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 "`[...]`".

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

(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

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 06.02.1984

6/2/84

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 $G_{\bullet\dots\bullet}$ each of whose Moore complexes in direction r,
$$G_{i_1,\dots,\underset{r}{\bullet},\dots,i_n}\ ,$$
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_*(fibre)), but there seems quite a gap between that and understanding even monodromy on π_1(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

$$\begin{tikzcd} \mathbf{Z} \arrow[r, "0"] \arrow[d, "0"'] & \mathbf{Z} \arrow[d, "1"] \\ \mathbf{Z} \arrow[r, "1"'] & \mathbf{Z} \end{tikzcd},$$

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

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

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

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