L E T T E R S

(*Collection*)

A. Grothendieck

This edition is a collection of letters of A. Grothendieck reunited by Mateo Carmona. Remarks, comments, and corrections are welcome.
`https://agrothendieck.github.io/`

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Contents

1950

1951

1953

1955

1956

1957

1958

1959

1960

1961

1962

1963

1964

1965

1966

6.1.1966

Dear Coates,

1 Here a few more comments to my talk on the conjectures. The following proposition shows that the conjecture $C_\ell(X)$ is independent of the chosen polarisation, and has also some extra interest, in showing the part played by the fact that $\mathrm{H}^i(X)$ should be "motive-theoretically" isomorphic to its natural dual $\mathrm{H}^{2n-i}(X)$ (as usual, I drop the twist for simplicity). *Letter to J. Coates, 6.1.1966*

Proposition. — *The condition $C_\ell(X)$ is equivalent also to each of the following conditions:*

a) *$D_\ell(X)$ holds, and for every $i < n$, there exists an isomorphism $\mathrm{H}^{2n-i}(X) \to \mathrm{H}^i(X)$ which is algebraic (i.e. induced by an algebraic correspondence class; we do not make any assertion on what it induces in degrees different from $2n-i$).*

b) *For every endomorphism $\mathrm{H}^i(X) \to \mathrm{H}^i(X)$ which is algebraic, the coefficients of the characteristic polynomial are rational, and for every $i < n$, there exists an isomorphism $\mathrm{H}^{2n-i}(X) \to \mathrm{H}^i(X)$ which is algebraic.*

Proof. — I sketched already how $D_\ell(X)$ implies the fact that for an algebraic endomorphism of $\mathrm{H}^i(X)$, the coefficients of the characteristic polynomial are rational numbers, therefore we know that a) implies b), and of course $C_\ell(X)$ implies a). It remains to prove that b) implies $C_\ell(X)$. Let $u : \mathrm{H}^{2n-i}(X) \to \mathrm{H}^i(X)$ be the given isomorphism which is algebraic, and $v : \mathrm{H}^i(X) \to \mathrm{H}^{2n-i}(X)$ is an algebraic isomorphism in the opposite direction, induced by L_X^{n-i}. Then $uv = w$ is an automorphism of $\mathrm{H}^i(X)$ which is algebraic, and the Hamilton-Cayley formula $u^h - \sigma_1(w)u^{h-1} + \ldots + (-1)^b\sigma_b(w) = 0$ (where the $\sigma_i(w)$ are the coefficients of the characteristic polynomial of w) such that w^{-1} is a linear combination of the w^i, with coefficients of the type $+/-\sigma_i(w)/\sigma_b(w)$ (N.B. $b = \operatorname{rank} \mathrm{H}^i$). The assumption implies that these coefficients are rational, which implies that w^{-1} is algebraic, and so is $w^{-1}u = v^{-1}$, which was to be proved.

N.B. In characteristic 0, the statement simplifies to: $C(X)$ equivalent to the existence of algebraic isomorphisms $\mathrm{H}^{2n-i}(X) \to \mathrm{H}^i(X)$, (as the preliminary in b) is then automatically satisfied). Maybe with some extra care this can be proved too in arbitrary characteristics.

Corollary. — *Assume X and X' satisfy condition C_ℓ, and let $u : \mathrm{H}^i(X) \to \mathrm{H}^{i+2D}(X') \to \mathrm{H}^i(X)$ $(D \in \mathbf{Z})$ be an isomorphism which is algebraic. Then u^{-1} is algebraic.*

Indeed, the two spaces can be identified "algebraically" (both directions!) to their dual, so that the transpose of u can be viewed as an isomorphism $u' : \mathrm{H}^{i+2D}(X') \to \mathrm{H}^i(X)$. Thus $u'u$ is an algebraic automorphism w of $\mathrm{H}^i(X)$, and by the previous argument we see that w^{-1} is algebraic, hence so is $u^{-1} = w^{-1}u'$.

As a consequence , we see that if $x \in \mathrm{H}^i(X)$ is such that $u(x)$ is algebraic (i being now assumed to be even), than so is x. The same result should hold in fact if u is a monomorphism, the reason being that in this case there should exists a left-inverse which is algebraic; this exists indeed in a case like $\mathrm{H}^{n-1}(X) \to \mathrm{H}^{n-1}(Y)$ (where we take the left inverse $\Lambda_X \phi_*$). But to get it in general, it seems w need moreover the Hodge index relation. (The complete yoga then being that we have the category of motives which is semi-simple!). Without speaking of motives, and staying down on earth, it would be nice to explain in the notes that $C(X)$ together with the index relation $I(X \times X)$ implies that the ring of correspondences classes for X is semi-simple, and how one deduces from this the existence of left and right inverses as looked for above.

This could be given in an extra paragraph (which I did not really touch upon in the talk), containing also the deduction of the Weil conjectures from the conjectures C and A.

A last and rather trivial remark is the following. Let's introduce variant $A'_\ell(X)$ and $A''_\ell(X)$ as follows:

$A'_\ell(X):$ if $2i \le n-1$, any element x of $\mathrm{H}^i(X)$ whose image in $\mathrm{H}^i(Y)$ is algebraic, is algebraic.

$A''_\ell(X):$ if $2i \ge n-1$, any algebraic element of $\mathrm{H}^{i+2}(X)$ is the image of an algebraic element of $\mathrm{H}^i(Y)$.

Let us consider also the specifications $A'_\ell(X)^\circ$ and $A''_\ell(X)^\circ$, where we restrict to the [] dimensions $2i = n-1$ if n odd, $2i = n-2$ if n even. All these conditions are in the nature of "weak" Lefschetz relations, and they are trivially implied by $A_\ell(X)$ resp. $C_\ell(X)$ (in the first case, applying ϕ we see that $L_X X$ is algebraic; in the second, we take $y = \Lambda_Y \phi^+(x)$). The remark then is that these pretendently "weak" variants in fact imply the full Lefschetz relations for algebraic cycles, namely:

Proposition. — *$C_\ell(X)$ is equivalent to the conjunction $C_\ell(Y) + A_\ell(X \times X)^\circ + A''_\ell(X \times X)^\circ$, hence (by induction) also to the conjunction of the conditions A'°_ℓ and A''°_ℓ for all of the varieties $X \times X$, $Y \times Y$, $Z \times Z$,... Analogous statement with $X \times Y$, $Y \times Z$ etc instead of $X \times X$, $Y \times Y$ etc.*

This comes from the remark that $A_\ell(X)^\circ$ follows from the conjunction of $A'_\ell(X)^\circ$ and $A''_\ell(X)^\circ$, as one sees by decomposing $L_X^2 : \mathrm{H}^{2m-2}(X) \to \mathrm{H}^{2m+2}(X)$ into $\mathrm{H}^{2m+2}(X) \xrightarrow{\phi^k} \mathrm{H}^{2m+2}(Y) \xrightarrow{\phi_\alpha} \mathrm{H}^{2m}(X) \xrightarrow{L_X} \mathrm{H}^{2m+2}(X)$ if $\dim X = 2m$ is even, and $\mathrm{H}^{2m+1-1}(X) \to \mathrm{H}^{2m+1+1}$ into $\mathrm{H}^{2m}(X) \xrightarrow{\phi^*} \mathrm{H}^{2m}(Y) \xrightarrow{\phi_*} \mathrm{H}^{2m+2}(X)$ if $\dim X = 2m+1$ is odd.

Sincerely yours

1967

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1974

7.8.74

Cher Deligne,

2 Étant peut-être empêché par mon jambe d'assurer un cours de 1er cycle au 1er trimestre, je vais peut-être à la place faire un petit séminaire d'algèbre, et envisage de le faire sur les fourbis de Mme Sinh, éventuellement transposés dans le contexte des "champs". À ce propos, je tombe sur le truc suivant, qui pour l'instant reste heuristique. Si M, N sont deux faisceaux abéliens sur un topos X, et $\tau_{\leqslant 2}\,\mathbf{Hom}(M,N) = E(M,N)$ est le complexe ayant les invariants *Lettre à P. Deligne, 7.8.1974*

$$\begin{cases} \mathbf{H}^i &= \mathbf{Ext}^i(M,N) \quad \text{pour } 0 \leqslant i \leqslant 2 \\ \mathbf{H}^i &= 0 \quad \text{si } i \notin [0,2]\,, \end{cases}$$

il doit y avoir un triangle distingué canonique

$$\text{(T)} \qquad \begin{tikzcd} & \mathbf{Hom}(M, {}_2N)[-2] \arrow[dl] & \\ E(M,N) \arrow[rr] & & E'(M,N) \arrow[ul] \end{tikzcd},$$

donc $E'(M,N)$ est un complexe dont les invariants $\mathbf{H}^i$ sont ceux de $E(M,N)$ en degré $i \neq 2$, et qui en degré 2 donne lieu à une suite exacte

$$(*) \qquad 0 \to \mathbf{Ext}^2(M,N) \to \overbrace{\mathbf{H}^2(E'(M,N))}^{P(M,N)} \xrightarrow{\sigma} \mathbf{Hom}(M, {}_2N) \to 0\,.$$

Heuristiquement, $E'(M,N)$ est le complexe qui exprime le "2-champ de Picard strict" formé des 1-champs de Picard (pas nécessairement stricts) "épinglés" par M, N sur des objets variables de X, en admettant que ta théorie pour les 1-champs de Picard stricts s'étend aux 2-champs de Picard stricts (ce qui pour moi ne fait guère de doute) ; de même $E(M,N)$ correspond aux champs de Picard stricts épinglés par M, N. La suite exacte $(*)$ se construit en tous cas canoniquement "à la main", où le terme médian est le faisceau des classes à "équivalence" près des champs de Picard épinglés par M, N, or étant l'invariant qui s'obtient en associant à toute section L d'un champ de Picard la symétrie de $L \otimes L$, interprété comme section de ${}_2N$. Je sais prouver (sauf erreur) que tout homomorphisme $M \to {}_2N$ provient d'un champ de Picard convenable (épinglé par M, N) (a priori l'obstruction est dans $\mathrm{Ext}^3(X; M,N)$, mais un argument "universel" prouve qu'elle est nulle). Cela prouve que l'extension $(*)$ est bien proche d'être splittée : toute section du troisième faisceaux, sur un objet quelconque de X, se remonte – en d'autres termes, l'extension a une section "ensembliste". Bien sûr, il y a mieux en fait : toute section sur un $U \in \mathrm{Ob}X$ "provient" d'un élément de $H^2(U, E'(M,N))$ (hypercohomolo - H^2).

Exemple. Soit A un anneau sur X, soient M, N respectivement les faisceaux K^0, K^1 associés au champ additif des A-Modules projectifs de type fini (p. ex.). Alors la construction de Mme Sinh nous fournit un champ de Picard épinglé par M, N, d'où une section canonique du terme médian $P(M,N)$ de $(*)$.

NB. Tout ce qui précède a les fonctorialités évidentes en $M, N, X, \dots$.

Question. Le triangle exact (T) et la suite exacte $(*)$ sont-ils connus par les compétences (Quillen, Breen, Illusie ...) ? Connaissent-ils des variantes "supérieures" ? (Un principe "géométrique" pour les obtenir pourrait être via des n-champs de Picard non nécessairement stricts ...)

Je profite de l'occasion pour soulever une question sur la "cohomologie relative". Soit $q : X \to Y$ un morphisme de topos. Si F est un faisceau abélien (ou un complexe d'iceux) sur Y, peut-on définir fonctoriellement en F la cohomologie relative $\Gamma(Y \bmod X, F)$ (de la catégorie dérivée de $(\mathrm{Ab})(Y)$ dans celle de (Ab)) ? L'interprétation "géométrique" en termes d'opérations sur des n-champs de Picard (n "grand") suggère que ça doit exister. Mais je ne vois de construction évidente " à la main" que dans les deux cas extrêmes :

(a) q est "(-1)-acyclique", i.e. pour tout F sur Y, $F \to q_* q^* F$ est injectif (NB C'est le cas de $Y/P \to Y$ si $P \to e_Y$ est un épimorphisme – c'est donc le cas de $\mathrm{B}_e \to \mathrm{B}_G$ plus haut.)
On prend
$$\Gamma(\mathrm{Coker}(F \to q_*(\underbrace{C(q^*(F))}_{\text{résolution injective}}))[-1]) .$$

(b) $\forall$ F injectif sur Y, $q^*(F)$ est injectif et $F \to q_* q^* F$ est un épimorphisme (exemple : q inclusion d'un ouvert $U \hookrightarrow e_Y$). On prend
$$\Gamma_Y(\mathrm{Ker}(\underbrace{C(F)}_{\text{résolution injective}} \to q_* q^*(C(F)))) .$$

Dans le cas général, la difficulté provient du fait que le cône d'un morphisme de complexes (tel que
$$F \;\to\; q_*(q^*(F)) \qquad\qquad)$$
n'est pas fonctoriel (dans la catégorie dérivée) par rapport à la flèche dont on veut prendre le cône. Et pourtant, dans le cas particulier actuel, il devrait y avoir un choix fonctoriel. Est-ce évident ?

Question pour Illusie : Dans sa théorie des déformations de schémas en groupes plats, il tombe sur des $\mathrm{H}^3(\mathrm{B}_G/X, -)$ resp. des $\mathrm{Ext}^2(X; -, =)$. Peut-on court-circuiter sa théorie via la théorie (supposée écrite) des Gr-champs – resp. via ta théorie des champs de Picard ? J'ai [phrase incomplète]

Je te signale que j'ai réfléchi aux Gr-champs sur X. Si G est un Groupe sur X, N un G-Module, les Gr-champs sur X " épinglés par G, N " forment a priori une 2-catégorie et même une 2-catégorie de Picard stricte, grâce à l'opération évidente à la Baer. On trouve que le complexe (de cochaînes) tronqué à 1 échelon à qui lui correspond est le tronqué
$$\tau_{\leqslant 2}(\Gamma(\mathrm{B}_G \bmod X, N)[1]) .$$

(NB la cohomologie de $\Gamma(\mathrm{B}_G \bmod X, N)$ commence en degré 1.) Plus géométriquement, un Gr-champ sur X épinglé par (G, N) est essentiellement "la même chose" qu'une 2-gerbe sur B_G, liée par N, et munie d'une trivialisation au dessus de $X \approx \mathrm{B}_e = (\mathrm{B}_G)/P$ (où P est l'objet de B_G "torseur universel sous G"). Ces 2-gerbes forment en fait une 3-catégorie de Picard a priori, mais il se trouve que dans celle-ci, les 3-flèches sont triviales (i.e. si source = but, ce sont des identités) – cela ne fait qu'exprimer $\mathrm{H}^0(\mathrm{B}_G/X, N) = 0$ (i.e. $\mathrm{H}^0(\mathrm{B}_G, N) \to \mathrm{H}^0(X, N)$ injectif...). Donc la 3-catégorie peut être regardée comme une 2-catégorie – et "c'est" celle des Gr-champs sur X épinglés par G, N. Si on veut localiser sur X, et décrire le 2-champs de Picard sur X des champs de Picard (sur des objets variables de X) épinglés par G, N, on trouve qu'il est exprimé par le complexe

$$\tau_{\leqslant 2}(p_{G*}\,\mathrm{Coker}(N \to q_{G*}\overbrace{C(q_G^* N)}^{\text{résolution injective}})),$$

où $p_G : \mathrm{B}_G \to X$ et $q_G : \mathrm{B}_e \approx X \simeq (\mathrm{B}_G)_P \to \mathrm{B}_G$. Toutes ces descriptions étant compatibles avec des variations de G, N, X, cela donne en principe une description de la 2-catégorie des Gr-champs, avec X, G, N variables...

1975

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1977

1978

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1981

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1983

Les Aumettes 19.2.1983

Dear Daniel,

[See *Pursuing Stacks* (1983)]

Letter to D. Quillen, 19.2.1983

3 Last year Ronnie Brown from Bangor sent me a heap of reprints and preprints by him and a group of friends, on various foundational matters of homotopical algebra. I did not really dig through any of this, as I kind of lost contact with the technicalities of this kind (I was never too familiar with the homotopy techniques anyhow, I confess) – but this reminded me of a few letters I had exchanged with Larry Breen in 1975, where I had developed an outline of a program for a kind of "topological algebra", viewed as a synthesis of homotopical and homological algebra, with special emphasis on topoi – most of the basic intuitions in this program arising from various backgrounds in algebraic geometry. Some of those intuitions we discussed, I believe, at IHES eight or nine years before, then, at a time when you had just written up your nice ideas on axiomatic homotopical algebra, published since in Springer's Lecture Notes. I write you under the assumption that you have not entirely lost interest for those foundational questions you were looking at more than fifteen years ago. One thing which strikes me, is that (as far as I know) there has not been any substantial progress since – it looks to me that an understanding of the basic structures underlying homotopy theory, or even homological algebra only, is still lacking – probably because the few people who have a wide enough background and perspective enabling them to feel the main questions, are devoting their energies to things which seem more directly rewarding. Maybe even a wind of disrepute for any foundational matters whatever is blowing nowadays*! In this respect, what seems to me even more striking than the lack of proper foundations for homological and homotopical algebra, is the absence I daresay of proper foundations for topology itself! I am thinking here mainly of the development of a context of "tame" topology, which (I am convinced) would have on the everyday technique of geometric topology (I use this expression in contrast to the topology of use for analysts) a comparable impact or even a greater one, than the introduction of the point of view of schemes had on algebraic geometry†. The psychological drawback here I believe is not anything like messiness, as for homological and homotopical algebra (as for schemes), but merely the inrooted inertia which prevents us so stubbornly from looking innocently, with fresh eyes, upon things, without being dulled and imprisoned by standing habits of thought, going with a familiar context – *too* familiar a context! The task of working out the foundations of tame topology, and a corresponding structure theory for "stratified (tame) spaces", seems to me a lot more

[*Tapis de Quillen* (1968)]

[Quillen (1967)]

*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 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…

†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.

urgent and exciting still than any program of homological, homotopical or topological algebra.

The motivation for this letter was the latter topic however. Ronnie Brown and his friends are competent algebraists and apparently strongly motivated for investing energy in foundational work, on the other hand they visibly are lacking the necessary scope of vision which geometry alone provides[‡]. They seem to me kind of isolated, partly due I guess to the disrepute I mentioned before – I suggested to try and have contact with people such as yourself, Larry Breen, Illusie and others, who have the geometric insight and who moreover, may not think themselves too good for indulging in occasional reflection on foundational matters and in the process help others do the work which should be done.

[‡] I have to apologise for this rash statement, as later correspondence made me realise 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!

At first sight it has seemed to me that the Bangor group[§] had indeed come to work out (quite independently) one basic intuition of the program I had envisioned in those letters to Larry Breen – namely that the study of n-truncated homotopy types (of semisimplicial sets, or of topological spaces) was essentially equivalent to the study of so-called n-groupoids (where n is any natural integer). This is expected to be achieved by associating to any space (say) X its "fundamental n-groupoid" $\Pi_n(X)$, generalizing the familiar Poincaré fundamental groupoid for $n = 1$. The obvious idea is that 0-objects of $\Pi_n(X)$ should be the points of X, 1-objects should be "homotopies" or paths between points, 2-objects should be homotopies between 1-objects, etc. This $\Pi_n(X)$ should embody the n-truncated homotopy type of X, in much the same way as for $n = 1$ the usual fundamental groupoid embodies the 1-truncated homotopy type. For two spaces X, Y, the set of homotopy-classes of maps $X \to Y$ (more correctly, for general X, Y, the maps of X into Y in the homotopy category) should correspond to n-equivalence classes of n-functors from $\Pi_n(X)$ to $\Pi_n(Y)$ – etc. There are very strong suggestions for a nice formalism including a notion of geometric realization of an n-groupoid, which should imply that any n-groupoid (or more generally of an n-category) is relativized over an arbitrary topos to the notion of an n-gerbe (or more generally, an n-stack), these become the natural "coefficients" for a formalism of non-commutative cohomological algebra, in the spirit of Giraud's thesis.

[§] 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.

But all this kind of thing for the time being is pure heuristics – I never so far sat down to try to make explicit at least a definition of n-categories and n-groupoids, of n-functors between these etc. When I got the Bangor reprints I at once had the feeling that this kind of work had been done and the homotopy category expressed in terms of ∞-groupoids. But finally it appears this is not so, they have been working throughout with a notion of ∞-groupoid too restrictive for the purposes I had in mind (probably because they insist I guess on strict associativity of compositions, rather than associativity up to a (given) isomorphism, or rather, homotopy) – to the effect that the simply connected homotopy types they obtain are merely products of Eilenberg-MacLane spaces, too bad! They do not seem to have realized yet that this makes their set-up wholly inadequate to a sweeping foundational set-up for homotopy. This brings to the fore again to work out the suitable definitions for n-groupoids – if this is not done yet anywhere. I spent the afternoon today trying to figure out a reasonable definition, to get a feeling at least of where the difficulties are, if any. I am guided mainly of course by the topological interpretation. It will be short enough to say how far

I got. The main part of the structure it seems is expressed by the sets F_i ($i \in \mathbb{N}$) of i-objects, the source, target and identity maps

$$\begin{aligned} \mathrm{s}_1^i, \mathrm{t}_1^i &: F_i \to F_{i-1} \quad (i \geqslant 1) \\ \mathrm{k}_1^i &: F_i \to F_{i+1} \quad (i \in \mathbb{N}) \end{aligned}$$

and the symmetry map (passage to the inverse)

$$\mathrm{inv}_i : F_i \to F_i \quad (i \geqslant 1),$$

satisfying some obvious relations: k_1^i is right inverse to the source and target maps $\mathrm{s}_1^{i+1}, \mathrm{t}_1^{i+1}$, inv_i is an involution and "exchanges" source and target, and moreover for $i \geqslant 2$

notation:
$d_a = \mathrm{k}_1^i(a)$,
$\check{u} = \mathrm{inv}_i(u)$

$$\begin{aligned} \mathrm{s}_1^{i-1}\mathrm{s}_1^i &= \mathrm{s}_1^{i-1}\mathrm{t}_1^i \left(\overset{\text{def}}{=} \mathrm{s}_2^i : F_i \to F_{i-2}\right) \\ \mathrm{t}_1^{i-1}\mathrm{s}_1^i &= \mathrm{t}_1^{i-1}\mathrm{t}_1^i \left(\overset{\text{def}}{=} \mathrm{t}_2^i : F_i \to F_{i-2}\right); \end{aligned}$$

thus the composition of the source and target maps yields, for $0 \leqslant j \leqslant i$, just *two* maps

$$\mathrm{s}_\ell^i, \mathrm{t}_\ell^i : F_i \to F_{i-\ell} = F_j \quad (\ell = i - j).$$

The next basic structure is the composition structure, where the usual compoisition of arrows, more specifically of i-objects ($i \geqslant 1$) $v \circ u$ (defined when $\mathrm{t}_1(u) = \mathrm{s}_1(v)$) must be supplemented by the Godement-type operations $\mu * \lambda$ when μ and λ are "arrows between arrows", etc. Following this line of thought, one gets the composition maps

$$(u, v) \mapsto (v *_\ell u) : (F_i, \mathrm{s}_\ell^i) \times_{F_{i-\ell}} (F_i, \mathrm{s}_\ell^i) \to F_i,$$

the composition of i-objects for $1 \leqslant \ell \leqslant i$, being defined when the ℓ-target of u is equal to the ℓ-source of v, and then we have

$$\left.\begin{aligned} \mathrm{s}_1^i(v *_\ell u) &= \mathrm{s}_1^i(v) *_{\ell-1} \mathrm{s}_1^i(u) \\ \mathrm{t}_1^i(v *_\ell u) &= \mathrm{t}_1^i(v) *_{\ell-1} \mathrm{s}_1^i(u) \end{aligned}\right\} \quad \ell \geqslant 2 \text{ i.e. } \ell - 1 \geqslant 1$$

and for $\ell = 1$

$$\begin{aligned} \mathrm{s}_1(v *_1 u) &= s_1(u) \\ \mathrm{t}_1(v *_1 u) &= t_1(v) \end{aligned}$$

(NB the operation $v *_1 u$ is just the usual composition $v \circ u$).

One may be tempted to think that the preceding data exhaust the structure of ∞-groupoids, and that they will have to be supplemented only by a handful of suitable *axioms*, one being *associativity* for the operation $\overset{i}{\underset{\ell}{*}}$, which can be expressed essentially by saying that that composition operation turns F_i into the set of arrows of a category having $F_{i-\ell}$ as a set of objects (with the source and target maps s_ℓ^i and t_ℓ^i, and with identity map $\mathrm{k}_\ell^{i-\ell} : F_{i-\ell} \to F_i$ the composition of the identity maps $F_{i-\ell} \to F_{i-\ell+1} \to \cdots \to F_{i-1} \to F_i$), and another being the Godement relation

$$(v' *_\alpha v) *_\nu (u' *_\alpha u) = (v' *_\nu u') *_\alpha (v *_\nu u)$$

(with the assumptions $1 \leqslant \alpha \leqslant \nu$, and $u, u', v, v''inF_i$ and

$$\begin{cases} \mathrm{t}_\alpha(u) = \mathrm{s}_\alpha(u') \\ \mathrm{t}_\alpha(v) = \mathrm{s}_\alpha(v') \end{cases} \quad \mathrm{t}_\nu(u) = \mathrm{s}_\nu(v) = \mathrm{s}_\nu(v') = \mathrm{t}_\nu(u')$$

implying that both members are defined), plus the two relations concerning the inversion of i-objects ($i \geqslant 1$) $u \mapsto \check{u}$,

$$u *_1 \check{u} \;=\; \mathrm{id}_{\mathrm{t}_1(u)}, \qquad \check{u} *_1 u \;=\; \mathrm{id}_{\mathrm{s}_1(u)}, \qquad (\check{v} *_\ell \check{u}) \;=\; ? \quad (\ell \;\geqslant\; 2)$$

It just occurs to me, by the way, that the previous description of basic (or "primary") data for an ∞-groupoid is already incomplete in some rather obvious respect, namely that the symmetry-operation $\mathrm{inv}_i : u \mapsto \check{u}$ on F_i must be complemented by $i-1$ similar involutions on F_i, which corresponds algebraically to the intuition that when we have an $(i+1)$-arrow λ say between two i-arrows u and v, then we must be able to deduce from it another arrow from $\check{u}$ to $\check{v}$ (namely $u \mapsto \check{u}$ has a "functorial character" for variable u)? This seems a rather anodine modification of the previous set-up, and is irrelevant for the main point I want to make here, namely: that for the notion of ∞-groupoids we are after, all the equalities just envisioned in this paragraph (and those I guess which will ensure naturality by the necessary extension of the basic involution on F_i) should be replaced by "homotopies", namely by $(i+1)$-arrows between the two members. These arrows should be viewed, I believe, as being part of the data, they appear here as a kind of "secondary" structure. The difficulty which appears now is to work out the natural coherence properties concerning this secondary structure. The first thing I could think of is the "pentagon axiom" for the associativity data, which occurs when looking at associativities for the compositum (for $\overset{i}{\underset{\ell}{*}}$ say) of four factors. Here again the first reflex would be to write down, as usual, an *equality* for two compositions of associativity isomorphisms, exhibited in the pentagon diagram. One suspects however that such equality should, again, be replaced by a "homotopy"-arrow, which now appears as a kind of "ternary" structure – before even having exhausted the list of coherence "relations" one could think of with the respect to the secondary structure! Here one seems caught at first sight in an infinite chain of ever "higher", and presumably, messier structures, where one is going to get hopelessly lost, unless one discovers some simple guiding principle for shedding some clarity in the mess.

I thought of writing you mainly because I believe that, if anybody, you should know if the kind of structure I am looking for has been worked out – maybe even *you* did? In this respect, I vaguely remember that you had a description of n-categories in terms of n-semisimplicial sets, satisfying certain exactness conditions, in much the same way as an ordinary category can be interpreted, via its "nerve", as a particular type of semisimplicial set. But I have no idea if your definition applied only for describing n-categories with strict associativities, or not[¶].

[¶]Definitely only for *strict* associativity.

Still some contents in the spirit of your axiomatics of homotopical algebra – in order to make the question I am proposing more seducing maybe to you! One comment is that presumably, the category of ∞-groupoids (which is still to be defined) is a "model category" for the usual homotopy category; this would be at any rate one plausible way to make explicit the intuition referred to before, that a homotopy type is "essentially the same" as an ∞-groupoid up to ∞-equivalence. The other comment: the construction of the fundamental ∞-groupoid of a space, disregarding for the time being the question of working out in full the pertinent structure on this messy object, can be paraphrased in any model category in your sense, and yields a functor from this category to the category of ∞-groupoids, and hence (by geometric realization, or by localization) also to the usual homotopy category‖. Was this functor obvious beforehand? It is of a non-trivial nature only when the model category is *not* pointed – as a matter of fact the whole construction can be carried out canonically, in terms of a "cylinder object" I for the final object e of the model category, playing the role of the unit argument. It's high time to stop this letter – please excuse me if it should come ten or fifteen years too late, or maybe one year too early. If you are not interested for the time being in such general nonsense, maybe you know someone who is …

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

Very cordially yours

1984

Les Aumettes 15.4.1984

Dear Lipman Bers,

4 Together with Yves Ladegaillerie (a former student of mine) we are running a microseminar on the Teichmüller spaces and groups, my own motivations coming mainly from algebraic geometry, and Ladegaillerie's from his interest in the topology of surfaces. Lately we have met with a problem which I would like to submit to you, as I understand you are the main expert on Thurston's hyperbolic geometry approach to Teichmüller space. Before stating the specific problem on hyperbolic "pants" (which things boil down to), let me tell you what we are really after. *Letter to L. Bers, 15.4.1984*

Assuming given a compact

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