APPENDIX to Chapter I : three letters to Larry Breen

In this appendix, I am encluding three letters to Larry Breen, dated 5.2 , 17.2 and 17-19.7. 1975. These letters are written in French, and the first two are reproduced textually, whereas the third appears here in English translation. I am grateful to Ronnie Brown who took the trouble last year to make such a translation (from a hardly legible ~~handwri~~ copy of the handwritten letter to Larry Breen) with the assistance of Larry Breen himself and J.L. Loday. I am now using his translation rather than the original letter, which no printer could possibly decipher ! Also, for the second letter I am using a typed copy made in 1975 by Larry Breen (who presumably had difficulties too deciphering the handwriting). Thanks are due to him for his interest and patience (with someone like me, very unknowledgeable in standard homotopy techniques), which ~~about~~ appeared in ~~from~~ the letters I got from him in response and his verbal explanations on related matters, as well as ~~from~~ in the trouble he took in ~~copying~~ retyping that letter, and sending me copies of my own letters to him, and allowing me to reproduce them in this volume of Pursuing Stacks. My only present contribution to this set of letters is adding a few comments (in the notes (22) to (31)), adding subtitles (with numbers App. 1 to App. 18), and correcting some ~~and correcting some~~ inaccuracies in the English translation (due mainly to my handwriting ...). Also, I skipped the beginning of the first letter, which does'nt seem ~~to me~~ of general relevance.

The first two letters are an attempt to explain to Larry Breen (who has a wide background in algebraic geometry and homological and homotopical algebra) some of the main points of the programme I had in mind around the notions of n-categories and n-stacks (which is what I am supposed to be pursuing now in my present work "Pursuing Stacks"). They were written under the impetus of ~~the~~ new intuition ~~which~~ (new to me at any rate) which then had just appeared to me, namely that (non strict) n-groupoids should model (in a suitable sense) n-truncated homotopy types.

The third letter, written in answer to a number of questions in Larry Breen's response to the first two, is of a wider scope. A large part of the letter outlines (very sketchyly) some main points of a duality program (including a cohomological formulation of "geometric" local and global classfield theory), which ~~appears~~ emerged by the end of the fifties and appears here for the first time in print, ~~and going back to the end of the fifties.~~ The later part of the letter ~~It~~ gives also some hints about the need of a framework of "tame topology" suitable for writing up a "dévissage theory" of stratified spaces, and for ~~realizing~~ working with étale tubular neighbourhoods, for the common purpose of coming to grips with ~~of~~ a suitable notion of "fine homotopy type" of a "tame" topological space or of a scheme, say, in terms of the inverse system ~~ordered set~~ of "indexed homotopy types" corresponding to all equisingular stratifications .

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NOTES to Chapter I and Appendix.

~~Chapter I~~

(1) ~~The most unexpected~~ When making this suggestion about there being a "wind of disrepute for any foundational matters whatever", I little suspected that the former friend to whom I was ~~ask~~ communicating my ponderings as they came, would take care of providing a most unexpected confirmation. As a matter of fact, this letter never got an answer, nor was it even read! Upon my inquiry nearly one year later, this colleague appeared sincerely ~~surprised~~ that I could have expected even for a minute that he might possibly read a letter of mine on mathematical matters, well knowing the kind of "general nonsense" mathematics that was to be expected from me ...

(2) These letters are reproduced as an "appendix" ~~at the end of this chapter~~.

(3) For some particulars about a program of "tame topology", I refer to "Esquisse d'un Programme", sections 5 and 6, which is included in Réflexions Mathématiques 1 .

(4) I have to apologize for this rash statement, as later correspondance made me realize that "Ronnie Brown and his friends" do have stronger contact with "geometry" than I suspected, even though they are not too familiar with algebraic geometry!

(5) The "Bangor group" is made up by Ronnie Brown and Tim Porter as the two fixed points, and a number of devoted research students. Moreover Ronnie Brown is working in close contact with J.L. Loday and J. Pradines in France.

(6) Definitely only for strict associativity.

(7) This idea is taken up again in section 12. The statement made here is a little rash, ~~and the si as well as~~ as for existence and uniqueness (in a suitable sense) of this functor. Compare note (17) below.

(8) Another important example is the structure of a "torsor" under a group G (torsor = principal homogeneous space). When this group G is fixed, the corresponding classifying topos B_G is the natural purely algebraic substitute for the familiar "classifying space" for the discrete group G.

(9) Such a theory was developped in a seminar I gave at Buffalo in 1973.

(10) That $\frac{1}{2}$ is of order 2 is heuristically clear, but will require a proof none the less !

(11) (Added 23.2.83) I do'nt believe it now any more - and I do not really care - compare comments in section 11.

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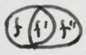
(12!) This is nonsense, as one sees already in the following picture where $f \cap f'' = S_o$:


(13!) This is nonsense again for $n \geq 1$, see PS at the end of section 13. Even this PS is still unaccurate, compare comments section 18.

(14!) Same mistake as ~~in~~ the one noticed in the previous note. The fibered products exist in $\underline{C}_\bullet$ only, and these should be preserved by the functors under consideration. Thus the "universal problem" ~~for~~ has to be rephrased somewhat ...

(15) I guess they are not equivalent, even when restricting to objects which are groupoids (i.e. so-called Gr-categories).

(16) An object of M is called fibrant (resp. trivial) it the map from it to the final object is fibrant (resp. a weak equivalence).

(17) This seems doubtful, unless all objects of M are fibrant (which is true for the most familiar cases I was having in mind). But even assuming this, we still need a unicity statement (in a suitable sense) for the functors $\underline{B}_o \to M$ thus obtained, in order to be sure that the corresponding functors $M \to (Ho)$ are all canonically isomorphic. These kind of questions may be viewed as closely related to the question of existence (and unicity) of "test functors" for a given test category A (here, the category of "standard hemispheres") into a given asphericity or contractibility structure, as discussed in section 90 (without yet getting there ~~the~~ any clear-cut handy existence and unicity theorems ~~which are~~ as are to be ~~hoped~~ hoped for). (In my notes, the set-up of asphericity and contractibility structures, which has been worked out in the months following, has been gradually replacing Quillen's approach to homotopy models. (I'll have to come back in due course to the question of the relationship between the two approaches, which deserves to be understood.)

(18) The word "gerb" here stands ~~for~~ as a "translation" of the French word "gerbe", as used in Giraud's book on non commutative cohomological algebra. With the terminology of next section, we would call it rather a "stack of groupoids" or "Gr-stack" (with a specification by n or ∞ if needed !).

(19!) This is false, as seen below.

(20!) This description is dubious, as it may give categories $\underline{C}_n$ which are larger than the ones we want.

(21!) It seems dubious however that the mere category structure of $\underline{C}$ will allow us to recover the ~~the~~ "primitive" subcategory $\underline{C}_o$, and it looks safer to add the latter as an extra structure to $\underline{C}$.

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(22) For the notion of a "lien" (or "tie"), which is one of the main ingredients of the non commutative cohomology panoply of Giraud's theory, I refer to his book (Springer, Grundlehren 179, 1971). A Picard category is a groupoid endowed with an operation ⊗, together with associativity, unity and commutativity data for this operation, which make it resemble to a commutative group. A "Champ de Picard" (or "Picard stack") is defined accordingly, by relativizing over an arbitrary space or topos (replacing the groupoid by a stack of groupoids over this topos). The necessary "general nonsense" on these notions is developped rather carefully in an exposé of Deligne in SGA 4 (SGA 4 XVIII 1.4). In this letter to Larry Breen, I am assuming "known" the notion of an n-stack (for $n \leq 3$ at any rate), and the corresponding notion of (strict) Picard n-stack, which should be describable (as was explained in Deligne's notes in the case n=1) by an n-truncated chain complex in the category of abelian sheaves on X (viewed mainly as an object of the relevant derived category). The "strictness" condition on usual Picard stacks refers to the restriction that the commutativity isomorphism within an object $L \otimes L'$, when $L=L'$, should reduce to the identity. It is assumed (without further explanation) that the condition carries over in a natural way to Picard n-stacks, in such a way as to allow an interpretation of these by truncated objects in a suitable derived category, as hinted above.

(23) When M is any abelian sheaf on a topos, the "Mac-Lane resolution" X(M) is a certain canonical left resolution of M by sheaves of $\underline{Z}$-modules which are "free", and more specifically, which are finite direct sums of sheaves of the type $\underline{Z}^{(T)}$, where T is any sheaf of the type M^n (finite product of copies of M). This canonical construction was introduced by MacLane (for abelian groups), and gained new popularity in the French school of algebraic geometry and homological algebra in the late sixties, because it gives a very handy way to relate the $\mathrm{Ext}^i(M,N)$ invariants (when N is another abelian sheaf on X) to the "spacial" cohomology of M (i.e. of the induced topos $X_{/M}$) with coefficients in N.

(24) Reflecting on the "right" version of the provisional Verdier notion of a triangulated category (which was supposed to describe adequately the relevant internal structure of the derived categories of abelian categories) is part of my present program for the notes on Pursuing Stacks, and will be the main task in one of the chapters of volume two. For some indications along these lines, see also section 69 (sketching the basic notion of a "derivator").

(25) As was seen in section 9, "uniqueness" here has to be understood in

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a considerably wider sense than I expected, when writing this letter to Larry Breen. It now appears that the whole theory of stacks of groupoids will depend on the choice of a "coherator" C , as seen in section 13.

(26) Tim Porter pointed out to me that "Dold Puppe" is an in accurate name for this basic theorem, which should be called Dold-Kan theorem.

(27) As was pointed out to me by Ronnie Brown, this structure was already well-known to J.H.C. Whitehead, under the name of "crossed modules", and extensive use and extensive generalizations of this notion (in quite different directions from those I was having in mind, in terms of Gr-stacks over an arbitrary topos) have been made by him and others. With respect to the question on next page, of generalizing this notion of "non commuative chain complex" from length one to length two, Ronnie says there is a work in preparation by D. Conduché " Modules croisés généralisés de longueur 2 " .

(28) Tim Porter pointed out to me that work on étale tubular neighbourhoods was done by D.A. Cox ; "Algebraic tubular neighbourhoods I,II', Math. Scand. 42 (1978) 211-228, 229-242. I've not seen yet this work, and can't say therefore whether it meets the rather precise expectations I have for a theory of tubular neighbourhoods, for the needs of a dévissage theory of stratified schemes (or, more generally, stratified topoi).

(29) This is the typical game ~~taxbe~~ embodied in the "derivator" associated to the theory (Hot) of usual homotopy types (compare section 69).

(30) Some more details on this program are outlined in "Esquisse d'un Programme" (section 5), in Réflexions Mathématiques 1 .

(31) I was informed by knowledgeable people soon later that the answer is well known to be negative, by working with "rational homotopy types" (the cohomology of which is made up with vector spaces over Q). It is well known indeed that a 1-connected rational homotopy type is not known from it's rational cohomology ring alone, which ~~is supposed (I confess I did'nt check) to~~ contains already all the information I was contemplating (*)

(32) This ~~is the non abelian version of the similar problem~~ "problem" is met with by the notion of a "derivator" , which "was in the air" already by the late sixties, but was never developped ~~at~~ (instead even derived categories became tabu in the seventies ...). Compare with note (24).

(*) At least (this is so) if we assume that ~~the~~ $H^i(X)$ is of finite dimension over Q for all i. But is there a counterexample still when X is a homotopy type "of finite type" ?

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