sense, and almost trivially so, I daresay – and a situation of van Kampen theorems is a typical case of a situation best expressed by stating that a certain topos (or topological space, in the initial case) is a direct limit of others [3]. The passage to van Kampen's theorem in terms of fundamental groupoids can be formally abstracted from this, by restricting attention to *locally constant* sheaves on the topoi (or spaces, by all means) under consideration [4]. This has been one of the key

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[3] This is what I have called since around 1960 a "situation of descent" – a sheaf on X "is the same" as a system of sheaves F_i on the X_i ("covering up" X), together with "gluing data"

$$F_i|_{X_i \times_X X_j} \xrightarrow{\sim} F_j|_{X_i \times_X X_j}$$

satisfying a "descent condition" with respect to the threefold products $X_i \times_X X_j \times_X X_k \ldots$

[4] These can be viewed as covering spaces of these topoi.

results in the theory of profinite fundamental groups of schemes, which I developed at about the same time as "theory of descent" [**78**], as I called it, before the topological language of topoi was developed. There comes, with this approach, a strong suggestion that van Kampen's theorem should be viewed as the byproduct of a substantially stronger "descent" statement (namely that a certain topos is a direct limit of others), deduced from the latter by replacing the topoi under consideration by their truncated homotopy types in homotopy dimension 1 (expressed adequately by their fundamental groupoids) and that higher order van Kampen theorems should follow in much the same, essentially "trivial" way, by passing to truncated homotopy types in higher dimensions $n \geqslant 2$, once a pretty simple, down-to-earth formalism of "direct limits" of homotopy types is developed; more correctly, of simplicial sets, or ∞-groupoids, or whatever category one is working with for expressing in algebraic terms the geometric notion of a "homotopy type" – yet understood as an actual object of a suitable category, not merely an isomorphism type of such – with the conviction that there should be an essentially unique notion of such "limits" [5] probably more readily expressed in terms of ∞-groupoids than in terms of simplicial sets [7], which is compatible [8] with the obvious notion for topoi referred to above, and with the "nerve" functor associating to any topos a profinite homotopy type à la Artin-Mazur [**5**], and of course, compatible [8], too, with the truncation functor from homotopy types (say, via ∞-groupoids) to n-homotopy types (namely via n-groupoids).

I wonder whether this is approximately what you achieved with your "higher order van Kampen theorem". It seems to me, in any case, that this $\varinjlim$-operation in the context of homotopy types is of a very fundamental character, with wide range of theoretical applications. To give just one example, relying on the existence of such a formalism, it is possible to give a very simple explicit algebraic description of the full homotopy types of the Mumford-Deligne compactifications [**60**] of the modular topoi for complex curves of given genus g, say, with ν "marked" points, in terms essentially of such a (finite) direct limit of $\mathrm{K}(\pi, 1)$-spaces, where π ranges over certain "elementary" Teichmüller groups (those, roughly, corresponding to modular dimension $\leqslant 2$),

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[5] I would use the name "integration" of homotopy types rather than "direct limit", as in the context, say, of simplicial sets, the notion is altogether a different one from the naive direct limit in the category $\hat{\mathrm{S}}_*$. Maybe this name associates with your own intuition of "bribes [6] and bits" piecing together to make up a global object . . .

[6] N. Éd. "Bribes" est le mot français pour "pieces".

[7] As a matter of fact, it occurs to me there is, on the contrary, a pretty evident candidate in the context of simplicial sets, by literally following this tie-up with integration of (direct systems of) topoi. But surely a direct construction in the n-groupoid context, extending the well-known one for 1-groupoids, should be made available, and presumably will describe the notions of $(n + 1)$-$\varinjlim$ (in accordance with the notions 2-$\varprojlim$ and 2-$\varinjlim$ in 2-categories such as the 2-category of categories or of (ordinary) groupoids . . .).

[8] Here, "compatible" possibly only up to homotopy, of course.

and to give analogous descriptions, too, of all those subtopoi of the previous one, deducible from its canonical "stratification" at infinity by taking unions of strata. In fact, such descriptions should apply to any kind of "stratified" space or topos, as it can be expressed (in an essentially canonical way, which apparently was never made explicit yet in this literature) as a (usually finite) direct limit of simpler spaces, namely the "strata", and "tubes" around strata, and "junctions" of tubes, *etc.* Such a formalism was alluded to in one of my letters to Larry, in connection with so-called "tame topology" – a framework which has yet to be worked out – and I was more or less compelled lately to work it out heuristically in some detail, in order to get precise clues for working out a description of the fundamental groupoids of Mumford-Deligne-Teichmüller modular topoi (namely, essentially, of the standard Teichmüller groups), suitable for the arithmetic aspects I had in mind (namely, for a grasp of the action of the Galois group $\mathrm{Gal}_{\overline{\mathbf{Q}}/\mathbf{Q}}$ on the profinite completion).

From the little I could guess from a superficial glance at the material you kindly sent me, and from your comments in your letters, I get the feeling that a substantial part indeed of what I had been contemplating as comprehensive foundations of "topological algebra" has been worked out (plus surely a lot more in somewhat different directions), by you and a handful of colleagues, and students, without attracting much attention so far. One reason probably is psychological – namely resistance against new bulky (?) formalisms, when there aren't at least one or two real big shots actively popularising the whole stuff. Another reason perhaps is of a more substantial type – what I tried to express by the (admittedly vague and superficial) feeling of mine of a lack of "sweeping perspective". It would seem that you are led mainly by the requirements of inner coherence and completeness of the formalisms you are developing, starting (if I see it right) from the existing homotopy formalism. Such requirements are often compelling and illuminating and give excellent clues, but they lead sometimes to something like a skeleton of bones, still lacking flesh and blood *etc.* for getting really alive and inspiring. Such flesh and blood is provided by "geometric" motivations stemming from wider areas of mathematics – and it seems to me that you yourself are aware of a lack in this respect among the small group of people who have been working so far on these foundational matters. Maybe such handicap can be overcome by trying to involve some people who have both a taste for elegant algebraic formalism (without being afraid of apparent bulkiness), and a strong contact with the flesh-and-blood of relevant geometry, and who, moreover, are free from the current snobism, consisting in opposing so called "serious mathematics" with so called "general nonsense". Such people are not too numerous, I'm afraid, but I can think at once of Larry Breen and Jean Giraud, and also of Luc Illusie (the two latter were both students of mine and are teaching in the Paris area), and also Quillen, who is at present at Bonn, till the end of this academical year, I believe. Quillen is an extremely nice chap (I know from Husemöller, who is at Bonn, too, this year, that Quillen is still as nice as he used

to), and an impressive mathematician, and just the ideal case of what I had in mind. When I knew him about twelve years ago, he was very open minded with respect to my own interests in foundational matters in homotopy algebra, and was developing a number of ideas of his own along similar lines [9]. I am sure if you or one of your friends has an opportunity to discuss with him, it will be very fruitful. By the way, Larry, Giraud and Illusie are very nice people, too, probably why they came to my mind first, together with Quillen, whereas other people have developed into big shots who are convinced they are too good for giving any thought to "general nonsense" – and who therefore, surely, are missing a lot of substance. (The introduction of the cipher 0 or the group concept was general nonsense, too, and for a thousand years or two mathematics was more or less stagnating, because nobody was around to make such childish steps . . .)

Please excuse my immoderate talkativeness, in contrast to my reluctance to dive into reading of technicalities! I feel out of the game, and still like to comment on it occasionally!

Yours very cordially

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 24.05.1982

24th May, 1982.

Dear Alexander,

This is to thank you very much for your long letters, and the copies of your letters to Breen of 1975. There is some difficulty with your final long letter, both because of difficulty of reading the copy and of the mathematics. Larry Breen says that he

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[9] Quillen had, among other nice ideas on foundations of "non-commutative homological algebra", an elegant formulation of the notion of (non-strict?) n-category in terms of certain special "n-fold" simplicial sets (*i.e.* contravariant functors from S_*^n to (Ens)), and also, if I remember well, a general philosophy of how to define the analogues of algebraic structures such as groups, torsors, rings, *etc.* in the context of n-categories (so as to get the right n-groupoid version of objects such as simplicial groups, rings and the like) – possibly also did he have a good idea about a non-abelian Dold theorem, which it might be worthwhile to compare with your ideas on the subject. Also he had a promising approach to higher K-invariants [**116**], which, he told me, was more or less equivalent to a more computational transcription of a somewhat abstract definition I had in mind in terms of "enveloping n-Picard categories" of a given additive category C, say, whose invariants π_i should yield the invariants $K^i(C)$. (The case $n = 1$ was worked out by a Vietnamese woman student of mine around that time, Mme Sinh [**120**] . . .)

hopes to be at the Marseille conference and I will discuss the matter with him there, and perhaps get an exposition of the main mathematical points.

In general terms, the points you are making seem to confirm that this is an area in which development is likely to take place in a variety of directions, and that the overall philosophy seems to confirm a number of points which came out initially from my study of the van Kampen theorem in dimension 1, and then were later extended to higher dimensions.

Referring to the enclosed offprint on "Higher dimensional group theory" [**20**], what struck me a long time ago was that theorem 1 on pushouts of fundamental groupoids seems to be an odd man out in theorems in Topology. The standard method of interrelating results in different directions was by means of an exact sequence or spectral sequence, and these did not give complete answers but only answers up to extensions. But theorem 1 gave a complete answer for the fundamental groupoid, and from this, information about various fundamental groups could be deduced purely algebraically. The reason why the method seems to work was that the fundamental groupoid had structure in dimension 0 and 1, and this enabled one to end up with a colimit theorem. It then seemed a reasonable game to play to try to invent, in a given geometric situation, gadgets with structure in all dimensions, which would then satisfy a colimit theorem. Of course, some sort of connectivity conditions would be required to obtain this theorem, and also it was not expected that such gadgets would solve all problems. What might be hoped was that such gadgets would solve some problems in directions hitherto unobtainable, or difficult. Such a programme can, I think, be reasonably said to have been shown successful as far as the homotopy crossed complex of a filtered space is concerned [**29**].

This is relevant to questions like: determine the homotopy type of a union in terms of the homotopy type of the individual bits. For example, one might hope to describe the first k-invariant in $\mathrm{H}^3(\pi_1 X, \pi_2 X)$ in terms of the k-invariants of X_i, when $X = X_1 \cup X_2$, $X_1 \cap X_2 = X_0$. I discussed this a long time ago with Mac Lane, and he said that he had tried it but found it very difficult. This attempt was presumably in terms of the cocycle description of k. However, if one filters X by skeletons as a CW-complex, then one can describe the k-invariant of X as the invariant of the crossed module $\pi_2(X, X^1) \longrightarrow \pi_1(X^1)$, regarding the element of a third cohomology group as being described as equivalence classes of crossed modules. But it then turns out from the Brown-Higgins results in dimension 2 that this crossed module of X is simply the pushout of the individual X_i. In this form, we do not really have a computational tool, but at least it shows the sort of results to be expected. An advantage of this type of procedure would seem to be that a crossed module is in many ways a similar kind of gadget to groups or modules. So if one is dealing with a situation with more structure, one can hope to define cohomology groups by putting such addititional structure on the crossed module, and then forming equivalence classes. For example,

one could introduce torsion conditions, one could work in a variety of algebras (this has been done by A. S. T. Lue [**59, 101, 102**]), or, I suppose, one could ask that all the groups and modules which occur are algebraic varieties. All this would give different forms of cohomology.

In the category of topological spaces, one can take a function space point of view and say that one of the reasons for studying the function space X^Y is that homotopy invariants of this space give rise to homotopy invariants of both X and Y. The classical cohomology groups are a special case of this, and so also are twisted cohomology groups, while one can also do stable versions in terms of spectra. There are some curious problems here, because it is not known whether this function space is contractible if X is a finite complex and Y is the infinite real projective space (this is Sullivan's conjecture).

If C is a crossed complex, and X is a CW-complex, it has seemed reasonable to define $\mathrm{H}^0(X, C) = [\pi\underline{X}, C]$, where $\pi\underline{X}$ is the homotopy crossed complex of the skeletal filtration of X, and square brackets denote homotopy classes. Granted that the category of crossed complexes has a reasonable internal Hom-functor, one can also define $\mathrm{H}^n(X, C) = \pi_n(\underline{\mathrm{Hom}}(\pi\underline{X}, C))$. This seems closely analogous to some of the definitions in your second letter to Breen, particularly when one notes that the categories of crossed complexes and of ∞-groupoids are equivalent, so that this can also be regarded as cohomology with coefficients in an ∞-groupoid. The question remains, though, as to whether these ∞-groupoids are the sort of gadgets that you do require for the purposes of algebraic geometry. It will be very interesting to see what Larry Breen makes of this kind of idea.

The equivalence between crossed complexes and simplicial T-complexes is a nonabelian form of the Dold-Kan theorem (I think it is fairer to call it this, rather than Dold-Puppe) [**61, 92, 62**]. It is not clear to me how this can be used in noncommutative homological algebra, unless one started to look strongly at T-complexes in other categories than that of sets, so that one can reasonably prolong functors to simplicial T-complexes in a way analogous to that of Dold-Puppe. But whereas there is some published work on category objects in various algebraic categories (Lavendhomme-Roisin, J. Algebra 67 (1980), 385-414 [**95**]), the corresponding analysis for ∞-groupoids has not been done.

A relevant idea here is that of identities among relations. In the case of groups, this leads inexorably to the notion of crossed module. At the present moment, the corresponding "combinatorial algebra theory" has not been worked out. It seems reasonable to expect that the appropriate crossed resolutions are more convenient gadgets with which to work, because they can be described directly in terms of presentations. This might be useful, say, for singularity theory.

My colleague Tim Porter has started on this study for the case of commutative algebras, and indeed I am very fortunate that his wide background has been a help in obtaining a further understanding of many of the points you have made. There is still a long way to go yet (!), but they confirm the probable utility of this area of study, involving on my part crossed complexes and appropriate such gadgets, and on his part the study of homotopy limits and related coherence problems. A typical problem here is to find a notion of weak T-complex so as to obtain an equivalence between the category of these and that of ∞-categories. This would give an appropriate notion of *nerve* of an ∞-category; actually, I think Tim has such a notion, but the question is, what extra thin structure does it have.

Your asking for an n-groupoid $\pi_n(X)$ which would give information on the n-type of X (*i.e.* truncated homotopy type) is interesting. Jean-Louis Loday has such a candidate in his recent paper "Spaces with finitely many homotopy groups", J. Pure Appl. Alg. 24 (1982), 179–202 [**99**]. The gadget $\pi_n(X)$ is in his terminology an *n-cat-group*, which is a group with n mutually compatible category structures, *i.e.* an n-tuple-category object in the category of groups. The starting point of his investigation was the equivalence between Gr-categories and crossed modules which you mention in your letter 3 to Breen – I heard that Verdier knew this in 1965, but Jean-Louis [10] learned it from the Brown-Spencer paper on G-groupoids [**42**]. He was interested in the question: what should be the "universal central extension of crossed modules"? This led him to 2-cat-groups, and an equivalence between these and "crossed squares". His general results relate n-cat-groups and n-cubes of fibrations of spaces satisfying certain connectivity conditions. We are hoping to formulate a van Kampen theorem for these gadgets [**41**] and so allow for a combination of his methods and those which have already been developed by Philip Higgins and me for crossed complexes. In a sense, Jean-Louis' work is in the Cartan-Serre tradition of using fibrations; Philip and my work is in the Henry Whitehead tradition of using cofibrations. What is now needed is a sensible combination of these two methods.

As part of his method, Jean-Louis finds for an n-cat-group G a non-abelian chain complex of groups $C_*(G)$ whose homology is the homotopy of the classifying space BG of G. However, he does not have a "Dold-Kan theorem" giving an equivalence between n-cat-groups and such chain complexes of groups, with extra structure. Work of Conduché (a student of Breen), on the Moore complex of a simplicial group, shows that such structure would have to be very complicated. It may be more sensible to rely on the n-cat-group structure, which seems related to geometrically understood objects, such as n-ad-homotopy groups.

I liked very much your remarks about "general nonsense" and "childish steps". It seems to me that the step from a group to a groupoid is precisely such a step, but there

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[10] N. Éd. Loday

still seems around a view that groupoids are rubbish, or at any rate they do not give anything which cannot be obtained by other methods. What encouraged me to go on developing the notions, admittedly from a viewpoint of homotopy theory, was meeting G.W. Mackey in 1967, when he told me of his work on ergodic groupoids [**104, 105**]. He was interested in the idea: if a transitive action of a group G corresponds to a subgroup of G, to what then does an ergodic action of G correspond? I would like to express his answer as follows.

A morphism $p : G \longrightarrow H$ of groupoids is said to be a *covering morphism* if it is *star-bijective, i.e.* for each $x \in \mathrm{Ob}(G)$ and arrow α of H starting at px, there is a unique β in G starting at x such that $p\beta = \alpha$. There is a category Cov_H of covering morphisms of H. This category is equivalent to that of functors $H \longrightarrow \mathrm{Set}$, or to that of operations of H on sets. These results are easy to formulate for internal groupoids in a category admitting finite limits. In particular, one replaces actions of H which are measurable in some sense by the appropriate covering groupoids of H, with some sort of measure structure. This is Mackey's main construction. He goes on to develop, with his students, a general theory of such ergodic groupoids, and this has led to work of Connes on C*-algebras and foliations [**48, 49, 50**]. In other words, there is a substantial analysis of measured groupoids, and this has significant geometric applications.

Similarly, one can propose that an action of an n-groupoid corresponds to a morphism of n-groupoids which is a covering with respect to one of the structures in the sense that it is an internal covering of groupoids in the category of $(n-1)$-groupoids. Perhaps the point I am trying to make is that the language of covering morphisms is possibly more convenient than that of operations (and so of torsors) because it is more internal.

I guess I am rambling on, and whereas no one in their senses would ask you to stop talking, I am sure they would ask me! I have translated your second letter to Breen and enclose a copy, in case the better duplication is of use to you. I am progressing slowly with the third letter.

With many thanks indeed for you cordial and interesting letters,

Yours sincerely,

R. Brown

Lettre de Ronald Brown à Alexandre Grothendieck, 17.08.1982

17th August, 1982.

Dear Alexander,

I have attempted a translation of one of your letters to Larry Breen, in order to make for ease of reference, and because the quality of the copy made it difficult to follow in places. I enclose two copies of this, as I would be very grateful indeed if you could return one copy to me with any amendments, additions, fill-ins, or comments which would suggest better ways of conveying the sense of what you wrote.

Your programme will be very much in my mind over the next few years, but I hope you will forgive my tremendous slowness in coming to terms with the vast range of ideas that you have initiated in these areas. I hope though to have much help in this matter from my colleague Tim Porter at Bangor and in collaboration with Jean-Louis Loday of Strasbourg.

There are a number of obstructions to the development of your programme, all of which present interesting and intriguing problems in their own right.

1. *The definition of a Picard n-groupoid.*

The definition of a Picard 1-groupoid is clear intuitively and has been written down in terms of a groupoid with an extra structure of tensor product $\otimes$ and duality $*$ satisfying the rules which make the groupoid a homotopy abelian group object up to coherent homotopy, with $\otimes$ as product and $*$ as inverse. This suggests a relation between the theory of homotopy everything H-spaces, developed by Boardman-Vogt [**13**] and Segal and others, and in particular should be expressed in terms of Segal's notion of a Γ-space [**119**]. I have discussed this with Vogt, but it seems that the notion of a Γ-space with involution $*$ is not in the literature. I hope that he will be giving me some more information on this in due course. If one can give an elegant description of a Γ^*-space, or Γ^*-category, or Γ^*-groupoid, then the corresponding n-fold object should be some functor from a product $\Gamma^* \times \Gamma^* \times \cdots \times \Gamma^*$ to spaces, or categories, or groupoids. Heller has some unpublished ideas on this, which again I hope to borrow.

Having obtained such a formalism, it would be necessary to compare it in detail with the examples you suggest in one of your letters to Breen.

2. *Crossed Modules.*

The notion of *crossed module* was first defined by J. H. C. Whitehead in 1946 [**137**] and is precisely the object you define as being equivalent to a Gr-category. In your last letter to Breen, you mention (p. 3 of my translation) that you have come across these in many situations, and in particular in the situation of formal groups. I would be very grateful for more information on your ideas in this direction. The notion of crossed

module was used more recently in homotopy theory by Quillen, who observed that if $F \longrightarrow E \longrightarrow B$ is a fibration, then the induced map $\pi_1 F \longrightarrow \pi_1 E$ of fundamental groups can be given the structure of a crossed module. This suggests there should be a notion of "crossed module up to homotopy" given by the action, for the above fibration, of $\Omega E \times \Omega F \longrightarrow \Omega F$, where Ω denotes the space of loops. However, this structure has not been explored at this space level, as far as I am aware, by homotopy theorists, and I hope to say more about this at a later date. The different viewpoints of a crossed module (as a double groupoid with connection, or as a 1-category in (Gr)) should give different ways of looking at this homotopy structure, and now suggest to me different ways of looking at some ideas from the theory of infinite loop spaces, and in particular perhaps obtaining some criteria for infinite loop maps.

3. *Moduli.*

A talk by Ruth Charney at a Conference on Algebraic Topology at Aarhus this month explained to me some of the background of the ideas mentioned in your letter on moduli spaces of curves and colimits of spaces of type $K(\pi, 1)$. I intend to see that I can get in this area as soon as I obtain her paper with Ronnie Lee [**45, 46**].

4. *n-cat-groups.*

These ideas of Loday [**99**] have to be part of my immediate aim, because they are linked with ideas in homotopy theory with which I am fairly familiar. Loday and I have hopes of formulating and proving a van Kampen theorem for these gadgets [**41**]. Since they are algebraic gadgets which do correspond to the truncated homotopy type, in a manner which you suggest for n-groupoids, this area is still philosophically related to those that you propose.

There are so many important ideas in this area, it seems to me, that I must put in an application to the S.E.R.C. for a postdoctoral three year research assistantship. I am not sanguine about the success of such a proposal, but I think I have to take the chance on its small possibility of success.

With all best wishes, and very many thanks again for the material you have sent me, and for the interest and encouragement, which is deeply appreciated,

Yours sincerely,

Ronnie

Lettre de Ronald Brown à Alexandre Grothendieck, 14.12.1982

14th December 1982

Dear Alexander,

First, I would like to send you the season's warm greetings.

Second, I enclose a copy of an informal translation made of your third letter to Larry Breen, in case this is of use to you or your students. Your letters are a marvellous example of enthusiasm and vision, and it will be very interesting to see how near we can get to what you have in mind.

Maybe one basic idea is that there are some really interesting non-abelian invariants in homotopy theory, not only the fundamental group, and that these non-abelian invariants need investigation in other areas where algebraic topology is applied.

The good news in this direction is that Jean-Louis Loday and I have just proved a van Kampen theorem for his 2-cat-groups, and the methods look very much as if they will generalise to n-cat-groups [**41**]. Since the latter gadgets model truncated homotopy types with trivial groups in dimensions greater than $n + 1$, we do seem to have something analogous to what you have in mind. Loday has generalised the equivalence between crossed modules and Gr-cats (= 1-cat-groups) to an equivalence between 2-cat-groups and what he called *crossed squares* [**99**, §5]. I won't go into the technical details, but one of the results is that one can compute certain third triad homotopy groups as a kind of non-abelian tensor product, where, if M and N are crossed P-modules, then $M \otimes N$ is, among other things, a crossed P-module, and the usual biadditivity properties of the tensor product are replaced by analogous properties of commutators. One of the interests for me in this kind of result, is maybe not that it seems by itself so significant, but that it is an example of a non-abelian result for which it is difficult to imagine any other method of proof.

In view of the small resources at Bangor, I am trying to get additional staff here, and for your information, I enclose a copy of a research proposal to the UKSERC, for the development of this programme on "non-commutative homological algebra". At the moment, it seems to me we have lots of questions and speculations, but also some solid ground to show that there are some new results in homotopy theory, and some new constructions in the corresponding algebra. I suspect that the development of these techniques in homotopy theory will occupy the major portion of my time, but if we get a Research Assistant, and one with the right background, then development in the area of toposes could be contemplated. At present, though, even the clear van Kampen theorem for crossed modules has not been developed in the context of

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 $\mathbf{\Pi}_n(X)$ of $X = \int_I X_i$ is canonically n-equivalent to $(n+1)$-$\varinjlim_I \mathbf{\Pi}_n(X_i)$. 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 $\mathbf{\Pi}_n$ and of $(n+1)$-$\varinjlim$ are available), I view the statement as more or less tautological when $\mathbf{\Pi}_1$ 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 $\qquad\longleftrightarrow\qquad$ 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 undispensable "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 Sociéte Mathématique de France. His main accent, if I remember well, was not on topology though, but on coherence properties for $\otimes$ and $\underline{\text{Hom}}$ 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 $\mathrm{H}_3 G$ 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 $(M, W \subset \mathrm{Fl}(M))$ 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 unformal 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 it 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* $B \longrightarrow$ (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 $\mathbf{\Delta}$ or $\mathbf{\square}$ 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 modelizing 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

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[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

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

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

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

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[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

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

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 $\mathrm{Ext}^2(H_i, H_{i+1})$'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 $L_i = \mathrm{Kern}\, s_i$ 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.

<div align="center">With very best wishes,</div>

<div align="center">R. Brown</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 02.05.1983

<div align="right">Les Aumettes, 2.5.1983</div>

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

<div align="center">Alexander</div>

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

<div align="right">7/May 1983</div>

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

$$ G \; \overset{s}{\underset{b}{\rightrightarrows}} \; P \; \overset{i}{\longrightarrow} \; G \qquad\qquad \begin{aligned} s &= \text{source} \\ b &= \text{but} \end{aligned} $$

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

(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

$$\mu(^p m) \,=\, p(\mu m)p^{-1}, \quad mm_1 m^{-1} \,=\, {}^{\mu m}m_1, \qquad m, m_1 \in M, \ p \in P \,.$$

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 $F \longrightarrow E \longrightarrow B$ is a fibration of pointed spaces, then $\pi_1 F \longrightarrow \pi_1 E$ 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 $\pi_2(X, Y) \longrightarrow \pi_1 Y$ 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

$$\text{groups } G \qquad \sim \qquad \text{pointed aspherical con-}\\ \text{nected spaces } K(G,1)$$

and we have a

$$\text{van Kampen} \quad \sim \quad \text{gluing} \quad \text{(integrating)}\\ \text{theorem} \qquad \qquad \text{homotopy types.}$$

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

$$\text{transitive action} \quad \sim \quad \text{subgroup.}$$

Mackey decided to develop a correspondence

$$\text{ergodic actions} \quad \sim \quad \text{“virtual” subgroup.}$$

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 $(g,s) : s \longrightarrow s^g$ 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 $\mu : M \longrightarrow P$ 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

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

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 $\mu : M \longrightarrow P$ 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 $P \times M \longrightarrow M$ 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

crossed modules	$\sim$	pointed, connected homotopy types with $\pi_i X = 0$ for $i > 2$

and we have a

van Kampen type theorem (due to Brown-Higgins [**29, 32**])	$\sim$	integrating homotopy types.

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

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

(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* $\theta : H \longrightarrow FX$, $^u p \mapsto uru^{-1}$ (*i.e.* the second relation for a crossed module is not satisfied). Factorising by the Peiffer relation

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

gives a crossed module $\partial : C \longrightarrow FX$, 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

$$\text{``groupoid objects} \quad \sim \quad \text{crossed modules''}$$

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, $i \in \{1, \ldots, n\}$, and morphisms

$$G \ \mathrel{\substack{s_i \\ \longrightarrow \\ \longrightarrow \\ b_i}} \ P_i$$

such that

(i) $s_i | P_i = b_i | P_i = 1_{P_i}$,

(ii) $[\mathrm{Ker}\, s_i, \mathrm{Ker}\, b_i] = 1$,

(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 $h : M \times N \longrightarrow P$ 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 $L \longrightarrow N$ is in some sense a "crossed module over the crossed module $M \longrightarrow P$ ". 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,

$$
G = \begin{pmatrix} L & M \\ M & P \end{pmatrix}_{\rtimes}.
$$

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 $\pi_3(\mathrm{S}^2) = \mathbf{Z}$ (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

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

where ${}^x(m \otimes n) = {}^xm \otimes {}^xn$. 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
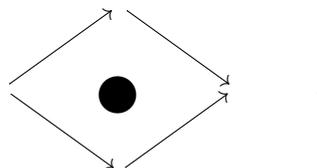
again conveys a non-commutative association.)

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

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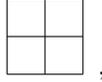
but apparently never got very far, at least he told me nothing more.)

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 $p : E \longrightarrow B$ 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

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

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 $\mathbf{R} \longrightarrow \mathbf{R}^{\mathbf{Z} \times \mathbf{Z}}$, $t \longrightarrow f_t$, is continuous (its adjoint $\mathbf{R} \times \mathbf{Z} \times \mathbf{Z} \longrightarrow \mathbf{R}$ is $(t, m, n) \mapsto m + nt$). In a topos, the function $X^Y \longrightarrow \mathcal{P}(X)$, $f \mapsto$ 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 $\mathcal{C}^\infty$. 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 $\mathbf{R} \longrightarrow \mathbf{R}^2$.

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 $K_1 \longrightarrow X$ with kernel K_2. Now resolve the *map $K_2 \longrightarrow K_1$* 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 Balliot 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.

<div style="text-align:center">Sincerely yours,</div>

<div style="text-align:center">Ronnie</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 10.05.1983

<div style="text-align:right">Les Aumettes, 10.5.1983</div>

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. $X/Y \to e$ 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 $X \to Y$ 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

<div align="center">Alexander</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 23.05.1983

<div align="right">Montpellier, 23/5/83</div>

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

$$(X_i) \quad \longrightarrow \quad (X_i')$$

with some indexing category I, if this is componentwise a weak equivalence $X_i \to X_i'$, then the induced $\int X_i \to \int X_i'$ 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 $I = \bullet \overset{\bullet}{\underset{\bullet}{<}}$, namely getting the substitute for fiber products in (Hot), including notably the well-known operation of taking the "homotopy fiber" of a map $X \to Y$, 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:

<div align="center">

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

</div>

With my best wishes

<div align="center">

Alexander

</div>

Lettre de Ronald Brown à Alexandre Grothendieck, 25.05.1983

<div align="right">25/5/1983</div>

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 $i_* : \pi_1 Y \longrightarrow \pi_1 Y^+$ 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 $i : Y \longrightarrow Y^+$ is not a weak equivalence if $\pi \neq 1$.

(ii) There are CW-complexes X, Y and maps $f : X \longrightarrow Y$ 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

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

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.

<div style="text-align:center">With very best wishes, and with great interest in your notes,</div>

<div style="text-align:center">Ronnie</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 14.06.1983

<div style="text-align:right">Montpellier, June 14, 1983</div>

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 $Y \to Z$ 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 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 $\mathrm{Gal}(\overline{\mathbf{Q}}/\mathbf{Q})$ 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(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

<div align="center">Alexander</div>

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*

$$
\begin{array}{rcl}
e \;\; : \;\; \mathcal{C}(Z \times Y, X) & \to & \mathcal{C}(Z, \mathcal{C}(Y, X)) \\
f & \mapsto & (z \mapsto (y \mapsto f(z, y)))
\end{array}
$$

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 $Z \times_{\mathrm{W}} Y$ of topological spaces, where $Z \times_{\mathrm{W}} Y = \mathsf{k}(Z \times Y)$, 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 $\mathcal{C}(Z \times_{\mathrm{S}} Y, X) \simeq \mathcal{C}(Z, \mathcal{C}(Y, X))$, where $Z \times_{\mathrm{S}} Y$

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 $\mathsf{k}(X)$, which means take the final topology with respect to all maps of compact Hausdorff spaces $C \longrightarrow X$ (one can show a *set* of such maps is aufficient to define $\mathsf{k}(X)$). So one *can* work in Top (the usual topological category), but for algebraic topology, k-Top is more useful, since the exponential law in k-Top implies, for example, that if f, g are quotient maps, so also is $f \times g$ (the categorical product in k-Top). In modern parlance, k-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 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 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

$$
\begin{array}{ccc}
Y & & X \\
\;_{q}\searrow & & \swarrow_{p}\; \\
 & B &
\end{array}
$$

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

$$\mathrm{Top}_B(Z \times_B Y, X) \;\simeq\; \mathrm{Top}_B(Z, (Y\;X))$$

(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 $(Y\;X)$ should be the disjoint union of $\mathrm{Top}(Y_b, X_b)$ for all $b \in B$, where $Y_b = q^{-1}(b)$ and $X_b = p^{-1}(b)$. What does it mean for a map $f : Y_b \longrightarrow X_b$ to be "near" a map $f' : Y_{b'} \longrightarrow X_{b'}$? Our first idea was that such an f is a "partial map" $f : X \longrightarrow Y$ (*i.e.* $\mathrm{D}_f \subset Y$) and that the compact-open topology extends to partial maps since $f(C) = f(C \cap \mathrm{D}_f)$ 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

$$\mathrm{P}\mathcal{C}(Y, X) \quad \xrightarrow{\sim} \quad \mathrm{Top}(Y, X^\wedge)$$
$$f \quad \mapsto \quad \hat{f},$$

where $\mathrm{P}\mathcal{C}(Y, X) = $ partial maps $Y \longrightarrow X$ with closed domain, and

$$\hat{f}(y) = \left\{ \begin{array}{ll} f(y) & \text{if } y \in \mathrm{D}_f \\ \omega & \text{if } y \notin \mathrm{D}_f \end{array} \right. .$$

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

$$\begin{array}{lll} \mathrm{P}\mathcal{C}(Z \times Y, X) & = & \mathrm{Top}(Z \times Y, X^\wedge) \\ & = & \mathrm{Top}(Z, \mathrm{Top}(Y, X^\wedge)) \qquad \text{for suitable } Y \\ & = & \mathrm{Top}(Z, \mathrm{P}\mathcal{C}(Y, X)), \end{array}$$

which is the exponential law for partial maps.

Now we go back to Top_B. The elements of $(Y\ X)$, if $Y \longrightarrow B$ and $X \longrightarrow B$ 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 $(Y\ X)$ the initial topology with respect to the two maps

$$(Y\ X) \longrightarrow \mathrm{P}\mathcal{C}(Y, X)$$
$$\downarrow$$
$$B$$

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

What one has found is that k-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

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[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 $(Y \ X) \longrightarrow B$ 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 $\pi_1 X_0 \longrightarrow \pi_1 X_1$ and $\pi_1 X_0 \longrightarrow \pi_1 X_2$ are *injective.* The proof uses the van Kampen theorem to describe $\pi_1 X$; some combinatorial group theory to prove that $\pi_1 X_1 \longrightarrow \pi_1 X$ and $\pi_1 X_2 \longrightarrow \pi_1 X$ 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 $\pi_1 X_0 \longrightarrow \pi_1 X_1$ and $\pi_1 X_0 \longrightarrow \pi_1 X_2$ is dropped. The Brown-Higgins theorem [**27**] gives information when these two maps are *surjective*; the conclusion is that

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

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

$$\begin{CD} C_2 @>\delta>> C_1 \\ @VVV @VVV \\ C_0 @>=>1> C_0 \end{CD}$$

of groupoids with objects C_0 and over 1_{C_0} such that C_2 is a family $C_2(p)$, $p \in C_0$, of groups and C_1 operates on C_2 so that if $a \in C_2(p)$, $x \in C_1(p, q)$ then $a^x \in C_2(q)$.

The usual rules are to hold

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

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

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

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 $\underline{U}^\lambda$ 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 $\pi_0 X_0 \longrightarrow \pi_0 X_1$, $\pi_0 X_0 \longrightarrow \pi_0 X_2$ 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 $X_1 \longrightarrow X_2$ is 0-connected, or each path $(I, 0, 1) \longrightarrow (X_2, X_1, p)$ is deformable into X_1, or $\pi_1(X_1, p) \longrightarrow \pi_1(X_2, p)$ is surjective). For $\nu = (\lambda_1, \ldots, \lambda_n) \in \Lambda^n$ let $U^\nu = U^{\lambda_1} \cap \cdots \cap U^{\lambda_n}$ and $U_i^\nu = U^\nu \cap X_i$, so we have a triple $\underline{U}^\nu$. Form the diagram of crossed modules over groupoids

$$(*) \qquad \bigsqcup_{\nu \in \Lambda^2} \pi\underline{U}^\nu \; \overset{a}{\underset{b}{\rightrightarrows}} \; \bigsqcup_{\lambda \in \Lambda} \pi\underline{U}^\lambda \; \overset{c}{\longrightarrow} \; \pi\underline{X}$$

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

Theorem. *If all triples $\underline{U}^\nu$ for $\nu \in \Lambda^4$ 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

$$\boxed{X_2}\,p \qquad \text{with addition} \qquad \boxed{\ \ \big|\ \ }$$
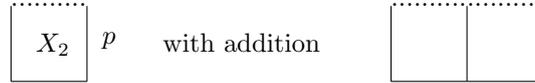
while $(\rho\underline{X})_2$ consists of homotopy classes of maps

$$
\begin{array}{ccc}
p & X_1 & p \\
X_1 & \boxed{X_2} & X_1 \\
p & X_1 & p
\end{array}
\qquad
\begin{array}{c}
(I^2, \dot{I}^2, \ddot{I}^2) \longrightarrow (X_2, X_1, X_0) \\
\text{homotopy rel } \ddot{I}^2
\end{array}
$$

which is more symmetric and more suitable for subdivision

$$\text{[grid diagram]}\ .$$
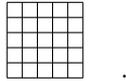
That is, in $\rho\underline{X}$ you can form multiple compositions $g = [g_{ij}]$. This is one of the key ideas which makes the proof work. The equivalence between crossed modules over groupoids and double groupoids takes $\pi\underline{X}$ to $\rho\underline{X}$, so you can work with either. (See my 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* $\pi\underline{U}^1 \circ \pi\underline{U}^2$ 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. White-head 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 $a^{-1} a_1^{-1} a a_1^{\mu a} = 1$ 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 $\pi_2(X, Y) \overset{\partial}{\longrightarrow} \pi_1 Y$ 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 $\mathrm{K}(\pi, 1)$ necessarily a $\mathrm{K}(\pi', 1)$?, 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 $\mathrm{H}_2 G$ (G a group). Consider again the pushout diagram

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

The Mayer-Vietoris sequence gives of course

$$
\cdots \longrightarrow \mathrm{H}_n(P) \longrightarrow \mathrm{H}_n(Q) \oplus \mathrm{H}_n(R) \longrightarrow \mathrm{H}_n(X) \longrightarrow \mathrm{H}_{n-1}(P) \longrightarrow \dots \ .
$$

The problem is to identify $\mathrm{H}_n(X)$ 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, $X = \mathrm{K}(Q *_P R, 1)$, 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 $\mathrm{H}_2(X) = \pi_2(X) = (M \cap N)/[M, N]$. This gives us

$$
\mathrm{H}_2(P) \longrightarrow \mathrm{H}_2(Q) \oplus \mathrm{H}_2(R) \longrightarrow \frac{M \cap N}{[M, N]} \longrightarrow \mathrm{H}_1(P) \longrightarrow \mathrm{H}_1(Q) \oplus \mathrm{H}_1(R) \longrightarrow 0
$$

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

$$
\mathrm{H}_2(P) \longrightarrow \mathrm{H}_2(R) \longrightarrow \frac{N}{[P, N]} \longrightarrow \mathrm{H}_1(P) \longrightarrow \mathrm{H}_1(R) \longrightarrow 0
$$

---

[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 $\mathrm{H}_2(P) = 0$ and

$$
\begin{aligned}
\mathrm{H}_2(R) &= \mathrm{Ker}\left(\frac{N}{[P,N]} \longrightarrow \frac{P}{[P,P]}\right) \\
&= \frac{N \cap [P,P]}{[P,N]} \ ,
\end{aligned}
$$

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

$$
\mathrm{H}_3 R \ = \ \mathrm{Ker}(N \overset{P}{\wedge} P \longrightarrow N) \ ,
$$

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 $\pi_1(E \times_B E) \overset{\longrightarrow}{\underset{\longleftarrow}{\longrightarrow}} \pi_1 E$ 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

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

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 testbed 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

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

functor

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

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

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

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 toplogists 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

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[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

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

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 ($\alpha \in \{0, \ldots, 4\}$) are connected.

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

The aim is to obtain and prove a coequaliser diagram

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

of Cat^2-groups, assuming all finite intersections $\underline{U}^\nu$, $\nu \in \Lambda^n$, 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 beasties). 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 $\mathrm{F}X$ on X and the free crossed $\mathrm{F}X$-module $\mathrm{C}R$ on the relators R (cf. Brown-Huebschmann [**35**]) giving a sequence

$$\mathrm{C}R \xrightarrow{\partial} \mathrm{F}X \longrightarrow G$$

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

Now $\mathrm{C}R$ itself fits in an exact sequence

$$1 \longrightarrow P \longrightarrow \mathrm{F}Y \longrightarrow \mathrm{C}R \longrightarrow 1 \,,$$

where $Y = \mathrm{F}X \times R$, and P is the "Peiffer group" (*ibid*). One can obtain a diagram

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

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

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

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 Cat^n-group language is preferable, and there should be a formal description of a "Cat^n-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 $\pi_3(\mathrm{S}^2) = \mathbf{Z}$ from van Kampen for Cat^2-groups in fact goes via crossed squares and the $\overset{P}{\otimes}$-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

(†)

$$
\begin{array}{ccccc}
F & \longrightarrow & Z & \longrightarrow & Z' \\
\downarrow & & \downarrow & & \downarrow \\
Y & \longrightarrow & C & \longrightarrow & B \\
\downarrow & & \downarrow & {\scriptstyle *} & \downarrow \\
Y' & \longrightarrow & A & \longrightarrow & X
\end{array}
$$

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

$$
\begin{array}{ccccc}
 & & \bullet & & \\
 & & \downarrow & & \\
\bullet & \longrightarrow & \bullet & \longrightarrow & \bullet \\
 & & \downarrow & & \\
 & & \bullet & &
\end{array}
$$
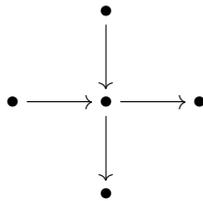
of the diagram (†) is

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

we get exact sequences

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

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

$$
0 \;\longrightarrow\; \pi_3 X \;\longrightarrow\; N \overset{P}{\otimes} M \;\longrightarrow\; M \;\longrightarrow\; M/[N,M] \;\longrightarrow\; 1 \,,
$$

$$
0 \;\longrightarrow\; \pi_2 X \;\longrightarrow\; M/[N,M] \;\longrightarrow\; P/N \;\longrightarrow\; \pi_1 X \;\longrightarrow\; 1 \,,
$$

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"?

<div align="center">Yours affectionately,</div>

<div align="center">Ronnie</div>

Lettre de Ronald Brown à Alexandre Grothendieck, 15.08.1983

<div align="right">15th August, 1983</div>

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.

<div align="center">Yours very cordially,</div>

<div align="center">R. Brown</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 06.09.1983

<div align="right">6.9.1983</div>

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 $E \longrightarrow B$ 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 $\mathbf{\Delta}$? Maybe I should do a little checking though, as it is the same "argument" of

idleness which made me admit that of course the Kan-Dold-Puppe theorem couldn't fail to be true in the cubical case. Still the question of understanding the exact realm of validity of Kan-Dold-Puppe remains just as intriguing, and maybe even more so, as the need of introducing extra structure ("connections", as you call them in you letter) in the cubical case, gives the idea than an answer may turn out subtler than expected.

Excuse me, I overlooked in my first reading of your long letter the practical question of writing a letter of support for your research program proposals. I am not definite about wishing to keep out of it, if you have the feeling it may help, rather than make the referees more moody still! It is all too evident I am not an expert on homotopy theory, and the books I am bold enough to write now on foundational matters are very likely to be looked at as "rubbish", too, by most experts, unless I show up with $\pi_{147}(S_{23})$ 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!

<div style="text-align:center">Very affectionately</div>

<div style="text-align:center">Alexander</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 08.09.1983

<div style="text-align:right">8.9.1983</div>

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 $\mathbf{P}^1_S$ 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 $\mathbf{P}^1_{\mathbf{Z}}$ 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 re-
markable 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 becom-
ing 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 geo-
metric motivations behind the algebraic constructions, and in your preprint you try
to suggest in unformal 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 $1 \longrightarrow N \longrightarrow F \longrightarrow G \longrightarrow 1$, 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 $\theta : H \longrightarrow F$, $(r, u) \mapsto u^{-1}ru$. 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

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

If we set $C = H/P$, we obtain a crossed module $\partial : C \longrightarrow F$ 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
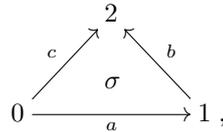
then we normally think of the boundary of σ as simply $c^{-1}ab$ (if you choose 2 as a base point and choose an appropriate orientation). But that is the folded expression, when you release 0 and 1 and allow them to vary. The correct expression for the boundary of σ is simply the diagram given above.

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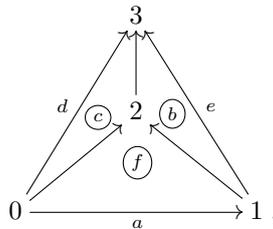
in which f corresponds to the face $(0, 1, 3)$, and then working out that the following composition is trivial.

$$e^{-1}bff^{-1}b^{-1}a^{-1}cff^{-1}c^{-1}dd^{-1}ae$$

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.

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

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!

<div align="center">Yours affectionately</div>

<div align="center">Alexander</div>

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 $M^\wedge \longrightarrow$ (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.

<div align="center">

Yours very affectionately

Alexander

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Lettre de Ronald Brown à Alexandre Grothendieck, 07.10.1983

<div align="right">

Bangor

</div>

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 is 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

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

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

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

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 $\mathrm{K}X$, 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 $\mathrm{S}X$ should be phrased as: if $\mathcal{F}$ is a family of subsets of X whose interiors cover X, and $\mathrm{S}_{\mathcal{F}}X$ denotes the singular $\mathcal{F}$-complex, of simplices $\sigma : \Delta^n \longrightarrow X$ such that

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

$$|\mathrm{S}_{\mathcal{F}}X| \longrightarrow |\mathrm{S}X|$$

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

I have thought of trying to characterise $\mathrm{S}X$ as a "weak T-complex". It has "thin" elements, intuitively those which factor through some map

$$\Delta^n \xrightarrow{\hspace{3cm}} X \quad ,$$

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 $\mathrm{Cov}(X)$ of open covers of X. One then computes $\mathrm{H}_* K_\lambda$ and $\varprojlim \mathrm{H}_* K_\lambda$. Alternatively, one can compute $\mathrm{C}_* K_\lambda$ (the chains of K_λ), $\varprojlim \mathrm{C}_* K_\lambda$ and $\check{\mathrm{H}}_* \varprojlim \mathrm{C}_* K_\lambda$. 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 $\mathrm{holim}\, \pi K_\lambda$. 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 to 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 socalled "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.

<div align="center">

Yours affectionately,

Ronnie

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Lettre de Ronald Brown à Alexandre Grothendieck, 27.11.1983 [19]

<div align="right">

U.C.N.W.
27 Nov 1983

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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

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[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

$$G \; \underset{i}{\overset{s,\,b}{\rightleftarrows}} \; N \qquad \text{with } si = 1_N = bi \text{ (so that } N \text{ can be considered as a subgroup of } G\text{)} ,$$

and such that $[\mathrm{Ker}\, s, \mathrm{Ker}\, b] = 1$. 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 $[\mathrm{Ker}\, s, \mathrm{Ker}\, b] = 1$ 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 $G' \; \overset{s',\,b'}{\underset{\leftarrow}{\rightrightarrows}} \; N'$, a pre-$\mathrm{Cat}^1$-group. Then factor G' by $[\mathrm{Ker}\, s', \mathrm{Ker}\, b']$ 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 $X \simeq \mathrm{B}G$ 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.

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[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!

<div style="text-align:center">

Very affectionately yours

Alexander

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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

$\varprojlim \mathrm{C}_*(K_\lambda)$ (rather than $\varprojlim \mathrm{H}_*(K_\lambda)$ or $\operatorname*{holim} \mathrm{C}_*(K_\lambda)$) seems to me nonsense. Instinct tells me (without having worked out anything) that $\operatorname*{holim} \mathrm{C}_*(K_\lambda)$ (or its homology) is the better choice, not $\varprojlim \mathrm{H}_*(K_\lambda)$. Another possibility of course would be to take $\operatorname*{holim}(K_\lambda)$, 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 $\pi_i(\operatorname*{holim}(K_\lambda))$, 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 *i*th *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 $\underline{\pi}_i(X_*, \sigma)$, which for $i \geq 1$ are sheaves of groups on S, with $\underline{\pi}_1$ 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

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

where the first member is the fiber of $\underline{\pi}_i$ 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, $\underline{\pi}_i(X/S,\sigma)$ – 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

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[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.

<div align="center">Yours very affectionately</div>

<div align="center">Alexander</div>

Lettre de Ronald Brown à Alexandre Grothendieck, 12.12.1983

<div align="right">12th December, 1983</div>

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 Flyn.

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

overwhelmed indeed, and now in reply scarcely know what to begin with for thanking and acknowledging for everything. I started reading "Mister God, this is Anna" the very day I got it – it has been many years now since I didn't read any book, as I always had more fascinating things to do. Found your big envelope in the mailbox last Sunday, January first, after a night spent till morning looking up nice geometric things in connection with systems of pseudolines. Reading the book a few hours in a row, standing by the sun-lit window facing the vineyards where I'd happened to open it first, was a most ominous way to start with the new year. The book strikes as something strange and beautiful – the strangeness, I believe, comes from its simplicity, which is so unusual. I had that same feeling a few times when reading books by Melville – someone looking at things with fresh eyes, not through any kind of glasses. In the case of Fynn's book, by the way, it seems hard to believe it is fiction, not just a candid account of something that happened. Is he someone known as an "author"? Now I know there is a little treasure on my desk ready for me to open it, wonder when I'll go on reading some more – namely tear myself from what I'm *doing*. We'll see . . .

As for all the valuable mathematical stuff you've kept sending me for over a year now, I feel a little ashamed that (for the time being at any rate) only such an infinitesimal portion of it gets to destination, is actually being used as material for a vision of things, for an understanding. To take just one example, I spent barely half an hour, or maybe one, reading through your nice informal report on knot theory, which I was and am wholly ignorant about – reading just enough to make me realize once again how many beautiful things have been done (and surely are being done still), which I could easily have become excited about myself and invested myself in, which I'll ever remain ignorant about. This is all the more so with my "do it yourself" hangup, which makes it sometimes hard for me to just receive information on this or that and keep it in mind, instead of sitting down on it days and weeks or more to dig through it my own way. Still I try my best, to get at least an approximate idea what you and Tim are sending me is about. Also the papers on mapping class groups by Steve Humphries [**86**] I have handed over to Yves Ladegaillerie [23], and presumably we'll make the junction later with the approach we are following at present, with such a strong motivation coming from "absolute" algebraic geometry . . . I've had a look, too, on your joint paper(s) on groups generated by transvections [**36, 37**], which looks like good stuff – but again, for the time being it doesn't trigger anything like "that's just the thing I've been lacking for doing this or that" . . .

By the way, I haven't written any more notes for nearly two months, having been involved in a lot of scratchwork – and for the last month, on systems of pseudo-lines, which has precious little to do with homotopy theory and the like! Maybe though I'll

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[23] Who read them!

slip in one section on these ponderings, which the uninterested reader may skip if he wants. (But maybe even the assumption that there remains a reader up to page 600 is a very bold one . . .)

Thank you a lot, too, busy as you are, to have taken the trouble of writing up a few comments on my notes (which I'll use when preparing a final typescript of volume 1), and even typing an alternative version of my presentation of Réflexions Mathématiques. Tim did so, too – and I think I'll rather stick to Tim's version, closer to the original and to the way I actually feel or sense the things I want to say. Thus "streams" (of thought, *etc.*) do not at all have the same connotations in my feeling as "seas" or "oceans" – they come from somewhere to go somewhere, quite in contrast to seas (that's actually where they go to ultimately!). That in strictly terrestrial geography, continents appear as surrounded by seas, rather than as confluences of streams, doesn't disturb me that much. Also the notion of a "leading thread" is by no means a literary metaphor, which could be replaced by a non-metaphorical word like "motivation", "conception" or the like, but a very strong reality: there is that thread and I am very careful to keep tight to it and never let it slip off my hands altogether. Sorry!

[...]

I'm sure I didn't reply to everything yet, but I better stop now to let this letter get off. Please, when you see Tim Porter, tell him I hope to answer soon to his painstaking letters, for the time being it seemed more urgent to answer you.

Yours very affectionately

Alexander

Lettre d'Alexandre Grothendieck à Ronald Brown, 15.01.1984

Les Aumettes Jan 15, 1984

Dear Ronnie,

Among the many points of your last long letter I didn't yet reply to, there is the practical one which I should have answered at once, please forgive my thoughtlessness. It would be nice indeed if you could drop by some time this year. I am not sure there is much point in a "formal invitation" though, as (a) presumably there is no one here at Montpellier interested in the kind of thing you may feel like talking about, except me, and (b) the Maths Department here has been acutely broke for years, and I doubt there is any money for inviting speakers from outside. At any rate, I don't feel the University here is the most congenial place for meeting, I am living about 100 miles

off and would be glad to welcome you at my place rather. You won't have any extra expenses, except some extra train fare, for which maybe it is not worthwhile making a fuss with SERC or whom not, except you make it a matter of principle. Point (a) isn't too serious I confess, after all you have a wide range of chords on your violin, not just hyperhard homotopy stuff – for instance, I can well imagine a beautiful talk of yours on knots or groupoids making everybody here quite happy – so if you really like giving a talk here, it could indeed make sense, if you choose your topic not too technical; and pushing hard enough maybe I could even squeeze out some money, as I never so far have caused such kind of expenditure. It's mainly that I am prejudiced and like my home a lot better than the University!

Looking forward to hearing from you

Yours very affectionately

<div align="center">Alexander</div>

Lettre de Ronald Brown à Alexandre Grothendieck, 06.02.1984

<div align="right">6/2/84</div>

Dear Alexander,

I was going to write when I heard from you, but think I should write it anyway.

First, I am delighted that "Mister God, this is Anna" appealed to you, as I thought it would. I know nothing more about the book and the author, then what is there. But I guess Fynn is what it says there, and very far from a professional writer. It reads to me a book of an experience. I took it down again tonight. Reading the first few chapters brought the tears to my eyes.

[...]

It has been very interesting working with French mathematicians. I was discussing this with Loday, and he was commenting on the French tradition of finding out what is really going on, of abstracting the essential point. By contrast, I asked an eminent British mathematician how he justified a point, and he said: "You just do a calculation." I had done this calculation and verified the point, and really wanted something more, something which to my colleague seemed a chimera. It seems I should have made more contacts across the water years ago!

Have you thought of including in your volumes your letters to Breen (or at least the first two and the relevant part of the third)? They do seem to me to give a good overall view of what you are seeking.

It is still early to decide on the significance of the Brown-Loday stuff [**41, 40**]. Certainly, it gives a new (and I think, surprising) twist to the van Kampen theorem story. It does give some new algebraic material on which to work. It does compute (in terms of generators and relations) some previously uncomputable groups. Since these groups are heavily involved with the failure of homotopy groups to satisfy excision, this seems a step in the right direction.

If one assumes that cat^n-groups model truncated homotopy types of CW-complexes, then it is presumably not unreasonable that they should be valuable in wider applications.

The whole theory of n-ads attracted attention in the early 1950s, but one reason for its losing interest must have been the problem of computation, as well as the value of other methods.

What I don't think I have done in my work so far is answered some question which people have been asking for some time. What I have done is made new contributions in areas where people thought they knew all the answers, but in fact hadn't asked the right question! So probably I'm best at nagging people with questions and having colleagues around who can help me find the answers. It is certainly amusing that lots of eminent people had lectured on function spaces and the compact-open topology, without looking at the question of changing the topology on $X \times Y$ by considering a subcategory of Top (the compactly generated spaces).

I am sorry to have sprung the idea of a visit without more careful explanation. There is money available from SERC for short visits to discuss collaborative projects, but it is not clear whether this is the right way to proceed, and in which you wish to, or I could reasonably, go. I am still not in as good a position as Tim to comment on your manuscript. My ideas of how to marry the two viewpoints are pretty vague.

What might be reasonable would be for me to fix sometime a visit to Pradines, to include also a visit to you and to Montpellier, if there are people there who would like a talk (on any of a variety of topics or levels). I would also like to meet Molino, for instance. I could for instance hire a car at Toulouse and make a round trip.

British Council are willing to consider financing such a trip, which would need for their consideration a formal invitation to discuss matters of mutual interest. I could for instance visit you at Les Aumettes and go on to Montpellier, or whatever you thought reasonable. The timing of such a visit is also not clear, as I am not sure of Pradines' plans. It may be in the end that it should be left to early July, say.

The arrangement with Loday went very well, as his style is for all day conversations at the blackboard, and also our two approaches dovetailed neatly. By contrast, the project with Pradines is tantalisingly sluggish, and clearly lacking some key concepts which would get it going. How does one go beyond the simple feeling that double

groupoids (and such like gadgets) are relevant to 2-dimensional phenomena in differential topology? Where would the Poincaré groupoid be if time were 2-dimensional? (That is a question of Atiyah.)

I get the impression that it is at the level of such intuitions that you and I are in clear agreement that there is work of substance to be done. I might be able in conversation to put over some clearer ideas which would provide a link-up, or at least put over the essentials of what I've done with colleagues to see if it suggests something to you. The financial side is not a worry anyway as this year the department here is in good financial health for research funding. However, if I can get support I would always like to do so.

Lettre d'Alexandre Grothendieck à Ronald Brown, 18.02.1984

Les Aumettes Feb 18, 1984

Dear Ronnie,

Thanks a lot for your lively letter and the note with Loday on obstruction to excision [**39**]. For one month or two now homotopy reprints are again piling up on my desk while I am busy otherwise, I hope within the next one month or two to find the leisure for finding out what they are about!

[...] It has been my habit all my life to be outspoken about what I think of someone, [...]. [There is] a difficulty you have, I have felt a few times, pinpointing exactly what you should demand or expect from a notion you are guessing after and still in the mist (and even after it got out of the mist partly). It may be something "psychological" and pretty deep, which seems to keep you at time (as was my impression at any rate) from pulling something out of the shadows and twilights right into the most brilliant sunlight! I am aware, however, that the difficulty in mathematical communication between us comes mainly from me, and I have the same difficulty with many others – a difficulty in grasping and assimilating ideas at a moment when I have no direct use for them and when they do not directly respond to some former experience of mine. This is a kind of inertia in me well known by my students, who fortunately have all been quite patient with me in this respect! This is the reason also why I feel (maybe I hadn't made this really clear before) that at present time is not ripe yet, as far as my own needs are concerned, for a meeting with you for mathematical discussion. When I welcomed the opportunity of a meeting, this was mainly as an occasion for getting better acquainted with each other. Of course, there is nothing really urgent for this, and we may postpone this for a moment when I am more ready than now to benefit from your mathematical insights.

As a matter of fact, I haven't worked on the mathematical notes for well over three months now, but am about to resume work on them, and hope to get a final typescript ready for the printer, of volume 1, within the next two months. For the last two weeks I have been involved in writing the "personal part" of the Introduction, in French. This for me is by far the most important thing of Pursuing Stacks. Maybe this reflection, whether published or not, is one main meaning of my resuming "public" mathematical work (for how many years I am wholly unable to foretell ...). It just occurred to me that when I am through with this reflection and testimonial of my life as a mathematician among mathematicians, I'll have it retyped separately and a hundred or so copies made, to send to those former or present friends and colleagues of mine, with whom I had or have closest contact. I'll then send you a copy in due course, which you are welcome to communicate to whom you like. However, unlike the bulk of the notes, which you were so kind to duplicate and circulate among a number of mathematicians whom you thought were interested (and a few were indeed!), please consider this introduction as somewhat confidential for the time being. According to the echoes I'll get, I may still drop from the typescript, before giving it to the printer, some too strongly personal references where other people are involved and named.

Your circulating these notes of mine has proved more useful than I would have expected. I got informed about active response to the notes in three places: two seminars on the notes, one with Baues in Bonn, and another with Bénabou in Strasbourg, which of course you know about; and a phone call from Joyal (by the way, do you know which university he is teaching in at present, where I could write him?) involved in work closely related to mine. The same could of course be said of your work, but the ties remain unclarified and emphasis and direction seem rather different.

To come back to the practical matter of your planned visit, let's agree, Ronnie, that you decide what suits you best. My schedules are flexible enough for arranging a talk, *etc.* practically at any moment, provided you inform me sufficiently in advance.

Please excuse my typing – it really is a lot more expeditive, and I am in a hurry to get back to this Introduction, sorry!

Yours very affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 06.03.1984

6/3/84

Dear Alexander,

I have a lot I want to say in reply to your letter of February 18, which I found deeply interesting, and to thank you very much indeed for the typed copies of your 2 "Esquisses ..." [**82**], which are greatly appreciated by Tim and me and will be (are being) carefully studied. In fact, I have just got back from a "Peripatic Seminar on Category Theory, Sheaves and Logic" run by Bénabou in Paris over the weekend. So if I don't stop now, you will not get the information that A. Joyal is at Columbia University, New York, at least this session. This explains how he, Tierney and Alex Heller are running a seminar. I had a long chat with Alex, who was in Paris for this Bénabou meeting, but will not attempt to convey what he said, since unlike Tim, I am not an expert in that area of model categories.

Could you send for Tim and me copies of the stuff by Ladegaillerie and by Malgoire and Voisin? I will also send separately another paper by Steve Humphries on curves on surfaces which at least is in the same area, I guess, as "cartes". It now seems there is a relation between the joint work with Steve and work on monodromy (one of your themes!) of singularities. W. Ebeling at Bonn has just sent me papers on this. The reason seems to be that groups generated by transvections occur widely. My criticism of the Brown-Humphries papers is that this link with, say, Dynkin diagrams, should be made clearer, even if it is not explicitly needed; but I will await a referee's report before taking further action.

[...] I will also try and get more out of your comment on my "difficulty" since any criticism which can help me to clarify ideas and notions is appreciated. It is not easy to give a more detailed and fair analysis in a way which would ensure improvement, so I will leave that to another day! What I very much respond to is someone who says "Yea", or "Nay", and, in the case of "Nay", is truly helpful!

More later,

Yours very affectionately,

Ronnie

Lettre de Ronald Brown à Alexandre Grothendieck, 27.03.1984

Strasbourg, 27 March 1984

Dear Alexander,

Here I am back again in Strasbourg, after visiting Paris to give a talk to (and attend) the Paris Algebraic K-Theory seminar, run by Karoubi-Soulé-Loday. I also stayed with the Siebenmanns on Saturday night. Two seminars on applying algebraic K-theory to algebraic geometry were rather incomprehensible to me.

The construction that is of immediate impact from my work with Loday seems to be the non-abelian tensor product of groups, $M \otimes N$, where one assumes M acts on N on the left, $(m, n) \mapsto {}^{m}n$, N acts on M on the left, ${}^{n}m$, and all groups act on themselves by conjugation. Hence the free product $M * N$ acts on M and on N. The tensor product $M \otimes N$ has generators (as a group) $m \otimes n$, $m \in M$, $n \in N$, with the relations analogous to the properties of commutators

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

where ${}^{p}(m \otimes n) = {}^{p}m \otimes {}^{p}n$. So if the actions of M on N and N on M are trivial, then $M \otimes N = M^{\mathrm{ab}} \otimes_{\mathbf{Z}} N^{\mathrm{ab}}$, the usual tensor product of the abelianisations. But in general, the answer is different. Loday and Guin-Waléry [**83**] have, effectively, the fact that if I, J are ideals in Λ (commutative, such that $I \cap J = 0$), then $\mathrm{St}(\Lambda, I) \otimes \mathrm{St}(\Lambda, J) \simeq I \otimes_{\Lambda} J$ (where the action is via a crossed module boundary to $\mathrm{St}(\Lambda)$). If G is a perfect group, then $G \otimes G \xrightarrow{[\,,\,]} G$ is the universal central extension. If G is finite, then $G \otimes G$ is finite. I worked out $D_{2m} \otimes D_{2m}$, where D_{2m} is the dihedral group of order $2m$ ($D_{2m} \otimes D_{2m}$ is abelian!). It looks like opening a new subject; e.g. behaviour with regard to exact sequences, derived functors of $G \otimes -$, exponent properties, *etc.* It is expected to be useful in defining non-abelian *homology* (since $\otimes$ is dual to hom, in the usual situation, and non-abelian cohomology involves (derived functors of) Der, *i.e.* actions are involved). There are analogues for other algebraic situations (Lie algebras, commutative algebras, *etc.*), not all of which have been worked out. The multiple analogues, e.g. $A \otimes B \otimes C$, have not been written down yet, but follow from aspects of Graham Ellis' thesis [**66**] on cat^{n}-groups and other higher dimensional analogues of crossed modules. Ellis looks likely to go to Strasbourg next session on a Royal Society European Fellowship, which will be good for all concerned.

There are lots of questions this work opens, including the whole question of algebraic models of homotopy types. I was of course very interested in your points on p. 44 of your "Esquisse ..." [**82**], but tend to take a different attitude. First, there is a lot of interest in 2-truncated homotopy types, as there are here some fascinating

but very hard questions in the homotopy of 2-complexes and (relatedly) in combinatorial group theory. Such 2-truncated homotopy types are well modeled by crossed modules, so it would be interesting to see algebraic geometry type applications in this situation – of course, here the fundamental group plays an essential rôle. I have already got new results in homotopy theory and the homology of discrete groups by these methods.

In higher dimensions, cat^n-groups seem to play the rôle we want – the gap in Loday's proof has been filled by Richard Steiner [**124**]. So it seems very reasonable to construct cohomology with coefficients in cat^n-groups, and I am playing around with possibilities, without at the moment too clear an idea of applications. However, I still regard cohomology as a special case of homotopy classification of maps, and this last is surely a basic problem in homotopy theory.

However, cat^n-groups are complicated – we only begin to understand clearly the case $n = 2$, and even here there is surely lots of work to do – like that on tensor products. It may be that in higher dimensions one will need for practical purposes to look at particular kinds of cat^n-groups. In this I expect the foundational work on crossed complexes to be essential, as a guide for the kind of behaviour that can be expected.

Other models present themselves, e.g.:

1) simplicial groups (the foundation for a great deal of work);

2) simplicial groups whose Moore complex is of length n (studied for $n = 1$ as crossed modules, for $n = 2$ by Conduché [**47**]);

3) truncated simplicial groups (also studied by Conduché);

4) n-simplicial groups;

5) n-simplicial groups $G_{\bullet \cdots \bullet}$ each of whose Moore complexes in direction r,

$$G_{i_1, \ldots, \underset{r}{\bullet}, \ldots, i_n} ,$$

is of length 1 (these are just cat^n-groups);

6) n-simplicial groups each of whose Moore complexes is a crossed complex.

At this stage I intend to keep an open mind over *which* models are useful *where,* particularly as the van Kampen theorem for cat^n-groups is not very old and won't be generally appreciated for some time to come. But I have found surprising the element lacking in my previous approaches, and which I have learned from Loday, namely the value of the n-simplicial approach. It really covers in a nice comprehensible way lots of results which I found getting incredibly complicated when attempted by pure geometry – I couldn't even draw or understand the pictures needed.

Also, I think that without having done the van Kampen theorem for crossed complexes [**29, 32**] (particularly with the application to the relative Hurewicz theorem),

then the corresponding theorem for cat^n-groups would not have been attempted. Conversely, I believe the crossed complex approach, though limited, does carry more information than chain complexes, even chain complexes with operators, and so one needs to set the theory out precisely and efficiently, so as to perceive other applications. Tim Porter is doing something here in commutative algebra.

What has not so far been attempted is a "schematization" of cat^n-groups (cat^n-algebras *etc.*). I will discuss this with Tim, but it is not an area where I am clear on the applications.

The "psychological" problem I have found is the old chicken and egg question; it went on for a long time. What was I setting up a van Kampen theorem for? I really didn't know, except in the hope it would give information on higher homotopy groups (as it does, we now see).

Why do I want to apply double groupoids in differential geometry? Maybe I tend to think of a style of approach or area I would like to see, rather than in terms of theorems, and this may have dangers without a fair knowledge of a topic. However, if a subject is to break out of a rut, it may need a hefty jolt on the wheels without too much worry for the contents of the coach.

I hope this isn't boring. I expect I need to quiz you personally on this topic, to see if I can grasp something more of your overall way of thought.

[...]

I don't see that I will be able to get to the South of France before September-November. I'm probably travelling too much instead of writing, although it does work well with the excellent contacts in France. The paper with Loday [**41**] now needs tidying up only and an Appendix from Zisman on the homotopy spectral sequence of a bisimplicial set (or simplicial space); the section on excision and Hurewicz has been consigned to a second paper [**40**], allowing more leisure to get the ideas and notation precise and clear, although in effect the applications are formal (apply van Kampen to the correct pushout n-cube).

Another aim, coming out of Steve Humphries work, is to get a van Kampen type theorem for application to monodromy. The abelian situation in relation to complex singularities is now quite well worked over (monodromy action on $H_*(\text{fibre})$), but there seems quite a gap between that and understanding even monodromy on $\pi_1(\text{fibre})$. On general principle, this seems tailor made for groupoid or van Kampen methods. Maybe this is related to p. 44 of your "Esquisse ..." [**82**].

April 8. I seem to have been sitting on this letter.

The problem with the n-simplicial approach is that the homotopy category version has not been done (if it is possible). This seems to make for difficulties in some

applications I have in mind of cat^n-groups. I am also fully aware that one should be doing the base point free approach – *i.e.* appropriate forms of cat^n-*groupoids* – these have not yet been defined in the appropriate way, since they are clearly not just $(n+1)$-fold groupoids.

Graham Ellis goes next session to work with Loday, assuming he gets his Ph.D., which seems likely. This should help to uncover lots more usable algebraic material.

I must post this letter, whether or not is useful.

Yours very affectionately,

Ronnie

Lettre d'Alexandre Grothendieck à Ronald Brown, 07.06.1984

Les Aumettes June 7, 1984

Dear Ronnie,

I am afraid I've been a very poor correspondent for the last four months or so. The main reason maybe is that I am still not through with the "introduction" to Pursuing Stacks, which is by now approaching 500 typescript pages – with a number of other things I want to include into volume 1 of Réflexions Mathématiques, I now expect this volume to include about 700 pages or so – about the same as the first planned volume of Pursuing Stacks (namely, volume 2 of Réflexions Mathématiques). I hope though (for the twentieth time, I guess) to be through with that "introduction" within the next few days (if nothing new appears in the meanwhile . . .).

[. . .]

I guess I stop for today.

Yours affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 11.06.1984

11 June, 1984

Dear Alexander,

I have sent separately a copy of the latest version of the Brown-Loday paper [**41**], which is now pretty well finished. I have just had a few minor modifications from Loday and Zisman. We decided to call it a day on this one, and leave material on excision and the Hurewicz theorem for n-cubes of maps to a later paper, as this needs the setting up of machinery on n-pushout cubes, and related material on n-homotopy pushouts. The final section 7 of the present paper gives a flavour of what to expect, and in this dimension it is easy to be quite explicit.

Also sent is some hand-written stuff on crossed squares, which shows how to compute a Whitehead product, and also gives a condition on a space X so that one can construct a three-equivalence of X to a specific $\mathrm{B}\Pi\mathcal{X}$. This implies for example that the sphere S^2 has its three type described by the universal crossed square

$$
\begin{array}{ccc}
\mathbf{Z} & \xrightarrow{\ 0\ } & \mathbf{Z} \\
{\scriptstyle 0}\downarrow & & \downarrow{\scriptstyle 1} \\
\mathbf{Z} & \xrightarrow[\ 1\ ]{} & \mathbf{Z} \,,
\end{array}
$$

$$
\begin{array}{ccccc}
h & : & \mathbf{Z} \times \mathbf{Z} & \to & \mathbf{Z} \\
 & & (m \quad , \quad n) & \mapsto & mn\,.
\end{array}
$$

This crossed square comes from applying the van Kampen theorem to the sphere regarded as the union of two hemi-spheres.

Now that Graham Ellis has got his thesis [**66**] written, giving a completely explicit form for crossed three-cubes, and a fairly explicit description of crossed n-cubes, one should be able to develop more applications in higher dimensions. I suspect also that crossed n-cubes will be too complicated to handle, and so one will be looking for more special kinds of crossed n-cubes with which to model specific spaces. Since Graham Ellis is going to Strasbourg next year on a Royal Society European Fellowship (conditional only on his obtaining his Ph.D.), the subject should begin to make quite rapid progress. I think Graham has done very well to get such a substantial piece of work done by the age of 24; to do this, we have of course taken a fairly narrow view of what he needs to know, but he will find it easy to spread himself at Strasbourg.

I have just spent a week in Germany, four days at Bielefeld and two days at Bonn. Herbert Abels at Bielefeld has done some interesting work on finite presentation of certain algebraic groups [**1**], using work of Borel-Tits [**14**] and Kneser [**94**], and wished to pursue this work to higher finiteness conditions, in particular finitely identified.

It turned out that the van Kampen theorem doesn't help in this particular case, since he wanted to compute the second homotopy group of a space which is the union of $K(\pi, 1)$s, but where the induced homomorphisms of fundamental groups are injective. For the van Kampen to apply to this kind of situation, one needs the induced homomorphisms to be surjective. This probably indicates that the van Kampen theorem is going to be one extra tool, rather than a solution to all problems. However, it is still early days, and I think I managed to give Abels and his student a key idea for one part of their problem, using results of C. T. C. Wall [**130**] on resolutions for extensions of groups.

At Bonn I gave a talk in the morning on algebraic models of homotopy types in the seminar that Baues is running on your manuscript. In the afternoon I gave a talk on the work with Loday. A further day's discussion with Baues gave me a much better idea of what he is up to, and how it fits in with my approach. Again we confirm that crossed complexes are the first level of approximation to homotopy theory (at least after that of chain complexes), but what I hadn't quite realised was that he obtains crossed complexes from his axioms for a cofibration category [**10**]. This in some sense explains the prevalence of such gadgets, and why they have cropped up in the deformation theory of algebras (for example). It suggests the importance of verifying that crossed complexes themselves can form a cofibration category, with a cylinder object, which gives added points to the work I have been doing with Philipp Higgins on verifying the basic properties of homotopies, higher homotopies, tensor products, *etc.* for crossed complexes [**34**]. This will then enable the rest of Baues' apparatus and deductions to be applied to crossed complexes. A further interesting point is that his methods will automatically lead, when trying to classify maps between crossed complexes, to the category of crossed complexes (as the first level of approximation to this homotopy classification problem). So we come across models rather like Loday's, but from a different direction. It also explains how such models are likely to crop up in other areas of homotopical algebra. A quick glance through Baues' material (of which there is rather a lot) suggests that one thing lacking in his general theory is a Hurewicz theorem. But from my approach, such a theorem is intimately related to the van Kampen theorem. So we see some more intriguing possibilities. It should all keep me busy for a while to come! Another useful prospect from the visit is that Abels does have the background in topological groups and differential topology to be a possible collaborator in some of my other projects, for example on the applications of double groupoids in differential topology.

I confess to have been puzzling over your comment about "bringing concepts from the mist", and to see if I could pinpoint some features of the approach which could either be improved on, or with which I would like to stick. For various psychological and personal reasons, I did have a considerable lack of confidence at an early period, and this perhaps led me away from attempting to tackle what other people might

call key problems, and maybe towards exposition which I always found enjoyable and heart-warming. This has also maybe led me to ask whether a subject is in its overall structure the way I would like, rather than to look at the unsolved problems posed by other people.

[...]

Your manuscript is seeping round the U.S.A., and has been described as "notorious" and "legendary". I have just had a letter from Bill Dwyer asking for more information, and saying it was right up his field. I referred him to Mac Lane, who does have a complete copy. It *is* an embarrassment that I have not in the past year been able to do more than pick out some correspondences or analogies which seemed of use to me. On the other hand, I am very pleased to have had the opportunity to pass the manuscript on to others (*e.g.* Tim) who have immediately at hand an appropriate background. What I probably ought to do, is consider how your methods could apply in a particular instance, like polysets, where there is a need for proving the expected equivalence of homotopy categories.

Lettre de Ronald Brown à Alexandre Grothendieck, 15.06.1984

15 June, 1984

Dear Alexander,

I am amazed and delighted to hear what you tell me of the amount of Réflexions Mathématiques that you are producing. You will again be causing problems for people in keeping up with you! I feel an immediate effect will be a widening of horizons. Indeed one may feel homotopy theory (or at least stable homotopy theory) has rather been an internal subject – though maybe this is not fair in view of the connections with differential topology, Algebraic K-theory, *etc.* It is interesting to see recently the input the other way; Connes' cyclic homology [**51**] is now used in K-theory and homotopy theory; Loday's cat^n-groups [**99**] (arising from algebraic K-theory) are applied in homotopy theory.

[...]

I'm beginning to get keyed up about the LMS Popular Lecture – which is sold out with 350 seats.

Yours affectionately,

Ronnie

Don't take time off from Réflexions Mathématiques to write long letters to me! Unless it helps!

Lettre de Ronald Brown à Alexandre Grothendieck, 11.07.1984

July 11, 1984

Dear Alexander,

[...]

There is some gloom in the Brown household – the nettle beer is finished! Still, the strawberries and raspberries are freely available from the garden, and I intend to make some elderflower beer.

Tim and I are going to a conference on category theory in Switzerland July 23–27. Mac Lane, Tierney, Joyal, Eilenberg, Lawvere will be there. I'll be giving a talk on the work with Loday, but saying more about models of homotopy types by crossed squares. I hope to work out some more on the general case, $n > 2$.

I worked out $Q_m \otimes Q_m$ for the quaternion group

$$Q_m \ = \ \langle x,\, y \ : \ x^{2m} = 1 \,,\ x^m = y^2 \,,\ xyx = y \rangle \ .$$

It turns out to be abelian. I also had to check 48 equations to verify the answer, which is not an appealing method. I have pushed this material to a colleague who is a group theorist at Nottingham; his reaction is positive.

The Popular Lecture went well. I had a foyer display with six 50 cm × 40 cm photographs illustrating early interest (8th century A.D.) in interlacing, from illustrated manuscripts, jewellery, stone crosses. Then before the lecture, there was a slide show of knots and interlacing again from various sources. The audience was 300–350.

Now I have to get back to research.

Graham Ellis has done, I think, a nice job on cat^n-groups and crossed cubes. His thesis [51] is being bound, and his oral examination is the 2nd week in August. I will send you a copy when it's available, but I daresay a lot of material is coming your

way lately! As I mentioned earlier, he is going to Strasbourg next session. I hope he will become ambitious to continue a career in mathematics.

Yours very affectionately,

Ronnie

Lettre d'Alexandre Grothendieck à Ronald Brown, 22.07.1984

Les Aumettes 22.7.1984

Dear Ronnie,

I am sorry I've been such a poor correspondent lately. As you know I've been rather intensely busy to get the so-called "introduction" to Pursuing Stacks finished – finally I fell sick all of a sudden, on June 10, from overwork – it was hard for me to stand or sit, and I had to keep in bed for about five weeks nearly, stopping all intellectual occupations, including even writing letters. Now I've started on convalescence, and answering to some of the many letters which have piled up in the meanwhile. This is the third time within three years such a thing is happening – it is becoming evident I've to make a drastic change of way of life, with a lot more time and investment in bodily, non-intellectual activity. Head just too strong and loses contact with needs of the body, who has come to the point evidently where it can't take any more of this. The main trouble is with sleep, the body badly needs it, but the head keeps awake and just doesn't connect with this need. I begin to realize that too great intellectual power is quite a trap, and it has come to a point where it may well be a deadly one, if I keep it unchecked as in the previous years.

[...]

Please tell Tim Porter I'm sorry to have been so late in (not yet!) replying to his previous letters, I hope to find time to drop him a few lines within the next days. I've still to rest a lot, most of the time I am not lying down I'm spending on household, gardening and the like, whereas sitting at a desk for writing (even the most innocuous, unintellectual letters!) is a (bodily) fatigue – the body is making it clear, this time, that I better don't start on the same run again!

For this reason also, I'll keep this letter short – hope you can decipher it.

With my best wishes for your vacations

Affectionately yours

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 06.09.1984

6/9/1984

Dear Alexander,

I hope that you are now better or getting better.

[...]

We had a quiet summer; my wife Margaret being busy with her academic work, we went away only for a short time. The weather has been very hot in August, and so bathing in the sea was pleasant, and I have been getting some writing done.

Philipp Higgins has been here for a week, and we are getting the algebra of crossed complexes pretty well straight, including a nice definition of the tensor product $C \otimes D$, and the internal hom, which enables us to rattle off results about homotopies, higher homotopies, fibrations, *etc.* It is the nature of this algebra that makes me think of crossed complexes as a good first approximation to non-abelian homotopy theory, and one which clarifies old results such as the non-abelian extensions of groups. This algebra is also expected to give hints as to the procedure to adopt for cat^n-groups. Of course, I could be wrong – the general case might follow different lines!

Some group theorist friends of mine are busy calculating the non-abelian tensor product $G \otimes G$ of groups (à la Brown-Loday [**41**]) for various finite G. I don't know where that will lead, but it is a good thing to get a feel for the construction.

At the end of next week I go to Strasbourg for discussions with Jean-Louis Loday. Now that the main van Kampen theorem is done, it should be possible to sort out principal directions of investigation. We also have some results to collate for a second paper [**40**].

Maybe it is better not to write too much now, until I am sure you are back on form. I hope you had a pleasant and relaxing summer.

Yours very affectionately,

Ronnie

Lettre de Ronald Brown à Alexandre Grothendieck, 19.09.1984

19 Sept 1984

Dear Alexander,

Greetings form Strasbourg! Maybe that is the main point of this letter!

But also enclosed are details of a planned Workshop next July. In the light of your earlier comments, I did not expect you to want to come. But any change of mind would be enthusiastically received by all of us!

I guess the main initial steps of the Brown-Loday programme are done, and it is not clear what will be the best line of advance. We don't expect (now) to prove in this article [**41**] for example that $\pi_6 S^3 = \mathbf{Z}/12\mathbf{Z}$. On the other hand, the new tensor product and related techniques could well prove useful for non-abelian *homology* (rather than cohomology), and Loday has a student working on that. Also some new techniques are at present ill-digested, particularly *induced* cat^n-groups. The variety of induction processes, and their clear descriptions in terms of generators and relations, should prove interesting and useful. I don't understand too well the relation between the algebra and the geometry, mainly because we are none of us much good at pushout n-cubes! I know there are special cases of colimits, but it is not easy to recognize a familiar space as some kind of n-pushout, except by giving a nice cover $U_1, \ldots, U_n$: "nice" means, the associated n-cube obtained by intersections is connected. It is all very curious.

We don't understand all the relations between cat^2-groups and Conduché's 2-crossed modules $C_2 \xrightarrow{\partial} C_1 \xrightarrow{\partial} C_0$ [**47**], which give another special kind of "non-abelian Dold-Kan-Puppe". Loday has a functor

$$
\begin{array}{ccc}
L \xrightarrow{\lambda} M & & \\
\lambda' \downarrow \quad \downarrow \mu & \mapsto & L \xrightarrow{(\lambda^{-1}, \lambda')} M \rtimes N \xrightarrow{(\mu, \nu)} P \, , \\
N \xrightarrow{\nu} P & &
\end{array}
$$

where $\{ \ , \ \} : (M \otimes N) \times (M \otimes N) \to L$ is, curiously, $((m, n), (m', n')) \mapsto h(m, nn'n^{-1})$, where h comes from the crossed square $h : M \times N \to L$. But we have no idea how to go back again. The 2-crossed modules seem to be closely related to Joyal-Tierney's gadget, mentioned in his letter [24] to you.

In the end, one also wants the algebra to do geometrical computations. This we can do, but have not solved *outstanding* problems this way. The main possibility is that the low dimensional calculations on induced crossed squares will shed light on combinatorial group theory. Still, it is early days, yet. The general idea of "cubical resolutions" is rather hard to conceptualise. Now that the two papers with Loday [**41, 40**] are written, I should be able to get down to experimentation, and, more importantly, a more detailed look at "Pursuing Stacks" and the questions you pose there. To answer the question about modelisers and cat^n-groups, one first ought to consider n-fold categories as models. Maybe a different formulation is needed here.

---

[24] N. Éd. Lettre de Joyal à Grothendieck, à paraître dans [**80**].

Lettre d'Alexandre Grothendieck à Ronald Brown, 29.09.1984

Les Aumettes 29.9.1984

Dear Ronnie,

I keep being an awfully bad correspondent – a week or so ago I got again a friendly letter from you, the third one I'm afraid since August which I left unanswered. In the meanwhile I'm being mainly busy recovering my health through gardening, which has proved the right activity for me for achieving a balance between mind and body, as you say. It is in itself a "complete" activity, and moreover one where you see things growing (so to say) out of your hands. I've never been able to compel myself to any physical exercise for just the sake of it, "gymnastics", and therefore mere weight lifting wouldn't be it, in my case at any rate. I feel my health is back to normal, including sound and regular sleep which is the key to the rest – and I feel, too, I'm going to stick to gardening as a regular ingredient of my daily work.

Thank you, too, for your concern with such "intendance" matters as secretarial assistance. This, however isn't really a problem, not for letter writing anyhow. Even if I did have unlimited secretarial assistance (as was the case while I was working at the IHES till 1970), I wouldn't make any use of it for my letter writing, not any more, say, than for writing up the mathematics as they come to my mind (something altogether different from retyping "au propre" something which is already written). The speed of the handwriting on a sheet of paper, or typing on a typewriter which does the "writing" – this speed and rhythm are just the same as the mind's, looking up things and getting hold of them through the use of words. It isn't just a question of "speed" anyhow, but the written word (by hand directly, or by typewriter) is for me an essential "material support" in the thinking process. It would be quite a strain for me to get along without such support, if I was compelled to by circumstance – and I am not even sure I would succeed!

For the last few days I've resumed work on "Récoltes et Semailles" – it shouldn't take me more than one week altogether, maybe two, plus getting it typed and duplicated *etc.* After this I'll have to write and put together the other things which are supposed to make up (with Récoltes et Semailles) vol. 1 of Réflexions Mathématiques. Presumably I'm not going to take up mathematical reflection on Pursuing Stacks before December – all the more so as I'll have first to prepare the first seven chapters for publication (namely, providing introduction, notes *etc.*), as vol. 2 of Réflexions Mathématiques. For the time being anyhow, I'm definitively not "in" yet!

Thank you, too, for sending your announcement (or rather, application?) of (resp. for) the "workshop". It is surely a good idea, and I hope it will materialize. Thank you

also for your comprehension for (or tolerance with) my total allergy to participating myself in workshops and similar happenings!

Please give my regards to Tim (to whom I still owe an answer to his last letter), and also to Margaret, with my thanks (as well as to you, of course) for your common concern with my health.

Yours very affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 01.01.1985

28/12/84 – 1/1/85

Dear Alexander,

I am starting to write this on a train to London. Margaret and I are taking a weekend off to see a couple of shows, art exhibitions and friends and relations by ourselves. Our children at home are Marcus (24), Natasha (20), Matthew (17), Marita (14), Camilla (10), so there is not much difficulty in leaving them to fend for themselves. The deep freeze is full, there are leeks and brussel sprouts in the garden, so the expectation is that they will do very well and suffer more from a surfeit of food, crazy whist, and television! They are an excellent, capable lot of people. Marcus looks like settling into a teaching career. Next session he takes a postgraduate teaching course in outdoor activities/chemistry – the outdoor activities had a very competitive entry, and it shows the standard of his rock climbing that he got onto it. Natasha is doing an Applied Languages Degree course (in London), concentrating on Russian and Spanish. This is a course which specialises in language proficiency and the general background of the countries, rather than their literature – most of their lectures and essays are in the language studied.

This last month or so I've been trying to get completed the two papers with Steve Humphries [**36, 37**], on symplectic transvections, which have been accepted by the Proc. London Math. Soc. They were sent off last January, and so by now I have further thought on a number of details. Also Steve wanted to include an extra result which I found difficult to get right. It is Dieudonné type stuff, but the results in the literature were not in the form needed, and the subject being a new one for me (I got into it through working over Steve's "sketches" in 1978 when his supervisor was killed in a mountaineering accident) it takes a long time to get clear. For example, I found even E. Artin's Geometric Algebra [**3**] was not written, in places, with the clarity

and precision which made it easy for me to understand, and which also covered the nonregular case.

This topic is related to (algebraic) monodromy, and as such has been studied by A'Campo, Wajnryb, Chmutov, Jannsen; our methods are closely related to those of the last 3 writers, but were found independently by Steve. I did start off a (not so strong) student in this area, but found I did not understand the singularity theory and algebraic geometry well enough to have clear ideas of how to proceed on the geometric monodromy side. For example, consider a problem which must be basic knowledge to you: let $f : \mathbf{C} \times \mathbf{C} \longrightarrow \mathbf{C}$, $(z, w) \mapsto z^2 - w^3$; describe the monodromy action of $\pi_1(\mathbf{C} \smallsetminus \{0\}, 1)$ on $\pi_1(f^{-1}(1))$. This ought to be doable by tracing out the relations between elliptic functions, lattices, *etc.*, but I don't know enough about that! So now I've set Ghafar Mosa problems on crossed modules in commutative algebras, which fits into a tradition of work by Lichtenbaum-Schlessinger [98], Gerstenhaber [73], Quillen [115] on the cohomology of commutative algebras.

One basic problem here is that crossed modules in groups (or cat^1-groups) are clearly related to geometric problems, via the fundamental crossed module (or cat^1-group) of a based pair. We also know that ideals are fundamental to algebraic geometry. But the notion of crossed modules in algebras is an "externalisation" of the notion of ideal. The question is: to what geometric object does such a crossed module correspond?

In the case of crossed modules in groups, the geometric notion of a pair (X, Y, x) and its fundamental crossed module $\pi_2(X, Y, x) \longrightarrow \pi_1(Y, x)$ is available. The corresponding cat^1-group, or double groupoid, took quite a long time to find. I expect there to be some simple natural functor

$$(\text{geometry}) \quad \longrightarrow \quad (\text{crossed modules in commutative algebras}) \, ,$$

but it may take quite a time to find.

I have just remembered your comments on procedures for bringing concepts out of the dark, and the question of aims. However, it is not always possible to be completely conscious of the motivation behind the search for a particular formal analogy, and I am inclined to follow my own peculiar (to myself, maybe) ways of proceeding, in the expectation that something sensible will emerge. As the old saying goes, "if a fool will but persist in his folly, he will become wise" [25]. Which I have probably said before!

In this case, the pursuit of "higher-dimensional algebra", of which our usual homology theory is a pale shadow, has to be carried out by following scant clues, and sniffing odd scents, with only a faint idea of what our quarry actually looks like. We want to build up a picture from two separate analyses – algebra and geometry. On

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[25] N. Éd. Citation de William Blake.

the algebra side, we have quite an array of material on which to build. The algebra of crossed modules, crossed complexes and cat^n-groups has now become fairly elaborate. For example, the theory of tensor products of crossed complexes, on which Philip Higgins and I are working [**34**], has lots of pleasant properties and expected results for higher homotopies – it's just that the *proofs* are elaborate and technical, involving that feel for formal algebra of which Philip is a master. The corresponding algebra for crossed complexes in other categories has yet to be worked out (it will be a task for Ghafar Mosa, I think [**109**]), but must be simpler, and also useful. What I hope is that it may be easier to get at the underlying geometry by developing the algebra – no new idea of course! At least this is something on which progress can be made immediately.

The further expectation is that crossed modules in commutative algebras are going to be nicely related to monodromy. That is, they should give information on maps. At the first stage, we are getting very basic information on Spec(cat^1-algebra) by studying some simple examples. I don't know of any information in the literature on this topic. If we get somewhere on this, then we should be able to analyse Spec(cat^n-algebra). These become n-tuple cogroupoids, a species of object which has already arisen in stable homotopy theory, at least for $n = 1$.

This reminds me that I have been invited to talk to the British Mathematical Colloquium in April. The last talk I gave to the B.M.C. was in 1967 on groupoids, so I decided this time to give another talk on groupoids, this time entitled "From groups to groupoids: a survey" [**24**]. One reason for talking on this is that the material should by now be well known, but isn't. Another reason is that the material is accessible in a 40 min talk, and should be of wide interest. An interesting contrast between groups and groupoids is that groupoids can carry interesting algebraic structures, whereas groups cannot. This suggests we may be forced to study curious mixed structures – sets with compatible groupoid, Lie algebroid, and commutative algebroid structures, for examples. But for the purposes of this talk, it seems to me the evidence that "groups are an interesting special case of groupoids" rather than "groupoids are sometimes useful as a generalisation of groups" is conclusive. Also, 18 years later I can be more attune to an audience; and can also see much more widely how groupoids arise. It was only after the previous lecture that someone came up to me and said: "That was very interesting. I have been using groupoids for years. My name is Mackey." Even since then, people have been developing the basic notions independently, for their own particular area, so it is about time a general view was publicised.

Another student is working on holonomy groupoids. We are still having trouble getting the full details of the construction written down. Here Pradines defined (in 1966 [**113**]) a differential piece of a groupoid to be a pair (G, W) such that G is a groupoid, $\mathrm{Ob}(G) \subseteq W \subseteq G$; W generates G as a groupoid; W has a manifold

structure; $\mathrm{Ob}(G)$ is a submanifold of W; the initial and final maps

$$\alpha,\, \beta\,:\, G\, \rightarrow\, X = \mathrm{Ob}(G)$$

induce surmersions $W \rightarrow X$; if $\delta : G \times_\alpha G \rightarrow G$ is $(x, y) \mapsto x^{-1}y$, with domain the pullback, then $(W \times_\alpha W) \cap \delta^{-1}(W)$ is open in $W \times_\alpha W$ and the restriction of δ is differentiable. From this data, and assuming each $\alpha^{-1}(x) \cap W$ is connected, Pradines claims one can construct a nice differentiable groupoid $\mathrm{Hol}(G, W)$ and a morphism of groupoids $\Phi : \mathrm{Hol}(G, W) \rightarrow G$ such that $\Phi_W : \Phi^{-1}(W) \rightarrow W$ is differentiable and Φ is an isomorphism if and only if the germ of W extends to make G a differential groupoid. We are having some trouble getting Pradines sketch method (verbal communication) to work, so I am going to Toulouse February 10–16 under British Council support.

I would very much like to call in on you for a chat or just a social call, depending on your inclinations at the time. One possibility is that Pradines would drive me over on a trip which would then go on to Montpellier, where I could meet Molino and others, and give a talk on symplectic groups and applications, or cat^n-groups, if anyone is interested. I haven't finalised my travel arrangements to Toulouse – I expect to travel on Sunday, Feb 10, and would be happy to leave Jean Pradines to make the detailed arrangements. [...]

Actually, things are quite busy now, after a Christmas break when I've hardly done any mathematics. Philip Higgins is coming next week, and at the end of the week, two group theorists, Dave Johnson and Edmund Robertson, are coming over. They have got interested in computing $G \otimes G$ for various non-abelian G, pushing further my previous calculations, so we hope to write a paper on that [**38**].

The stuff with Philip looks like three papers. It does go on rather, but I don't see any way of cutting down the full story, since one has to set up tensor products, internal hom, higher homotopies, fibrations, and relate these both to the topology and to the more standard case of chain complexes with a group(oid) as operators. It gives some idea of how complex is even this first step towards a non-abelian cohomology. What we can do is show how the applications follow from the formal properties (Paper I), give proofs of formal properties (Paper II) and relations to chain complexes (Paper III). It is a long essay on geometry leading to algebra, in the spirit of Combinatorial Homotopy II [**137**], which itself was written 6 or 10 years before Cartan-Eilenberg [**44**]. The only difficulty is that maybe the applications won't seem so impressive for the length of justification. But I don't see any other options but to write it down.

Further understanding should come from the commutative algebra analogues. Tim is working on developing these.

I also enclose a couple of research applications for your information on current goals. I would have liked to have sent these to you and asked for your comments to go as an appendix to the applications – but there was not enough time in view of the deadlines, and it did not seem right to have comments sent in afterwards, unless the

SERC specifically ask you as referee. In Canada they have a system where you can put down names of people you would like as referees, and names you would not like. There is nothing like that here.

I hope that all is well with you, and, from Margaret and I, the very best wishes for the New Year

Yours very affectionately,

<div align="center">Ronnie</div>

P.S. 2/1/85 I telephoned SERC. The main proposal is liked by SERC, but they have not got enough money on this round. It will come up again in their March meeting. So things are improving! In fact, the proposals are improving, as lines of work have become clearer. I should explain that the case for support is restricted to 6 pages, but you are allowed appendices giving more details for referees. It is a good exercise writing these, but quite time consuming, particularly with the space limitation.

The workshop/conference on homotopical algebra is going ahead with partial support from the London Math. Soc. It should be fun.

This term we are organising Royal Institution Mathematics Master classes for Young People in Gwynedd: 5 Saturdays fortnightly, for 45–50 13-year-olds. It is quite a challenge to find and present appropriate material. I am one of 3 presenters for the first session on January 26, and will do angles in a spherical triangle. The course is meant to be activity oriented, not just a lecture.

<div align="center">Ronnie</div>

Did I mention that at the beginning of November, Tim obtained his richly deserved promotion to Readership? This is awarded on distinction in research, and I suppose is equivalent to the French professeur deuxième class.

<div align="center">R.</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 12.01.1985

<div align="right">12.1.1985</div>

Dear Ronnie,

Thanks a lot for your long and friendly letter, which I got only two or three days ago, and finished reading only today (due to various work and interruptions). Let

me come at once to the practical matter of your travel to Toulouse next month, with possible jump to Montpellier and (possibly) to my place. I am at present in (what I believe to be) the finalizing stage of my retrospective notes on my past *etc.,* and for nearly one year I haven't really thought about mathematics properly speaking. Also since October I am attaché de recherches au CNRS (Centre National de la Recherche Scientifique), and haven't yet once been in Montpellier, as I was very intensely busy with writing up those notes (which, with some extras, should make up volumes 1 and 2 of Réflexions Mathématiques). Thus I am not at all at present in the right state of mind for mathematical communication. On the other hand, from Toulouse to my place is about 500 km (340 miles) distance – if this is not prohibitive for you and (possibly) Pradines, for just a "social call" as you say, I'll be of course delighted to welcome you and have a look at each other! Montpellier is on your way, about 350 km or so (170 miles), quite a drive, too. You may not find the faculty there so pleasant a place to be worth driving all that far, all the more as there is not me or someone else living in the city or nearby to welcome you at his home there. Interest and knowledge on homotopy theory is close to absolute zero there, however, things of definitely geometric (and not too strongly algebraic) flavor, as your symplectic reflections, may have some interest for the Molino group people, and maybe one or two others; they'll find they've listened to a nice talk. An introductory talk on knots (or on the mapping class group) may be better still, but I doubt any of this will go beyond academic interest. If this doesn't sound discouraging to you, and if you feel like getting acquainted with Molino (who's quite a nice person) and possibly Ladegaillerie (whom I like a lot, too), please tell me so in a line or drop me a call, and I'll see with Molino and Pradines how to arrange something. Maybe this will become an occasion for me, too, to pay a visit to my "université d'attache".

Thanks a lot for your good wishes, and please accept mine for you, Margaret, Marcus, Natacha, Matthew (N.B. I've a son Matthieu, too, who is 19), Marita and Camilla. I hope the kids had a good time while you and Margaret were away! I'll stop at that, for the time being, as this letter should leave soon. I am in good shape, although there is not much garden work to do with the wave of cold which swept over the land lately.

Yours very affectionately

<div align="center">Alexander</div>

Please give my regards to Tim Porter, too. I hope he got my belated answer to his previous letter, and that he isn't too annoyed with me for having been so long in answering. And my congratulations for his promotion!

Lettre d'Alexandre Grothendieck à Ronald Brown, 02.03.1985

Les Aumettes 2.3.1985

Dear Ronnie,

It was nice to get your letter, the pictures of your whereabouts, and manifold gifts – you're really spoiling me! Your winemaker's recipes are quite fascinating, and I'm sure I'm going to have fun with it – I'll tell you what comes out. Thanks, too, for your book on topology, which looks quite nice and readable. I hope though that this hasn't been an unconscience [26] to your friend Morris to whom the book belonged. Maybe he knows it by heart now?

I've been going on working on my notes. Just written up a review of the "four operations" (not the *six* in duality theory!), the fourth and last of which is the one called "opération du Colloque Pervers" [**125**]. It (the four together) came out longer than I expected – about 30 typed pages. Maybe I'll have this typed separately and send it out before the bulk of my notes is finished typing *etc.* – all the more so as this is part of the 3$^{\text{rd}}$ part of Récoltes et Semailles, which I'm not going to send out with the first and second, as it is of a more personal character still – but only on request. (Of course, I'll send you a copy of it as soon as it is out, as I know already you *are* interested.)

It was nice to meet you, Ronnie, and I'm glad you enjoyed it, too. And thanks again for your kindness!

Affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 17.10.1985

17/10/85

Dear Alexander,

This is to express my great appreciation and thanks for the volumes of "Récoltes et Semailles" received yesterday! I have already been dipping into them, not so easy with "ma pauvre commande de la langue française", but I am fascinated by many of the questions you address, as you well know. I also greatly appreciated your handwritten dedication.

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[26] N. Éd. C.à.d. "inconscience" en français, "inadvertence" en anglais.

There are all sorts of further things to ponder. A "Manual for the beginner in mathematical research" has yet to be written, and there is maybe largely an oral tradition among those fortunate to be properly led. Did I ever tell you of the apocryphal dedication to a Ph.D. thesis which Michael Barratt told me of 20 or so years ago: "I am deeply indebted to Professor X whose wrong conjectures and fallacious proofs led me to the theorems he had overlooked." (A test of good supervision!)

I do wonder about the effect of all of this analysis of yours on those to whom you refer at length. This may lead to a broad discussion of mathematical ethics, and of the responsibilities of those in a position of power and influence to encourage the young.

But the important thing is to help for a renewed vision in our subject. I meet so many who once they have done their Ph.D. problem do not know what to do with themselves, because an analysis of aims has not been part of their training (it was not part of mine, either!). There may be many who argue differently, but I enclose a letter from the Notices AMS which argues in similar vein, I think.

There is a lot more I would like to say. I might use the revision of my book and particularly the additional notes and comments to give encouraging points, if my editor and publisher will allow me.

Another old saying I find instructive: If a fool but persists in his folly, he will become wise. It is useful for a student to see how his supervisor copes with failure, since there is not usually much difficulty in handling success.

I am really looking forward to a proper reading of Récoltes et Semailles.

Yours affectionately,

Ronnie

Lettre d'Alexandre Grothendieck à Ronald Brown, 22.12.1985

Les Aumettes Dec 22, 1985

Dear Ronnie,

Two months have passed since I got your warm and interesting letter acknowledging receipt of Récoltes et Semailles. During this time, most of my energy was spent in "meditating", something which I had been pushing off for a very long time, and which had become quite urgent. Accordingly, I greatly neglected my correspondence (an old tune of mine, I'm afraid) – I hope you will pardon me for being so late in replying.

According to the response I got so far from Récoltes et Semailles, I am rather pessimistic about an overall effect it may have in the mathematical community, and hardly expect anything like a "broad discussion of mathematical ethics", which you are contemplating. Only a surprisingly small number among those mathematicians to whom I sent a copy of Récoltes et Semailles took the trouble to write, and the dominating tone is embarrassment, and a desire to "drown the fish" ("noyer le poisson", as the French expression says). Exceedingly few people who would be willing to admit (even to themselves, I am sure) that there has been going on a large-scale fraud, with the connivance of a large fraction of the mathematical establishment – even for those not directly involved in the fraud, such a thing is just too big to face. Except yourself, and of course Mebkhout and me, the only people you may know by name, who expressed disapproval of a fraud, are Samuel, Leray and (lastly named, and not least!) Illusie. The case of Illusie is remarkable, as he is one of the three people (with Deligne and Verdier) who has been the most directly involved in the fraud. The weird fact is that he acknowledges the existence of the fraud and sincerely regrets that it took place, but just ignores the crushing evidence showing that he was one of the main artisans of the Burial. It is just amazing, once again, to see how one may fool oneself and renounce the use of even the coarsest common sense, doing the worse while being convinced of one's good faith and good intentions. If this were not so, many unbelievable things such as wars and the like, couldn't possibly take place.

It seems to me that there has never been, in any kind of study, an "analysis of (its) aims" as part of the study, as far as I know. It seems to me unrealistic to expect such a feature to appear on a large scale, in any subject whatever – and now less than ever. All one can do is to be attentive to this aspect of things oneself, and share one's own thoughtfulness with those one is supposed to teach. This is very little, of course. The fact is that we are functioning in a setup which is more and more crazy, and this just cannot be helped, whatever one may try to do.

I wish you and Margaret and the kids nice Christmas and New Year festivities, and a very happy New Year. And please don't let yourself be discouraged to write again because of my slowness in responding!

Affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 15.02.1986

16 December 1985, posted 15/2/86

Dear Alexander,

[...] It looks like I'll be at Bangor for a good few years yet. From our children's point of view, such a settled existence is probably just as well, with their own friends and activities in the village and area, although our last but two, Matthew, is off to University in October, to read Electronics, or Electronics/Mathematics. Actually, we'll have 5 children home for Christmas, but we hope to go off for a long weekend in Paris soon afterwards.

In January, Baues has a meeting on Algebraic Homotopy with about 40 people, including for example Joyal and Duskin, Loday, Porter, people in rational homotopy, so it all looks good, and a nice complement to the Bangor meeting.

I'm beginning really to understand Loday's functor [**99**]

$$\Pi \; : \; (n\text{-cubes of spaces}) \; \longrightarrow \; (\mathrm{cat}^n\text{-groups}) \; .$$

The nice point is that starting from an n-cube X, one makes X into a *fibrant n-cube* $\overline{\mathsf{X}}$ in the sense of Edwards-Hastings (SLNM 542 [**63**]) in which each map

$$\overline{\mathsf{X}}_\alpha \; \longrightarrow \; \lim_{\sigma > \alpha} \overline{\mathsf{X}}_\sigma$$

is a fibration. Rather cunningly, such a fibrant n-cube is just what one needs for all of Loday's construction, and the algebra ties in beautifully with the geometry. Also, fibrant n-cubes are used in Steiner's construction [**124**] of a functor

$$\text{spaces} \; \longrightarrow \; (n\text{-resolutions}) \; ,$$

where an n-resolution is a fibrant n-cube such that the associated n-cube-of-fibrations (stick in all the fibres), also written $\overline{\mathsf{X}}$, has $\overline{\mathsf{X}}_\alpha$ a $\mathrm{K}(\pi,1)$ for $\alpha \leq \underline{0}$, $\alpha \neq (-1,-1,\ldots,-1)$.

We have a long way to go before the n-cubical (= hyper-relative) methods have the status in homotopical algebra as projective resolutions already have, but there are a few clues pointing to possibilities. I would also dearly love to get the differential topology/geometry applications going; I am almost beginning to believe your original judgement that such a plan needs a wider experience and spread than I have so far attempted. Not to worry – I do like exploring from a soundly constructed base, and the present applications seem to me of that type. How long will we have to wait for a solid application of the non-abelian tensor product $L \otimes L$ for L a Lie algebra? In homotopy theory, we have the charming result

$$\pi_3 \mathrm{SK}(G,1) \; = \; \mathrm{Kern}(G \otimes G \xrightarrow{\;[\,,\,]\;} G) \qquad (= \mathrm{J}_2 G, \text{ say})$$

and the exact sequence

$$\mathrm{H}_3 G \;\longrightarrow\; \Gamma G^{\mathrm{ab}} \;\longrightarrow\; \mathrm{J}_2 G \;\longrightarrow\; \mathrm{H}_2 G \;\longrightarrow\; 0 \;,$$

where Γ is Whitehead's universal quadratic functor. In view of the profound influence $\mathrm{H}_2 G$ has had, I think we can expect quite an impact of $\mathrm{J}_2 G$. Also, calculations in $G \otimes G$ are fun! What's more, $G \otimes G$ occurs in embryonic form in algebraic K-theory, cyclic homology, ..., so we just need an impact in algebraic geometry. For this we might need a localised form – work under consideration!

We've got a nice group here, with Tim, Graham Ellis and Nick Gilbert, who just did a Ph.D. in automorphisms of free products of groups, and now is SERC Research Assistant here.

19 Dec.

Edmund Robertson of St. Andrews, who did some computer calculations of $G \otimes G$, reported on our joint work [**38**] at a group theory conference, and John Conway was amazed as he had not seen the construction before. It is amusing the definition was forced by the topology (*i.e.* application of generalised van Kampen), as when you think about it, it is an "obvious" thing to do. The commutator map $G \times G \longrightarrow G$ is a "biderivation" – so turn it into a homomorphism, *i.e.* factor through $G \otimes G \longrightarrow G$, a homomorphism. The fact that $G \otimes G$ is finite if G is finite gives it then a bit of reality.

I've got my book revision almost finished, with comments bringing it up to date [**25**]. I have commented on Steenrod, as this seemed a suitable place to do so, but adopting a cool rather than injured line. The episode is interesting, a judgement either way can hardly affect my career now, and I can't complain about my early advancement either. The point of publicity is to set the record straight, and to heighten awareness that this sort of thing does occur. It may also need more than just awareness. I tried to explain my annoyance to Dale Husemöller last summer, and he seemed to think that if Steenrod wanted to write up this stuff, why not? It is this attitude of "let the big guys do what they want" which seems to need stamping out. There was a song by Tom Lehrer popular among mathematicians in the 1950s with the chorus "plagiarise – don't let anything evade your eyes!" I wrote a letter to the Michigan Math. J. last summer, but have not got any acknowledgement, let alone a reply. That again is interesting.

What I have also tried to convey in my additional comments in my book is that the notion of topological space is unlikely to be the final resting place for the intuitive notion of continuity in mathematics and science. One aspect of the inappropriateness of "topologies" for all circumstances was envisaged in my 1961 work on k-spaces, and this is now part of the philosophy of "categorical topology". The other aspect is your vision of a topos as a fusion of geometry, topology and arithmetic, and this also I have

mentioned. So I hope students will get from my revised book an idea of the fun of good exposition, using the right concepts, but also an idea of the temporary nature of even good exposition. Isn't this one of the mistakes of the Bourbaki tradition? Started at a time when an improvement in exposition was essential, it eventually became authoritarian and rigid, so that it really becomes like the schoolboy question: What happens if you put many worms on a straight line? Answer: one of them is bound to wiggle and spoil it all! I'm afraid even Bourbaki cannot keep the mathematical worms straight – they can't even do the fundamental groupoid properly, which I thought I had wrapped up in my (1968) book (or should I say, lined up?).

Have you ever seen any of Jack Morava's work? He has really managed to apply ideas of groupoid schemes (with references to Catégories Tannakiennes [**118**] by a student of yours . . .) in stable homotopy theory. [...] I find the range of ideas he uses in a fairly recent Annals paper [27] quite difficult to follow and understand; but I think you would appreciate it. Of course, most people in homotopy theory do stable theory, of which I am a non-practitioner. Stable homotopy looks aghast at the fundamental group and tries to get as far from it as possible. Having failed to keep up with that lot, I have ended up by trying to make homotopy theory as much *like* the fundamental groups as follows, an idea beautifully completed by Loday with his cat^n-groups, and giving a computational aspect with the van Kampen theorem.

1/2/1986

The above has been lying around in my briefcase with an intention of Xeroxing and posting. Now it's February!

I just got a reply from the Mich. Math. J., suggesting that contemplation of an 18 year old injury is unhealthy, and I should do the Christian thing and forgive Steenrod. This misses the point. I shall reply that an unwillingness to discuss, frankly, matters of history and ethics is unhealthy for the subject, and such discussions should be particularly welcomed by editors of journals.

Your volumes of Récoltes et Semailles will lead people to discuss the ethical issues involved, even if you don't hear about this directly. This was certainly true at Baues' meeting on "Algebraic homotopy" at Bonn. You have written a unique work, so things won't be quite the same again.

I go to Montreal Feb 15–22 at the invitation of André Joyal, and expect to explain about crossed modules and cat^n-groups. I'm getting clearer about the foundations of the subject, in the sense that I think I can explain the basic ideas. Somebody ought to show how cat^n-groups exist in an (arbitrary?) model category for homotopy.

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[27] "Noetherian localisations of categories of cobordism comodules", Annals of Math. 121 (1985), 1–39 [**108**].

Lettre de Ronald Brown à Alexandre Grothendieck, 01.05.1986

1st May, 1986

Dear Alexander,

This is just a note to thank you for your volume 01 of Récoltes et Semailles. I found your remarks on mathematics and your own development very interesting indeed, and it is very helpful to have an indication of the overall vision which motivates the particular technical work. This is too often what gets omitted in published work.

I don't know that you should be too worried, upset, or surprised that a lot of people do not take what you have written in perhaps the spirit you intended. Indeed, I am not too sure what precisely you expect people to do, in the sense of being clear about a reasonable list of options for them. I cannot see from my overall glance at the whole work what you would place at the top of your list of desirable outcomes which were also within the bounds of reasonable probabilities.

Part of the trouble is that the sort of matters about which you write and the tendencies are confirmed by tradition. [...] Indeed, Joyal tells me that colleagues have justified their actions by reference to [biographies], and pointing out that this is the way the world runs. It seems to be fairly common in any area for top people to give themselves airs, and to make as if the normal laws did not apply to them. Of course, this has a great advantage if you can get people to accept it! I prefer to take the view that mathematicians do suffer from attacks of human nature, or, to put it another way, that to the student of the vagaries of human nature, even mathematicians have something to offer! Not a lot, maybe, but something.

Enclosed are copies of correspondence with the Michigan Math. J. which I hope have entertainment value. My historical note is deliberately written in a cool, ironic tone, and I excised the indignation, which can get wearisome on one's friends. On the other hand, my last letter to them deliberately laid it on thick.

I am afraid I cannot go along with your comments on page 47 of 01 of Récoltes et Semailles on the "mathematical community", partly because I am not entirely sure what they mean in practice, except that you do not like what you claim is going on. Among mathematicians, there are a variety of people, reacting to the pressures put upon them in their own individual way. Indeed, a further interesting question is to analyse what these pressures are, for example, why should Steenrod do something which, when examined, is quite absurd? To some extent, these pressures reflect the "macho", the competitive element in mathematics, a proportion of which is inevitable and desirable – if you want to reach into the unknown, then there is a clear desire to be first to do so. I am in danger here of being obvious and boring, but there are two points I want to make forcefully:

1) There are lots of mathematicians with whom I get on well, whose company I enjoy, and who behave with care and probity (I would not describe this as an intersection or as a union!). So a vague, generalised attack on mathematicians is not helpful. As a class, I don't expect they are particularly worse or better than other similar classes.

2) The pressures and influences which lead to lapses, and lack of help to young people, are worth analysing. One may smile at Hilbert's comment "You read too much literature!" when a research student told Hilbert that his dissertation results had been published – but such remarks are remembered, and used, by lesser people. Very often, the overall tone is set by those at the top. Obviously, I'm influenced by Henry Whitehead and Michael Barratt, great ones for talking in pubs – that is a matter of luck, and I hope it has helped me. An overall aim should be to forward the subject as a whole, by making it easier for young people to develop and flourish. Reputations of oldsters are not so important.

I should say that my little note got quite a lot of support among the 57 participants at a recent "Conference on Categorical Topology", in Italy, but this is not a subject too well regarded by some of the top boys in mathematics. I tend to take a philosophical tone, of "you win some, you lose some". Indeed, the whole thing might be regarded as something of a storm in a teacup, all about who did what when and why, when compared with some of the serious problems that various people have to face.

But still some expression should be made about clearly improper actions. A comic tone might help, and be amusing, and I enclose a copy of a draft article along these lines. Whether it will see the light of day is another matter, but it was quite amusing to write. Life is hard if one can't get some fun out of one's colleagues.

I do get the feeling you are in danger of oversell, and giving the impression that you are trying to show that you anyway are clean, only everyone else is dirty. That's not too good for those who have to live and work in the rough and tumble. The real challenge is to come out and do something about it by getting around, meeting people, and so setting an example.

Yours affectionately,

R. Brown

Lettre de Ronald Brown à Alexandre Grothendieck, 01.05.1986

1/5/86

Dear Alexander,

[...]

The main difference between our approaches to the overall programme on non-abelian methods, is that it turns out that for my purposes, the notion of *crossed module* is central. On the other hand, in both "Esquisse d'un programme" [**82**] and "Letter to Quillen" you are fairly dismissive of crossed complexes and in particular of crossed modules! Yet, just as groups (groupoids) describe 1-types, so crossed modules (over groupoids) describe 2-types, and are related to lots of known algebra, both in groups, and elsewhere. Without crossed modules, it would have been difficult to move into Loday's crossed squares [**99**] and the Ellis-Steiner crossed n-cubes [**67**]. So I find a lot to do exploiting these notions, before moving on to other kinds of structures, e.g. lax ones, to which we may be forced in the long run. So I like to think I am working in the *spirit* of your programme, without actually following the *letter.*

I am still looking for an effective use of crossed modules (or an analogue) in algebraic geometry – this motivates the thesis in preparation of G. H. Mosa [**109**], but his actual work is rather different.

People are, I think, beginning to sit up and take notice of these cat^n-group ideas. I gave a talk at Oberwolfach in September and so my U.K. colleagues looked startled – the results give a bit of an impression now of rising like Venus fully formed from the sea; particularly if presented without explanation for the benefit of pragmatic, problem-motivated topologists. I'm giving a talk on these ideas at Edinburgh next week, and Loday has talked at Berkeley.

Also enclosed is a copy of a draft article on groupoids [**24**], which needs some reorganising, and lots of references. I have had detailed comments from various people, and if you wished to add any, this would be very welcome. I won't worry if you don't get round to reading the various stuff enclosed – e.g. another more considered letter commenting on 01 of Récoltes et Semailles.

Yours affectionately,

Ronnie

Lettre d'Alexandre Grothendieck à Ronald Brown, 05.05.1986

Les Aumettes May 5, 1986

Dear Ronnie,

Just got your letter and the lot of interesting material which you sent with it. Thanks a lot for all, and also for not being mad at me for not having replied yet to your letter of December 16th. Those six months nearly have run away as if they had been a few days! Of course you are on my list of people to whom I have to answer – but the list is only getting longer over the months, while I scarcely write at all, except for really urgent matters. Maybe there is nothing urgent now either, just a pleasure I take to write back this time at once.

Your are certainly right with your remarks on that page L 47 of my "post-scriptum" to the Letter, I already felt myself there was a somewhat emphatic tone to it which wasn't too helpful. If you hadn't come along as a reminder on this, I may well have forgotten to make the necessary changes before the text goes to print – at present I am rereading the last part and hope to be through within a week or so. Don't be afraid about my being "worried, upset or surprised" on this and that – and be sure I don't "expect" really people to "do" anything. Anyhow, whatever comes up comes as a surprise – sometimes unpleasant, and sometimes welcome. So far, what came from you I liked getting, even if some of the mathematics passes over my head (still more now maybe, as I am more "out" than ever), and even though I don't reply at once. I found your "historical note" very adequate, namely a clear and short account of the main facts, understandable to anyone (without having to enter into any technicalities). Also, I found the correspondence with the editor of the Michigan Mathematical Journal quite instructive, and I feel you hit the right tone. I wonder if it wouldn't be a good idea to send out your historical note, together with a copy of the correspondence with the editor (which is reasonably compact) to a fair number of people. I feel this correspondence, and particularly your two letters of February 3rd and March 12th, very adequately complements the "historical note", which you purposely kept rather dry and impersonal (and surely, rightly so). I definitely feel the action you have taken so far very "healthy" indeed, and would encourage you to give it maximum publicity – well knowing, of course, that it will get very little (or sneering) response from the higher spheres, and probably from the not so high spheres as well (as they are just taking the tone from above, mostly). It was a pleasure, too, to read "Don't let anything evade your eyes" (but what's the meaning of "Screwtape"?), it seems though you didn't quite finish this introduction to a treatise. I do hope you'll write down whatever else comes up in your mind on this seemingly inexhaustible topic. Circulating such a text might be a lot more efficient still than your historical note

(with or without the correspondence), and I would be delighted to help circulating it among the people I know, if it is O.K. with you.

[...]

I also looked at once through your survey "From groups to groupoids" [**24**] and found it written in quite a stimulating way. Sorry, I am too much out of mathematics, at the present moment, for having any useful comment to offer! Sorry, too, you found I was "dismissive" with crossed modules in Esquisse d'un Programme [**82**] and the letter to Quillen – I believe, rather, I didn't mention them at all (as far as I remember) – which isn't the same. They just didn't play any crucial role in my ponderings so far, and I am afraid that despite your untiring and friendly efforts, I haven't yet got the right feeling for them, and maybe never will. (It may take years before I get back to the program "Pursuing Stacks", interrupted through Reaping and Sowing, and now there are a number of things which I feel are more urgent that I write them up, as visibly nobody else will do it. You know I have this hang-up for lax structures, which possibly isn't so much better than the non-lax ones, as Étienne Li told me.) I guess I'll have to go to the end of what is in my mind, though, and see what comes out, before getting involved with the kind of structures you keep telling me about. My way of doing maths has been so far to listen more to the things than to people, and it seemed to me that (for those things I have been after so far, at any rate) they never told me yet that crossed modules or complexes was to be a key notion where I was working. This doesn't mean at all that I deny (or "dismiss") their key importance elsewhere, and I am quite willing to believe you on word about this. But such "belief" is a long way from a genuine feeling or understanding of what they're all about...

I didn't read any biography of Hilbert, or of any other mathematician except one of Galois, a very long time ago [28]. [...] Certainly, I do feel Hilbert's point when talking about "reading the literature too much" (but of course I am ignorant of the context [...]). And I am pretty sure, too, that not much of what Hilbert did, could be found in the "literature" prior to him. At any rate, the problem is not at all in this direction – of being sufficiently mindful to know the totality of relevant literature, to be sure you'll never forget to acknowledge, if perchance anything of what you're doing has been done more or less by Such-and-Such. At a certain level, you are perfectly sure nobody ever looked into the things you are looking into – and at another, more routine level, to make sure that there isn't some place in the literature of the last hundred years, where some of what you need is done more or less, is a wholly hopeless and sterile undertaking. The literature is just too big, and spending your life on it wouldn't get through anyhow (nor even, catch up with what is currently published!). The point I want to make is that the question of probity isn't really so much a question of *rules* one should stick to, for instance, keep oneself informed of

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[28] N. Éd. L. Infeld, *Whom the Gods love.*

everything related to one's subject (granting this was at all possible), so as to be able to give proper credit for whatever was published, say. You know as well as me and Mr. Screwtape, that you can stick to such a rule, and give due credit for trifles, and still act as a perfect crook. The question of probity is not a question of rules, but of spirit. This doesn't mean that rules are useless, or that they should be disregarded, when there are such written or unwritten rules. They are a "pis aller". [...] Maybe this is an unwarranted apriori of mine, which I'll still have to get rid off, too – but I'm convinced that in the past (say, preceding the last twenty years or so) there haven't been great mathematicians (the word here taken in a purely technical sense, if this makes at all sense) who were at the same time crooks in their profession.

By the way, I never meant to question, anywhere in Récoltes et Semailles, that there are today mathematicians "who behave with care and probity" (I cite from your last letter), and at places I am quite outspoken on this. Could you tell me where exactly you got the impression of "a vague, generalized attack on mathematicians" – which certainly wasn't what I had in mind. What I *did* want to state, though, is a pervading, overall corruption of what could be called the "collective ethics" – according to which things nowadays have become accepted as "normal", which "in my time" were just not thinkable, and would have been regarded as outrageous fraud, if they should ever have happened. Now it is true that, in a certain sense, every single mathematician is coresponsible of the state of the collective ethics. Even though such responsibility is rarely clearly acknowledged, I believe it is however felt (more or less unconsciously) by most mathematicians, which is one reason why almost all react with embarrassment or defensively, and probably feel "attacked", when the ethics of the profession is being examined (through "cases", say, generally regarded as "normal"), even though they are not personally involved.

You say that I am in danger of "giving the impression that I am trying to show that I anyway am clean, only everyone else is dirty (or at any rate, others are)". Now isn't this the situation, practically at any moment when you start questioning things around you which, you feel, are not O.K.! In Récoltes et Semailles, part I, I have been examining with some care my own past, and found a number of things which were unexpected to me, and which could be termed "not O.K.". Later I came to tumble upon the Burial, and upon a number of things which had been going on and which I "didn't like" (as you put it). Now, regretfully, I must admit that there hasn't been this kind of thing going on in that past of mine, as far as I can see at any rate. Should I therefore refrain from speaking out, lest I give the impression you told about? I guess that's not what you really meant – as you yourself took the trouble to make things clear about that affair with Steenrod. But what exactly is your point? Sorry I am so stupid! I do see your point though when speaking about "getting around, meeting people" *etc.* But there are a number of ways of "getting around" – my own way (which responds to my own temperament, as it is evolving more and more) is to

do so without leaving my room (and therefore, not really "meeting" anyone, except in writing). Fifteen years ago, I went about it quite differently, actually meeting lots of people. So maybe we may admit that there are many ways of proper action, not only from one person to another, but also for the same person, according to circumstances, age, mood, and the like.

I guess its time to stop these ramblings. Thank you once again, Ronnie, for your helpful and stimulating comments. I look forward to hearing again from you, when you find a moment. By the way, your chervil seeds did fine in my garden, and I greatly appreciate their flavor. It took a while, but they seem happy now.

Yours affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 27.05.1986

27th May, 1986

Dear Alexander,

Thanks a lot for your letter of May 5. I am delighted that you found the various material interesting. One distinguished person did write to me to say that "it does seem to me that you are perhaps unduly concerned about this matter of undue credit which has been given to Steenrod... I hope that you will not press the issue with quite such insistence... If it seems that he has been needlessly attacked, many of his contemporaries (and I am one) would be likely to rise to his defence". On the other hand, people to whom I have shown or talked about the last letter from the Michigan Math. Journal are simply amazed that such a policy could be held by a serious scientific journal.

The article "Don't let anything evade your eyes" was a quick draft, and I should explain the references. The title turns out to be a misquote from a song of Tom Lehrer, with its refrain "plagiarise – let no-one else's work evade your eyes". The name Screwtape refers to a book called "The Screwtape Letters" by C. S. Lewis, which purports to be a series of letter from a devil called Screwtape to his apprentice devil explaining the various ways in which he can turn good into evil. I think though that the article ought to be more certain in tone and in manner of address, and it should perhaps be prefaced with an introduction in which the author (or authors, if Tim Porter agrees to come along) explain that the manuscript was found in an obscure part of the library, that the writers disclaim any responsibility for the scurrilous message, and are simple circulating it so that people can ensure that this kind of

foolishness does not get into the wrong hands. I don't mind the thing being copied in its present unfinished form, but still anonymous. A final version needs a little more care.

[...]

I also enclose an amusing account from a letter of John Isbell, with whom I am arguing about Steenrod's paper.

I think my point about the "vague, generalised attack" was really in reference to the remarks on "gangrene", and I think the important thing that we agree is that it is useful to make these precise. In any scientific community, peer review is important, and reputation is crucial to a person's career. Those with established reputations are anxious to show that they are still doing more. There is also, as you rightly point out, an idea that ethical situations should not be discussed, and this is the real danger, because it allows people to get away with it if they choose to work against obvious standards of decency. Then there can arise an atmosphere of fear, almost like a police state, in which the friends of the Chief of the Police will certainly defend him from any suggestions of untoward behaviour. Indeed, in this atmosphere, and I think I get something like this reaction from some people with regard to the Steenrod matter, the victim is blamed for being embarrassing.

Maybe I didn't get it right in the extract from my letter which you quote about "clean and dirty", but I think this was just a reaction against what seems to me a generalised attack. There is a problem also of finding remedies. I think maybe this has to be in terms of creating an atmosphere in which young people can flourish, and in which established people are prepared to go out of their way to advertise the work of younger people. This seems a triviality, but an opposite attitude is all too common. And this is what you rightly point out.

I am delighted the chervil did well in your garden. We like it very much. Did those strange little onion bulbs take at all?

Yours very affectionately,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 17.06.1986

Mormoiron 17.6.1986

Dear Ronnie,

Thanks a lot for your letter, and your Screwtape presentation with Tim. I've been very strongly involved with mathematics lately, working out another approach

still to "topology" and "form" (different from topological spaces, from topoi and from moderate spaces as proposed in Esquisse d'un Programme [**82**]), just getting the basic language straight – but to work it out in full, just as for moderate topology and a lot more than for topos theory, it would require years of work. I am quite fascinated by now and if I let it have its way, I would scarcely eat nor sleep, let alone write any letters. Still, I don't want to have you wait again for six months, and therefore write to you right away (well, your letter is of May 27, so it's already three weeks off, sorry!), even if it be but a hasty reply. [...]

I wonder if you couldn't find a less academic title for your Screwtape note, which I find deserves it!

Let me defend myself a bit, about your charge of a "vague, generalized attack" on the poor math community. The offensive lines are on page L 47, whereas on pages 48–50 I am giving, it seems to me, a host of striking facts (involving a substantial portion of that "community") to substantiate the feeling (which is by no means a metaphor, but very real indeed) of a "gangrene". The image that you use of an atmosphere of fear and the Chief of Police isn't very far away from the one which came to me very forcibly. However, the style and tone in that paragraph on page L 47 does have a little too dramatic taste, and it is fortunate that you called my attention to it. I changed it accordingly, but without removing the "gangrene" – sorry, but this feeling of mine is just too forcible not to be expressed . . . As for "remedies", I am afraid to find such is a wholly hopeless undertaking. All one can do is to bring in oneself a breath of fresh air within a generalized stagnation – those who find the stagnation to their taste will be loath of the stir however modest, and some others will feel refreshed. Thus I found your Screwtape notes refreshing indeed, and I am sure others will feel so, too. As for "creating an atmosphere", there is an atmosphere surrounding every one of us, whether we are aware of it or not, and what this atmosphere is like is really where our own personal responsibility lies – and this responsibility is quite enough! Any attempts to try and ameliorate the atmosphere around other people, let alone within a large body of individuals, seem rather futile to me. It is the kind of thing I wouldn't get involved in any more . . .

I did plant those onion bulbs last year, but must have done it the wrong way, none of them got out. The chervil at the end is going to its end, I expect some of it will grow again. Has been fun!

I guess I leave it at that for today. [...]

Yours very affectionately

Alexander

P.S. Thanks, too, for the copy of the letter to Peavey and from Isbell – I found them interesting indeed.

[À cette lettre était joint un rapport de Grothendieck sur les tra-
vaux de Ronald Brown dont une partie présente un intérêt indépendant :

[...]

b) The idea of making systematic use of groupoids (notably, fundamental groupoids
of spaces, based on a given set of basepoints), however evident it may look today,
is to be seen as a significant conceptual advance, which has spread into the most
manifold areas of mathematics. R. Brown's generalization and restatement of
the classical van Kampen theorem is one example, among many. The Gabriel-
Zisman autodual treatment of the basic exact sequence in homotopy theory is
another. In my own work in algebraic geometry, I have made extensive use
of groupoids – the first one being the theory of passage to the quotient by a
"pre-equivalence relation" (which may be viewed as being no more, no less than
a groupoid in the category one is working in, the category of schemes, say),
which at once led me to the notion (nowadays quite popular) of the nerve of a
category. The last time has been in my work on the Teichmüller tower, where
working with a "Teichmüller groupoid" (rather than a Teichmüller group) is a
"must", and part of the very crux of the matter ...

c) The problematic of "higher van Kampen theorems", which for a long time has
been the red thread through R. Brown's work, appears to me as being of basic
significance. I had hit upon this problematic independently in the mid-seventies,
with motivations stemming from a wholly different quarter, as a part of a general
programme of a kind of "topological algebra", viewed as a synthesis of some
of the main intuitions and the main structures (some yet to be worked out)
occurring in homotopy theory, in the theory of n-fold categories and n-fold
"stacks", and in topos theory.

This programme (which I have started pushing through in the volume 1 of
"Pursuing Stacks") has some substantial overlap with R. Brown's. Getting
aware of this was the starting point, in 1982, of a very stimulating correspon-
dence between R. Brown and myself, which has been continuing till now. It is
this correspondence mainly, and the friendly and competent interest of Ronnie
Brown in mathematical ramblings, which was the decisive impetus to take up
again and push ahead some of the old ponderings of mine, materializing in the
writing up of "The Modelizing Story" (the volume alluded to above).

[...]

Montpellier June 4th

Lettre de Ronald Brown à Alexandre Grothendieck, 16.07.1986

16th July 1986

Dear Alexander,

[...]

I haven't done anything more to the Screwtape note, but will probably gently circulate it for the moment as it is. Several things are keeping me pretty busy. My student Harasani has just had his oral examination, with John Isbell and Tim Porter as examiners. John Isbell's report describes the thesis as a "substantial contribution to topology", but we all found that the thesis has some defects of presentation and detail, so Harasani has to modify it and re-present it before he can get a Ph.D. He is able to go back to Saudi Arabia I think reasonably pleased with himself, knowing that the result is pretty certain in a few months' time. I have also got a student Ghafer Mosa, who is working on producing for algebroids [**109**] (*i.e.* categories enriched over R-modules) what Philip Higgins and I have done for groupoids and multiple groupoids. The motivation here is to obtain something that looks like "n-dimensional algebra", with one would like to think eventual applications to algebraic geometry, differential geometry, the kitchen sink, *etc. etc.* In practice, there is a lot of technical work which keeps Mosa pretty busy, and which looks so nice that it should make a good Ph.D. thesis on its own merits. The other student, Aof, is having quite a lot of difficulties in understanding and getting on with the problems set.

I have also agreed to make a revised version of my book [**25**] for a publisher Ellis Horwood, who have American connections with Wiley. The previous proposals with publishers fell down, and the new people want very much a revised version, and so I am just now getting down to this. The revision will enable me to point up the use of groupoids much more so than I could have done in 1967, and in particular I have just found a lovely proof of the description of the fundamental group of an orbit space for a discontinuous group action. It once more shows the benefit of using groupoids! Next week I go to a category theory conference at Cambridge, and in early August I go to the ICM at Berkeley, the first one I have attended since 1958. I am down for a ten minute talk, but it should be an occasion to meet lots of old friends.

It was delightful to read of your new strong involvement with mathematics in your new approach to "topology" and "form", and I look forward very much to hearing more about this. I warn you that I still have your letter on file which says "my machine building days are over", or words to that effect, and shall tease you with it from time to time. I would, incidentally, be interested to know how your position is with regard to the CNRS? I hope that they continue to give you full time to write and communicate in your own way.

The copy of "Pursuing Stacks" will go to Tsuji. Quite a lot of copies have gone out. I am sending out with recent copies also the letters to Breen.

I have at last sorted out an account of Dedecker's theory of Čech non-abelian cohomology of a space with coefficients in a sheaf of crossed modules [**58**], or, more generally, as is convenient, a sheaf of crossed complexes. The point is to say that a cover of a space defines an equivalence relation on the disjoint union of the sets of the cover, and this equivalence relation can be regarded as a groupoid. Any groupoid has a standard crossed resolution, *i.e.* doing what is usual in homological algebra, except using crossed complexes, and doing it for groupoids rather than groups. This standard crossed resolution is defined in the paper "Crossed complexes and non-abelian extensions" by Brown-Higgins [**33**]. So instead of talking about covers and refinements, one talks about these groupoids and their crossed resolutions, and all of Dedecker's formulae seem to come out in a very clear way. I get indications from various current papers that this kind of approach is going to become pretty useful, for example in differential geometry. I suppose one should then work on an étale version, suitable for schemes! More later!

Yours affectionately,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 03.08.1986

August 3, 1986

Dear Ronnie,

Thanks a lot for your letter with the onions. I will plant them in the garden this very evening – I hope they'll have survived the heat. The trouble is, as I am in a meditation period and open and read my mail only every few days, the poor bulbs have remained in the envelope for about two days longer than needed – I feel very stupid now I didn't think taking them out at once. But despite all they seem still alive.

Thanks a lot, too, for sending Pursuing Stacks to Yuichi Tsuji. Just got a letter from him, telling me he got it – must have come over very fast indeed!

Wishing you and your family a good summertime.

Yours affectionately

Alexander

Lettre d'Alexandre Grothendieck à Ronald Brown, 22.10.1986

Les Aumettes Oct 22, 86

Dear Ronnie,

I've been late again to answer to your nice postcard from San Francisco, and your present letter reminds this to me. I've now been meditating for over three months and it may still last a few months more. Got many dreams which keep me busy – a great many among them on the theme of death. Keeps me lively though . . .

Of course you are welcome to quote me as you like it. I'm glad your work keeps making you happy! The same with me, but not the same kind of work, for the time being. I find dreams a lot harder to get into than mathematics, and I feel it requires a lot more "rigour". But it isn't the kind of rigour which consists in keeping carefully to rules.

Your onions finally came beautifully all of them, to my surprise – as I first had neglected them, poor them. But besides contemplating the green leaves, what am I supposed to do with these plants – eat them some way or other? Please pardon my ignorance!

No hurry to read my last yin-yang story. If ever knots, *etc.* leave you the leisure to read it, I'm sure you'll enjoy it – if not, maybe someone else will read it.

Is this an assumption of yours that the stone-agers were skillful with strings, too? At any rate, I've great respect for these people, which surely were not less skillful and less clever than us, and possibly a lot happier! They are the people of the mythical "golden age" I guess – we may be ashamed of them (as many are) or envy them, but surely not return to that age!

Yours affectionately

Alexander

P.S. I got a note from N. Kuiper, telling me (like you) that everybody has been getting so very nice with me. Everybody seems to agree there has been kind of and unfortunate misunderstanding, due to my being "oversensitive to credit" . . . (The latter is a quote from an "awfully nice" letter of Thomason, who claims to have read 90% of Récoltes et Semailles . . .)

Lettre de Ronald Brown à Alexandre Grothendieck, 05.11.1986

5/11/86

Dear Alexander,

It is an absent-minded thing to do to send plants without explanation! Mind you, there is something of Zen about just contemplating their onion-ness. But the usual thing is to treat them like chives, and cut off as much of the stalk as you like, to chop with soups, stews, salads, *etc.* to give a mild onion flavour. You could try them fresh. In the summer the stalks produce at the top many little bulbils which you plant elsewhere. They are very tough, and we throw away many each year. I also sent a lemon balm plant ("citronella"), but it may have not survived. You may have the only garden in the South of France with Welsh Onions (or Tree Onions)!

What the stoneagers did with string is a bit of a deduction – but what else you do with an axehead but tie it to a stick to use as an axe? Since the quality of the bond affects survival, they were likely to get a good idea of tying things together.

Remains of a fishing net were found in Finland in the 1920s, dated 7.250 B.C. The net was 20 metres $\times$ $1\frac{1}{2}$ metres, with a 6cm mesh, made of willow, with bark floats. The knot used was the bowline, standard in fishing nets today. What social organisation and long period of technological development could have led to such an artefact, so far north not so long after the ice ages? I guess fish were a plentiful, easily prepared, form of protein, and methods of catching them could evolve from simple beginnings over a long period of time.

Whether they had time for a "golden age" I don't know. Archaeology tells us that 35 was an old age for that time, that most of their work was done by teenagers. Their scientific urge seems to be shown by the standing stones and circles, which have now been well proven to be associated with astronomy or "celestial" events – it is not too surprising considering the amount of time they must have spent in the open. How can one imagine the dawn of imagination?

It is interesting working with a graphics designer on this knot exhibition, to see how another professional thinks.

We are playing with some ideas on automorphisms. In a monoidal closed category $\mathcal{C}$ (*i.e.* $\otimes$ and HOM exist and are adjoint, $\mathcal{C}(X \otimes Y, Z) \simeq \mathcal{C}(X, \mathrm{HOM}(Y, Z))$) you have an automorphism object $\mathrm{AUT}(X)$ of $X \in \mathrm{Ob}(\mathcal{C})$ with "group structure" $\mathrm{AUT}(X) \otimes \mathrm{AUT}(X) \longrightarrow \mathrm{AUT}(X)$. If $G =$ group, $\mathrm{HOM}(G, G)$ is a groupoid and $\mathrm{AUT}(G)$ is a group in groupoids, *i.e.* a crossed module. If C is a crossed module, $\mathrm{AUT}(C)$ is a crossed module with $\mathrm{AUT}(C) \otimes \mathrm{AUT}(C) \longrightarrow \mathrm{AUT}(C)$, an entertaining structure, and comprehensible, since Brown-Higgins have written down $\otimes$ exactly. So $\mathrm{AUT}(C)$ is a

3-type. So "higher dimensional symmetry" looks like homotopy theory. Maybe I've said this before.

Universities here are in a dismal state with cuts, financial mainly, the order of the day, and lots of time is spent in writing memoranda, and in survival, that is, looking to the future and doing a good job by the students we get, and improving what we do. To say nothing of political infighting necessary to survive or even flourish in an overall shortage of resources.

Yours affectionately,

Ronnie

Lettre de Ronald Brown à Alexandre Grothendieck, 08.04.1987

8 April, 1987

Dear Alexander,

It is a long time since I had written and I thought I ought to keep you up to date. [...]

You may be interested in the new programme that has just been submitted to the EEC for a large sum of money. It took a lot of work to get these four pages written, including visits to Strasbourg and Bonn, but I think it now strikes quite a good balance between the general and the specific. It is interesting that Baues' programme has somewhat converged towards the Brown-Loday direction, and so the aim of combining his very specific methods built up from a very long acquaintance with obstruction theory methods, with the general algebraic techniques of cat^n-groups initiated by Loday and developed with the GVKT (= Generalized Van Kampen Theorem) looks good to me. I hope you don't mind that I have also put you down as a possible referee for this project.

Mathematically I have been somewhat busy with 5 Ph.D.s since last June and also Cordier's Doctorat d'État. Harasani did a thesis under me on "Topos theoretic methods in general topology" [**84**], chiefly on various ways of looking at the possibility of obtaining a truly convenient category of spaces over a given space B. Mosa did a thesis on "Crossed complexes in higher dimensional algebroids" [**109**]. I don't suppose many people will see the point or regard the trouble as worth it. But to me it seems an exploratory step in the direction of "higher dimensional algebra". This has been well and truly justified in the group(oid) case, and every analogy from history suggests that the algebra case should be relevant to wider problems. There is still my old

conceptual problem: granted that an ideal in a polynomial ring $K[X]$ corresponds to an affine algebraic variety, and that a crossed module $\delta : C \to K[X]$ generalises an ideal, what geometric entity does such a crossed module correspond to? In case the question awakes a response, I will put down the formal definition of such a crossed module. The algebra $K[X]$ is to operate on the algebra C [29] such that $c^{a+b} = c^a + c^b$, $c^{ab} = (c^a)^b$, $(c+d)^a = c^a + d^a$, $(cd)^a = c^a d = cd^a$, $\delta(c^a) = (\delta c)a$, $cd = c^{\delta d} = d^{\delta c}$. There are also the usual rules about the operation of the field K.

You will probably find me nagging on this point every so often, which might some-day stimulate me actually to find the answer. It is this kind of question which in my case leads to the search for the right "higher dimensional algebra", and so for some algebraic form of a higher dimensional GVKT in another context than that already found, so leading to a new local-global method in mathematics, and one nicely available in a non-abelian context. I think it is this kind of dream which keeps me happy and writing, but which does not fit too well with the U.K. mathematical climate. This is not just my opinion, as a colleague [...], who originated from Bonn said that he found the climate missing something from what he had at Bonn, a kind of global, questioning viewpoint, I think.

Related to this, I was asked to lead an evening discussion at the British Mathematical Colloquium at St. Andrews last week on "The Public Image of Mathematics". This invitation was occasioned by my circular for a Knot Exhibition we are designing at Bangor, and which mentioned that one of the purposes was to improve the general impression of mathematics, and so get greater support for what we are trying to do. In thinking about how I should stimulate discussion, I designed an examination paper – copy enclosed. What do you think of it? I don't know if you recall the Houyhnhnms – they were the horses in the final chapter of Gulliver's Travels, by Dean Swift, in which the misanthrope Dean imagines what he believes is a picture of an ideal society, where the horses rule, and the role of man is played by the Yahoos, an ugly, monkeylike, servile, but awkward tribe! So I was trying to put over the worry that mathematics courses as at present might have many dubious aspects, and is it possible to do something about it? There was a bit of a shocked silence when I had finished (Ronnie Brown doing his "How to make friends and influence people" act?), but I think some people liked it, and others were not going to be so easily disturbed. At least it might set up a new ripple in the waters.

Part of the problem may also be that all of us find it difficult to state or to formulate the role(s?) of mathematics, and this must lead to some confusion in our teaching and so in the minds of students. The physicist says he is investigating the basic laws of nature; the biologist says he is investigating the facts and laws of life. What then should the mathematician say? A comment from you on this would be truly

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[29] N. Éd. L'algèbre C n'a pas nécessairement un élément unité.

appropriate and treasured. Maybe we should go along with George Boole and say we are investigating the laws (and facts) of thought? It would at least be a slogan with some kind of power and inspiration. At present it would seem that as a general rule there is nothing, although many realise this fascination subconsciously and are thereby drawn to the subject. Much in these ideas accords with the distinctions you make in Récoltes et Semailles, I feel. So once again, I come with the pleasure to what seems to me a common resonance of ideas between us, which has made our correspondence such a pleasure. Of course, I am pretty well aware of how much wider and deeper you have gone than I in this drawing from the dark of new and unsuspected ideas, but am, I won't say content, but at least pleased to have had the opportunity to follow one idea into uncharted territory, and one whose potential, even if generally unrealised, seems to me even greater. It is these coquettish dreams of a "higher dimensional algebra" which are for me the lure behind the specific tasks which I find myself able to formulate and to carry out. And it is the reduction of dreams to technical exercises which is the trend that worries me in the wide run of mathematics courses. How can we get away from this so as to give some inspiration even to the weak and timid? Not too easily, I guess, but I hope that at Bangor we can have some fun trying. There is no doubt we get some weak and timid students; but since my colleagues at other and more notable institutions say the same about their apparently much stronger students, maybe there is hope for all of us yet.

That was a long paragraph. Wordprocessors may lead one astray. It will at least be more readable than my handwriting. And since this package has lots of fonts and symbols, I can happily write some mathematics straight onto the screen.

Are you getting back into mathematics at all? I have a new student, John Shrimpton, who resigned his well paid computer job to take up mathematical research. I thought he needed a project with some kind of risk and breadth, so we are looking at some work of some East German crystallographers who have used groupoids to study what they call *order-disorder* phenomena. We have not yet formulated for ourselves the clear mathematical point behind this, but my belief is that it is related to the problem of studying local-global phenomena for symmetry groups. Intuitively, this is related to the van Kampen theorem, and so we expect it to be properly handled by groupoid methods. Your tantalising remarks on the Teichmüller groupoid are, I hope, another lead. The optimal scenario in this study would be to give methods of attack on the mathematical study of quasi-crystals and other aperiodic phenomena.

We are beginning to get clear about *higher order symmetry,* a speculation which saw the light of day in my Cambridge talk in 1985. I find it helps to fix vague ideas if I talk about them in public, so this is why I depart from the common trend; in any case, mathematical meetings can get pretty boring, as I try to tell people, if you hear only about what has been done, and not about what might be done. But to return

to the topic: the automorphisms of a set form a group. But a set is a 0-type and a group is (corresponds to) a 1-type.

Let us push the analogy. The set $\mathrm{Aut}(G)$ of automorphisms of a group G forms under composition a group. But this group is canonically part of the crossed module $G \to \mathrm{Aut}(G)$. A crossed module corresponds to a 2-type. (Once again I will beg, pray, solicit, treat *etc.* for an application of crossed modules in algebraic geometry. After all, you did claim the Brown-Higgins stuff was not too good because it dealt only with a limited range of homotopy types. But 2 is bigger than 1, and groups deal only with 1-types.) All this is easy and well-known. But the next stage is a technical treat: the automorphisms of a crossed module form part of a structure corresponding to a 3-type. Isn't that nice? This is joint work of Nick Gilbert and me, and independently and by a different route, of Kathy Norrie. We don't yet have the algebra to do the n-th stage. The models that occur here are Conduché's 2-crossed modules. But I am not going to write the structure of those here. It is all quite complicated, but is I believe relevant to non-abelian cohomology, and to the detailed structures developed for analysing homotopy types by Hans Baues.

I think I should finish this letter. Your garden must now be beginning to green. I hope the Welsh onions have been of some value, and that the chervil continuous to proliferate. We just leave the seed heads around to self-sow. Also enclosed is a piece of a bronze fennel. If you nurture it in a pot it might get established. If so, it grows big.

Yours affectionately,

Ronnie

Lettre d'Alexandre Grothendieck à Ronald Brown, 17.04.1987

Les Aumettes April 17.4.1987

Dear Ronnie,

Thanks for your long and thoughtful letter, and please excuse my not reacting for such a long time to your previous ones! One reason is that I have been out of mathematics since July last year, and working very hard meditating on my dreams. This has proved unexpectedly fruitful – as a matter of fact, my view and grasp of things has deeply changed during the few last months. Presumably, I am not going to be back to mathematics at all, certainly not for the next few years at any rate, and accordingly I decided to ask for my retirement next year (when I'll be 60). I may say I got a calling for devoting myself entirely to developing and expounding

a religious-centered view of the soul and the universe, inspired mainly (besides my personal experience and former meditation) by a number of revelations that have come to me through my dreams. I expect to start writing an account of my experience with dreams this very year. After eight months of working on my dreams, I've been for the last three weeks mainly busy with reading, especially the writings of some so-called "mystics", and the Scriptures. It may take me a few weeks or months more, before I sit down and start writing. I'll never will have read so much at a time in my whole life!

So you see I'm less than ever in tune with your own ponderings, very sorry Ronnie! And I doubt at present I'll ever get around reading some of the heap of reprints you sent me, both now as the older ones. I feel the turn that my life has just taken is maybe even more drastic and unexpected than it was in 1970, when I quit the mathematical milieu. What happened now is that, in a way, I am renouncing my own will, trying the very best I can to direct my life according to the will of God and to his particular intentions.

Thus I don't feel in a position responding to the mathematical part of your letter – just too much out of this whole stuff! Your question of a more philosophical nature, as to what mathematics is all about, is more delicate. I doubt it is possible to give a striking one-sentence characterization of mathematics, really hitting some essentials, any more than for physics or biology. The ones you mentioned for these are highly dubious, in my opinion, because they claim a lot more than could be reasonably claimed; and the same for mathematics and Boole's description, because the mathematical form of a thought is a highly specialized and by no means typical one – and anyhow, mathematicians are practically never interested in laws or facts of thought, but in mathematical facts and laws, *i.e.* facts and laws concerning mathematical beings, and not at all "thought". A large part of my reflection "Les Portes sur l'Univers" (an appendix to "La Clef du yin et du yang") is an examination of thought itself, and it is very far from a mathematical reflection (even though it turns out that there are some pretty mathematical structures associated naturally to it). From a metaphysical standpoint, I would say that mathematics is exactly what God didn't have to create, because it was all there from the beginning and He couldn't but take it as it is. (Maybe it may be said that mathematics is just part of God's own nature, namely the part of it to which human reason has access just by its own feeble means . . .) God had an infinite choice about how to build a universe, with its laws (spiritual, physical, biological . . .), and maybe there are many or even infinitely many such, of which we only know (ever so little) one. But whatever way he thinks up his Universe, He's got to use the same mathematics, with $2 + 2 = 4$, and not 3 or 5. It is not in His power to change this, any more than to change his own nature – and surely He never had any wish to do so!

Another way to see mathematics, from a more materialistic point of view, is to say that its role within the laws and structures of the Universe, is similar to the role of the architect's design, with respect to building a house, or to the house actually built. With this proviso, moreover, that mathematics rather stands for the art of the architect, which makes him able to produce appropriate designs for many different kinds of houses and buildings of any kind, including such as have never been built and which will never be. But all this isn't a short one-sentence statement as you are looking for, and I'm afraid I can't provide you with any!

The garden does fine and so do the onions you sent me. The chervil did't prosper too well, but still one leaf I believe came out this spring, I'll see if it grows. But I didn't find that piece of bronze fennel you announced, maybe you forgot to put it into the envelope? I've still a lot of fun with gardening, even though it remains a pretty unrefined kind of gardening, without dedicating too much attention to it. Still, the things grow, and there are plenty of vegetables to eat . . .

Hope you are having fun in your garden, too, and that everything is O.K. From your letter I felt you are in good spirits. [. . .]

Affectionately to you and your family

<div align="center">Alexander</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 23.04.1987

<div align="right">Les Aumettes April 23, 1987</div>

Dear Ronnie,

Today arrived your letter with the plants: bronze fennel, lemon balm, lovage, thanks a lot for the trouble you have been taking! I planted them right away – looked a little tired, but I'll do my best to have them recover. I'll let you know how they do.

Tomorrow I'm making a 24-hour travel to Paris and back, to meet a friend I know for nearly 40 years and didn't meet in the last 15 years. By that occasion, I'll bring, in the long last, the final typescript of Récoltes et Semailles to the publisher. So maybe it'll come out some time this year. (For a while I hadn't been sure whether I was going to publish it or not, and finally decided I would . . .)

Affectionately

<div align="center">Alexander</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 26.05.1988

May 26, 1988

Dear Ronnie,

It was nice hearing from you again. Sorry I have been long not giving any news, and not answering yet to your friendly occasional notes. I am very much involved with work on dreams, and all that goes with it, and almost stopped writing letters, except on urgent practical matters.

I really don't believe that corruption of ethics has much to do with "lack of training" you are referring to – it just doesn't occur on the same level of reality. Even spending a thousand billion dollars on "training" throughout the world, won't heighten the spiritual level by one millimeter. I believe you know this, but under the pressure of current opinion, have a tendency to forget about it. As we all have a tendency to forget what we know and what matters most.

Yours affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 05.08.1988

5th August 1988

Dear Alexander,

I was glad to have your note, and am happy to continue sending occasional notes or cards, with a reply if and when the impulse takes you.

You will not be too surprised to find that some people of high standing are taking the line of "lay off Steenrod". It is irritating and embarrassing because they don't seem to realise that in republishing my book I am writing the historical notes which have to say something explicit and clear without any obvious gnashing of teeth. I have decided to be factual, making the comparison between the papers clear, and saying that: "Colleagues inform me that a reading of [St] [**123**] left them unaware that the words 'convenient category', and the principal properties of such an object, were already described in [Brown 2] [**18**]." I am also mentioning the fact that the Michigan Math. J. refused to publish a correction, because of their policy to publish no corrections of any sort.

By contrast, G. Chogoshvili, from Tbilisi, now in his 70s, wrote that the correspondence with the above journal was a "remarkable document", and that he was now not

so surprised that Steenrod did not refer to papers of Kolmogorov although ideas must have been known to Steenrod when he wrote on regular cycles and the homology of metric spaces. Perhaps someone from the Soviet Union is more sensitive to aspects of totalitarianism than people from the West who assume we are "free". Another aspect is that it says something of the underlying and not so apparent character of Steenrod that he can leave a legacy of dissension, as a kind of time bomb.

I found it difficult to get over clearly a whole point of view on the back of a picture postcard! I was thinking of several aspects. One is that there is little discussion of ethics in mathematical training because of the implicit assumption, I suppose, that ethical discussion is not required. Indeed, there is little discussion of the methodology of mathematical research, which is rather pathetic, and confusing to young people starting in research. At least, it was confusing to me. Thus, attitudes and behaviours tend to build up implicitly without discussion of issues and without analysis. Of course, even if there is discussion of issues, it does not follow that people will behave ethically, as is shown on many occasions in medicine, where the ethical side is well reported and is much more crucial in its outcome.

Another aspect behind my remarks on the card was my own experience with my mentally handicapped child Adrian. We were very fortunate in meeting two experienced workers in the field of mental handicap who were able to communicate a positive attitude and to discuss the issues, facts, principles and behaviours involved in coping with this situation. This reinforces my approach that sound knowledge is an important factor in any difficult kind of situation. Such knowledge might be intuited, but for those who are confused, and whose behaviour is thus unpredictable, the contact with a firm basis of knowledge can be an enormous relief, a rock on which to rest.

In my discussing with research students I thus spend a lot of time discussing the methodology and rationale of mathematical research. This is what is nowadays called the "top down" approach, as contrasted to the "bottom up". In the end, both are needed. But to get back to the main point, I am interested in how attitudes and realistic forms of behaviour can be built up through pressures and discussion. Conversely, negative behaviours can be unwittingly built up if their existence it not recognised, and the pressures which lead to them are not understood. (Such negative behaviours and attitudes did build up as a result of our confusion with regard to Adrian, but changed dramatically when we were given the right lead.) It is the unwillingness of the mathematical community to discuss openly such happenings as those we have been concerned with that must be a block to progress, since there is then no basis on which to start analysing the reasons for such behaviour, nor to develop the attitudes and insights which are required to ensure these behaviours are less likely. I realise you may find this philosophy unduly behaviouristic, but it is pragmatic and does suggest a guide to action.

There are many factors which lead to this grab-grab behaviour. One is the fact that truth is seen as a prize in itself, rather than the search for truth, and men will fight over prizes. Another is the dependence of careers on the value of results obtained. But I wonder if another factor is not the dehumanisation of mathematics, apparent in vast runs of textbooks and monographs, and concomitant with the belief in a final view of a piece of mathematics, as in the Bourbaki monument. Thus our training of mathematics totally leaves out history and the idea of scholarship, so destroying a sense of tradition. (I know Bourbaki has excellent historical commentaries, but it is history as leading to the present pinnacle, with no way forward. There is no sense of new worlds awaiting discovery.)

However, to some extent I agree with you, in that what we have talked about occurs among those who should know better. It is quite standard for the great and powerful to suggest that "Steenrod's friends will rise in his defence", for example. We shall see what they can do. It reminds me of a bit of Shakespeare:

> Glendower: "I can call the devils from the deep!"
> Hotspur: "Aye, but will they come?"

But let us leave all these matters, and I don't expect a reply. All I am saying is that young people absorb the attitudes of the seniors around them, and I am still a maverick at odds to an extent with much of the attitudes and methods general in our time. But I find much sympathy in the younger generation, perhaps more among those not brought up at our top centres!

Let me go on to a point on which I would like a reply. Bill Lawvere and I were discussing *Pursuing Stacks* and the likelihood of your making it generally available. I get requests for it every so often, but the duplicating singly is expensive (our department is under lots of financial pressures) and we both felt that the material should be more readily available. Would you like, or be willing, for us to prepare a retyped job, with possibly some editing to remove repetition, and make it available at cost price? Or would you like a proper publishing job? There are many mathematicians who would greatly welcome any way of getting some idea of your insights, not only in Pursuing Stacks, but also in some of your other material, such as the "Long March through Galois Theory" [**81**], and also the new ideas on topology and form to which you referred briefly in a letter. Would it be sensible and possible, say, to let Bill Lawvere loose on copies of *any* notes and jottings you have?

Yours affectionately,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 09.08.1988

Les Aumettes 9.8.1988

Dear Ronnie,

I appreciate your thoughtful letter, arriving today, allowing me (among other things) a glimpse into the sound and well digested experience of life, and of teaching, behind the somewhat casual-looking comments of your previous postcard. And please excuse my somewhat hasty (and maybe schoolmasterly) reply!

For the last four months all my energy is being absorbed in writing up the plentiful dreams that come during night, and doing my best to get the message from some of them – a rich harvest again, not any less than last year. I don't know how much longer this will last still, but while it lasts, my correspondence and my involvement with anything else is reduced to a minimum (near to zero). That's why this reply is going to be rather short, and provisional. Mainly to thank you and Bill Lawvere for your help so far, and for the suggestion about Pursuing Stacks. To take any sensible decision in this respect I will have to pause and think about it a little, which I have no leisure to do at present. When I am under less pressure, however (presumably within a few months), I'll contact you again and give you (I hope) some clear answer, as for what to do with Pursuing Stacks. At any rate (as is already clear to you, it seems) I am about sure *not* to find within the next 10 or 15 years the few weeks needed to put the present manuscript into publishable shape (with foreword, putting it into perspective and footnotes with retrospective comments and corrections of errors, among which a pretty big one . . .).

As for "letting Bill Lawvere loose on *any* notes and jottings", I am afraid it wouldn't make much sense, as for most of these, I'm afraid, nobody except myself will be able to make a sense out of them (even without mentioning the difficulty of deciphering the handwriting). Also, the "Long March through Galois Theory"-notes (of 1981) [**81**] are very much behind the ponderings (e.g. on the Teichmüller-Lego game) that grew out of them, and at any rate wouldn't make much sense to someone who isn't fairly familiar with the general yoga of arithmetical geometry (as developed by me between 1958 and 1970, say). On their request, I sent a copy (nearly 2000 pages, a big secretarial work to get a decent copy out of overcrowded pages . . .) to the Chudnovsky brothers (Columbia University), who never so much as sent me notice they got it, despite my inquiry later. (This was soon after Reaping and Sowing was sent out . . .) These notes are the first systematic reflection on the ties between $\mathrm{Gal}(\bar{\mathbf{Q}}/\mathbf{Q})$ and non-abelian fundamental groups of typical algebraic varieties, such as $\mathbf{P}^1 \smallsetminus \{\text{three points}\}$. If

Lawvere is interested to look through it, maybe David Chudnovsky would be willing to send him the heap – it is certainly O.K. with me.

So long – I'll write again as soon as I can manage.

Affectionately

<div align="center">Alexander</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 29.01.1989

<div align="right">Les Aumettes Jan 29, 1989</div>

Dear Ronnie,

Sorry I am so very late in replying to your and Lawvere's suggestion concerning a provisional small-circulation edition of "Pursuing Stacks" or rather of the very limited portion of it (essentially "The Modelizing Story") which I finally wrote down in 1983. The previous three or four months have been still denser than usual, and I practically didn't have a minute to think of anything else but what was occurring in connection with my dream-work. Now it has quietened down somewhat, and I am in a position to clear away by and by the impressive heap of correspondence which has accumulated in the meanwhile. Please accept first of all my very hearty wishes for a happy New Year for you and your wife and family. And, by the same occasion, my thanks for the help and encouragement you have kept giving me over the years, mathematically as otherwise.

Every once in a while I still get demands for a copy of Pursuing Stacks, and it seems to me that Lawvere's offer should be quite helpful. He is probably the mathematician best suited for the task, and all the more so if, as I do hope, he should feel like adding editorial comments of his own. At any rate, it has become clear for more than two years now that it is out of question that I may still find in my life the time (a month or two, I guess) to make "The Modelizing Story" ready for publication in book form. My life has definitively taken a wholly different direction! Thus I am quite grateful for Bill Lawvere's offer. My only condition for accepting is that, except possibly for minor corrections in orthography and English grammar (when these are obviously violated), no change or deletion should be made of the notes I wrote. I know it is a rather peculiar way of writing mathematics (or anything else, for that matter), but that's how I did it (and still do it, although not in mathematics now), and people have to take it or leave it. Of course, there are numerous awkwardnesses and even coarse errors in the course of the exposition, but nearly all of them smooth out as the

reflection proceeds. My intention was, of course, to point out such cases, as they occur, in footnotes, referring to later places where the correction or smoothing out occurs. The editor is of course welcome to do so wherever he thinks fit, and to add whatever mathematical comments may occur to him. Also, I intended to put into focus the main ideas and results developed in The Modelizing Story, and give a hint of those still to come in the two other planned volumes, in a mathematical introduction, which was and will never be written (at any rate not by me). If Lawvere feels like it, he may add a mathematical introduction, including whatever comments and ideas of his own fit in that place, besides possible editorial footnotes within the text. Moreover, it may be a good idea that I write a short foreword (of half a page), to thank you and Lawvere for your help with Pursuing Stacks, and explain in a few words the rather particular fate of this work of mine.

I remember, Ronnie, that you once felt uncomfortable about my mentioning in the course of my ponderings the event of the death of your son Gabriel, which I just knew from you. If you still prefer this passage to be deleted, this is of course O.K. with me.

If Lawvere feels like going on with the project, after this letter, maybe it is simpler for him to contact me directly. I am not sure I know his present address. You are welcome to send him a copy of this letter, so he should know first hand how I feel about this project. By the way, it had been my intention to add to the planned book a fair number of appendices, namely ponderings of mine (e.g. some letters to Larry Breen) closely connected with the overall philosophy of "Pursuing Stacks". If Lawvere feels like including some such material, we may decide together, which.

Looking forward to hearing from you again,

Yours very affectionately

<div style="text-align:center">Alexander</div>

Lettre d'Alexandre Grothendieck à Ronald Brown, 24.07.1989

<div style="text-align:right">Les Aumettes July 24, 1989</div>

Dear Ronnie,

I just read again your two substantial letters of March 14 and April 12, and took the pleasure, too, to look through the preface of the reedition of your topology book [**25**]. Some health troubles (not durable, I think) have forced me to take some leisure, and reminded me of letters I had to answer, some for quite a long time. Since Easter 1988, rhythm and speed of inner events have become even more extravagant, and I couldn't

but follow as best I could. Now at long last there has been a break, for about a week or so I've been taking breath again, although the state of suspense is greater than after this crazy sequence of up-and-downs and coups de théâtre, unceasing?

Mathematics look very remote, nearly all of the time. But yesterday I had the visit of Bill, with an Italian colleague, which brought back to my mind (now in a provisional state of leisure) some of the things I had been pondering about. I'm sorry Bill and his friend must have been bewildered and disappointed, as I had just started on a 33 days period of total silence, and didn't open the mouth during the two hours or so of their stay. Bill did the talking, and I commented in writing, as best I could. He seemed to want to know what "Pursuing Stacks" was all about, and I am not sure he knows it now, still less that he feels encouraged spending time on an edition as planned. I told him I wouldn't do more than write a short foreword, mainly acknowledgement to you and him – and leave everything else to him! It may have sounded cynical – but I just have no time to spend on maths any more, that's all . . .

Of course, this letter is no adequate response to your friendly and interesting letters, as usual alas! I am glad to see you keep going heartful and unabashed with your 1001 manifold projects and tasks: the one man I know, really, who seems to me up to the challenge to be spiritually of use, as a mathematician and a sentient being, to his fellow beings. Quite an achievement indeed. Poor as my response to your innumerable creative suggestions has been so far, alas!, I still have an inkling that maybe, within the following years, your tasks and mine might join, and draw us together. (While so far, the opposite could seem to be true.) Incredible things are going to happen, Ronnie, to every single soul on earth, before the end of this century – and we'll both be around to take part in it, I'm sure. But for the time being I won't say more on this tremendous, burning topic.

Except, though, that I would like to recommend you a book, quite relevant to what I am alluding to, maybe even the one book I know of most relevance to it of all. It is *"Flight into Freedom"*, the autobiography of Eileen Caddy, one of the three founders of the "Findhorn Community", you may have heard of. The only one book of similar significance I know, which may be viewed as complementary to the previous one, is *"The Findhoven Garden"*. I am ordering the two books for you at the London bookshop, "Compendium", which has been very helpful for the last two years to provide me with various English and American titles. You should get them within a few weeks, and I hope you'll find time to read them, and appreciate them. I discovered these books only this year, in January and May, and have been quite fascinated and overwhelmingly grateful at once, as they are so very closely connected to the revelation I had through dreams, and since through direct communications. The world is a lot wider and more mysterious than ever I had dreamed only two or three years ago . . .

Thank you very much, too, for the photographs of the two beautiful sculptures of John Robinson. Sorry I've been so long in responding to your friendliness and kindness!

Yours very affectionately

Alexander

Lettre d'Alexandre Grothendieck à Ronald Brown, 12.09.1989

Les Aumettes le 12.9.89

Dear Ronnie,

Thanks for your friendly letter, and for the beautiful booklet on John Robinson's sculptures, which I immediately read and looked through. I'm very glad, too, that the two Findhoven books have caused a lively response in you. What you say about the "ordinariness" of Eileen has to be taken with a grain of salt, though. One thing is that her book is also the story of an amazing spiritual growth within the space of one lifetime; it bears testimony to the fact that all of us, who are just "ordinary" people (put apart intellectual superficialities) as she was in her young age, we are all bound for the very highest spiritual destiny. Second, there were evidently very precious gifts dormant in her from the very start, but of quite a different order from mental brilliancy or originality, which are the ones people generally have in mind when speaking about someone being "ordinary" or "outstanding". I'm pretty sure that in God's eyes, Eileen from the very start was by no means "ordinary"; and at times the "guidance" states very clearly that she has been preparing [30]

Lettre d'Alexandre Grothendieck à Ronald Brown, 09.04.1991

Les Aumettes April 9, 1991

Dear Ronnie,

As always, it was a pleasure to get a lifesign from you, and the beautiful picture with the severe and serene landscape around Iona Abbey. I am glad, too, all seems to be well with you and Margaret and the tribe. As for me, health and spirit in best shape. One news is that for the last five months, I've taken up some maths again,

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[30] N. Éd. Le reste de la lettre manque.

which I had dropped totally during four full years. I've set out to develop the program of "topological algebra" (as I like to call it) which I told you about here and there, and which I had started on with Pursuing Stacks – but without outlining there the overall program, except in a scattered way by bribes [31] and bits. (As I thought I would do the work by myself.) I made pretty fast headway and things are steadily taking shape. But the work still ahead appears more extensive as work progresses. And I doubt there will be time enough left for me to get much further than now, as I expect that events (this time not unforeseen) will put an end soon to my mathematical (and badly needed!) vacations. If not here, I trust I'll carry it on beyond!

Affectionately as ever to you and Margaret,

your

<div align="center">Alexander</div>

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[31] N. Éd. "Bribes" est le mot français pour "pieces".

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