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 "`[...]`".

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

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

Yours very affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 01.01.1985

28/12/84 – 1/1/85

Dear Alexander,

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

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

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

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

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

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

but it may take quite a time to find.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Yours very affectionately,

Ronnie

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

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

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

Ronnie

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

R.

Lettre d'Alexandre Grothendieck à Ronald Brown, 12.01.1985

12.1.1985

Dear Ronnie,

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

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

Yours very affectionately

Alexander

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

Lettre d'Alexandre Grothendieck à Ronald Brown, 02.03.1985

Les Aumettes 2.3.1985

Dear Ronnie,

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

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

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

Affectionately

Alexander

Lettre de Ronald Brown à Alexandre Grothendieck, 17.10.1985

17/10/85

Dear Alexander,

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

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(26) N. Éd. C.à.d. "inconscience" en français, "inadvertence" en anglais.

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

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

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

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

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

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

Yours affectionately,

Ronnie

Lettre d’Alexandre Grothendieck à Ronald Brown, 22.12.1985

Les Aumettes Dec 22, 1985

Dear Ronnie,

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

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

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

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

Affectionately

Alexander