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

Alexander Grothendieck

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

15th April, 1982.

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 $\mathrm{H}^0(X; C)$ 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 $K(\pi, 1)$-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 $\hat{S}_*$. 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

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

Lettre de Ronald Brown à Alexandre Grothendieck, 24.05.1982

<div align="right">24th May, 1982.</div>

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 $H^0(X, C) = [\pi\underline{X}, C]$, 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 $H^n(X, C) = \pi_n(\underline{Hom}(\pi\underline{X}, C))$. 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 non-abelian 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 non-commutative 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 $\mathrm{C}_*(G)$ 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 $\int_I$