

## SKETCH OF A PROGRAMME

by Alexandre Grothendieck

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## SKETCH OF A PROGRAMME

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1. As the present situation makes the prospect of teaching at the research level at the University seem more and more illusory, I have resolved to apply for admission to the CNRS, in order to devote my energy to the development of projects and perspectives for which it is becoming clear that no student (nor even, it seems, any mathematical colleague) will be found to develop them in my stead.

In the role of the document “Titles and Articles”, one can find after this text the complete reproduction of a sketch, by themes, of what I considered to be my principal mathematical contributions at the time of writing that report, in 1972. It also contains a list of articles published at that date. I ceased all publication of scientific articles in 1970. In the following lines, I propose to give a view of at least some of the principal themes of my mathematical reflections since then. These reflections materialised over the years in the form of two voluminous boxes of handwritten notes, doubtless difficult to decipher for anyone but myself, and which, after several successive stages of settling, are perhaps waiting for their moment to be written up together at least in a temporary fashion, for the benefit of the mathematical community. The term “written up” is somewhat incorrect here, since in fact it is much more a question of developing the ideas and the multiple visions begun during these last twelve years, to make them more precise and deeper, with all the unexpected rebounds which constantly accompany this kind of work – a work of discovery, thus, and not of compilation of piously accumulated notes. And in writing the “Mathematical Reflections”, begun since February 1983, I do intend throughout its pages to clearly reveal the process of thought, which feels and discovers, often blindly in the shadows, with sudden flashes of light when some tenacious false or simply inadequate image is finally shown for what it is, and things which seemed all crooked fall into place, with that mutual harmony which is their own.

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In any case, the following sketch of some themes of reflection from the last ten or twelve years will also serve as a sketch of my programme of work for the coming years, which I intend to devote to the development of these themes, or at least some of them. It is intended on the one hand for my colleagues of the National Committee whose job it is to decide the fate of my application, and on the other hand for some other colleagues, former students, friends, in the possibility that some of the ideas sketched here might interest one of them.

2. The demands of university teaching, addressed to students (including those said to be “advanced”) with a modest (and frequently less than modest) mathematical baggage, led me to a Draconian renewal of the themes of reflection I proposed to my students, and gradually to myself as well. It seemed important to me to start from an intuitive baggage common to everyone, independent of any technical language used to express it, and anterior to any such language – it turned out that the geometric and topological intuition of shapes, particularly two-dimensional shapes, formed such a common ground. This consists of themes which can be grouped under the general name of “topology of surfaces” or “geometry of surfaces”, it being understood in this last expression that the main emphasis is on the topological properties of the surfaces, or the combinatorial aspects which form the most down-to-earth technical expression of them, and not on the differential, conformal, Riemannian, holomorphic aspects, and (from there) on to “complex algebraic curves”. Once this last step is taken, however, algebraic geometry (my former love!) suddenly bursts forth once again, and this via the objects which we can consider as the basic building blocks for all other algebraic varieties. Whereas in my research before 1970, my attention was systematically directed towards objects of maximal generality, in order to uncover a general language adequate for the world of algebraic geometry, and I never restricted myself to algebraic curves except when strictly necessary (notably in étale cohomology), preferring to develop “pass-key” techniques and statements valid in all dimensions and in every place (I mean, over all base schemes, or even base ringed topoi...), here I was brought back, via objects so simple that a child learns them while playing, to the beginnings and origins of algebraic geometry, familiar to Riemann and his followers!

Since around 1975, it is thus the geometry of (real) surfaces, and starting in 1977 the links between questions of geometry of surfaces and the algebraic geometry of algebraic curves defined over fields such as  $\mathbb{C}$ ,  $\mathbb{R}$  or extensions of  $\mathbb{Q}$  of finite type, which were my principal source of inspiration and my constant guiding thread. It is with surprise and wonderment that over the years I discovered (or rather, doubtless, rediscovered) the prodigious, truly inexhaustible richness, the unsuspected depth of this theme, apparently so anodine. I believe I feel a central sensitive point there, a privileged point of convergence of the principal currents of mathematical ideas, and also of the principal structures and visions of things which they express, from the most specific (such as the rings  $\mathbb{Z}$ ,  $\mathbb{Q}$ ,  $\overline{\mathbb{Q}}$ ,  $\mathbb{R}$ ,  $\mathbb{C}$  or the group  $\mathrm{Sl}(2)$  over one of these rings, or general reductive algebraic groups) to the most “abstract”, such as the algebraic “multiplicities”, complex analytic or real analytic. (These are naturally introduced when systematically studying “moduli va-

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 rieties” for the geometric objects considered, if we want to go farther than the notoriously insufficient point of view of “coarse moduli” which comes down to most unfortunately killing the automorphism groups of these objects.) Among these modular multiplicities, it is those of Mumford-Deligne for “stable” algebraic curves of genus  $g$  with  $\nu$  marked points, which I denote by  $\widehat{M}_{g,\nu}$  (compactification of the “open” multiplicity  $M_{g,\nu}$  corresponding to non-singular curves), which for the last two or three years have exercised a particular fascination over me, perhaps even stronger than any other mathematical object to this day. Indeed, it is more the system of all the multiplicities  $M_{g,\nu}$  for variable  $g, \nu$ , linked together by a certain number of fundamental operations (such as the operations of “plugging holes”, i.e. “erasing” marked points, and of “glueing”, and the inverse operations), which are the reflection in absolute algebraic geometry in characteristic zero (for the moment) of geometric operations familiar from the point of view of topological or conformal “surgery” of surfaces. Doubtless the principal reason of this fascination is that this very rich geometric structure on the system of “open” modular multiplicities  $M_{g,\nu}$  is reflected in an analogous structure on the corresponding fundamental groupoids, the “Teichmüller groupoids”  $\widehat{T}_{g,\nu}$ , and that these operations on the level of the  $\widehat{T}_{g,\nu}$  are sufficiently intrinsic for the Galois group  $\mathbb{I}$  of  $\overline{\mathbb{Q}}/\mathbb{Q}$  to act on this whole “tower” of Teichmüller groupoids, respecting all these structures. Even more extraordinary, this action is faithful – indeed, it is already faithful on the first non-trivial “level” of this tower, namely  $\widehat{T}_{0,4}$  – which also means, essentially, that the outer action of  $\mathbb{I}$  on the fundamental group  $\widehat{\pi}_{0,3}$  of the standard projective line  $\mathbb{P}^1$  over  $\mathbb{Q}$  with the three points  $0, 1$  and  $\infty$  removed, is already faithful. Thus the Galois group  $\mathbb{I}$  can be realised as an automorphism group of a very concrete profinite group, and moreover respects certain essential structures of this group. It follows that an element of  $\mathbb{I}$  can be “parametrised” (in various equivalent ways) by a suitable element of this profinite group  $\widehat{\pi}_{0,3}$  (a free profinite group on two generators), or by a system of such elements, these elements being subject to certain simple necessary (but doubtless not sufficient) conditions for this or these elements to really correspond to an element of  $\mathbb{I}$ . One of the most fascinating tasks here is precisely to discover necessary and sufficient conditions on an exterior automorphism of  $\widehat{\pi}_{0,3}$ , i.e. on the corresponding parameter(s), for it to come from an element of  $\mathbb{I}$  – which would give a “purely algebraic” description, in terms of profinite groups and with no reference to the Galois theory of number fields, to the Galois group  $\mathbb{I} = \text{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$ .

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 Perhaps even a conjectural characterisation of  $\mathbb{I}$  as a subgroup of  $\text{Aut}_{\text{ext}} \widehat{\pi}_{0,3}$  is for the moment out of reach <sup>(1)</sup>; I do not yet have any conjecture to propose. On the other hand another task is immediately accessible,

which is to describe the action of  $\mathbb{I}$  on all of the Teichmüller tower, in terms of its action on the “first level”  $\hat{\pi}_{0,3}$ , i.e. to express an automorphism of this tower, in terms of the “parameter” in  $\hat{\pi}_{0,3}$  which picks out the element  $\gamma$  running through  $\mathbb{I}$ . This is linked to a representation of the Teichmüller tower (considered as a groupoid equipped with an operation of “glueing”) by generators and relations, which will in particular give a presentation by generators and relations in the usual sense of each of the  $\hat{T}_{g,\nu}$  (as a profinite groupoid). Here, even for  $g = 0$  (so when the corresponding Teichmüller groups are “well-known” braid groups), the generators and relations known to date which I have heard of appear to me to be unusable as they stand, because they do not present the characteristics of invariance and of symmetry indispensable for the action of  $\mathbb{I}$  to be directly legible on the presentation. This is particularly linked to the fact that people still obstinately persist, when calculating with fundamental groups, in fixing a single base point, instead of cleverly choosing a whole packet of points which is invariant under the symmetries of the situation, which thus get lost on the way. In certain situations (such as descent theorems for fundamental groups à la van Kampen) it is much more elegant, even indispensable for understanding something, to work with fundamental groupoids with respect to a suitable packet of base points, and it is certainly so for the Teichmüller tower. It would seem (incredible, but true!) that even the geometry of the first level of the Teichmüller tower (corresponding thus to “moduli” either for projective lines with four marked points, or to elliptic curves (!)) has never been explicitly described, for example the relation between the genus 0 case and the geometry of the octahedron, and that of the tetrahedron. A fortiori the modular multiplicities  $M_{0,5}$  (for projective lines with five marked points) and  $M_{1,2}$  (for curves of genus 1 with two marked points), which actually are practically isomorphic, appear to be virgin territory – braid groups will not enlighten us on their score! I have begun to look at  $M_{0,5}$  at stray moments; it is a real jewel, with a very rich geometry closely related to the geometry of the icosahedron.

$\frac{5}{6}$

The a priori interest of a complete knowledge of the two first levels of the tower (i.e., the cases where the modular dimension  $N = 3g - 3 + \nu$  is  $\leq 2$ ) is to be found in the principle that the entire tower can be reconstituted from these two first levels, in the sense that via the fundamental operation of “glueing”, level 1 gives a complete system of generators, and level 2 a complete system of relations. There is a striking analogy, and I am certain it is not merely formal, between this principle and the analogous principle of Demazure for the structure of reductive algebraic groups, if we replace the term “level” or “modular dimension” with “semi-simple rank of the reductive group”. The link becomes even more striking, if we recall that

the Teichmüller group  $T_{1,1}$  (in the discrete, transcendental context now, and not in the profinite algebraic context, where we find the profinite completions of the former) is no other than  $\mathrm{Sl}(2, \mathbb{Z})$ , i.e. the group of integral points of the simple group scheme of “absolute” rank 1  $\mathrm{Sl}(2)_{\mathbb{Z}}$ . Thus, the fundamental building block for the Teichmüller tower is essentially the same as for the “tower” of reductive groups of all ranks – a group of which, moreover, we may say that it is doubtless present in all the essential disciplines of mathematics.

This principle of construction of the Teichmüller tower is not proved at this time – but I have no doubt that it is valid. It would be a consequence (via a theory of dévissage of stratified structures – here the  $\widehat{M}_{g,\nu}$  – which remains to be written, cf. par. 5) of an extremely plausible property of the open modular multiplicities  $M_{g,\nu}$  in the complex analytic context, namely that for modular dimension  $N \geq 3$ , the fundamental group of  $M_{g,\nu}$  (i.e. the usual Teichmüller group  $T_{g,\nu}$ ) is isomorphic to the “fundamental group at infinity”, i.e. that of a “tubular neighbourhood of infinity”. This is a very familiar thing (essentially due to Lefschetz) for a non-singular affine variety of dimension  $N \geq 3$ . True, the modular multiplicities are not affine (except for small values of  $g$ ), but it would suffice if such an  $M_{g,\nu}$  of dimension  $N$  (or rather, a suitable finite covering) were a union of  $N - 2$  affine open sets, making  $M_{g,\nu}$  “not too near a compact variety”.

Having no doubt about this principle of construction of the Teichmüller tower, I prefer to leave to the experts, better equipped than I am, the task of proving the necessary (if it so happens that any are interested), to rather study, with all the care it deserves, the structure which ensues for the Teichmüller tower by generators and relations, this time in the discrete, not the profinite framework – which essentially comes down to a complete understanding of the four modular multiplicities  $M_{0,4}$ ,  $M_{1,1}$ ,  $M_{0,5}$ ,  $M_{1,2}$  and their fundamental groupoids based at suitably chosen “base points”. These offer themselves quite naturally, as the complex algebraic curves of the type  $(g, n)$  under consideration, having automorphism group (necessarily finite) larger than in the generic case (\*). Including the holomorphic

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(\*) It is also necessary to add the “base points” coming from operations of glueing of “blocks” of the same type in smaller modular dimension. On the other hand, in modular dimension 2 (the cases of  $M_{0,5}$  and  $M_{1,2}$ ), it is advisable to exclude the points of certain one-parameter families of curves admitting an exceptional automorphism of order 2. These families actually constitute remarkable rational curves on the multiplicities considered, which appear to me to be an important ingredient in the structure of these multiplicities.

$\frac{7}{8}$

sphere with three marked points (coming from  $M_{0,3}$ , i.e. from level 0), we find twelve fundamental “building blocks” (6 of genus 0, 6 of genus 1) in a “game of Lego-Teichmüller” (large box), where the points marked on the surfaces considered are replaced by “holes” with boundary, so as to have surfaces with boundary, functioning as building blocks which can be assembled by gentle rubbing as in the ordinary game of Lego dear to our children (or grandchildren...). By assembling them we find an entirely visual way to construct every type of surface (it is essentially these constructions which will be the “base points” for our famous tower), and also to visualise the elementary “paths” by operations as concrete as “twists”, or automorphisms of blocks in the game, and to write the fundamental relations between composed paths. According to the size (and the price!) of the construction box used, we can even find numerous different descriptions of the Teichmüller tower by generators and relations. The smallest box is reduced to identical blocks, of type  $(0,3)$  – these are the Thurston “pants”, and the game of Lego-Teichmüller which I am trying to describe, springing from motivations and reflections of absolute algebraic geometry over the field  $\mathbb{Q}$ , is very close to the game of “hyperbolic geodesic surgery” of Thurston, whose existence I learned of last year from Yves Ladegaillerie. In a microseminar with Carlos Contou-Carrère and Yves Ladegaillerie, we began a reflection one of whose objects is to confront the two points of view, which are mutually complementary.

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I add that each of the twelve building blocks of the “large box” is equipped with a canonical cellular decomposition, stable under all symmetries, having as its only vertices the “marked points” (or centres of the holes), and as edges certain geodesic paths (for the canonical Riemannian structure on the sphere or the torus considered) between certain pairs of vertices (namely those which lie on the same “real locus”, for a suitable real structure of the complex algebraic curve considered). Consequently, all the surfaces obtained in this game by assembling are equipped with canonical cellular structures, which in their turn (cf. §3 below) enable us to consider these surfaces as associated to complex algebraic curves (and even over  $\overline{\mathbb{Q}}$ ) which are canonically determined. There is here a typical game of intertwining of the combinatorial and the complex algebraic (or rather, the algebraic over  $\overline{\mathbb{Q}}$ ).

The “small box” with identical blocks, which has the charm of economy, will doubtless give rise to a relatively complex description for the relations (complex, but not at all inextricable!). The large box will give rise to more numerous relations (because there are many more base points and remarkable paths between them), but with a more transparent structure. I foresee that in modular dimension 2, just as in the more or less familiar



case of modular dimension 1 (in particular with the description of  $\mathrm{Sl}(2, \mathbb{Z})$  by  $(\rho, \sigma \mid \rho^3 = \sigma^2, \rho^4 = \sigma^6 = 1)$ ), we will find a generation by the automorphism groups of the three types of relevant blocks, with simple relations which I have not clarified as I write these lines. Perhaps we will even find a principle of this type for all the  $T_{g,\nu}$ , as well as a cellular decomposition of  $\widehat{M}_{g,\nu}$  generalising those which present themselves spontaneously for  $\widehat{M}_{0,4}$  and  $\widehat{M}_{1,1}$ , and which I already perceive for modular dimension 2, using the hypersurfaces corresponding to the various real structures on the complex structures considered, to effect the desired cellular decomposition.

3. Instead of following (as I meant to) a rigorous thematic order, I let myself be carried away by my predilection for a particularly rich and burning theme, to which I intend to devote myself prioritarilly for some time, starting at the beginning of the academic year 84/85. Thus I will take the thematic description up again where I left it, at the very beginning of the preceding paragraph.

My interest in topological surfaces began to appear in 1974, when I proposed to Yves Ladegaillerie the theme of the isotopic study of embeddings of a topological 1-complex into a compact surface. Over the two following years, this study led him to a remarkable isotopy theorem, giving a complete algebraic description of the isotopy classes of embeddings of such 1-complexes, or compact surfaces with boundary, in a compact oriented surface, in terms of certain very simple combinatorial invariants, and the fundamental groups of the protagonists. This theorem, which should be easily generalisable to embeddings of any compact space (triangulable to simplify) in a compact oriented surface, gives as easy corollaries several deep classical results in the theory of surfaces, and in particular Baer's isotopy theorem. It will finally be published, separately from the rest (and ten years later, seeing the difficulty of the times...), in *Topology*. In the work of Ladegaillerie there is also a purely algebraic description, in terms of fundamental groups, of the "isotopic" category of compact surfaces  $X$ , equipped with a topological 1-complex  $K$  embedded in  $X$ . This description, which had the misfortune to run counter to "today's taste" and because of this appears to be unpublishable, nevertheless served (and still serves) as a precious guide in my later reflections, particularly in the context of absolute algebraic geometry in characteristic zero.

The case where  $(X, K)$  is a 2-dimensional "map", i.e. where the connected components of  $X \setminus K$  are open 2-cells (and where moreover  $K$  is equipped with a finite set  $S$  of "vertices", such that the connected components of  $K \setminus S$  are open 1-cells) progressively attracted my attention over the following years. The isotopic category of these maps admits a particularly simple

algebraic description, via the set of “markers” (or “flags”, or “biarcs”) associated to the map, which is naturally equipped with the structure of a set with a group of operators, under the group

$$\underline{C}_2 = \langle \sigma_0, \sigma_1, \sigma_2 \mid \sigma_0^2 = \sigma_1^2 = \sigma_2^2 = (\sigma_0\sigma_2)^2 = 1 \rangle,$$

which I call the (non-oriented) cartographic group of dimension 2. It admits as a subgroup of index 2 the oriented cartographic group, generated by the products of an even number of generators, which can also be described by

$$\underline{C}_2^+ = \langle \rho_s, \rho_f, \sigma \mid \rho_s\rho_f = \sigma, \sigma^2 = 1 \rangle,$$

(with

$$\rho_s = \sigma_2\sigma_1, \quad \rho_f = \sigma_1\sigma_0, \quad \sigma = \sigma_0\sigma_2 = \sigma_2\sigma_0,$$

$\frac{10}{11}$

operations of elementary rotation of a flag around a vertex, a face and an edge respectively). There is a perfect dictionary between the topological situation of compact maps, resp. oriented compact maps, on the one hand, and finite sets with group of operators  $\underline{C}_2$  resp.  $\underline{C}_2^+$  on the other, a dictionary whose existence was actually more or less known, but never stated with the necessary precision, nor developed at all. This foundational work was done with the care it deserved in an excellent DEA thesis, written jointly by Jean Malgoire and Christine Voisin in 1976.

This reflection suddenly takes on a new dimension, with the simple remark that the group  $\underline{C}_2^+$  can be interpreted as a quotient of the fundamental group of an oriented sphere with three points, numbered 0, 1 and 2, removed; the operations  $\rho_s, \sigma, \rho_f$  are interpreted as loops around these points, satisfying the familiar relation

$$\ell_0\ell_1\ell_2 = 1,$$

while the additional relation  $\sigma^2 = 1$ , i.e.  $\ell_1^2 = 1$  means that we are interested in the quotient of the fundamental group corresponding to an imposed ramification index of 2 over the point 1, which thus classifies the coverings of the sphere ramified at most over the points 0, 1 and 2 with ramification equal to 1 or 2 at the points over 1. Thus, the compact oriented maps form an isotopic category equivalent to that of these coverings, subject to the additional condition of being finite coverings. Now taking the Riemann sphere, or the projective complex line, as reference sphere, rigidified by the three points 0, 1 and  $\infty$  (this last thus replacing 2), and recalling that every finite ramified covering of a complex algebraic curve itself inherits the structure of a complex algebraic curve, we arrive at this fact, which eight years later still appears to me as extraordinary: every “finite” oriented map is canonically realised on a complex algebraic

$\frac{11}{12}$ 

curve! Even better, as the complex projective line is defined over the absolute base field  $\mathbb{Q}$ , as are the admitted points of ramification, the algebraic curves we obtain are defined not only over  $\mathbb{C}$ , but over the algebraic closure  $\overline{\mathbb{Q}}$  of  $\mathbb{Q}$  in  $\mathbb{C}$ . As for the map we started with, it can be found on the algebraic curve, as the inverse image of the real segment  $[0, 1]$  (where 0 is considered as a vertex, and 1 as the middle of a “folded edge” of centre 1), which itself is the “universal oriented 2-map” on the Riemann sphere (\*). The points of the algebraic curve  $X$  over 0, 1 and  $\infty$  are neither more nor less than the vertices, the “centres” of the edges and those of the faces of the map  $(X, K)$ , and the orders of the vertices and the faces are exactly the multiplicities of the zeros and the poles of the rational function (defined over  $\mathbb{Q}$ ) on  $X$ , which expresses its structural projection to  $\mathbb{P}_{\mathbb{C}}^1$ .

This discovery, which is technically so simple, made a very strong impression on me, and it represents a decisive turning point in the course of my reflections, a shift in particular of my centre of interest in mathematics, which suddenly found itself strongly focused. I do not believe that a mathematical fact has ever struck me quite so strongly as this one, nor had a comparable psychological impact <sup>(2)</sup>. This is surely because of the very familiar, non-technical nature of the objects considered, of which any child’s drawing scrawled on a bit of paper (at least if the drawing is made without lifting the pencil) gives a perfectly explicit example. To such a dessin, we find associated subtle arithmetic invariants, which are completely turned topsy-turvy as soon as we add one more stroke. Since these are spherical maps, giving rise to curves of genus 0 (which thus do not lead to “moduli”), we can say that the curve in question is “pinned down” if we fix three of its points, for instance three vertices of the map, or more generally three centres of facets (vertices, edges or faces) – and then the structural map  $f : X \rightarrow \mathbb{P}_{\mathbb{C}}^1$  can be interpreted as a well-determined rational function

 $\frac{12}{13}$ 

$$f(z) = P(z)/Q(z) \in \mathbb{C}(z),$$

quotient of two well-determined relatively prime polynomials, with  $Q$  unitary, satisfying algebraic conditions which in particular reflect the fact that  $f$  is unramified outside of 0, 1 and  $\infty$ , and which imply that the coefficients

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(\*) There is an analogous description of finite non-oriented maps, possibly with boundary, in terms of real algebraic curves, more precisely of coverings of  $\mathbb{P}_{\mathbb{R}}^1$  ramified only over 0, 1,  $\infty$ , the surface with boundary associated to such a covering being  $X(\mathbb{C})/\tau$ , where  $\tau$  is complex conjugation. The “universal” non-oriented map is here the disk, or upper hemisphere of the Riemann sphere, equipped as before with the embedded 1-complex  $K = [0, 1]$ .

of these polynomials are algebraic numbers; thus their zeros are algebraic numbers, which represent respectively the vertices and the centres of the faces of the map under consideration.

Returning to the general case, since finite maps can be interpreted as coverings over  $\overline{\mathbb{Q}}$  of an algebraic curve defined over the prime field  $\mathbb{Q}$  itself, it follows that the Galois group  $\mathbb{I}$  of  $\overline{\mathbb{Q}}$  over  $\mathbb{Q}$  acts on the category of these maps in a natural way. For instance, the operation of an automorphism  $\gamma \in \mathbb{I}$  on a spherical map given by the rational function above is obtained by applying  $\gamma$  to the coefficients of the polynomials  $P, Q$ . Here, then, is that mysterious group  $\mathbb{I}$  intervening as a transforming agent on topologico-combinatorial forms of the most elementary possible nature, leading us to ask questions like: are such and such oriented maps “conjugate” or: exactly which are the conjugates of a given oriented map? (Visibly, there is only a finite number of these).

I considered some concrete cases (for coverings of low degree) by various methods, J. Malgoire considered some others – I doubt that there is a uniform method for solving the problem by computer. My reflection quickly took a more conceptual path, attempting to apprehend the nature of this action of  $\mathbb{I}$ . One sees immediately that roughly speaking, this action is expressed by a certain “outer” action of  $\mathbb{I}$  on the profinite compactification of the oriented cartographic group  $\underline{C}_2^+$ , and this action in its turn is deduced by passage to the quotient of the canonical outer action of  $\mathbb{I}$  on the profinite fundamental group  $\hat{\pi}_{0,3}$  of  $(U_{0,3})_{\overline{\mathbb{Q}}}$ , where  $U_{0,3}$  denotes the typical curve of genus 0 over the prime field  $\mathbb{Q}$ , with three points removed. This is how my attention was drawn to what I have since termed “anabelian algebraic geometry”, whose starting point was exactly a study (limited for the moment to characteristic zero) of the action of “absolute” Galois groups (particularly the groups  $\text{Gal}(\overline{K}/K)$ , where  $K$  is an extension of finite type of the prime field) on (profinite) geometric fundamental groups of algebraic varieties (defined over  $K$ ), and more particularly (breaking with a well-established tradition) fundamental groups which are very far from abelian groups (and which for this reason I call “anabelian”). Among these groups, and very close to the group  $\hat{\pi}_{0,3}$ , there is the profinite compactification of the modular group  $\text{Sl}(2, \mathbb{Z})$ , whose quotient by its centre  $\pm 1$  contains the former as congruence subgroup mod 2, and can also be interpreted as an oriented “cartographic” group, namely the one classifying triangulated oriented maps (i.e. those whose faces are all triangles or monogons).

Every finite oriented map gives rise to a projective non-singular algebraic curve defined over  $\overline{\mathbb{Q}}$ , and one immediately asks the question: which are the algebraic curves over  $\overline{\mathbb{Q}}$  obtained in this way – do we obtain them all,

who knows? In more erudite terms, could it be true that every projective non-singular algebraic curve defined over a number field occurs as a possible “modular curve” parametrising elliptic curves equipped with a suitable rigidification? Such a supposition seemed so crazy that I was almost embarrassed to submit it to the competent people in the domain. Deligne when I consulted him found it crazy indeed, but didn’t have any counterexample up his sleeve. Less than a year later, at the International Congress in Helsinki, the Soviet mathematician Bielyi announced exactly that result, with a proof of disconcerting simplicity which fit into two little pages of a letter of Deligne – never, without a doubt, was such a deep and disconcerting result proved in so few lines!

In the form in which Bielyi states it, his result essentially says that every algebraic curve defined over a number field can be obtained as a covering of the projective line ramified only over the points 0, 1 and  $\infty$ . This result seems to have remained more or less unobserved. Yet it appears to me to have considerable importance. To me, its essential message is that there is a profound identity between the combinatorics of finite maps on the one hand, and the geometry of algebraic curves defined over number fields on the other. This deep result, together with the algebraic-geometric interpretation of maps, opens the door onto a new, unexplored world – within reach of all, who pass by without seeing it.

It was only close to three years later, seeing that decidedly the vast horizons opening here caused nothing to quiver in any of my students, nor even in any of the three or four high-flying colleagues to whom I had occasion to talk about it in a detailed way, that I made a first scouting voyage into this “new world”, from January to June 1981. This first foray materialised into a packet of some 1300 handwritten pages, baptised “The Long March through Galois theory”. It is first and foremost an attempt at understanding the relations between “arithmetic” Galois groups and profinite “geometric” fundamental groups. Quite quickly it became oriented towards a work of computational formulation of the action of  $\text{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$  on  $\hat{\pi}_{0,3}$ , and at a later stage, on the somewhat larger group  $\widehat{\text{Sl}}(2, \mathbb{Z})$ , which gives rise to a more elegant and efficient formalism. Also during the course of this work (but developed in a different set of notes) appeared the central theme of anabelian algebraic geometry, which is to reconstitute certain so-called “anabelian” varieties  $X$  over an absolute field  $K$  from their mixed fundamental group, the extension of  $\text{Gal}(\overline{K}/K)$  by  $\pi_1(X_{\overline{K}})$ ; this is when I discovered the “fundamental conjecture of anabelian algebraic geometry”, close to the conjectures of Mordell and Tate recently proved by Faltings <sup>(3)</sup>. This period also saw the appearance of the first reflection on the Teichmüller groups, and the first intuitions on the many-faceted structure of the “Teichmüller tower”

– the open modular multiplicities  $M_{g,\nu}$  also appearing as the first important examples in dimension  $> 1$ , of varieties (or rather, multiplicities) seeming to deserve the appellation of “anabelian”. Towards the end of this period of reflection, it appeared to me as a fundamental reflection on a theory still completely up in the air, for which the name “Galois-Teichmüller theory” seems to me more appropriate than the name “Galois Theory” which I had at first given to my notes. Here is not the place to give a more detailed description of this set of questions, intuitions, ideas – which even includes some tangible results. The most important thing seems to me to be the one pointed out in par. 2, namely the faithfulness of the outer action of  $\mathbb{I} = \text{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$  (and of its open subgroups) on  $\hat{\pi}_{0,3}$ , and more generally (if I remember rightly) on the fundamental group of any “anabelian” algebraic curve (i.e. whose genus  $g$  and “number of holes”  $\nu$  satisfy the equality  $2g + \nu \geq 3$ , i.e. such that  $\chi(X) < 0$ ) defined over a finite extension of  $\mathbb{Q}$ . This result can be considered to be essentially equivalent to Bielyi’s theorem – it is the first concrete manifestation, via a precise mathematical statement, of the “message” which was discussed above.

I would like to conclude this rapid outline with a few words of commentary on the truly unimaginable richness of a typical anabelian group such as  $\text{Sl}(2, \mathbb{Z})$  – doubtless the most remarkable discrete infinite group ever encountered, which appears in a multiplicity of avatars (of which certain have been briefly touched on in the present report), and which from the point of view of Galois-Teichmüller theory can be considered as the fundamental “building block” of the “Teichmüller tower”. The element of the structure of  $\text{Sl}(2, \mathbb{Z})$  which fascinates me above all is of course the outer action of  $\mathbb{I}$  on its profinite compactification. By Bielyi’s theorem, taking the profinite compactifications of subgroups of finite index of  $\text{Sl}(2, \mathbb{Z})$ , and the induced outer action (up to also passing to an open subgroup of  $\mathbb{I}$ ), we essentially find the fundamental groups of all algebraic curves (not necessarily compact) defined over number fields  $K$ , and the outer action of  $\text{Gal}(\overline{K}/K)$  on them – at least it is true that every such fundamental group appears as a quotient of one of the first groups (\*). Taking the “anabelian yoga” (which remains conjectural) into account, which says that an anabelian algebraic curve over a number field  $K$  (finite extension of  $\mathbb{Q}$ ) is known up to isomorphism when we know its mixed fundamental group (or what comes to the same thing, the outer action of  $\text{Gal}(\overline{K}/K)$  on its profinite geometric fundamental group), we can thus say that all algebraic curves defined over number fields are “contained” in the profinite compactification

(\*) In fact, we are considering quotients of a particularly trivial nature, by abelian subgroups which are products of “Tate modules”  $\hat{\mathbb{Z}}(1)$ , corresponding to “loop-groups” around points at infinity.

$\widehat{\mathrm{Sl}(2, \mathbb{Z})}$ , and in the knowledge of a certain subgroup  $\Gamma$  of its group of outer automorphisms! Passing to the abelianisations of the preceding fundamental groups, we see in particular that all the abelian  $\ell$ -adic representations dear to Tate and his circle, defined by Jacobians and generalised Jacobians of algebraic curves defined over number fields, are contained in this single action of  $\Gamma$  on the anabelian profinite group  $\widehat{\mathrm{Sl}(2, \mathbb{Z})}$ ! <sup>(4)</sup>

$\frac{17}{18}$

There are people who, faced with this, are content to shrug their shoulders with a disillusioned air and to bet that all this will give rise to nothing, except dreams. They forget, or ignore, that our science, and every science, would amount to little if since its very origins it were not nourished with the dreams and visions of those who devoted themselves to it.

4. From the very start of my reflection on 2-dimensional maps, I was most particularly interested by the “regular” maps, those whose automorphism group acts transitively (and consequently, simply transitively) on the set of flags. In the oriented case and in terms of the algebraic-geometric interpretation given in the preceding paragraph, it is these maps which correspond to Galois coverings of the projective line. Very quickly also, and even before the appearance of the link with algebraic geometry, it appears necessary not to exclude the infinite maps, which in particular occur in a natural way as universal coverings of finite maps. It appears (as an immediate consequence of the “dictionary” of maps, extended to the case of maps which are not necessarily finite) that for every pair of natural integers  $p, q \geq 1$ , there exists up to non-unique isomorphism one and only one 1-connected map of type  $(p, q)$ , i.e. all of whose vertices are of order  $p$  and whose faces are of order  $q$ , and this map is a regular map. It is pinned down by the choice of a flag, and its automorphism group is then canonically isomorphic to the quotient of the cartographic group (resp. of the oriented cartographic group, in the oriented case) by the additional relations

$$\rho_s^p = \rho_f^q = 1.$$

$\frac{18}{19}$

The case where this group is finite is the “Pythagorean” case of regular spherical maps, the case where it is infinite gives the regular tilings of the Euclidean plane or of the hyperbolic plane (\*). The link between combinatorial theory and the “conformal” theory of regular tilings of the hyperbolic plane was foreshadowed, before the appearance of the link between finite maps and finite coverings of the projective line. Once this link is understood,

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(\*) In these statements, we must not exclude the case where  $p, q$  can take the value  $+\infty$ , which is encountered in particular in a very natural way as tilings associated to certain regular infinite polyhedra, cf. below.

it becomes obvious that it should also extend to infinite maps (regular or not): every map, finite or not, can be canonically realised on a conformal surface (compact if and only if the map is finite), as a ramified covering of the complex projective line, ramified only over the points 0, 1 and  $\infty$ . The only difficulty here was to develop the dictionary between topological maps and sets with operators, which gave rise to some conceptual problems in the infinite case, starting with the very notion of a “topological map”. It appears necessary in particular, both for reasons of internal coherence of the dictionary and not to let certain interesting cases of infinite maps escape, to avoid excluding vertices and faces of infinite order. This foundational work was also done by J. Malgoire and C. Voisin, in the wake of their first work on finite maps, and their theory indeed gives everything that we could rightly expect (and even more...)

In 1977 and 1978, in parallel with two C4 courses on the geometry of the cube and that of the icosahedron, I began to become interested in regular polyhedra, which then appeared to me as particularly concrete “geometric realizations” of combinatorial maps, the vertices, edges and faces being realised as points, lines and planes respectively in a suitable 3-dimensional affine space, and respecting incidence relations. This notion of a geometric realisation of a combinatorial map keeps its meaning over an arbitrary base field, and even over an arbitrary base ring. It also keeps its meaning for regular polyhedra in any dimension, if the cartographic group  $\underline{C}_2$  is replaced by a suitable  $n$ -dimensional analogue  $\underline{C}_n$ . The case  $n = 1$ , i.e. the theory of regular polygons in any characteristic, was the subject of a DEA course in 1977/78, and already sparks the appearance of some new phenomena, as well as demonstrating the usefulness of working not in an ambient affine space (here the affine plane), but in a projective space. This is in particular due to the fact that in certain characteristics (in particular in characteristic 2) the centre of a regular polyhedron is sent off to infinity. Moreover, the projective context, contrarily to the affine context, enables us to easily develop a duality formalism for regular polyhedra, corresponding to the duality formalism of combinatorial or topological maps (where the roles of the vertices and the faces, in the case  $n = 2$  say, are exchanged). We find that for every projective regular polyhedron, we can define a canonical associated hyperplane, which plays the role of a canonical hyperplane at infinity, and allows us to consider the given polyhedron as an affine regular polyhedron.

The extension of the theory of regular polyhedra (and more generally, of all sorts of geometrico-combinatorial configurations, including root systems...) of the base field  $\mathbb{R}$  or  $\mathbb{C}$  to a general base ring, seems to me to have an importance comparable, in this part of geometry, to the analogous



extension which has taken place since the beginning of the century in algebraic geometry, or over the last twenty years in topology (\*), with the introduction of the language of schemes and of topoi. My sporadic reflection on this question, over some years, was limited to discovering some simple basic principles, concentrating my attention first and foremost on the case of pinned regular polyhedra, which reduces to a minimum the necessary conceptual baggage, and practically eliminates the rather delicate questions of rationality. For such a polyhedron, we find a canonical basis (or flag) of the ambient affine or projective space, such that the operations of the cartographic group  $\underline{C}_n$ , generated by the fundamental reflections  $\sigma_i$  ( $0 \leq i \leq n$ ), are written in that basis by universal formulae, in terms of the  $n$  parameters  $\alpha_1, \dots, \alpha_n$ , which can be geometrically interpreted as the doubles of the cosines of the “fundamental angles” of the polyhedron. The polyhedron can be reconstituted from this action, and from the affine or projective flag associated to the chosen basis, by transforming this flag by all the elements of the group generated by the fundamental reflections. Thus the “universal” pinned  $n$ -polyhedron is canonically defined over the ring of polynomials with  $n$  indeterminates

$$\mathbb{Z}[\underline{\alpha}_1, \dots, \underline{\alpha}_n],$$

its specialisations to arbitrary base fields  $k$  (via values  $\alpha_i \in k$  given to the indeterminates  $\underline{\alpha}_i$ ) giving regular polyhedra corresponding to various combinatorial types. In this game, there is no question of limiting oneself to finite regular polyhedra, nor even to regular polyhedra whose facets are of finite order, i.e. for which the parameters  $\alpha_i$  are roots of suitable “semicyclotomic” equations, expressing the fact that the “fundamental angles” (in the case where the base field is  $\mathbb{R}$ ) are commensurable with  $2\pi$ . Already when  $n = 1$ , perhaps the most interesting regular polygon (morally the regular polygon with only one side!) is the one corresponding to  $\alpha = 2$ , giving rise to a parabolic circumscribed conic, i.e. tangent to the line at infinity. The finite case is the one where the group generated by the fundamental reflections, which is also the automorphism group of the regular polyhedron considered, is finite. In the case where the base field is  $\mathbb{R}$  (or  $\mathbb{C}$ , which comes to the same thing), and for  $n = 2$ , the finite cases have been well-known since antiquity – which does not exclude that the schematic point of view unveils new charms; we can however say that when specialising the icosahedron (for example) to finite base fields of arbitrary characteristic, it remains

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(\*) In writing this, I am aware that rare are the topologists, even today, who realise the existence of this conceptual and technical generalisation of topology, and the resources it offers.

an icosahedron, with its own personal combinatorics and the same simple group of automorphisms of order 60. The same remark applies to finite regular polyhedra in higher dimension, which were systematically studied in two beautiful books by Coxeter. The situation is entirely different if we start from an infinite regular polyhedron, over a field such as  $\mathbb{Q}$ , for instance, and “specialise” it to the prime fields  $\mathbb{F}_p$  (a well-defined operation for all  $p$  except a finite number of primes). It is clear that every regular polyhedron over a finite field is finite – we thus find an infinity of finite regular polyhedra as  $p$  varies, whose combinatorial type, or equivalently, whose automorphism group varies “arithmetically” with  $p$ . This situation is particularly intriguing in the case where  $n = 2$ , where we can use the relation made explicit in the preceding paragraph between combinatorial 2-maps and algebraic curves defined over number fields. In this case, an infinite regular polyhedron defined over any infinite field (and therefore, over a sub- $\mathbb{Z}$ -algebra of it with two generators) thus gives rise to an infinity of algebraic curves defined over number fields, which are Galois coverings ramified only over 0, 1 and  $\infty$  of the standard projective line. The optimal case is of course the one deduced by passage to the field of fractions  $\mathbb{Q}(\alpha_1, \alpha_2)$  of its base ring. This raises a host of new questions, both vague and precise, none of which I have up till now had leisure to examine closely – I will cite only this one: exactly which are the finite regular 2-maps, or equivalently, the finite quotients of the 2-cartographic group, which come from regular 2-polyhedra over finite fields (\*)? Do we obtain them all, and if yes: how?

These reflections shed a special light on the fact, which to me was completely unexpected, that the theory of finite regular polyhedra, already in the case of dimension  $n = 2$ , is infinitely richer, and in particular gives infinitely many more different combinatorial forms, in the case where we admit base fields of non-zero characteristic, than in the case considered up to now, where the base fields were always restricted to  $\mathbb{R}$ , or at best  $\mathbb{C}$  (in the case of what Coxeter calls “complex regular polyhedra”, and which I prefer to call “regular pseudo-polyhedra defined over  $\mathbb{C}$ ”) (\*\*). Moreover, it seems that this extension of the point of view should also shed new light

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(\*) These are actually the same as those coming from regular polyhedra defined over arbitrary fields, or algebraically closed fields, as can be seen using standard specialisation arguments.

(\*\*) The pinned pseudo-polyhedra are described in the same way as the pinned polyhedra, with the only difference that the fundamental reflections  $\sigma_i$  ( $0 \leq i \leq n$ ) are here replaced by pseudo-reflections (which Coxeter assumes of finite order, since he restricts himself to finite combinatorial structures). This simply leads to the introduction for each of the  $\sigma_i$  of an additional numerical invariant  $\beta_i$ , such that the universal  $n$ -pseudo-polyhedron can

on the already known cases. Thus, examining the Pythagorean polyhedra one after the other, I saw that the same small miracle was repeated each time, which I called the combinatorial paradigm of the polyhedra under consideration. Roughly speaking, it can be described by saying that when we consider the specialisation of the polyhedra in the or one of the most singular characteristic(s) (namely characteristics 2 and 5 for the icosahedron, characteristic 2 for the octahedron), we read off from the geometric regular polyhedron over the finite field ( $\mathbb{F}_4$  and  $\mathbb{F}_5$  for the icosahedron,  $\mathbb{F}_2$  for the octahedron) a particularly elegant (and unexpected) description of the combinatorics of the polyhedron. It seems to me that I perceived there a principle of great generality, which I believed I found again for example in a later reflection on the combinatorics of the system of 27 lines on a cubic surface, and its relations with the root system  $E_7$ . Whether it happens that such a principle really exists, and even that we succeed in uncovering it from its cloak of fog, or that it recedes as we pursue it and ends up vanishing like a Fata Morgana, I find in it for my part a force of motivation, a rare fascination, perhaps similar to that of dreams. No doubt that following such an unformulated call, the unformulated seeking form, from an elusive glimpse which seems to take pleasure in simultaneously hiding and revealing itself – can only lead far, although no one could predict where...

However, occupied by other interests and tasks, I have not up to now followed this call, nor met any other person willing to hear it, much less to follow it. Apart from some digressions towards other types of geometrico-combinatorial structures, my work on the question has been limited to a first effort of refining and housekeeping, which it is useless for me to describe further here <sup>(5)</sup>. The only point which perhaps still deserves to be mentioned is the existence and uniqueness of a hyperquadric circumscribing a given regular  $n$ -polyhedron, whose equation can be given explicitly by simple formulae in terms of the fundamental parameters  $\alpha_i$  (\*). The case which interests me most is when  $n = 2$ , and the moment seems ripe to rewrite a new version, in modern style, of Klein's classic book on the icosahedron and the other Pythagorean polyhedra. Writing such an exposé on regular 2-polyhedra would be a magnificent opportunity for a young researcher to familiarise himself with the geometry of polyhedra as well as

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also be defined over a ring of polynomials with integral coefficients, in the  $n + (n + 1)$  variables  $\alpha_i$  ( $1 \leq i \leq n$ ) and  $\beta_j$  ( $0 \leq j \leq n$ ).

(\*) An analogous result is valid for pseudo-polyhedra. It would seem that the “exceptional characteristics” we discussed above, for specialisations of a given polyhedron, are those for which the circumscribed hyperquadric is either degenerate or tangent to the hyperplane at infinity.

their connections with spherical, Euclidean and hyperbolic geometry and with algebraic curves, and with the language and the basic techniques of modern algebraic geometry. Will there be found one, some day, who will seize this opportunity?

5. I would like to say a few words now about some topological considerations which have made me understand the necessity of new foundations for “geometric” topology, in a direction quite different from the notion of topos, and actually independent of the needs of so-called “abstract” algebraic geometry (over general base fields and rings). The problem I started from, which already began to intrigue me some fifteen years ago, was that of defining a theory of “dévissage” for stratified structures, in order to rebuild them, via a canonical process, out of “building blocks” canonically deduced from the original structure. Probably the main example which had led me to that question was that of the canonical stratification of a singular algebraic variety (or a complex or real singular space) through the decreasing sequence of its successive singular loci. But I probably had the premonition of the ubiquity of stratified structures in practically all domains of geometry (which surely others had seen clearly a long time before). Since then, I have seen such structures appear, in particular, in any situation where “moduli” are involved for geometric objects which may undergo not only continuous variations, but also “degeneration” (or “specialisation”) phenomena – the strata corresponding then to the various “levels of singularity” (or to the associated combinatorial types) for the objects in question. The compactified modular multiplicities  $\widehat{M}_{g,\nu}$  of Mumford-Deligne for the stable algebraic curves of type  $(g, \nu)$  provide a typical and particularly inspiring example, which played an important motivating role when I returned to my reflection about stratified structures, from December 1981 to January 1982. Two-dimensional geometry provides many other examples of such modular stratified structures, which all (if not using rigidification) appear as “multiplicities” rather than as spaces or manifolds in the usual sense (as the points of these multiplicities may have non-trivial automorphism groups). Among the objects of two-dimensional geometry which give rise to such modular stratified structures in arbitrary dimensions, or even infinite dimension, I would list polygons (Euclidean, spherical or hyperbolic), systems of straight lines in a plane (say projective), systems of “pseudo straight lines” in a projective topological plane, or more general immersed curves with normal crossings, in a given (say compact) surface.

The simplest non-trivial example of a stratified structure is obtained by considering a pair  $(X, Y)$  of a space  $X$  and a closed subspace  $Y$ , with a suitable assumption of equisingularity of  $X$  along  $Y$ , and assuming moreover

(to fix ideas) that both strata  $Y$  and  $X \setminus Y$  are topological manifolds. The naive idea, in such a situation, is to consider “the” tubular neighbourhood  $T$  of  $Y$  in  $X$ , whose boundary  $\partial T$  should also be a smooth manifold, fibred with compact smooth fibres over  $Y$ , whereas  $T$  itself can be identified with the conical fibration associated to the above one. Setting

$$U = X \setminus \text{Int}(T),$$

one finds a manifold with boundary, whose boundary is canonically isomorphic to the boundary of  $T$ . This being said, the “building blocks” are the manifold with boundary  $U$  (compact if  $X$  is compact, and which replaces and refines the “open” stratum  $X \setminus Y$ ) and the manifold (without boundary)  $Y$ , together with, as an additional structure which connects them, the “glueing” map

$$f : \partial U \longrightarrow Y$$

which is a proper and smooth fibration. The original situation  $(X, Y)$  can be recovered from  $(U, Y, f : \partial U \rightarrow Y)$  via the formula

$$X \cong U \coprod_{\partial U} Y$$

(amalgamated sum over  $\partial U$ , mapping into  $U$  and  $Y$  by inclusion resp. the glueing map).

This naive vision immediately encounters various difficulties. The first is the somewhat vague nature of the very notion of tubular neighbourhood, which acquires a tolerably precise meaning only in the presence of structures which are much more rigid than the mere topological structure, such as “piecewise linear” or Riemannian (or more generally, space with a distance function) structure; the trouble here is that in the examples which naturally come to mind, one does not have such structures at one’s disposal – at best an equivalence class of such structures, which makes it possible to rigidify the situation somewhat. If on the other hand one assumes that one might find an expedient in order to produce a tubular neighbourhood having the desired properties, which moreover would be unique modulo an automorphism (say a topological one) of the situation – an automorphism which moreover respects the fibred structure provided by the glueing map, there still remains the difficulty arising from the lack of canonicity of the choices involved, as the said automorphism is obviously not unique, whatever may be done in order to “normalise” it. The idea here, in order to make canonical something which is not, is to work systematically in the framework of the “isotopic categories” associated to the categories of a topological nature which are naturally present in such questions (such as the category of

admissible pairs  $(X, Y)$  and homeomorphisms of such pairs etc.), retaining the same objects, but defining as “morphisms” the isotopy classes (in a sense which is dictated unambiguously by the context) of isomorphisms (or even morphisms more general than isomorphisms). I used this idea, which is taken up successfully in the thesis of Yves Ladegaillerie (see beginning of par. 3), in a systematic way in all my later reflections on combinatorial topology, when it came to a precise formulation of translation theorems of topological situations in terms of combinatorial situations. In the present situation, my hope was to be able to formulate (and prove!) a theorem of equivalence between two suitable isotopic categories, one being the category of “admissible pairs”  $(X, Y)$ , and the other the category of “admissible triples”  $(U, Y, f)$ , where  $Y$  is a manifold,  $U$  a manifold with boundary, and  $f : \partial U \rightarrow Y$  a smooth and proper fibration. Moreover, I hoped that such a statement could be naturally extended, modulo some essentially algebraic work, to a more general statement, which would apply to general stratified structures.

It soon appeared that there could be no question of getting such an ambitious statement in the framework of topological spaces, because of the sempiternal “wild” phenomena. Already when  $X$  itself is a manifold and  $Y$  is reduced to a point, one is confronted with the difficulty that the cone over a compact space  $Z$  can be a manifold at its vertex, even if  $Z$  is not homeomorphic to a sphere, nor even a manifold. It was also clear that the contexts of the most rigid structures which existed then, such as the “piecewise linear” context were equally inadequate – one common disadvantage consisting in the fact that they do not make it possible, given a pair  $(U, S)$  of a “space”  $U$  and a closed subspace  $S$ , and a glueing map  $f : S \rightarrow T$ , to build the corresponding amalgamated sum. Some years later, I was told of Hironaka’s theory of what he calls, I believe, (real) “semi-analytic” sets which satisfy certain essential stability conditions (actually probably all of them), which are necessary to develop a usable framework of “tame topology”. This triggered a renewal of the reflection on the foundations of such a topology, whose necessity appears more and more clearly to me.

After some ten years, I would now say, with hindsight, that “general topology” was developed (during the thirties and forties) by analysts and in order to meet the needs of analysis, not for topology per se, i.e. the study of the topological properties of the various geometrical shapes. That the foundations of topology are inadequate is manifest from the very beginning, in the form of “false problems” (at least from the point of view of the topological intuition of shapes) such as the “invariance of domains”, even if the solution to this problem by Brouwer led him to introduce new geometrical ideas. Even now, just as in the heroic times when one anxiously witnessed

for the first time curves cheerfully filling squares and cubes, when one tries to do topological geometry in the technical context of topological spaces, one is confronted at each step with spurious difficulties related to wild phenomena. For instance, it is not really possible, except in very low dimensions, to study for a given space  $X$  (say a compact manifold), the homotopy type of (say) the automorphism group of  $X$ , or of the space of embeddings, or immersions etc. of  $X$  into some other space  $Y$  – whereas one feels that these invariants should be part of the toolbox of the essential invariants attached to  $X$ , or to the pair  $(X, Y)$ , etc. just as the function space  $\underline{\text{Hom}}(X, Y)$  which is familiar in homotopical algebra. Topologists elude the difficulty, without tackling it, moving to contexts which are close to the topological one and less subject to wildness, such as differentiable manifolds, PL spaces (piecewise linear) etc., of which it is clear that none is “good”, i.e. stable under the most obvious topological operations, such as contraction-glueing operations (not to mention operations like  $X \rightarrow \text{Aut}(X)$  which oblige one to leave the paradise of finite dimensional “spaces”). This is a way of beating about the bush! This situation, like so often already in the history of our science, simply reveals the almost insurmountable inertia of the mind, burdened by a heavy weight of conditioning, which makes it difficult to take a real look at a foundational question, thus at the context in which we live, breathe, work – accepting it, rather, as immutable data. It is certainly this inertia which explains why it tooks millenia before such childish ideas as that of zero, of a group, of a topological shape found their place in mathematics. It is this again which explains why the rigid framework of general topology is patiently dragged along by generation after generation of topologists for whom “wildness” is a fatal necessity, rooted in the nature of things.

My approach toward possible foundations for a tame topology has been an axiomatic one. Rather than declaring (which would indeed be a perfectly sensible thing to do) that the desired “tame spaces” are no other than (say) Hironaka’s semianalytic spaces, and then developing in this context the toolbox of constructions and notions which are familiar from topology, supplemented with those which had not been developed up to now, for that very reason, I preferred to work on extracting which exactly, among the geometrical properties of the semianalytic sets in a space  $\mathbb{R}^n$ , make it possible to use these as local “models” for a notion of “tame space” (here semianalytic), and what (hopefully!) makes this notion flexible enough to use it effectively as the fundamental notion for a “tame topology” which would express with ease the topological intuition of shapes. Thus, once this necessary foundational work has been completed, there will appear not one “tame theory”, but a vast infinity, ranging from the strictest of all, the one which deals with “piecewise  $\overline{\mathbb{Q}_r}$ -algebraic spaces” (with  $\overline{\mathbb{Q}_r} = \overline{\mathbb{Q}} \cap \mathbb{R}$ ), to the one which

$\frac{29}{30}$

$\frac{30}{31}$

appears (whether rightly or not) to be likely to be the vastest of all, namely using “piecewise real analytic spaces” (or semianalytic using Hironaka’s terminology). Among the foundational theorems which I envision in my programme, there is a comparison theorem which, to put it vaguely, would say that one will essentially find the same isotopic categories (or even  $\infty$ -isotopic) whatever the tame theory one is working with. In a more precise way, the question is to put one’s finger on a system of axioms which is rich enough to imply (among many other things) that if one has two tame theories  $\mathcal{T}$ ,  $\mathcal{T}'$  with  $\mathcal{T}$  finer than  $\mathcal{T}'$  (in the obvious sense), and if  $X, Y$  are two  $\mathcal{T}$ -tame spaces, which thus also define corresponding  $\mathcal{T}'$ -tame spaces, the canonical map

$$\underline{\text{Isom}}_{\mathcal{T}}(X, Y) \rightarrow \underline{\text{Isom}}_{\mathcal{T}'}(X, Y)$$

induces a bijection on the set of connected components (which will imply that the isotopic category of the  $\mathcal{T}$ -spaces is equivalent to the  $\mathcal{T}'$ -spaces), and is even a homotopy equivalence (which means that one even has an equivalence for the “ $\infty$ -isotopic” categories, which are finer than the isotopic categories in which one retains only the  $\pi_0$  of the spaces of isomorphisms). Here the Isom may be defined in an obvious way, for instance as semisimplicial sets, in order to give a precise meaning to the above statement. Analogous statements should be true, if one replaces the “spaces” Isom with other spaces of maps, subject to standard geometric conditions, such as those of being embeddings, immersions, smooth, etale, fibrations etc. One also expects analogous statements where  $X, Y$  are replaced by systems of tame spaces, such as those which occur in a theory of dévissage of stratified structures – so that in a precise technical sense, this dévissage theory will also be essentially independent of the tame theory chosen to express it.

$\frac{31}{32}$

The first decisive test for a good system of axioms defining the notion of a “tame subset of  $\mathbb{R}^n$ ” seems to me to consist in the possibility of proving such comparison theorems. I have settled for the time being for extracting a temporary system of plausible axioms, without any assurance that other axioms will not have to be added, which only working on specific examples will cause to appear. The strongest among the axioms I have introduced, whose validity is (or will be) most likely the most delicate to check in specific situations, is a triangulability axiom (in a tame sense, it goes without saying) of a tame part of  $\mathbb{R}^n$ . I did not try to prove the comparison theorem in terms of these axioms only, however I had the impression (right or wrong again!) that this proof, whether or not it necessitates the introduction of some additional axiom, will not present serious technical difficulties. It may well be that the technical difficulties in the development of satisfactory foundations for tame topology, including a theory of dévissage for



tame stratified structures are actually already essentially concentrated in the axioms, and consequently already essentially overcome by triangulability theorems à la Lojasiewicz and Hironaka. What is again lacking is not the technical virtuosity of the mathematicians, which is sometimes impressive, but the audacity (or simply innocence...) to free oneself from a familiar context accepted by a flawless consensus...

The advantages of an axiomatic approach towards the foundations of tame topology seem to me to be obvious enough. Thus, in order to consider a complex algebraic variety, or the set of real points of an algebraic variety defined over  $\mathbb{R}$ , as a tame space, it seems preferable to work with the “piecewise  $\mathbb{R}$ -algebraic” theory, maybe even the  $\overline{\mathbb{Q}_r}$ -algebraic theory (with  $\overline{\mathbb{Q}_r} = \overline{\mathbb{Q}} \cap \mathbb{R}$ ) when dealing with varieties defined over number fields, etc. The introduction of a subfield  $K \subset \mathbb{R}$  associated to the theory  $\mathcal{T}$  (consisting in the points of  $\mathbb{R}$  which are  $\mathcal{T}$ -tame, i.e. such that the corresponding one-point set is  $\mathcal{T}$ -tame) make it possible to introduce for any point  $x$  of a tame space  $X$ , a residue field  $k(x)$ , which is an algebraically closed subextension of  $\mathbb{R}/K$ , of finite transcendence degree over  $K$  (bounded by the topological dimension of  $X$ ). When the transcendence degree of  $\mathbb{R}$  over  $K$  is infinite, we find a notion of transcendence degree (or “dimension”) of a point of a tame space, close to the familiar notion in algebraic geometry. Such notions are absent from the “semianalytic” tame topology, which however appears as the natural topological context for the inclusion of real and complex analytic spaces.

Among the first theorems one expects in a framework of tame topology as I perceive it, aside from the comparison theorems, are the statements which establish, in a suitable sense, the existence and uniqueness of “the” tubular neighbourhood of closed tame subspace in a tame space (say compact to make things simpler), together with concrete ways of building it (starting for instance from any tame map  $X \rightarrow \mathbb{R}^+$  having  $Y$  as its zero set), the description of its “boundary” (although generally it is in no way a manifold with boundary!)  $\partial T$ , which has in  $T$  a neighbourhood which is isomorphic to the product of  $T$  with a segment, etc. Granted some suitable equisingularity hypotheses, one expects that  $T$  will be endowed, in an essentially unique way, with the structure of a locally trivial fibration over  $Y$ , with  $\partial T$  as a subfibration. This is one of the least clear points in my temporary intuition of the situation, whereas the homotopy class of the predicted structure map  $T \rightarrow Y$  has an obvious meaning, independent of any equisingularity hypothesis, as the homotopic inverse of the inclusion map  $Y \rightarrow T$ , which must be a homotopism. One way to a posteriori obtain such a structure would be via the hypothetical equivalence of isotopic categories which was considered at the beginning, taking into account the fact that the functor

$(U, Y, f) \mapsto (X, Y)$  is well-defined in an obvious way, independently of any theory of tubular neighbourhoods.

It will perhaps be said, not without reason, that all this may be only dreams, which will vanish in smoke as soon as one sets to work on specific examples, or even before, taking into account some known or obvious facts which have escaped me. Indeed, only working out specific examples will make it possible to sift the right from the wrong and to reach the true substance. The only thing in all this which I have no doubt about, is the very necessity of such a foundational work, in other words, the artificiality of the present foundations of topology, and the difficulties which they cause at each step. It may be however that the formulation I give of a theory of dévissage of stratified structures in terms of an equivalence theorem of suitable isotopic (or even  $\infty$ -isotopic) categories is actually too optimistic. But I should add that I have no real doubts about the fact that the theory of these dévissages which I developed two years ago, although it remains in part heuristic, does indeed express some very tangible reality. In some part of my work, for want of a ready-to-use “tame” context, and in order to have precise and provable statements, I was led to postulate some very plausible additional structures on the stratified structure I started with, especially concerning the local retraction data, which do make it possible to construct a canonical system of spaces, parametrised by the ordered set of flags  $\text{Drap}(I)$  of the ordered set  $I$  indexing the strata; these spaces play the role of the spaces  $(U, Y)$  above, and they are connected by embedding and proper fibration maps, which make it possible to reconstitute in an equally canonical way the original stratified structure, including these “additional structures” (7). The only trouble here, is that these appear as an additional artificial element of structure, which is no way part of the data in the usual geometric situations, as for example the compact moduli space  $\widehat{M}_{g,\nu}$  with its canonical “stratification at infinity”, defined by the Mumford-Deligne divisor with normal crossings. Another, probably less serious difficulty, is that this so-called moduli “space” is in fact a multiplicity – which can be technically expressed by the necessity of replacing the index set  $I$  for the strata with an (essentially finite) category of indices, here the “MD graphs” which “parametrise” the possible “combinatorial structures” of a stable curve of type  $(g, \nu)$ . This said, I can assert that the general theory of dévissage, which has been developed especially to meet the needs of this example, has indeed proved to be a precious guide, leading to a progressive understanding, with flawless coherence, of some essential aspects of the Teichmüller tower (that is, essentially the “structure at infinity” of the ordinary Teichmüller groups). It is this approach which finally led me, within some months, to the principle of a purely combinatorial construction of the tower

of Teichmüller groupoids, in the spirit sketched above (cf. par. 2).

Another satisfying test of coherence comes from the “topossic” viewpoint. Indeed, as my interest for the multiplicities of moduli was first prompted by their algebrico-geometric and arithmetic meaning, I was first and foremost interested by the modular algebraic multiplicities, over the absolute basefield  $\mathbb{Q}$ , and by a “dévissage” at infinity of their geometric fundamental groups (i.e. of the profinite Teichmüller groups) which would be compatible with the natural operations of  $\Gamma = \text{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$ . This requirement seemed to exclude from the start the possibility of a reference to a hypothetical theory of dévissage of stratified structures in a context of “tame topology” (or even, at worst, of ordinary topology), beyond a purely heuristic guiding thread. Thus the question arose of translating, in the context of the topoi (here étale topoi) which were present in the situation, the theory of dévissage I had arrived at in a completely different context – with the additional task, in the sequel, of extracting a general comparison theorem, patterned after well-known theorems, in order to compare the invariants (in particular the homotopy types of various tubular neighbourhoods) obtained in the transcendent and schematic frameworks. I have been able to convince myself that such a formalism of dévissage indeed had some meaning in the (so-called “abstract”!) context of general topoi, or at least noetherian topoi (like those occurring in this situation), via a suitable notion of canonical tubular neighbourhood of a subtopos in an ambient topos. Once this notion is acquired, together with some simple formal properties, the description of the “dévissage” of a stratified topos is even considerably simpler in that framework than in the (tame) topological one. True, there is foundational work to be done here too, especially around the very notion of the tubular neighbourhood of a subtopos – and it is actually surprising that this work (as far as I know) has still never been done, i.e. that no one (since the context of étale topology appeared, more than twenty years ago) apparently ever felt the need for it; surely a sign that the understanding of the topological structure of schemes has not made much progress since the work of Artin-Mazur...

Once I had accomplished this (more or less heuristic) double work of refining the notion of dévissage of a stratified space or topos, which was a crucial step in my understanding of the modular multiplicities, it actually appeared that, as far as these are concerned, one can actually take a short cut for at least a large part of the theory, via direct geometric arguments. Nonetheless, the formalism of dévissage which I reached has proved its usefulness and its coherence to me, independently of any question about the most adequate foundations which make it completely meaningful.

$\frac{36}{37}$

6. One of the most interesting foundational theorems of (tame) topology which should be developed would be a theorem of “dévissage” (again!) of a proper tame map of tame spaces

$$f : X \rightarrow Y,$$

via a decreasing filtration of  $Y$  by closed tame subspaces  $Y^i$ , such that above the “open strata”  $Y^i \setminus Y^{i-1}$  of this filtration,  $f$  induces a locally trivial fibration (from the tame point of view, it goes without saying). It should be possible to generalise such a statement even further and to make it precise in various ways, in particular by requiring the existence of an analogous simultaneous dévissage for  $X$  and for a given finite family of (tame) closed subspaces of  $X$ . Also the very notion of locally trivial fibration in the tame sense can be made considerably stronger, taking into account the fact that the open strata  $U_i$  are better than spaces whose tame structure is purely local, because they are obtained as differences of two tame spaces, compact if  $Y$  is compact. Between the notion of a compact tame space (which is realised as one of the starting “models” in an  $\mathbb{R}^n$ ) and that of a “locally tame” (locally compact) space which can be deduced from it in a relatively obvious way, there is a somewhat more delicate notion of a “globally tame” space  $X$ , obtained as the difference  $\hat{X} \setminus Y$  of two compact tame spaces, it being understood that we do not distinguish between the space defined by a pair  $(\hat{X}, Y)$  and that defined by a pair  $(\hat{X}', Y')$  deduced from it by a (necessarily proper) tame map

$$g : \hat{X}' \rightarrow \hat{X}$$

$\frac{37}{38}$

inducing a bijection  $g^{-1}(X) \rightarrow X$ , taking  $Y' = g^{-1}(Y)$ . Perhaps the most interesting natural example is the one where we start from a separated scheme of finite type over  $\mathbb{C}$  or  $\mathbb{R}$ , taking for  $X$  the set of its real or complex points, which inherits a global tame structure with the help of schematic compactifications (which exist according to Nagata) of the scheme we started with. This notion of a globally tame space is associated to a notion of a globally tame map, which in turn allows us to strengthen the notion of a locally trivial fibration, in stating a theorem of dévissage for a map  $f : X \rightarrow Y$  (now not necessarily proper) in the context of globally tame spaces.

I was informed last summer by Zoghman Mebkhout that a theorem of dévissage in this spirit has been recently obtained in the context of real and/or complex analytic spaces, with  $Y^i$  which here are analytic subspaces of  $Y$ . This result makes it plausible that we already have at our disposal techniques which are powerful enough to also prove a dévissage theorem in the tame context, apparently more general, but probably less arduous.

The context of tame topology should also, it seems to me, make it possible to formulate with precision a certain very general principle which I frequently use in a great variety of geometric situations, which I call the “principle of anodine choices” – as useful as vague in appearance! It says that when for the needs of some construction of a geometric object in terms of others, we are led to make a certain number of arbitrary choices along the way, so that the final object appears to depend on these choices, and is thus stained with a defect of canonicity, that this defect is indeed serious (and to be removed requires a more careful analysis of the situation, the notions used, the data introduced etc.) whenever at least one of these choices is made in a space which is not “contractible”, i.e. whose  $\pi_0$  or one of whose higher invariants  $\pi_i$  is non-trivial, and that this defect is on the contrary merely apparent, and the construction itself is “essentially” canonical and will not bring along any troubles, whenever the choices made are all “anodine”, i.e. made in contractible spaces. When we try in actual examples to really understand this principle, it seems that each time we stumble onto the same notion of “ $\infty$ -isotopic categories” expressing a given situation, and finer than the more naive isotopic (= 0-isotopic) categories obtained by considering only the  $\pi_0$  of the spaces of isomorphisms introduced in the situation, while the  $\infty$ -isotopic point of view considers all of their homotopy type. For example, the naive isotopic point of view for compact surfaces with boundary of type  $(g, \nu)$  is “good” (without any hidden boomerangs!) exactly in the cases which I call “anabelian” (and which Thurston calls “hyperbolic”), i.e. distinct from  $(0, 0)$ ,  $(0, 1)$ ,  $(0, 2)$ ,  $(1, 0)$  – which are also exactly the cases where the connected component of the identity of the automorphism group of the surface is contractible. In the other cases, except for the case  $(0, 0)$  of the sphere without holes, it suffices to work with 1-isotopic categories to express in a satisfying way via algebra the essential geometrico-topological facts, since the said connected component is then a  $K(\pi, 1)$ . Working in a 1-isotopic category actually comes down to working in a bicategory, i.e. with  $\underline{\text{Hom}}(X, Y)$  which are (no longer discrete sets as in the 0-isotopic point of view, but) groupoids (whose  $\pi_0$  are exactly the 0-isotopic Hom). This is the description in purely algebraic terms of this bicategory which is given in the last part of the thesis of Yves Ladegaillerie (cf. par. 3).

If I allowed myself to dwell here at some length on the theme of the foundations of tame topology, which is not one of those to which I intend to devote myself prioritarilly in the coming years, it is doubtless because I feel that it is yet another cause which needs to be pleaded, or rather: a work of great current importance which needs hands! Just as years ago for the new foundations of algebraic geometry, it is not pleadings which will surmount

the inertia of acquired habits, but tenacious, meticulous long-term work, which will from day to day bring eloquent harvests.

I would like to say some last words on an older reflection (end of the sixties?), very close to the one I just discussed, inspired by ideas of Nash which I found very striking. Instead of axiomatically defining a notion of “tame theory” via a notion of a “tame part of  $\mathbb{R}^n$ ” satisfying certain conditions (mainly of stability), I was interested by an axiomatisation of the notion of “non-singular variety” via, for each natural integer  $n$ , a subring  $\mathcal{A}_n$  of the ring of germs of real functions at the origin in  $\mathbb{R}^n$ . These are the functions which will be admitted to express the “change of chart” for the corresponding notion of  $\mathcal{A}_n$ -variety, and I was first concerned with uncovering a system of axioms on the system  $\mathcal{A} = (\mathcal{A}_n)_{n \in \mathbb{N}}$  which ensures for this notion of variety a suppleness comparable to that of a  $C^\infty$  variety, or a real analytic one (or a Nash one). According to the familiar type of construction which one wants to be able to do in the context of  $\mathcal{A}$ -varieties, the relevant system of axioms is more or less reduced or rich. One doesn’t need much if one only wants to develop the differential formalism, with the construction of jet bundles, De Rham complexes etc. If we want a statement of the type “quasi-finite implies finite” (for a map in the neighbourhood of a point), which appeared as a key statement in the local theory of analytic spaces, we need a more delicate stability axiom, in Weierstrass’ “Vorbereitungssatz” (\*). In other questions, a stability axiom by analytic continuation (in  $\mathbb{C}^n$ ) appears necessary. The most Draconian axiom which I was led to introduce, also a stability axiom, concerns the integration of Pfaff systems, ensuring that certain (even all) Lie groups are  $\mathcal{A}$ -varieties. In all this, I took care not to suppose that the  $\mathcal{A}_n$  are  $\mathbb{R}$ -algebras, so a constant function on a  $\mathcal{A}$ -variety is “admissible” only if its value belongs to a certain subfield  $K$  of  $\mathbb{R}$  (which is, if one likes,  $\mathcal{A}_0$ ). This subfield can very well be  $\mathbb{Q}$ , or its algebraic closure  $\overline{\mathbb{Q}}_r$  in  $\mathbb{R}$ , or any other subextension of  $\mathbb{R}/\mathbb{Q}$ , preferably even of finite or at least countable transcendence degree over  $\mathbb{Q}$ . This makes it possible, for example, as before for tame spaces, to have every point  $x$  of a variety (of type  $\mathcal{A}$ ) correspond to a residue field  $k(x)$ , which is a subextension of  $\mathbb{R}/K$ . A fact which appears important to me here, is that even in its strongest form, the system of axioms does not imply that we must have  $K = \mathbb{R}$ . More precisely, because all the axioms are stability axioms, it follows that for a given set  $S$  of germs of real analytic functions at the origin (in various spaces  $\mathbb{R}^n$ ), there exists a smaller theory

$\frac{40}{41}$

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(\*) It could seem simpler to say that the (local) rings  $\mathcal{A}_n$  are Henselian, which is equivalent. But it is not at all clear a priori in this latter form that the condition in question is in the nature of a stability condition, and this is an important circumstance as will appear in the following reflections.

$\mathcal{A}$  for which these germs are admissible, and that it is “countable”, i.e. the  $\mathcal{A}_n$  are countable, whenever  $S$  is. A fortiori,  $K$  is then countable, i.e. of countable transcendence degree over  $\mathbb{Q}$ .

The idea here is to introduce, via this axiomatic system, a notion of an “elementary” (real analytic) function, or rather, a whole hierarchy of such notions. For a function of 0 variables i.e. a constant, this notion gives that of an “elementary constant”, including in particular (in the case of the strongest axiomatic system) constants such as  $\pi$ ,  $e$  and many others, taking values of admissible functions (such as exponentials, logarithms etc.) for systems of “admissible” values of the argument. One feels that the relation between the system  $\mathcal{A} = (\mathcal{A}_n)_{n \in \mathbb{N}}$  and the corresponding rationality field  $K$  must be very tight, at least for  $\mathcal{A}$  which can be generated by a finite “system of generators”  $S$  – but one must fear that even the least of the interesting questions one could ask about this situation still remains out of reach <sup>(1)</sup>.

$\frac{41}{42}$

These old reflections have taken on some current interest for me due to my more recent reflection on tame theories. Indeed, it seems to me that it is possible to associate in a natural way to a “differentiable theory”  $\mathcal{A}$  a tame theory  $\mathcal{T}$  (doubtless having the same field of constants), in such a way that every  $\mathcal{A}$ -variety is automatically equipped with a  $\mathcal{T}$ -tame structure and conversely for every  $\mathcal{T}$ -tame compact space  $X$ , we can find a rare tame closed subset  $Y$  in  $X$ , such that  $X \setminus Y$  comes from an  $\mathcal{A}$ -variety, and moreover such that this  $\mathcal{A}$ -variety structure is unique at least in the following sense: two such structures coincide in the complement of a rare tame subset  $Y' \supset Y$  of  $X$ . The theory of dévissage of stratified tame structures (which was discussed in the preceding par.), in the case of smooth strata, should moreover raise much more precise questions of comparison of tame structures with structures of differentiable (or rather,  $\mathbb{R}$ -analytic) type. I suspect that the type of axiomatisation proposed here for the notion of “differentiable theory” would give a natural framework for the formulation of such questions with all desirable precision and generality.

7. Since the month of March last year, so nearly a year ago, the greater part of my energy has been devoted to a work of reflection on the foundations of non-commutative (co)homological algebra, or what is the same, after all, of homotopic algebra. These reflections have taken the concrete form of a voluminous stack of typed notes, destined to form the first volume (now being finished) of a work in two volumes to be published by Hermann, under the overall title “Pursuing Stacks”. I now foresee (after successive extensions of the initial project) that the manuscript of the whole of the two volumes, which I hope to finish definitively in the course of this year, will be about

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1500 typed pages in length. These two volumes are moreover for me the first in a vaster series, under the overall title “Mathematical Reflections”, in which I intend to develop some of the themes sketched in the present report.

Since I am speaking here of work which is actually now being written up and is even almost finished, the first volume of which will doubtless appear this year and will contain a detailed introduction, it is undoubtedly less interesting for me to develop this theme of reflection here, and I will content myself with speaking of it only very briefly. This work seems to me to be somewhat marginal with respect to the themes I sketched before, and does not (it seems to me) represent a real renewal of viewpoint or approach with respect to my interests and my mathematical vision of before 1970. If I suddenly resolved to do it, it is almost out of desperation, for nearly twenty years have gone by since certain visibly fundamental questions, which were ripe to be thoroughly investigated, without anyone seeing them or taking the trouble to fathom them. Still today, the basic structures which occur in the homotopic point of view in topology are not understood, and to my knowledge, after the work of Verdier, Giraud and Illusie on this theme (which are so many beginnings still waiting for continuations...) there has been no effort in this direction. I should probably make an exception for the axiomatisation work done by Quillen on the notion of a category of models, at the end of the sixties, and taken up in various forms by various authors. At that time, and still now, this work seduced me and taught me a great deal, even while going in quite a different direction from the one which was and still is close to my heart. Certainly, it introduces derived categories in various non-commutative contexts, but without entering into the question of the essential internal structures of such a category, also left open in the commutative case by Verdier, and after him by Illusie. Similarly, the question of putting one’s finger on the natural “coefficients” for a non-commutative cohomological formalism, beyond the stacks (which should be called 1-stacks) studied in the book by Giraud, remained open – or rather, the rich and precise intuitions concerning it, taken from the numerous examples coming in particular from algebraic geometry, are still waiting for a precise and supple language to give them form.

I returned to certain aspects of these foundational questions in 1975, on the occasion (I seem to remember) of a correspondence with Larry Breen (two letters from this correspondence will be reproduced as an appendix to Chap. I of volume 1, “History of Models”, of Pursuing Stacks). At that moment the intuition appeared that  $\infty$ -groupoids should constitute particularly adequate models for homotopy types, the  $n$ -groupoids corresponding to truncated homotopy types (with  $\pi_i = 0$  pour  $i > n$ ). This same intu-



ition, via very different routes, was discovered by Ronnie Brown and some of his students in Bangor, but using a rather restrictive notion of  $\infty$ -groupoid (which, among the 1-connected homotopy types, model only products of Eilenberg-Mac Lane spaces). Stimulated by a rather haphazard correspondence with Ronnie Brown, I finally began this reflection, starting with an attempt to define a wider notion of  $\infty$ -groupoid (later rebaptised stack in  $\infty$ -groupoids or simply “stack”, the implication being: over the 1-point topos), and which, from one thing to another, led me to Pursuing Stacks. The volume “History of Models” is actually a completely unintended digression with respect to the initial project (the famous stacks being temporarily forgotten, and supposed to reappear only around page 1000...).

$\frac{44}{45}$

This work is not completely isolated with respect to my more recent interests. For example, my reflection on the modular multiplicities  $\widehat{M}_{g,\nu}$  and their stratified structure renewed the reflection on a theorem of van Kampen in dimension  $> 1$  (also one of the preferred themes of the group in Bangor), and perhaps also contributed to preparing the ground for the more important work of the following year. This also links up from time to time with a reflection dating from the same year 1975 (or the following year) on a “De Rham complex with divided powers”, which was the subject of my last public lecture, at the IHES in 1976; I lent the manuscript of it to I don’t remember whom after the talk, and it is now lost. It was at the moment of this reflection that the intuition of a “schematisation” of homotopy types germinated, and seven years later I am trying to make it precise in a (particularly hypothetical) chapter of the History of Models.

The work of reflection undertaken in Pursuing Stacks is a little like a debt which I am paying towards a scientific past where, for about fifteen years (from 1955 to 1970), the development of cohomological tools was the constant Leitmotiv in my foundational work on algebraic geometry. If in this renewal of my interest in this theme, it has taken on unexpected dimensions, it is however not out of pity for a past, but because of the numerous unexpected phenomena which ceaselessly appear and unceremoniously shatter the previously laid plans and projects – rather like in the thousand and one nights, where one awaits with bated breath through twenty other tales the final end of the first.

8. Up to now I have spoken very little of the more down-to-earth reflections on two-dimensional topological geometry, directly associated to my activities of teaching and “directing research”. Several times, I saw opening before me vast and rich fields ripe for the harvest, without ever succeeding in communicating this vision, and the spark which accompanies it, to one of my students, and having it open out into a more or less long-term com-

$\frac{45}{46}$

mon exploration. Each time up through today, after a few days or weeks of investigating where I, as scout, discovered riches at first unsuspected, the voyage suddenly stopped, upon its becoming clear that I would be pursuing it alone. Stronger interests then took precedence over a voyage which at that point appeared more as a digression or even a dispersion, than a common adventure.

One of these themes was that of planar polygons, centred around the modular varieties which can be associated to them. One of the surprises here was the irruption of algebraic geometry in a context which had seemed to me quite distant. This kind of surprise, linked to the omnipresence of algebraic geometry in plain geometry, occurred several times.

Another theme was that of curves (in particular circles) immersed in a surface, with particular attention devoted to the “stable” case where the singular points are ordinary double points (and also the more general theme where the different branches at a point mutually cross), often with the additional hypothesis that the immersion is “cellular”, i.e. gives rise to a map. A variation on the situations of this type is that of immersions of a surface with non-empty boundary, and first of all a disk (which was pointed out to me by A’Campo around ten years ago). Beyond the question of the various combinatorial formulations of such situations, which really represent no more than an exercise of syntax, I was mainly interested in a dynamical vision of the possible configurations, with the passage from one to another via continuous deformations, which can be decomposed into compositions of two types of elementary operations and their inverses, namely the “sweeping” of a branch of a curve over a double point, and the erasing or the creation of a bigon. (The first of these operations also plays a key role in the “dynamical” theory of systems of pseudo-lines in a real projective plane.) One of the first questions to be asked here is that of determining the different classes of immersions of a circle or a disk (say) modulo these elementary operations; another, that of seeing which are the immersions of the boundary of the disk which come from an immersion of the disk, and to what extent the first determine the second. Here also, it seems to me that it is a systematic study of the relevant modular varieties (of infinite dimension here, unless a purely combinatorial description of them can be given) which should give the most efficient “focus”, forcing us in some sense to ask ourselves the most relevant questions. Unfortunately, the reflection on even the most obvious and down-to-earth questions has remained in an embryonic state. As the only tangible result, I can cite a theory of canonical “dévissage” of a stable cellular immersion of a circle in a surface into “undecomposable” immersions, by “telescoping” such immersions. Unfortunately I did not succeed in transforming my lights on the question into a DEA

thesis, nor other lights (on a complete theoretical description, in terms of fundamental groups of topological 1-complexes, of the immersions of a surface with boundary which extend a given immersion of its boundary) into the beginnings of a doctoral thesis...

A third theme, pursued simultaneously over the last three years at different levels of teaching (from the option for first year students to the three third-cycle theses now being written on this theme) deals with the topologico-combinatorial classification of systems of lines or pseudo-lines. Altogether, the participation of my students here has been less disappointing than elsewhere, and I have had the pleasure of occasionally learning interesting things from them which I would not have thought of. Things being what they are, however, our common reflection was limited to a very elementary level. Lately, I finally devoted a month of intensive reflection to the development of a purely combinatorial construction of a sort of “modular surface” associated to a system of  $n$  pseudo-lines, which classifies the different possible “relative positions” (stable or not) of an  $(n + 1)$ -st pseudo-line with respect to the given system, in other words: the different possible “affinisations” of this system, by the different possible choices of a “pseudo-line at infinity”. I have the impression of having put my finger on a remarkable object, causing an unexpected order to appear in questions of classification which up to now appeared fairly chaotic! But the present report is not the place to dwell further on this subject.

Since 1977, in all the questions (such as the two last themes evoked above) where two-dimensional maps occur, the possibility of realising them canonically on a conformal surface, so on a complex algebraic curve in the compact oriented case, remains constantly in filigree throughout my reflection. In practically every case (in fact, in all cases except that of certain spherical maps with “few automorphisms”) such a conformal realisation implies in fact a canonical Riemannian metric, or at least, canonical up to a multiplicative constant. These new elements of structure (without even taking into account the arithmetic element which was considered in par. 3) are of a nature to deeply transform the initial aspect of the questions considered, and the methods of approaching them. A beginning of familiarisation with the beautiful ideas of Thurston on the construction of Teichmüller space, in terms of a very simple game of hyperbolic Riemannian surgery, confirms me in this presentiment. Unfortunately, the very modest level of culture of almost all the students who have worked with me over these last ten years does not allow me to investigate these possibilities with them even by allusion, since the assimilation of even a minimal combinatorial language already frequently encounters considerable psychical obstacles. This is why, in some respect and more and more in these last years, my teaching activity

has often acted like a weight, rather than a stimulus for the unfolding of a somewhat advanced or even merely delicate geometric reflection.

9. The occasion appears to be auspicious for a brief assessment of my teaching activity since 1970, that is, since it has taken place in a university. This contact with a very different reality taught me many things, of a completely different order than simply pedagogic or scientific. Here is not the place to dwell on this subject. I also mentioned at the beginning of this report the role which this change of professional milieu played in the renewal of my approach to mathematics, and that of my centres of interest in mathematics. If I pursue this assessment of my teaching activity on the research level, I come to the conclusion of a clear and solid failure. In the more than ten years that this activity has taken place, year after year in the same university, I was never at any moment able to suscite a place where “something happened” – where something “passed”, even among the smallest group of people, linked together by a common adventure. Twice, it is true, around the years 1974 to 1976, I had the pleasure and the privilege of awakening a student to a work of some consequence, pursued with enthusiasm: Yves Ladegaillerie in the work mentioned earlier (par. 3) on questions of isotopy in dimension 2, and Carlos Contou-Carrère (whose mathematical passion did not await a meeting with myself to blossom) an unpublished work on the local and global Jacobians over general base schemes (of which one part was announced in a note in the CR). Apart from these two cases, my role has been limited throughout these ten years to somehow or other conveying the rudiments of the mathematician’s trade to about twenty students on the research level, or at least to those among them who persevered with me, reputed to be more demanding than others, long enough to arrive at a first acceptable work written black on white (and even, sometimes, at something better than acceptable and more than just one, done with pleasure and worked out through to the end). Given the circumstances, among the rare people who persevered, even rarer are those who will have the chance of carrying on the trade, and thus, while earning their bread, learning it ever more deeply.

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10. Since last year, I feel that as regards my teaching activity at the university, I have learned everything I have to learn and taught everything I can teach there, and that it has ceased to be really useful, to myself and to others. To insist on continuing it under these circumstances would appear to me to be a waste both of human resources and of public funds. This is why I have applied for a position in the CNRS (which I left in 1959 as freshly named director of research, to enter the IHES). I know moreover

that the employment situation is tight in the CNRS as everywhere else, that the result of my request is doubtful, and that if a position were to be attributed to me, it would be at the expense of a younger researcher who would remain without a position. But it is also true that it would free my position at the USTL to the benefit of someone else. This is why I do not scruple to make this request, and to renew it if it is not accepted this year.

In any case, this application will have been the occasion for me to write this sketch of a programme, which otherwise would probably never have seen the light of day. I have tried to be brief without being sybilline and also, afterwards, to make it easier reading by the addition of a summary. If in spite of this it still appears rather long for the circumstances, I beg to be excused. It seems short to me for its content, knowing that ten years of work would not be too much to explore even the least of the themes sketched here through to the end (assuming that there is an “end”...), and one hundred years would be little for the richest among them!

Behind the apparent disparity of the themes evoked here, an attentive reader will perceive as I do a profound unity. This manifests itself particularly by a common source of inspiration, namely the geometry of surfaces, present in all of these themes, and most often front and centre. This source, with respect to my mathematical “past”, represents a renewal, but certainly not a rupture. Rather, it indicates the path to a new approach to the still mysterious reality of “motives”, which fascinated me more than any other in the last years of this past (\*). This fascination has certainly not vanished, rather it is a part of the fascination with the most burning of all the themes evoked above. But today I am no longer, as I used to be, the voluntary prisoner of interminable tasks, which so often prevented me from springing into the unknown, mathematical or not. The time of tasks is over for me. If age has brought me something, it is lightness.

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(\*) On this subject, see my commentaries in the “Thematic Sketch” of 1972 attached to the present report, in the last section “motivic digressions”, (loc. cit. pages 17-18).

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(<sup>1</sup>) The expression “out of reach” here (and also later for a completely different question), appears to me to be decidedly hasty and unfounded. I have noted myself on other occasions that when oracles (here myself!) declare with an air of deep understanding (or doubt) that such and such a problem is “out of reach”, it is actually an entirely subjective affirmation. It simply means, apart from the fact that the problem is supposed to be not yet solved, that the person speaking has no ideas on the question, or probably more precisely, that he has no feelings and no motivation with regard to it, that it “does nothing to him” and that he has no desire to do anything with it – which is frequently a sufficient reason to want to discourage others. As in the remark of M. de la Palisse, this did not stop the beautiful and regretted conjectures of Mordell, Tate, and Shafarevitch from succumbing although they were all reputed to be “out of reach”, poor things ! – Besides, in the very days which followed the writing up of the present report, which put me into contact with questions from which I had distanced myself during the last year, I noticed a new and remarkable property of the outer action of an absolute Galois group on the fundamental group of an algebraic curve, which had escaped me until now and which undoubtedly constitutes at least a new step towards the formulation of an algebraic characterisation of  $\text{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$ . This, with the “fundamental conjecture” (mentioned in par. 3 below) appears at present as the principal open question for the foundation of an “anabelian algebraic geometry”, which starting a few years ago, has represented (by far) my strongest centre of interest in mathematics.

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(<sup>2</sup>) With the exception of another “fact”, at the time when, around the age of twelve, I was interned in the concentration camp of Rieucros (near Mende). It is there that I learnt, from another prisoner, Maria, who gave me free private lessons, the definition of the circle. It impressed me by its simplicity and its evidence, whereas the property of “perfect rotundity” of the circle previously had appeared to me as a reality mysterious beyond words. It is at that moment, I believe, that I glimpsed for the first time (without of course formulating it to myself in these terms) the creative power of a “good” mathematical definition, of a formulation which describes the essence. Still today, it seems that the fascination which this power exercised on me has lost nothing of its force.

(<sup>3</sup>) More generally, beyond the so-called “anabelian” varieties, over fields of finite type, anabelian algebraic geometry (as it revealed itself some years ago) leads to a description, uniquely in terms of profinite groups, of the category of schemes of finite type over the absolute base  $\mathbb{Q}$  (or even  $\mathbb{Z}$ ), and from there, in principle, of the category of all schemes (by suitable passages to limits). It is thus a construction which “pretends” to ignore the rings (such

as  $\mathbb{Q}$ , algebras of finite type over  $\mathbb{Q}$ , etc.) and the algebraic equations which traditionally serve to describe schemes, while working directly with their étale topoi, which can be expressed in terms of systems of profinite groups. A grain of salt nevertheless: to be able to hope to reconstitute a scheme (of finite type over  $\mathbb{Q}$  say) from its étale topos, which is a purely topological invariant, we must place ourselves not in the category of schemes (here of finite type over  $\mathbb{Q}$ ), but in the one which is deduced from it by “localisation”, by making the morphisms which are “universal homeomorphisms”, i.e. finite, radicial and surjective, be invertible. The development of such a translation of a “geometric world” (namely that of schemes, schematic multiplicities etc.) in terms of an “algebraic world” (that of profinite groups and systems of profinite groups describing suitable topoi (called “étale”) can be considered as the ultimate goal of Galois theory, doubtless even in the very spirit of Galois. The sempiternal question “and why all this?” seems to me to have neither more nor less meaning in the case of the anabelian geometry now in the process of birth, than in the case of Galois theory in the time of Galois (or even today, when the question is asked by an overwhelmed student...); the same goes for the commentary which usually accompanies it, namely “all this is very general indeed!”.

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(<sup>4</sup>) We thus easily conceive that a group like  $\mathrm{Sl}(2, \mathbb{Z})$ , with its “arithmetic” structure, is positively a machine for constructing “motivic” representations of  $\mathrm{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$  and its open subgroups, and that we thus obtain, at least in principle, all the motivic representations which are of weight 1, or contained in a tensor product of such representations (which already makes quite a packet!) In 1981 I began to experiment with this machine in a few specific cases, obtaining various remarkable representations of  $\Pi$  in groups  $G(\hat{\mathbb{Z}})$ , where  $G$  is a (not necessarily reductive) group scheme over  $\mathbb{Z}$ , starting from suitable homomorphisms

$$\mathrm{Sl}(2, \mathbb{Z}) \rightarrow G_0(\mathbb{Z}),$$

where  $G_0$  is a group scheme over  $\mathbb{Z}$ , and  $G$  is constructed as an extension of  $G_0$  by a suitable group scheme. In the “tautological” case  $G_0 = \mathrm{Sl}(2)_{\mathbb{Z}}$ , we find for  $G$  a remarkable extension of  $\mathrm{Gl}(2)_{\mathbb{Z}}$  by a torus of dimension 2, with a motivic representation which “covers” those associated to the class fields of the extensions  $\mathbb{Q}(i)$  and  $\mathbb{Q}(j)$  (as if by chance, the “fields of complex multiplication” of the two “anharmonic” elliptic curves). There is here a principle of construction which seemed to me very general and very efficient, but I didn’t have (or take) the leisure to unravel it and follow it through to the end – this is one of the numerous “hot points” in the foundational programme of anabelian algebraic geometry (or “Galois theory”, extended

version) which I propose to develop. At this time, and in an order of priority which is probably very temporary, these points are:

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a) Combinatorial construction of the Teichmüller tower.

b) Description of the automorphism group of the profinite compactification of this tower, and reflection on a characterisation of  $\mathbb{I} = \text{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$  as a subgroup of the latter.

c) The “motive machine”  $\text{Sl}(2, \mathbb{Z})$  and its variations.

d) The anabelian dictionary, and the fundamental conjecture (which is perhaps not so “out of reach” as all that!). Among the crucial points of this dictionary, I foresee the “profinite paradigm” for the fields  $\mathbb{Q}$  (cf. b)),  $\mathbb{R}$  and  $\mathbb{C}$ , for which a plausible formalism remains to be uncovered, as well as a description of the inertia subgroups of  $\mathbb{I}$ , via which the passage from characteristic zero to characteristic  $p > 0$  begins, and to the absolute ring  $\mathbb{Z}$ .

e) Fermat’s problem.

(<sup>5</sup>) I would like to point out, however, a more delicate task (apart from the task pointed out in passing on cubic complexes), on the combinatorial interpretation of regular maps associated to congruence subgroups of  $\text{Sl}(2, \mathbb{Z})$ . This work was developed with a view to expressing the “arithmetic” operation of  $\mathbb{I} = \text{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$  on these “congruence maps”, which is essentially done via the intermediary of the cyclotomic character of  $\mathbb{I}$ . A point of departure was the combinatorial theory of the “bi-icosahedron” developed in a C4 course starting from purely geometric motivations, and which (it afterwards proved) gives rise to a very convenient expression for the action of  $\mathbb{I}$  on the category of icosahedral maps (i.e. congruence maps of index 5).

(<sup>6</sup>) Let us note in relation to this that the isomorphism classes of compact tame spaces are the same as in the “piecewise linear” theory (which is not, I recall, a tame theory). This is in some sense a rehabilitation of the “Hauptvermutung”, which is “false” only because for historical reasons which it would undoubtedly be interesting to determine more precisely, the foundations of topology used to formulate it did not exclude wild phenomena. It need (I hope) not be said that the necessity of developing new foundations for “geometric” topology does not at all exclude the fact that the phenomena in question, like everything else under the sun, have their own reason for being and their own beauty. More adequate foundations would not suppress these phenomena, but would allow us to situate them in a suitable place, like “limiting cases” of phenomena of “true” topology.

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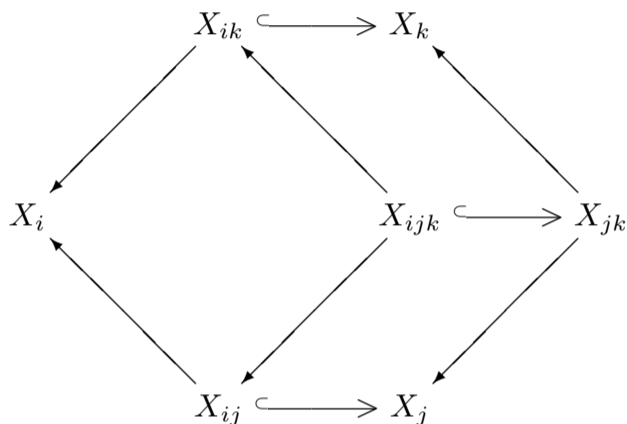


(7) In fact, to reconstruct the system of spaces

$$(i_0, \dots, i_n) \mapsto X_{i_0, \dots, i_n}$$

contravariant on  $\text{Drap}(I)$  (for the inclusion of flags), it suffices to know the  $X_i$  (or “unfolded strata”) and the  $X_{ij}$  (or “joining tubes”) for  $i, j \in I$ ,  $i < j$ , and the morphisms  $X_{ij} \rightarrow X_j$  (which are “bounding” inclusions) and  $X_{ij} \rightarrow X_i$  (which are proper fibrations, whose fibres  $F_{ij}$  are called “joining fibres” for the strata of index  $i$  and  $j$ ). In the case of a tame multiplicity, however, we must also know the “junction spaces”  $X_{ijk}$  ( $i < j < k$ ) and their morphisms in  $X_{ij}$ ,  $X_{jk}$  and above all  $X_{i,k}$ , included in the following hexagonal commutative diagram, where the two squares on the right are Cartesian, the arrows  $\hookrightarrow$  are immersions (not necessarily embeddings here), and the other arrows are proper fibrations:

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(N.B. This diagram defines  $X_{ijk}$  in terms of  $X_{ij}$  and  $X_{jk}$  over  $X_j$ , but not the arrow  $X_{ijk} \rightarrow X_{ik}$ , since  $X_{ik} \rightarrow X_k$  is not necessarily an embedding.)

In the case of actual stratified tame spaces (which are not, strictly speaking, multiplicities) we can conveniently express the unfolding of this structure, i.e. the system of spaces  $X_{i_0, \dots, i_n}$  in terms of the tame space  $X_*$  sum of the  $X_i$ , which is equipped with a structure of an ordered object (in the category of tame spaces) having as graph  $X_{**}$  of the order relation the sum of the  $X_{ij}$  and the  $X_i$  (the latter being on the diagonal). Among the essential properties of this ordered structure, let us only note here that  $\text{pr}_1 : X_{**} \rightarrow X_*$  is a (locally trivial) proper fibration, and  $\text{pr}_2 : X_{**} \rightarrow X_*$  is a “bounding” embedding. We have an analogous interpretation of the unfolding of a stratified tame multiplicity, in terms of a category structure (replacing a simple ordered structure) “in the sense of tame multiplicities”, such that the composition map is given by the morphisms  $X_{ijk} \rightarrow X_{ik}$  above.