

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. Küntzer, 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. Küntzer, 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, 15.02.1986

16 December 1985, posted 15/2/86

Dear Alexander,

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

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

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

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

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

$$\bar{X}_\alpha \longrightarrow \lim_{\sigma > \alpha} \bar{X}_\sigma$$

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

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

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

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

$$\pi_3 \text{SK}(G, 1) = \text{Kern}(G \otimes G \xrightarrow{[\cdot, \cdot]} G) \quad (= J_2 G, \text{ say})$$

and the exact sequence

$$H_3G \longrightarrow \Gamma G^{\text{ab}} \longrightarrow J_2G \longrightarrow H_2G \longrightarrow 0,$$

where Γ is Whitehead's universal quadratic functor. In view of the profound influence H_2G has had, I think we can expect quite an impact of J_2G . Also, calculations in $G \otimes G$ are fun! What's more, $G \otimes G$ occurs in embryonic form in algebraic K-theory, cyclic homology, ..., so we just need an impact in algebraic geometry. For this we might need a localised form – work under consideration!

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

19 Dec.

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

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

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

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

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

1/2/1986

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

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

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

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

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

Lettre de Ronald Brown à Alexandre Grothendieck, 01.05.1986

1st May, 1986

Dear Alexander,

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

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

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

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

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

- 1) There are lots of mathematicians with whom I get on well, whose company I enjoy, and who behave with care and probity (I would not describe this as an intersection or as a union!). So a vague, generalised attack on mathematicians is not helpful. As a class, I don't expect they are particularly worse or better than other similar classes.
- 2) The pressures and influences which lead to lapses, and lack of help to young people, are worth analysing. One may smile at Hilbert's comment "You read too much literature!" when a research student told Hilbert that his dissertation results had been published – but such remarks are remembered, and used, by lesser people. Very often, the overall tone is set by those at the top. Obviously, I'm influenced by Henry Whitehead and Michael Barratt, great ones for talking in pubs – that is a matter of luck, and I hope it has helped me. An overall aim should be to forward the subject as a whole, by making it easier for young people to develop and flourish. Reputations of oldsters are not so important.

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

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

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

Yours affectionately,

R. Brown

Lettre de Ronald Brown à Alexandre Grothendieck, 01.05.1986

1/5/86

Dear Alexander,

[...]

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

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

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

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

Yours affectionately,

Ronnie

Lettre d’Alexandre Grothendieck à Ronald Brown, 05.05.1986

Les Aumettes May 5, 1986

Dear Ronnie,

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

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

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

[...]

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

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

⁽²⁸⁾ N. Éd. L. Infeld, *Whom the Gods love*.

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

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

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

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

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

Yours affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 27.05.1986

27th May, 1986

Dear Alexander,

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

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

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

[...]

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

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

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

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

Yours very affectionately,

R. Brown

Lettre d'Alexandre Grothendieck à Ronald Brown, 17.06.1986

Mormoiron 17.6.1986

Dear Ronnie,

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

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

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

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

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

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

Yours very affectionately

Alexander

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

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

[...]

- b) The idea of making systematic use of groupoids (notably, fundamental groupoids of spaces, based on a given set of basepoints), however evident it may look today, is to be seen as a significant conceptual advance, which has spread into the most manifold areas of mathematics. R. Brown's generalization and restatement of the classical van Kampen theorem is one example, among many. The Gabriel-Zisman autodual treatment of the basic exact sequence in homotopy theory is another. In my own work in algebraic geometry, I have made extensive use of groupoids – the first one being the theory of passage to the quotient by a “pre-equivalence relation” (which may be viewed as being no more, no less than a groupoid in the category one is working in, the category of schemes, say), which at once led me to the notion (nowadays quite popular) of the nerve of a category. The last time has been in my work on the Teichmüller tower, where working with a “Teichmüller groupoid” (rather than a Teichmüller group) is a “must”, and part of the very crux of the matter...
- c) The problematic of “higher van Kampen theorems”, which for a long time has been the red thread through R. Brown's work, appears to me as being of basic significance. I had hit upon this problematic independently in the mid-seventies, with motivations stemming from a wholly different quarter, as a part of a general programme of a kind of “topological algebra”, viewed as a synthesis of some of the main intuitions and the main structures (some yet to be worked out) occurring in homotopy theory, in the theory of n -fold categories and n -fold “stacks”, and in topos theory.

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

[...]

Montpellier June 4th

Lettre de Ronald Brown à Alexandre Grothendieck, 16.07.1986

16th July 1986

Dear Alexander,

[...]

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

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

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

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

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

Yours affectionately,

R. Brown

Lettre d’Alexandre Grothendieck à Ronald Brown, 03.08.1986

August 3, 1986

Dear Ronnie,

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

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

Wishing you and your family a good summertime.

Yours affectionately

Alexander

Lettre d'Alexandre Grothendieck à Ronald Brown, 22.10.1986

Les Aumettes Oct 22, 86

Dear Ronnie,

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

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

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

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

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

Yours affectionately

Alexander

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

Lettre de Ronald Brown à Alexandre Grothendieck, 05.11.1986

5/11/86

Dear Alexander,

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

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

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

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

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

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

3-type. So “higher dimensional symmetry” looks like homotopy theory. Maybe I’ve said this before.

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

Yours affectionately,

Ronnie

Lettre de Ronald Brown à Alexandre Grothendieck, 08.04.1987

8 April, 1987

Dear Alexander,

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

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

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