

## Did Earlier Thoughts Inspire Grothendieck?

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*“... mon attention systématiquement était  
... dirigée vers les objets de généralité maximale ...”*  
Grothendieck on page 3 of [11]; see [68], page 8

### Introduction

When I first met Alexander Grothendieck more than fifty years ago I was not only deeply impressed by his creativity, his knowledge and many other aspects of his mathematics, but I also wondered where all his amazing ideas and structures originated from. It seemed to me then as if new abstract theories just emerged in his mind, and then he started to ponder them and simply build them up in their most pure and general form without any recourse to examples or earlier ideas in that particular field. Upon reading his work, I saw my impression confirmed by the direct and awe-inspiring precision in which his revolutionary structures evolved.

*Where does inspiration come from ?* We can ask this question in general. The question has fascinated me for many years, and it is particularly intriguing in connection with the mathematics of Alexander Grothendieck.

Forty years ago the picture was even more puzzling for me. At that time, we had been confronted with thousands of pages of abstract mathematics from his hand. It was not easy at all to understand this vast amount of material. Hence it was a relief for me to read, much later, what Mumford wrote to Grothendieck about this: “ ... I should say that I find the style of the finished works, esp. EGA, to be difficult and sometimes unreadable, because of its attempt to reach a superhuman level of completeness.” See: Letter Mumford to Grothendieck, 26 December 1985, [44], page 750.

Those who had the privilege to follow closely these developments could see the grand new views. Here is what Mumford wrote about Grothendieck’s visit to Harvard about fifty years ago in connection with a new proof of Zariski’s “Main Theorem”: Then Grothendieck came along and he reproved

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this result now by a *descending* induction on an assertion on the higher cohomology groups with Zariski's theorem resulting from the  $H^0$  case: this seemed like black magic." See the paper [45] by Mumford, this volume.

The fact that there should exist a cohomological proof of this theorem by Zariski was conjectured by Serre; see [1], page 112 (here we see already where the inspiration came from). See [73], bottom of page 21.

The magic described by Mumford can also be found in a description by Deligne. "Je me rappelle mon effarement, en 1965-66 après l'exposé de Grothendieck [SGA5] prouvant le théorème de changement de base pour  $Rf_!$ : dévissages, dévissages, rien ne semble se passer et pourtant à la fin de l'exposé un théorème clairement non trivial est là." See [23], page 12.

About this passage Luc Illusie communicated to me: "... base change for  $Rf_!$  is a trivial consequence of proper base change, and proper base change was proved by Artin in his exposés in SGA 4, not SGA 5. ... January 2005, was the beginning of the first part of SGA 5, and as far as I remember (I wrote preliminary notes for them) Grothendieck recalled the global duality formalism, and then embarked in the local duality formalism (construction of dualizing complexes). Also, the proof of the proper base change theorem is not just a long sequence of trivial 'dévissages' leading to a trivial statement : the dévissages are not trivial, and proper base change for  $H^1$  is a deep ingredient."

It was clear to many of us that the tools which Grothendieck developed in this branch of mathematics revolutionized algebraic geometry and a part of number theory and offered us a clear and direct approach to many questions which were unclear to us before.

But it was also frustrating for us that the maestro himself left the scene too early, with EGA unfinished and many developments that he had initiated left hanging in the air, leaving us with the feeling that now we had to find our own way.

The question of whether Grothendieck's brilliant ideas had simply occurred to him out of the blue or whether they had some connection to earlier thought continued to puzzle me, and over the years I started to approach each of his theories or results with this particular question in mind. The results were illuminating. Every time I started out expecting to find that a certain method was originally Grothendieck's idea in full, but then, on closer examination, I discovered each time that there could be found in earlier mathematics some preliminary example, specific detail, part of a proof, or anything of that kind that preceded a general theory developed by Grothendieck. However, seeing an inspiration, a starting point, it also showed what sort of amazing quantum leap Grothendieck did take in order to describe his more general results or structures he found.

In this short note I will discuss, describe and propose the following.

### § 1. Some questions Grothendieck asked

In a very characteristic way Grothendieck asked many questions. Some of these are deep and difficult. Some other questions could be answered easily, in many cases with a simple example. We describe some of these questions.

**§ 2. How to crack a nut?**

Are we theory-builders or problem-solvers? We discuss Grothendieck's very characteristic way of doing mathematics in this respect.

**§ 3 Some details of the influence of Grothendieck on mathematics.**

We make some remarks on the style of Grothendieck in approaching mathematics. His approach had a great influence especially in the way of doing algebraic geometry and number theory.

**§ 4. We should write a scientific biography.**

Here we come to the question asked in the title of this paper. We propose that a scientific biography should be written about the work of Grothendieck, in which we indicate the "flow" of mathematics, and the way results by Grothendieck are embedded in this on the one hand and the way Grothendieck created new directions and approaches on the other hand. Another terminology could be: we should give a genetic approach to his work.

This would imply each time discussing a certain aspect of Grothendieck's work, indicating possible roots, then describing the leap Grothendieck made from those roots to general ideas, and finally setting forth the impact of those ideas. This might present future generations a welcome description of topics in 20th century mathematics. It would show the flow of ideas, and it could offer a description of ideas and theories currently well-known to specialists in these fields now; that knowledge and insight should not get lost. Many ideas by Grothendieck have already been described in a more pedestrian way. But the job is not yet finished. In order to make a start, I intend to give some examples in this short note which indicate possible earlier roots of theories developed by Grothendieck. We give some examples supporting our (preliminary) Conclusion (4.1), that all theory developed by Grothendieck in the following areas has earlier roots:

**§ 5. The fundamental group.**

**§ 6. Grothendieck topologies.**

**§ 7. Anabelian geometry.**

**§ 8. If the general approach does not work.**

It may happen that a general approach to a given problem fails. What was the reaction of Grothendieck, and how did other mathematicians carry on?

In this note we have not documented extensively publications of Grothendieck, because in this volume and in other papers a careful and precise list of publications is to be found. For more details see e.g. [6], [31].

In this note we only discuss research by Grothendieck in the field of algebraic geometry.

An earlier draft of this note was read by L. Illusie, L. Schneps and J-P. Serre. They communicated to me valuable corrections and suggestions. I thank them heartily for their contributions.

## 1. Some questions Grothendieck asked

During his active mathematical life, Grothendieck asked many questions. Every time, it was clear that he had a general picture in mind, and he tried to see whether his initial idea would hold against the intuition of colleagues, would be supported or be erased by examples. Many times we see a remarkable insight, a deep view on general structures, and sometimes a lack of producing easy examples, not doing simple computations himself.

We may ask ourselves how it was possible that Grothendieck could possibly work without examples. As to this question: now that we have the wonderful [10] and letters contained in [44] it is possible to see that there is more to the creative process of Grothendieck than I originally knew.

Also in this line of thought we should discuss what happened in case Grothendieck constructed a general machinery, which for certain applications however did not give an answer to questions one would like to see answered. Some examples will be given in Section 8.

**(1.1). Local and global topological groups.** In [32], on page 1039 of the first part we find the story of how Grothendieck in 1949, then 21 years old, came to C. Ehresmann and A. Borel during a break between lectures in the Bourbaki seminar asking: “Is every local topological group the germ of a global topological group?” I find this typical of his approach to mathematics. Seeing mathematical structures, Grothendieck was interested in knowing their interrelations. And one of the best ways of finding out is going to the true expert, asking a question and obtaining an answer which would show him the way to proceed. See the beautiful paper of Jackson describing this episode, also characterizing Grothendieck’s “social niceties” and much more. The question which was asked has a counterexample, as Borel knew. Many times we see this pattern: Grothendieck would test the beauty and coherence of mathematics by asking a question to a “real expert” and obtain an answer which either would show him the way to proceed, or save him from going on in a wrong direction.

**(1.2). Correspondence with Serre.** The volume [10] is a rich source of information. We obtain a glimpse of the exchange of ideas between these mathematicians. It is fascinating reading, it gives insight into the way they feel about mathematics, and it gives food for further thought. We highlight just a few of the many questions Grothendieck asked in these letters. Also see (3.9).

**(1.2).1.** See [10], p. 7. Grothendieck wrote on 18.2.1955: “...Sait-on si le quotient d’une variété de Stein par un groupe discret ‘sans point fixe’ est de Stein?”

To which Serre responds on 26.02.1955: “...ça peut même être une variété compacte! Cf. courbes elliptiques, et autres...”

**(1.2).2.** See [10], p. 42. Grothendieck wrote on 23.7.1956: “Quant à plonger une variété algébrique complète dans un espace projectif, j’avoue que je ne vois pas de méthode encore.”

Did Grothendieck expect this to be true? In 1957 Nagata constructed an example of a *complete normal surface* which cannot be embedded into a projective space, and in his Harvard PhD-thesis in 1960 Hironaka constructed complete, *non-singular threefolds* which cannot be embedded into any projective space. See [29], 3.4.1.

**(1.2).3.** See [10], p. 67. Grothendieck wrote on 5.11.1958: “...me font penser qu’il est possible de remonter canoniquement toute variété  $X_0$  définie sur un corps parfait de caractéristique  $p \neq 0$ ...” For a further discussion see (8.3).

It is not clear what Grothendieck had in mind here. We know he was much too optimistic, see [75]. But we see his theory of formal liftings (not canonical, sometimes obstructed) and his “existence theorem in formal geometry” foreshadowed here.

**(1.2).4.** See [10], p. 145. Grothendieck had the hope (in 1964, or earlier) of proving the Weil conjectures by first showing that any variety could be dominated by a product of curves, see [10], p. 271. We can understand his insight that indeed that would solve problems. But Serre gave an example of an algebraic surface which does not satisfy this condition, see [10], page 145. We see the mechanism of Grothendieck asking a question before embarking on this general idea, and Serre finishing off the attempt by an example. As far as I know this example was never published. And it seems it was not known to C. Schoen in 1995, see [70]. It would be nice to understand Serre’s example in the light of this new approach by Schoen.

**(1.2).5.** See [10], p. 169. Grothendieck wrote on 13.08.1964: “...si  $V$  est un schéma algébrique projectif et lisse sur le corps local  $K$ , et si  $G(\overline{K}, K)$  opère de façon non ramifiée sur tous les  $H^i(\overline{V})$ , on peut se demander si  $V$  n’a pas forcément une *bonne réduction*. C’est probablement un peu trop optimiste, mais tout de même, je ne vois pas de contre-exemple immédiat.”

For every curve of genus at least two degenerating into a tree of regular curves of lower genus, its Jacobian has good reduction; hence the condition of trivial monodromy is satisfied (the local Galois group operates in a non-ramified way). However the curve does not have good reduction.

**(1.2).6.** See [10], p. 203. Grothendieck wrote on 3-5.10.1964: “...est-il connu si la fonction  $\zeta$  de Riemann a une infinité de zéros?”

On which Serre later made the comment: “... Grothendieck ne s’est jamais intéressé à la théories analytiques des nombres.” See [10], p. 277.

Already this small selection shows that some questions asked by Grothendieck have an easy answer that can be provided by anyone knowing simple examples on the one hand, and deep thoughts and attempts on the other hand.

**(1.3). Correspondence between Grothendieck and Mumford.** We will discuss in (8.6) a question Grothendieck asked in 1970 to Mumford. See [44], page 745. Mumford gave an easy example which showed that this idea by Grothendieck did not match mathematical reality. This exchange shows that Grothendieck's thoughts, without simple computations or examples for support, were geared towards new insight in the objects he was studying at that time.

Perhaps these two sentences from their correspondence characterize their interaction particularly well.

Grothendieck to Mumford 25.04.1961: "It seems to me that, because of your lack of some technical background on schemata, some proofs are rather awkward and unnatural, and the statements you give not as simple and strong as they should be." See [44], page 636/637.

Mumford to Grothendieck on 11.02.1986: "I hope you know how vivid and influential a figure you were in my life and my development at one time." See [44], page 758.

**(1.4). We may ask ourselves how it was possible that Grothendieck could possibly work without examples.** As to this question: now that we have the wonderful [10] and letters contained in [44] it is possible to see that there is more to the creative process of Grothendieck than I originally knew. His contacts with colleagues, such as Serre and Mumford, and the information he obtained saved him from spending time on trying to develop structures which do not exist (as follows by counterexamples). We can admire Grothendieck for asking the right questions to the right colleagues.

Here is another explanation. Serre remarked to me (private correspondence): "Grothendieck could prove such nice theorems ... the strong consistency of mathematics".

And perhaps Grothendieck knew examples better than can be concluded from his correspondence and from his style of writing. L. Illusie communicated to me: "In his filing cabinets, located behind his desk, Grothendieck kept many handwritten notes, where he had studied specific examples: he sometimes told me that he was weak on surfaces, but as everybody knows, he was not so weak in local algebra, and he knew enough of curves, abelian varieties and algebraic groups to be able to test his ideas. Also, his familiarity (and constant interest) in analysis and topology was a strong asset. All these examples appeared when you discussed with him."

But perhaps we had best cite Grothendieck himself, where "harmony" could be the inspiring source:

"Et toute science, quand nous l'entendons non comme un instrument de pouvoir et de domination, mais comme aventure de connaissance de notre espèce à travers les âges, n'est autre chose que cette harmonie, plus ou moins

vaste et plus ou moins riche d'une époque à l'autre, qui se déploie au cours des générations et des siècles, par le délicat contrepoint de tous les thèmes apparus tour à tour, comme appelés du néant.

(ReS; see [32], Part 1, page 1038, also for a translation).

The construction of very general ideas was a strong point of the mathematics of Grothendieck. In this line of thought we discuss what happened in case Grothendieck constructed a general machinery, which for certain applications however did not give an answer to questions one would have liked to see answered. If a counterexample showed that a general approach could not work, or that a general idea did not describe the true structure, if mathematics was not as simple and beautiful as Grothendieck would have liked to see, then what was Grothendieck's reaction? We will see some examples of this in Section 8, and describe how progress could still be made by others.

## 2. How to crack a nut?

**(2.1).** Here we study the way mathematicians try to solve a problem, or develop further mathematical insight.

In ReS, see [13], Grothendieck described two (extreme) ways of cracking a big nut (“...une grosse noix...”). The first way he described is basically by brute force. The second way is to immerse the nut in a softening fluid: “on plonge la noix dans un liquide émoullent”, until the nut opens just by itself. And Grothendieck leaves the reader to guess which is his method. See ReS, and see [23], pp. 11/12.

However, I think, mathematical reality is not as simple as described in this metaphor. FLT, Fermat's Last Theorem, or the Weil conjectures were not solved in not just one of these two ways.

I would like to give a description of the creative aspect of mathematical activity which has been on my mind for the last 50 years; a concept slightly different from the nut-story. To put it in an extreme form:

**Method (1)** One method is to construct a “machine”, a general concept, find a universal truth. Then “simply” feed the problem studied into it, and wait, see what happens.

**Method (2)** Or, one can study special cases, make an inventory of known examples, and try to connect the problem to a general principle. Or one can at first try to find a proof, see where it gets stuck, then use the obstructions in an attempt to construct a counterexample, and by this zig-zag method discover more about the structure of the objects studied, and hope that these attempts eventually converge to a conclusion.

Does a mathematician discover or create a result? This is an interesting question on which many ideas already exist. However, this question and related lines of idea will not be further discussed in this note. The first method is very appealing. It is the one we should start with: “finding a preexisting pattern”.

Yuri Manin wrote: “I see the process of mathematical creation as a kind of recognizing a preexisting pattern”; see [38]. In my opinion Grothendieck followed this line of research consistently. He discovered many mathematical structures, and he created important tools for us to proceed in our search for mathematical truth.

In a sense this is very reassuring: if Grothendieck studied a certain question or structure, and there is the possibility of a smooth, direct, general solution, he will have found it.

Grothendieck taught us how successful mathematical research along the lines of Method (1) can be. Also, this seems to be the heart of our profession: creating the evolution of our understanding of mathematical structures. – However, clinging only to this method has its drawbacks. If you are not successful, what can you do? – You can try to generalize the problem, and find a structure which solves the more general question. But we have learned that mathematical reality sometimes (or often? according to your taste and experience) does not fit into the approach (1). I have the impression that in many cases when this first method did not work out well, Grothendieck would let the problem rest, waiting “until the nut opens just by itself”; and he sometimes left the question completely untouched afterwards.

The second method has been applied quite often. Many results have been achieved this way.

Here is another description of this activity of mathematicians, given by Andrew Wiles. “Perhaps I could best describe my experience of doing mathematics in terms of entering a dark mansion. One goes into the first room, and it’s dark, completely dark. One stumbles around bumping into the furniture, and gradually, you learn where each piece of furniture is, and finally, after six months or so, you find the light switch. You turn it on, and suddenly, it’s all illuminated. You can see exactly where you were. At the beginning of September, I was sitting here at this desk, when suddenly, totally unexpectedly, I had this incredible revelation. It was the most important moment of my working life...” (BBC-documentary by S. Singh and John Lynch: Fermat’s Last Theorem. Horizon, BBC 1996.)

We have seen that FLT was not proved, and as far as we know, cannot be proved by just constructing a general theory and “feeding the problem into the machine”. Not only did Andrew Wiles try to “learn where each piece of furniture is”, but all those attempts during more than three centuries before can be seen as “stumbling around bumping into the furniture”. This *evolutionary process* is fascinating to watch and to describe. We can mention Fermat, Euler, Legendre, Dirichlet, Sophie Germain, Kummer, Serre, Shimura-Taniyama-Weil, Frey, Ribet and many others (and they paved the road for Wiles). The final achievement is a combination of growing insight, knowing which roads should not be taken, and then coming up with a combination of general concepts and deep insight on the one hand, and “tricks” and precise knowledge of all the pieces of the “furniture” on the



other hand. How different from either brute force or expecting that the nut will open just by itself.

**(2.2). Conclusion.** Grothendieck created new tools and gave us deep insight, and we can be grateful for that. However, reality in mathematical research shows that there are problems which need more than only general insight. If Method (1) fails, it seems wise to apply Method (2) (and many mathematicians, tenaciously, have done so); we describe some examples of this in Section 8.

### 3. Some details of the influence of Grothendieck on mathematics

“...le jour où une démonstration nous apprend au-delà de tout doute que telle chose que nous imaginions était bel et bien l’expression fidèle et véritable de la réalité elle-même...” ReS, page 211.

In this section we describe some characteristics of the way Grothendieck was working and thinking while doing algebraic geometry in his fruitful years, and we speculate about the ways in which this formed and changed our views on these topics.

**(3.1). Representable functors.** We describe a general approach known in algebraic topology, algebra, and many other fields, that started already more than 70 years ago, but was adapted and used to its full consequences by Grothendieck.

There were many occasions in mathematics where a “solution satisfying a universal property” was constructed. Topologists knew that vector bundles come from a general one. Bourbaki made use of the solution of a “universal problem” (such as a tensor product).

Samuel wrote in 1948: “It has been observed” (with a footnote to unpublished work by Bourbaki) “that constructions so apparently different enter in the same frame”; see the first lines of [64].

To French mathematicians in the 1960s, but especially to Grothendieck, we owe the mantra that defining a functor and proving it is representable should be the heart, or at least the beginning of any construction. In algebraic geometry before Grothendieck, there were many constructions where no a priori “universal property” was formulated, or where defining conditions and corollaries of such properties appear in the same lines. For us, nowadays, it is hard to assess the influence of even this “small” aspect of the French lucidity of view, and the systematic use of it made by Grothendieck.

We taste this atmosphere in the description by Samuel of Igusa’s construction of what we now would call the coarse moduli scheme  $\mathcal{M}_2 \rightarrow \text{Spec}(\mathbb{Z})$ :

“Signalons aussitôt que le travail d’IGUSA ne résoud pas, pour les courbes de genre 2, le ‘problème des modules’ tel qu’il a été posé par GROTHENDIECK à diverses reprises dans ce Séminaire.” (See the first

lines of [65].) This aspect of abstract methods was to have a direct influence on our profession for years to come.

Illusie wrote to me: "...there are two aspects in the technique of representable functors:

- (1) of course, defining the functor makes clear the object we are searching for,
- (2) but independently of whether that functor is representable or not, what Grothendieck taught us is that we can do geometry on the functor itself: e.g. (formal) smoothness, étaleness, etc. This was the 'quantum leap' as you said before."

**(3.2). Non-representable moduli functors.** Grothendieck's views helped us to understand essential features much better than we knew them before. This portrays a phenomenon that we will encounter many times when observing how abstract methods of Grothendieck's were digested, adapted and used. But several times we also see that the abstract and clean approach does not completely cover mathematical reality. E.g., sometimes we want to construct an object which does not represent a functor that is easily defined beforehand.

I remember once I met Grothendieck in a Paris street; both of us were going to the same lecture, and he was very excited by a construction made by a young American mathematician. It was Mumford, who wrote in 1961 to Grothendieck about his proof of "the key theorem in a construction of the arithmetic scheme of moduli  $M$  of curves of any genus." Grothendieck was excited about this idea, apparently completely new to him. Later Mumford pinned down the notion of a "coarse moduli scheme", necessary in case the obvious moduli functor is not representable by a variety (or by a scheme). See [44], pp. 635-638 where we see this excitement of Grothendieck reflected in several letters to Mumford. Grothendieck explained that for "higher levels" he could represent moduli functors, but for all levels he could not perform the necessary quotient construction, see [44], pp. 635/636.

Later in this note we will see instances where Grothendieck's abstract theory clarifies a lot, but sometimes "non-canonical steps" are necessary to give full access to mathematical reality; see Section 8.

**(3.3). Morphisms instead of objects.** "...comme Grothendieck nous l'a appris, les objets d'une catégorie ne jouent pas un grand rôle, ce sont les morphismes qui sont essentiels." See page 335 of [76].

One of the first theorems that Grothendieck proved in algebraic geometry, and which gave him a lot of prestige, was the Grothendieck-Hirzebruch-Riemann-Roch theorem. One of the first essential ideas is that such a theorem should not be about a variety (as all the "old" results were), but that it should describe properties of a morphism; see [7]; see (3.8). The idea of

considering morphisms rather than objects dominated many considerations by Grothendieck in algebraic geometry, and we have seen so many results coming out of this point of view.

In many cases, it is hard now to realize how mathematicians were thinking and working some time ago, let alone long ago. For a long period of time algebraic geometry was the study of varieties. However Grothendieck has taught us to think “functorially”. The way Grothendieck would start a seminar talk is well-known: “Let  $X$  vertical arrow  $S$  be a scheme over  $S$ ”. And since then, some of us (most of us) see the importance of this way of looking at things, although we still use the term “variety”.

Illusie writes: “*Grothendieck pensait toujours en termes relatifs: un espace au-dessus d’un autre*”; see [31], second page. Where algebraic geometers, and certainly mathematicians working in number theory, were interested in properties of one variety, or one equation, or at best a class of varieties or equations, Grothendieck showed us the essence of changing our point of view. Certainly here we can indicate earlier roots. Just one example: a “complete variety” was defined by Chevalley, see Chap. IV of [22]. It opened the possibility of studying varieties which appeared naturally in constructions, which were not necessarily projective but still had the property that “no points are missing”. In the hands of Grothendieck, it was no longer a variety that matters, but a morphism, and Chevalley’s definition was generalized to the notion of a “proper morphism”. Indeed this is a generalization: an algebraic variety  $V$  defined over a field  $K$  is complete if and only if the morphism  $V \rightarrow \text{Spec}(K)$  is proper.

In 1970 we had a Summer School on Algebraic Geometry. I remember Swinnerton-Dyer starting a talk by writing, in a very Grothendieckian way:  $X$  vertical arrow  $S$ , and continuing for just one minute saying very complicated things about schemes over schemes. We were amazed: even this famous number theorist had converted to the new faith? Then Swinnerton-Dyer continued his talk on “Rational points on Del Pezzo surfaces of degree 5” by saying that he wanted to compute something, that schemes for him were not very helpful, and soon equations were solved, determinants computed, and the result followed.

**(3.4). The most general situation.** “Alors que dans mes recherches d’avant 1970, mon attention systématiquement était dirigée vers les objets de généralité maximale, afin de dégager un langage d’ensemble adéquat pour le monde de la géométrie algébrique...pour développer des techniques et énoncés ‘passe-partout’ valables en toutes dimensions et en tous lieux...”; see [11], pp. 2/3. In many cases this has enriched our point of view. However, sometimes we feel that working on a specific problem in “maximal generality” is not always helpful.

**(3.5). Commuting diagrams.** Grothendieck gives us the feeling that mathematics satisfies all possible rules of simplicity and elegance. And certainly we have learned a lot from him by looking at our profession this way. However, Serre writes on 23.7.1985:

“On ne peut pas se borner à dire que les diagrammes qu’on écrit ‘doivent’ commuter...”; see [10], page 244.

Let me add to this a description of a personal episode from the time, in 1960/61, when I was a student in Paris. The goal of my research was modest: constructing the Picard scheme of  $X$  in case the Picard scheme of  $X_{\text{red}}$  is known to exist (first for curves, later for arbitrary algebraic schemes). Grothendieck had claimed in September 1960 to me that he had already proved everything I was after, which however turned out later not to be the case. After I finished my proof, Serre insisted to Grothendieck that I should give a talk on my first (small) result in the Grothendieck seminar. In my talk, I explained that in a large diagram with two quite different cohomology sequences with down arrows connecting them, the crucial square was not commutative in general, as I had checked in several examples. However, I proved that in the relevant square the two images were the same, and that was all that I needed in that situation.

The week after my performance in his seminar Grothendieck gave a talk in Cartan’s seminar; there he needed my result. In [12], Th. 3.1 on page 16-13 we see an extra condition (not “de généralité maximale”) which helps to avoid this non-commutativity. After my result appeared in print, Grothendieck used it in 1962 to prove the theorem without this extra condition, see [4], page 232-17.

**(3.6). Schemes.** Classical algebraic geometry studied varieties over a field. However, in many cases in geometry and number theory, particularly when considering varieties moving in a family, or equations together with their reduction mod  $p$  (in Grothendieck’s language this amounts to just taking a special fiber of a morphism between schemes), it is necessary to use a more general machinery. Already in [47], and in many later publications, we find an attempt to formulate this; it was also studied by E. Kähler. When sheaf theory became available, ringed spaces, substituted for the notion of sets of solutions of polynomial equations, paved the way for a more general concept. According to Pierre Cartier, the word scheme was first used in the 1956 Chevalley Seminar, in which Chevalley was pursuing Zariski’s ideas ([17]). Serre communicated to me: “I was well aware when I wrote FAC of the notion (but not the word) of Spec and of its use; I had read Krull’s Idealtheorie, which is probably the first place where the technique of going from a ring to its local rings was systematically used (and in order to prove non-trivial theorems, such as Krull’s theorems on dimension.)” In [82] we read on page 43: “Schemes were already in the air, though always with restrictions on the rings involved. In February 1955, Serre mentions that the

theory of coherent sheaves works on the spectrum of commutative rings in which every prime ideal is an intersection of maximal ideals.”

It was Grothendieck who saw the importance of the more general definition. Still, algebraic geometers in the beginning complained that the notion of a point should be related to a maximal ideal. However Grothendieck (of course) noted that a ring homomorphism  $R \rightarrow R'$  in general does not give a map between the set of maximal ideals, e.g. as is the case when  $R$  is an integral domain, unequal to  $R'$ , its field of fractions. General principles and thinking of morphisms instead of objects made Grothendieck replace old habits by clean new ideas.

Here we see the “earlier roots” that inspired Grothendieck, and his jump to the general concept we use now. In [1] on page 106 Grothendieck describes these ideas originating in work by Nagata-Chevalley-Serre and many others. See Cartier’s description of the development of these ideas ([18], page 398).

In [10], page 26, Grothendieck writes on 16.1.1956 “...le contexte général des spectres d’anneau à la Cartier-Serre.” And Serre writes as comment in this edition: “ cela s’appellera plus tard des schémas affines.” In [10], page 53, Grothendieck writes on 22.11.1956 “...Cartier a fait le raccord des schémas avec les variétés...” We see the inspiring atmosphere of the Paris mathematical community at that time for Grothendieck.

*Did everyone adopt the theory of schemes?* For some algebraic geometers it was hard to adjust to this modern terminology. And there were several reasons for that. Partly because the machinery was too general: in some cases an easy and direct approach would give a better and easier framework for understanding, for describing easy structures, and for writing things down in a plain language. Also, it was not so easy to change from old habits into the new discipline.

In 1960 I made an appointment with Néron, and I asked him to explain to me his theory of “minimal models”. I had the feeling it was important, but I must confess that I understood very little of his explanation at that time. Then, reading his [48] I could better understand the result, but it was hard to digest the proof. I know that during that time, his colleagues tried to convince him to publish his results in the language of schemes, but in fact we can see that Néron’s publication used terminology that closely followed the language of Weil and Shimura. In 1966 M. Artin wrote in his review of this result: “ It would be very useful to have a clear exposition of his theory in the language of schemes.” It was by reading [77] (see p. 494) that I obtained a clearer view of this notion. In SGA 7, Vol. I Exp. IX by Grothendieck (see IX.1.1), and in fact already in [63], we can see the formulation of the result in the language of schemes. But it was only in later work by Raynaud, and in 1986 (see [15]) that a discussion completely in modern terminology became available.

**(3.7). Going on with general theory, leaving applications to others.**

We know that Grothendieck had a grand plan for completing the foundations of algebraic geometry in EGA; e.g. see [10], page 83, where Grothendieck writes in 1959 that he expects to have EGA finished in 3 or at most 4 years. I have the impression that laying these foundations became more important than having this work actually “aboutir à la démonstration des conjectures de Weil” (as in the footnote on page 9 of EGA I). The plan for the 13 chapters of EGA can be found on page 6 of EGA I. We know that he did not finish writing EGA – alas ! – only 4 chapters ever appeared.

In ReS more than once we find a sentence like “Au moment de quitter la scène mathématique en 1970 l’ensemble de mes publications (dont bon nombre en collaboration) sur le thème des schémas devait se monter à quelques deux mille pages” (es ReS page 44, footnote 21). However, some of the material which should have appeared in later volumes of EGA, but was in fact never written down in that setting, was luckily already divulged in SGA and in FGA. These are rich sources of information.

**(3.8). Certain applications he did not publish himself.**

We can mention *the Riemann-Roch theorem*, discussed and published by Borel and Serre ([7]). Also see SGA 6, and [26], 15.2 and 18.3.

Part of the monodromy theorem: *every eigenvalue of a monodromy matrix is a root of unity*, a wonderful application of the theory of the fundamental group, which intertwines Galois theory and classical monodromy, see the appendix of [77]; see SGA 7 I, Exp. I, Section 1; see (5.1) for the fundamental group; see (5.2) for comments on the monodromy theorem.

CM *abelian varieties are, up to isogeny, defined over a finite extension of the prime field*; see [55], also published with his permission.

Dieudonné wrote: “Il ne publia pas lui-même sa démonstration...” (of the Riemann-Roch-Grothendieck theorem) “...premier exemple de ce qui allait devenir chez lui une coutume: poussé par les idées qui se pressaient en foule dans son esprit, il laissait souvent à ses collègues ou élèves le travail de leur mise au point dans tous les détails” (es [27], Vol. I, pp. 6).

We see that Grothendieck in those years 1958 - 1970 spent all his energy on the main lines of his plans, and we can be grateful for that. For other things “he was never in a hurry to publish”, see [69], p. 22.

**(3.9). “Toujours lui!”** Grothendieck had contact with Serre on many occasions, mainly by phone it seems, but also by correspondence. Serre’s insight, his results, and certainly his incredible ability to see through a question or a problem, and come up either with a counterexample or a critical remark, was often crucial for Grothendieck. In [10] we see just a small part of this interaction. Here is one of the Serre’s results which had a deep influence on the work of Grothendieck(see [74]):

”C’était là une réflexion qui a dû se faire vers le moment de ma réflexion sur une formulation des ”conjectures standard”, inspirées l’une et l’autre par l’idée de Serre (toujours lui!) d’un analogue ‘kählérien’ des conjectures de Weil.” See ReS, pp 209/210.

**(3.10).** “**On pourra commencer à faire de la géométrie algébrique!**” In his letter of 18.8.1959 (see [10], page 83), Grothendieck tells Serre his schedule for the next 4 years: in those years he expects to write down the planned volumes of EGA, and also things which were later partly published in [4] and in volumes of SGA. And the letter concludes:

“Sans difficultés imprévues ou enlèvement, le multiplodoque devrait être fini d’ici 3 ans, ou 4 ans maximum. On pourra commencer à faire de la géométrie algébrique!”

This plan for material to be published in the 12 chapters (many volumes) of EGA appeared in 1960, on page 6 of EGA 1. Now, though, we know that the first four chapters of EGA already took 7 years to be published, and contained more than 1800 pages in 8 volumes. The remaining eight chapters were never written or published.

In January 1984, Grothendieck wrote: “Mais aujourd’hui je ne suis plus, comme naguère, le prisonnier volontaire de tâches interminables, qui si souvent m’avaient interdit de m’élancer dans l’inconnu, mathématique ou non” (see [11], page 51).

This shows that Grothendieck did find it a heavy task to lay the foundations of algebraic geometry in his style. Indeed, as Serre writes:

“ J’ai l’impression que, malgré ton énergie bien connue, tu étais tout simplement fatigué de l’énorme travail que tu avais entrepris” (see [10], 8.2.1986, page 250).

Although the original plan for EGA was far from finished, I think that Grothendieck did hand down enough of his ideas of these foundations to us in a way for which we can use them and proceed. Also we see that basically everything he produced in those twelve fruitful years did belong to “known territory” to him. Did he consider his activity before 1970 as “faire de la géométrie algébrique”?

Cartier remarks that Grothendieck , after leaving the field of “nuclear spaces” and everything connected with that, “in rather characteristic fashion, never paid attention to the descendant of his ideas, and showed nothing but indifference and even hostility towards theoretical physics, a subject guilty of the destruction of Hiroshima!” Was Grothendieck’s behavior after 1970 with respect to the “descendance” of his ideas in algebraic geometry very different?

**(3.11).** Let me mention at least three very different aspects of Grothendieck’s work in algebraic geometry.

**Foundational work.** The way Grothendieck revolutionized this field is amazing. And, how is it possible that someone writes, within say 10 years, thousands of pages of non-trivial mathematics with no flaws, theory just flowing on and on?

**Imagination.** His published work, say between 1960 and 1970, was based on his deep insight, which enabled Grothendieck to see clearly the structure of this material. But Grothendieck also conveyed his ideas in manuscripts of many pages. We will see how just one idea (the anabelian conjecture) gave rise to a flow of activities and results. So many more deep ideas are still not fully understood. Grothendieck supplied many starting points which will keep us busy for many years; e.g. see § 7. I think that large parts of [14] are still not understood.

**Questions.** Grothendieck was very open in asking questions spurred by his curiosity. And here we see a strange mixture of deep insight (into structures and in theory) on the one hand and some innocent ignorance (in easy examples, in very concrete matters in mathematics) on the other. For me, it has always been a puzzling mystery how someone with such deep insight can proceed in mathematics without basic contact with elementary examples, and how it is possible that someone with such deep insight could miss easy aspects which are obvious to mathematicians who are used to living with examples and finding motivation in simple and easy structures. Putting things together, one can conclude that Grothendieck was not hampered by details which could obstruct his incredible insight in abstract matters. And perhaps we can be grateful that he did not know such easy examples, so that they did not obstruct him when finding his way through the mazes of abstract thoughts. See Section 1.

**(3.12). Sometimes too abstract?** When examples and direct applications are not there to form an obstruction to developing abstract mathematics, sometimes theory can go too far. For just ordinary people this point comes quite soon; many times I have seen a student doing much better after being asked to produce at least one example of the theory developed. It quite often happens that I ask a former student something, and the answer is just a beautiful, complicated example illustrating what I am asking. I call it “Feynman’s method”: while following a talk, or reading a paper, you test every statement against a non-trivial example that you know very well.

Many attempts by Grothendieck put the right perspective on the matter at hand. But sometimes I have the feeling that he went too far. Many years ago, I asked Monique Hakim to explain to me what she worked on for her Ph.D. She explained to me some material which much later appeared in her book [28]. During that explanation I saw the connection with deformation theory as explained by Kodaira and Spencer, see [37]. Before Schlessinger’s paper and the Grothendieck-Mumford deformation theory was available, the



Kodaira-Spencer paper was a valuable source of information and inspiration. You can see how the authors find the right concept. However, they have to struggle with a mixture of methods: we see families where the base is a differentiable manifold, the fibers are algebraic varieties, and on the total space these structures are mixed in an obvious but not so easy way. This was all at a time when a “scheme over  $\text{Spec}(k[\epsilon])$ ” did not yet exist; David Mumford writes: “But now Grothendieck was saying these first order deformations *were actually families*, families whose parameter space was the embodied tangent vector  $\text{Spec}(k[\epsilon]/(\epsilon^2))$  (see [45]).

Quite understandably, Grothendieck tried to find a unifying framework in which such families naturally find their place. The idea is to replace every geometric object by the category of, say, coherent sheaves on it. The category of varieties then becomes a category of categories. And we see fundamental problems arise: one doesn’t want to talk about “isomorphisms of categories”, but rather of equivalences. The idea is nice, but I doubt whether any geometer can truly work, do computations, or consider structures in such an abstract universe. History has shown us that while we have gratefully accepted many structures handed down to us by Grothendieck, common sense and practical necessity sometimes forces us to back up our abstract theory by more concrete methods, examples and computations.

Several of the considerations above can be summarized by the following words of Leila Schneps: “...Grothendieck’s style...his view of the most general situations, explaining the many ‘special cases’ others have worked on, his independence from (and sometimes ignorance of) other people’s written work, and above all, his visionary aptitude for rephrasing classical problems on varieties or other objects in terms of morphisms between them, thus obtaining incredible generalizations and simplifications of various theories.” (See [69], page 5.)

**(3.13). Grothendieck inspired many of us.** Not only did earlier results form a basis for ideas by Grothendieck, but even more, Grothendieck’s new theories gave rise to many new developments. One could draw a diagram of this:

earlier ideas – structures invented/discovered by Grothendieck – later  
developments.

This gives a clear picture of the flow of mathematical ideas.

An answer to Grothendieck as to whether his “pupils” did continue his work could be that indeed, a lot of us did build upon the work he did, although not precisely in his style; in many cases with a different approach, in some cases with less insight, but certainly with great respect. Also see [10], page 244, where Serre writes: “Non continuation de ton œuvre par tes anciens élèves. Tu as raison: ils n’ont pas continué. Cela n’est guère surprenant: c’était toi

qui avais une vision d'ensemble du programme, pas eux (sauf Deligne, bien sûr).”

#### 4. We should write a scientific biography

**(4.1).** *We should start writing a scientific biography of Grothendieck.* It would be worthwhile to write a mathematical biography of Grothendieck in terms of his scientific ideas. This would imply each time discussing a certain aspect of Grothendieck’s work, indicating possible roots, then describing the leap Grothendieck made from those roots to general ideas, and finally setting forth the impact of those ideas. This might present future generations with a welcome description of topics in 20th century mathematics. It would show the flow of ideas, and it could offer a description of ideas and theories currently well-known to specialists in these fields now; that knowledge and insight should not get lost. The present volume already is a first step in this direction.

Many ideas by Grothendieck have already been described in a more pedestrian way. But the job is not yet finished. In order to make a start, I intend to give some (well-known) examples in §§ 5, 6, 7, which indicate possible earlier roots of theories developed by Grothendieck. This is just a small and superficial selection: many more examples should be described and worked out in greater detail.

Or, should we speak of “a genetic approach to algebraic geometry”? In [83] we see: “Otto Toeplitz did not teach calculus as a static system of techniques and facts to be memorized. Instead, he drew on his knowledge of the history of mathematics, and presented calculus as an organic evolution of ideas beginning with the discoveries of Greek scholars such as Archimedes, Pythagoras, and Euclid, and developing through the centuries in the work of Kepler, Galileo, Fermat, Newton, and Leibniz. Through this unique approach, Toeplitz summarized and elucidated the major mathematical advances that contributed to modern calculus.” I thank Viktor Blåsjö for indicating this reference to me. Instead of what I phrase as “Grothendieck and the flow of mathematics”, I could also choose to say “a genetic approach to Grothendieck’s results”.

#### 5. The fundamental group

“.. une définition algébrique du groupe fondamental...”  
Grothendieck 22.11.56, see [10], page 55

For a description of this topic, see [3], Vol. 1, and see the paper by Murre in this volume [46].

We are familiar with classical ideas like Galois theory and the theory of the fundamental group of a pointed topological space. In Grothendieck’s theory of the fundamental group, these two theories are combined in one framework. It is due to Grothendieck that we have this beautiful and important tool at

our disposal, combining pillars of algebra and topology into a new concept, with many more applications and much more insight than were possible before.

**(5.1). The arithmetic and the geometric part.** In the unified fundamental group defined by Grothendieck for (say) a variety  $X$  over a ground field  $K$ , the Galois group of that field appears as a quotient:

$$1 \rightarrow \pi_1(\overline{X}, a) \longrightarrow \pi_1(X, a) \xrightarrow{p_X} \text{Gal}(K^{\text{sep}}/K) = \pi_1(\text{Spec}(K)) \rightarrow 1$$

(see [3], Vol. 1, Th. 6.1 for an even more general situation): Grothendieck defined  $\pi_1(X, a)$  for an arbitrary scheme  $X$  with a geometric point  $a$ . Here we see that starting with classical ideas and placing them in a new framework, a powerful tool becomes available.

**(5.2). An application: the monodromy theorem.** In this theorem, we study a family over a punctured disk (or over the field of fractions of a discrete valuation ring) and we consider in which way the fundamental group of the base (or the Galois group of that field) acts on, say, the homology of the fibers. This situation was studied in many separate cases (Landman, Steenbrink, Brieskorn and many others). One version of the monodromy theorem says that

(1) *the eigenvalues of a monodromy matrix are roots of unity.*

Proofs were not easy. However as soon as Grothendieck's theory of the fundamental group combined the fundamental group of the base (or the Galois group of the field of definition) and the geometric fundamental group of a fiber into one concept, a proof was just an elementary exercise in linear algebra. See [77], page 515 for this idea by Grothendieck published by Serre and Tate; see [68], pp. 79-83 for an elementary proof of a simplified version, and for some references to earlier work. – This is a beautiful example of what Grothendieck means by: “the nut opens just by itself”. Or one could say that it seems “like black magic”. This theorem is proved by an easy exercise in linear algebra.

The result was proved and used in a more general setting. Usually what we call the “Grothendieck monodromy theorem” is the fact that a variety (or an  $\ell$ -adic representation coming from algebraic geometry) over a local field is potentially semi-stable. For more explanation and references, see [31]. As a comment to my use of the term “monodromy theorem”, Luc Illusie communicated to me:

“The monodromy theorem: ‘a wonderful application of the theory of the fundamental group’: here you are mixing and confusing two things:

- (1) the ‘exercise in linear algebra’ saying that the action of inertia on  $\ell$ -adic representations over a local field with finite residue field (or such that the local field is small enough in the sense that it does not contain all roots of unity of order a power of  $\ell$ ) is quasi-unipotent (appendix of [77]);
- (2) the theorem that the same statement holds for representations arising

from  $\ell$ -adic cohomology with proper supports or no supports of schemes separated and of finite type over the local field (whether or not the residue field satisfies the ‘smallness’ assumption).

Grothendieck gave two proofs of (2), both using much more than ‘the theory of the fundamental group’. One (the ‘arithmetic’ one, as Grothendieck called it) consisted in a delicate reduction to (1), using the main theorems of SGA 4 and Néron’s smoothification method, the second one (the ‘geometric’ one) was conditional, based on resolution of singularities, and only worked unconditionally in characteristic zero. This second proof was inspired to Grothendieck by Milnor’s conjecture on the monodromy of an isolated singularity (Grothendieck told me he had greatly enjoyed Milnor’s book), and used the full force of Grothendieck’s theory of  $R\Psi$  and  $R\Phi$ , together with the calculation of nearby cycles in the general semistable reduction case (nowadays we can make Grothendieck’s proof work unconditionally, using de Jong – getting uniform bounds for the index of the open subgroup of the inertia group which acts unipotently).”

**(5.3). The fundamental group under specialization. An application.** (Computation of the prime-to- $p$  part of the geometric fundamental group of a curve in characteristic  $p$ .) One of Grothendieck’s results that he seemed very satisfied with was his computation of the prime-to- $p$  part of the geometric fundamental group of a curve in positive characteristic. *Let  $X_0$  be an irreducible, complete, non-singular algebraic curve over an algebraically closed field of characteristic  $p$ , and let  $Y$  be an irreducible, complete, non-singular algebraic curve over  $\mathbb{C}$  of the same genus. Then the group  $\pi_1(X_0)^{(p)}$  is isomorphic to  $\pi_1(Y)^{(p)}$*  (see [3], Vol. 1, Cor. 3.10). The structure of this group is well-known, as follows by classical, topological considerations. Note, however, that there seems to be no known proof giving this structure only using algebraic and geometric methods of algebraic geometry; this is the key to the result quoted above.

Here we see that that a question can lead naturally the discovery of new methods, new insight. Grothendieck developed “specialization of the fundamental group” (see [3], Vol. 1, Th. 3.8). In this theorem, for a scheme that is proper and smooth over a discrete valuation ring with residue characteristic  $p$ , the prime-to- $p$  part of the fundamental group of the geometric generic fiber maps isomorphically onto the prime-to- $p$  part of the fundamental group of the geometric special fiber.

This example shows in what way Grothendieck revolutionized this part of algebraic geometry “just” by describing the right concepts. Such ideas (unramified maps, coverings in topology, Galois groups) certainly were known in special cases, but the “quantum leap” from those previous ideas to the concept of the algebraic fundamental group is startling. For us, nowadays, it is hard to imagine how to proceed in algebraic geometry without such a tool at hand.

It is clear that Galois theory, the theory of the topological fundamental group, and existing monodromy-singularities considerations were a source of inspiration for Grothendieck.

(5.4). The result mentioned in (5.3) studies the geometric fundamental group of an algebraic curve, of a Riemann surface, as an abstract group. The wonderful paper [41] convinced me that it is even better to consider the *geometric fundamental group*, in characteristic zero, *as a subgroup* of  $\mathrm{PSL}_2(\mathbb{R})^0$ .

## 6. Grothendieck topologies

(6.1). When working in the algebraic context, the classical topology is replaced by the Zariski topology. But then cases arise that demand yet other adaptations. For example, consider a quotient by an algebraic group, such as an isogeny  $\varphi : E \rightarrow E'$  of elliptic curves (a quotient by a finite group scheme). When working over  $\mathbb{C}$  *in the classical topology*, this map is locally trivial. However if  $\varphi$  is not an isomorphism, this is not locally trivial in the Zariski topology. And this applies to many quotient maps in algebraic geometry. However, we would like to work with the notion of a fiber space, as was done earlier in so many cases in classical topology. This problem was recognized immediately after introducing the Zariski topology. Already in [72], we see how to circumvent this by proposing “une définition plus large, celle des espaces *localement isotriviaux*, qui échappe à ces inconvénients.” The general theory was then extended by Serre to this new notion of “isotrivial”, “trivial in the étale topology” in modern language. Already in that article Serre answered many questions, e.g. when is a quotient map locally trivial in the Zariski topology? See “groupes spéciaux”, and the fact that *every special group is connected and linear* ([72], Section 4). He also observed the limitations of this new notion; e.g. see [72], 2.6: quotient maps which are purely inseparable do not fall under the considerations of locally isotrivial coverings just discussed (in modern terminology, e.g. a quotient map under the action of a non-étale local group scheme). Serre also constructed a first cohomology group in this article, and asked whether one can define higher cohomology groups and whether they give the desired “vraie cohomologie” necessary for a proof of the Weil conjectures.

Note that what “localement isotriviaux” really means is “locally trivial in some Grothendieck topology”. It was M. Artin who found the correct notion of “étale localization” (see [31] for a description).

The way Grothendieck approached this new concept is characteristic of his way of developing new ideas: a rather simple remark, and a need for further technique in order to solve problems becomes clear. Grothendieck sets out to develop a new method in the most general situation possible, and many pages of abstract mathematics are created (it is clear that he had

a grand view of possibilities), and a new tool is created that can be applied and used in many situations.

(6.2). Here we clearly see the roots of further developments constructed and described by Grothendieck. The simple remark that a quotient map need not be locally trivial in the Zariski topology, and the remedy by Serre leads to a new concept: “Grothendieck topologies”. Hundreds of pages on this topic can be found in SGA 4. It is one of the most important tools in fields like logic and algebraic geometry. Also, we can see by this example how we become accustomed to a new concept. I remember the first time I saw a topology as a set of maps which do not give necessarily subsets; it was new to me. After some time you get accustomed to it, and it seems as if it must always have been that way.

“... j’admettais de confiance que pour le plongement usuel du groupe projectif dans le groupe linéaire, il y a une section rationnelle, puisque tout le monde semblait convaincu que ça devait toujours se passer comme ça pour une fibration par un groupe linéaire...” Letter of Grothendieck to Serre of 30.1.1956, see [10], page 29.

## 7. Anabelian geometry

(7.1). After 1970 Grothendieck wrote down many new ideas: “On pourra commencer à faire de la géométrie algébrique!” Many of these ideas have not yet been unravelled and certainly many of them not at all understood. Let me describe one of these, where we can clearly indicate the “roots” and where we now have a fairly good understanding of some of the implications and general structures involved.

In order to state the idea, Grothendieck introduced the notion “anabelian”. In particular this applies to the (the fundamental group of) a curve of genus at least two. Of course Grothendieck also mentions that we should prove such results more generally for arbitrary “hyperbolic” varieties. Grothendieck baptizes these curves, these situations, these groups “anabelian” because such “groupes fondamentaux...sont très éloignés des groupes abéliens...” (see [11], p. 14, or [68], page 17). Later on, a more technical definition of an “anabelian group” became available:

**Definition.** A group  $G$  is called *anabelian* if every finite index subgroup  $H \subset G$  has trivial center.

**Definition.** A topological group  $G$  is called *anabelian* if every finite index, closed subgroup  $H \subset G$  has trivial center.

### Examples.

(1) For a number field  $K$ , i.e.  $[K : \mathbb{Q}] < \infty$ , its absolute Galois group  $G = G_K = \text{Gal}(\overline{K}/K)$  is anabelian. This follows from results known to F. K. Schmidt, see [61] to which Neukirch refers, see [50].

(2) On page 77 of [40] we find the definition of a *sub- $p$ -adic field*. In particular any number field (a finite extension of  $\mathbb{Q}$ ), or a finite extension of  $\mathbb{Q}_p$  is a sub- $p$ -adic field. Following Mochizui and Tamagawa we have:

*For every sub- $p$ -adic field  $K$ , its absolute Galois group is anabelian;* see [40], Lemma 15.8 on page 80.

(3) For a hyperbolic curve  $X$  over an algebraically closed field, the fundamental group is anabelian. E.g. for complete curves of genus at least 2 over an algebraically closed field (of arbitrary characteristic), see [25], Lemma 1 on page 133.

In the terminology of S. Mochizuki – H. Nakamura – A. Tamagawa such groups are called “slim groups”.

It might be that a more refined definition of an “anabelian group” is necessary in order to be able to prove the full analogue of the anabelian Grothendieck conjecture in higher dimensions.

(7.2). Let  $K$  be a field and let  $X$  be a geometrically irreducible algebraic curve, smooth over  $K$ . Let  $k$  be an algebraic closure of  $K$ . The following statements are equivalent:

- (1) The fundamental group of  $X_k$  is non-commutative.
- (2) The fundamental group of  $X_k$  is anabelian.
- (3) The genus of  $X$  is either 2, or the genus is 1 and  $X$  is not proper over  $K$ , or its genus is zero and at least three geometric points have to be added to obtain a complete model.
- (4) (In case  $K \subset \mathbb{C}$ .) The Euler characteristic is negative:  $\chi(X(\mathbb{C})) < 0$ .
- (5) (Definition.) The curve is called *hyperbolic*.

Over an arbitrary field, (3) is usually used as the definition of a hyperbolic curve.

In [11], and in the letter June 27, 1983 of Grothendieck to Faltings (see [68], pp. 49-58) we see the following “anabelian” conjecture. For a scheme  $X$  (with base point, which will be omitted in the notation) over a field  $K$  we write

$$p_X : \pi_1(X) \rightarrow G_K := \text{Gal}(K)$$

for the natural map of fundamental groups as in (5.1). For schemes  $X$  and  $Y$  over  $K$  we write

$$\text{Isom}_{G_K}(\pi_1(X), \pi_1(Y))$$

for continuous isomorphisms which commute with  $p_X$ , respectively  $p_Y$ . We write  $\text{Inn}(\pi_1(X))$  for the group of inner automorphisms.

(7.3). **Anabelian conjecture.**(Grothendieck). *Let  $K$  be a number field, i.e.  $[K : \mathbb{Q}] < \infty$  and let  $X$  and  $Y$  be hyperbolic algebraic curves over  $K$ . Then the natural map*

$$\text{Isom}_K(X, Y) \longrightarrow \text{Isom}_{G_K}(\pi_1(X), \pi_1(Y))/\text{Inn}(\pi_1(Y))$$

is bijective.

I will not describe here the rich history and the flow of ideas, proofs and results on this topic due to F. Bogomolov, Y. Ihara, S. Mochizuki, H. Nakamura, Takayuki Oda, F. Pop, Michel Raynaud, M. Saïdi, A. Shiho, A. Tamagawa, Y. Tschinkel, V. Voevodskii and many others, starting from the moment Grothendieck made his conjecture on this topic, and made public his ideas on this and other related topics. Basically this conjecture, as well as several generalizations and considerations in analogous situations, have now been proved or settled.

**(7.4). Neukirch and Uchida.** In trying to determine the “roots” of the anabelian conjecture, we can find at least two different sources. For the arithmetic of number fields, as far as this is encoded in the absolute Galois groups, there is a theorem of Artin and Schreier (from 1927). Then, in 1969–1977 Neukirch and Uchida proved that two number fields are isomorphic if and only if their absolute Galois groups are isomorphic as profinite groups; see [49], [50], [84]. This is called the Neukirch-Ikeda-Iwawasa-Uchida result. For a survey of the history of these, see [62].

Note, however, that the corresponding statement *does not hold for local fields*: two finite extensions of  $\mathbb{Q}_p$  can have isomorphic absolute Galois groups without being isomorphic; see [51], XII.2, “closing remark”. I thank Jakob Stix for helpful discussions and for providing references on this subject.

**(7.5). Tate and Faltings.** In 1966, Tate formulated a conjecture, that he proved for abelian varieties over finite fields; see [81]. In 1983, the conjecture was proved by Faltings over number fields (see [24]).

**(7.6). Theorem** (The Tate conjecture; Tate, Zarhin, Mori, Serre, Faltings). *Let  $K$  be a field of finite type over its prime field. Let  $\ell$  be a prime number not equal to the characteristic of  $K$ . Let  $X$  and  $Y$  be abelian varieties over  $K$ . Then the natural map*

$$\mathrm{Hom}(X, Y) \otimes \mathbb{Z}_\ell \xrightarrow{\sim} \mathrm{Hom}(T_\ell(X), T_\ell(Y))$$

*is an isomorphism.*

This conjecture was generalized by Tate to the situation of algebraic cycles, but that generalization will not be discussed here.

We note that the analog of the above result *does not hold over local fields*, as was remarked by Lubin and Tate: *there exists a finite extension  $L \supset \mathbb{Q}_p$  and an abelian variety  $A$  over  $L$  such that the natural inclusion*

$$\mathrm{End}(A) \otimes \mathbb{Z}_\ell \subsetneq \mathrm{End}(T_\ell(A))$$

*is not an equality.* In fact, we can choose  $A$  to be an elliptic curve with  $\mathrm{End}(A \otimes \bar{L}) = \mathbb{Z}$  and  $\mathrm{End}_L(T_\ell(A))$  of rank two over  $\mathbb{Z}_\ell$ . For details and references see [21], 3.17.



**(7.7). Theorem** (Tate). *Let  $K$  be a finite field, and let  $X$  and  $Y$  be abelian varieties over  $K$ . Then the natural map*

$$\mathrm{Hom}(X, Y) \otimes \mathbb{Z}_p \xrightarrow{\sim} \mathrm{Hom}(X[p^\infty], Y[p^\infty])$$

*is an isomorphism.*

See [85], Th. 6.

**(7.8).** After Neukirch and Uchida's result, which we could now call *anabelian theory for number fields [??] finite fields*, and after Faltings' proof of the Tate conjecture over number fields, we can see how Grothendieck's anabelian conjecture for hyperbolic curves *over number fields* arises naturally. Basically, this conjecture and several generalizations have been proved. However, the following result came as a big surprise (at least to me).

**(7.9). Theorem** (Mochizuki). *Let  $L$  be a finite extension of  $\mathbb{Q}_p$ , and let  $X$  and  $Y$  be hyperbolic algebraic curves over  $L$ . Then the natural map*

$$\mathrm{Isom}_L(X, Y) \longrightarrow \mathrm{Isom}_{G_L}(\pi_1(X), \pi_1(Y)) / \mathrm{Inn}(\pi_1(Y))$$

*is bijective.*

This amazing counterpart of (7.3) can be found in [40].

**(7.10).** In the Tate conjecture/theorem (see (7.6)), it is essential to work over a number field, or over a field finitely generated over a prime field, but not over a  $p$ -adic field. Grothendieck knew this idea, and we can assume (conclude?) that his anabelian conjecture had the Tate conjecture as a stimulating source. Also see "Brief an G. Faltings", [68], pp. 49-58. In this letter to Faltings, Grothendieck stressed that we should work over a field of finite type over a prime field. It seems that it did not occur even to Grothendieck himself that a result like Mochizuki's theorem (see (7.9)) could be true for curves over a  $p$ -adic field; in fact the analog for abelian varieties does not hold over a  $p$ -adic field.

**(7.11). The section conjecture.** Consider the exact sequence in (5.1). Any  $K$ -rational point in  $X$  will give rise to a section of the map

$$p_X : \pi_1(X, a) \longrightarrow \mathrm{Gal}(K^{\mathrm{sep}}/K).$$

The *Grothendieck anabelian section conjecture* expects that the map

$$X(K) \longrightarrow \Gamma(\pi_1(X, a) \rightarrow G_K) / (\text{conjugation by } \pi_1(\bar{X}))$$

from  $X(K)$  to the set  $\Gamma(-)$  of *sections* for  $p_X$  up to conjugacy thus obtained *is a bijection in the case of hyperbolic curves*. Grothendieck already knew that this map was injective. No final result seems to be known at present about this conjecture. For a survey see [78].

*In §§ 5, 6, 7 we have described three examples of earlier thoughts which inspired Grothendieck to create a completely new theory, or a conjecture which opened new areas of research for us.*

### 8. If the general approach does not work

“ ... obtaining even good results ‘the wrong way’  
 – using clever tricks to get around deep theoretical obstacles –  
 could infuriate Grothendieck.”  
 See [69], p.18.

**(8.1).** What happens if general patterns and theories do not suffice to settle a specific problem? Grothendieck gives us the impression that at such a point, one might need to develop a more general structure; to “escape” into a more general problem. However, there are mathematicians who, especially in specific situations, like to proceed by studying examples or making non-canonical choices, and sometimes a proof or construction comes out of all this even though it is neither expected nor obtained by general principles. A challenging difficulty in a problem, something which for years and years obstructs any solution, has always seemed to me to be more a stimulating and beautiful aspect of mathematics than a negative one. In past decades we have seen many examples of proofs that diverge from Grothendieck’s “general approach” philosophy. Just to indicate the flavor, I will discuss a few of these.

All of the problems and questions in this section were studied by Grothendieck, but he did not solve them. These results show that while general theory is certainly needed, additional considerations such as a “trick” or a worked-out example were necessary in order to arrive at a solution.

Already in Section 2 we discussed various manners of finding new ways in mathematical research. Sometimes the activity of a mathematician is to create a new abstract theory, or if one prefers, to describe a structure which already exists but has not been discovered as yet. To solve a given problem, it is sometimes better to first understand the general pattern, and then just “turn the machine” in order to get the desired answer. As Grothendieck said, “Une fois cette théorie développée, j’espère bien que les conjectures de Weil viendront toutes seules”: these words express what he was hoping for (see [10], 9.8.1960, page 104).

On 23.7.1985 Serre wrote to Grothendieck:

“Je sais bien que l’idée même de ‘contourner une difficulté’ t’est étrangère – et c’est peut-être cela qui te choque le plus dans les travaux de Deligne (autre exemple: dans sa démonstration des conjectures de Weil, il ‘contourne’ les ‘conjectures standard’ – cela te choque, mais cela me ravit).

(En fait, malgré ce que tu dis dans L28, mes façons de penser ne sont pas très différentes – profondeur à part – de celles de Deligne. Et elles sont

assez éloignées des tiennes – ce qui explique d’ailleurs que nous nous soyons très bien complétés pendant 10 ou 15 ans, comme tu le dis très gentiment dans ton premier chapitre.)” See [10], pp. 244/245.

Serre communicated to me: “About theorems being proved by general methods or by tricks. The word trick is pejorative. But one should keep in mind that a ‘trick’ in year  $N$  often becomes a ‘theory’ in year  $N + 20$ . This is typically what has happened with Deligne’s proof, and Wiles’ proof.”

**(8.2). The Weil conjectures.** As could already be seen in [1], the Weil conjectures were the starting point for Grothendieck to revolutionize algebraic geometry. Following this hint, we could deduce that Grothendieck was interested in “problem solving” research. We have seen that this was not true at all. Although these conjectures remained the driving force behind many of his endeavors between 1960 and 1970, we see that even when necessary methods became available, Grothendieck did not immediately sit down and try a head-on approach for a solution. As long as “the nut did not open just by itself”, the time was not ripe: “j’espère bien que les conjectures de Weil viendront toutes seules”. It seemed necessary to Grothendieck to develop more general theory, or more general conjectures which, once proved, would yield the Weil conjectures as an easy corollary. For the “standard conjectures” see [5], [35], [36].

In 1959 Dwork proved essential parts of the Weil conjectures; for a description and for references, see [8]. It seems that Grothendieck was not very interested in this work; at least we have no record that he ever seriously plunged into it; see [69], p. 17. Perhaps this was an essential aspect of his devotion to his own plan: examples or work by other mathematicians only partially interested him, I think, insofar as it supported his ideal view on further development, or revealed the intrinsic beauty of general structures studied, or if it could stimulate him to transform this “source” into a grand new idea.

We know how the story of the Weil conjectures did eventually proceed. Deligne proved these conjectures in the end; however, he diverged from the road proposed and wanted by Grothendieck. Instead of Grothendieck expressing admiration for Deligne for this great achievement, only a negative reaction came out (to say the least); I find this one of the most regrettable episodes in the development of modern algebraic geometry. We are grateful to Deligne for this wonderful result, for this token of insight combining abstract and deep insight on the one hand and a direct approach (sometimes called “a trick”, but that is not fully adequate) on the other; it gives us confidence to try to proceed with such insight and energy. But we can also be grateful for Grothendieck formulating the ‘standard conjectures’, which are still a source for further inspiration. This fascinating aspect – the Weil conjectures and everything they created – of the “flow of mathematics”

is a great example of the essence of our profession and the way various mathematicians work and react, in such different ways, to challenges.

**(8.3). Lifting abelian varieties to characteristic zero.** Suppose we have an abelian variety  $A_0$  defined over a field  $\kappa$  of positive characteristic. Does there exist a lifting to an abelian variety defined over a field of characteristic zero? I.e. does there exist an abelian scheme over a mixed characteristic base having  $A_0$  as special fiber? Grothendieck was interested in such questions as early as 1958 (see [10], p. 67).

There is a natural approach to this question. One studies deformations (in mixed characteristic) of  $A_0$ , like Kodaira-Spencer, and later Schlessinger, Grothendieck and Mumford taught us to do. Illusie communicated to me: “...Grothendieck studied formal deformations before Schlessinger; of course, it’s Schlessinger who gave a really manageable criterion, and I remember that Grothendieck was surprised, vexed, and finally happy at that.”

The result is that indeed, a *formal* abelian scheme can be constructed in mixed characteristic, and in fact, as Grothendieck showed, this problem is unobstructed (see [54]). However, we need to algebrize the result in order to end up with a true abelian variety in characteristic zero. As the Lefschetz-Chow-Grothendieck method is available, it suffices to make a formal deformation of  $(A_0, \mu_0)$ , where  $\mu_0 : A_0 \rightarrow A_0^t$  is a polarization; for a beautiful description of Grothendieck’s existence theorem in formal geometry, see Part 4 written by Illusie in [6]. Grothendieck and Mumford proved that this problem is unobstructed in the case where  $A_0$  admits a *principal polarization*, or at least a polarization of degree prime to  $p$ ; in that case the problem is settled satisfactorily (see [54]).

However there are (many) cases where  $A_0$  does not admit a principal polarization, and where the deformation problem defined by  $(A_0, \mu_0)$  can be obstructed. Stepwise deformations do not give much information: if the next infinitesimal step is obstructed, how can we change the previous steps in order to be able to proceed unobstructed? It seems as though here, the machine comes to a stop. This was as far as Grothendieck could bring the state of affairs.

But at this point, ideas of David Mumford entered the scene; we see a pattern that is visible in many other cases. One uses the ideas, and the structures and tools given to us by Grothendieck, but one adds a new ingredient, which has a completely different flavor. Mumford started by describing the theory of “displays”: choosing a basis for the Dieudonné module of the  $p$ -divisible group of  $A_0$ , one describes in this coordinate system an arbitrary “deformation” of the Frobenius in characteristic  $p$  (or in mixed characteristic) which still divides  $p$ ; this can be done directly; from the “formula  $V = p/F$ ” one can construct (over a perfection of the deformation ring) a  $p$ -divisible group which defines this deformation, and this can be descended to the deformation ring. This method gives access to direct computations: stepwise deformations are all encoded in one system.

Later this tool was further developed by T. Zink: it gave rise to the general and very useful theory of “windows”.

After developing this general theory Mumford proceeded to use it to show that any polarized abelian variety  $(A_0, \mu_0)$  can be deformed in characteristic  $p$  to a polarized *ordinary* abelian variety. Note that this deformation is not canonical and not unique; it depends on choices, and is very much *not* in the style of Grothendieck.

Once this point is reached, one can use a general theory by Serre and Tate which shows that any polarized *ordinary* abelian variety admits a (canonical) lifting to characteristic zero, which concludes the proof. We see the ingenious combination of general theory, tricks, computations and general structures. This theorem was proved/expected by Mumford (see [69b] in [44]; also see [52]); this program was outlined by Mumford, and details were worked out in [53]:

**Theorem** (Mumford; Norman-Oort) **(8.3).1.** *Suppose given a polarized abelian variety  $(A_0, \lambda)$  over a field  $\kappa$  of characteristic  $p$ . Then there exists an integral domain  $R$  of mixed characteristic, with a residue class map  $R \rightarrow \kappa$  and a polarized abelian scheme  $(A, \lambda) \rightarrow \text{Spec}(R)$  such that  $(A, \lambda) \otimes_R \kappa \cong (A_0, \lambda)$ .*

**Remark.** On several occasions, Grothendieck considered the question of the existence of (canonical) liftings. In his letter to Serre of 5 December 1958, he wrote: “...me font penser qu’il est possible de remonter canoniquement toute variété  $X_0$  définie sur un corps parfait de caractéristique  $p \neq 0$  en une sorte de ‘variété holomorphe’  $X$  définie sur un anneau local complet quelconque  $\mathcal{O}$  ayant le même corps résiduel. Si on a la chance que cette ‘variété holomorphe’ provient d’une variété algébrique  $X$  définie sur  $\mathcal{O}$ , alors cette dernière est unique, dépend fonctoriellement de  $X_0$ , etc.” (see [10], p. 67).

It is hard to understand what Grothendieck had in mind at that moment. For an algebraic curve, it is not clear what a “canonical lift” should be. For an elliptic curve (abelian variety of dimension one) which is supersingular there is no “canonical lift” to characteristic zero. Serre gave an example of a surface which does not admit a lift to characteristic zero at all (see [75]). Further examples by Serre, non-singular projective varieties which could not be lifted to characteristic zero, are described by L. Illusie in [6], Part 4, Chapter 8: *Grothendieck’s existence theorem in formal geometry with a letter of Jean-Pierre Serre*. In Coroll. 8.6.7 these examples are studied, and results are extended to varieties of dimension at least two.

The theorem alluded to in the previous paragraph, that an ordinary (polarized) abelian variety in positive characteristic admits a canonical lift to characteristic zero, was explained by Serre to Grothendieck right after the Woods Hole conference (see [10], pp. 161-164).

**Example.** In 1965, Grothendieck and Serre tried to have at least a candidate for an abelian variety in positive characteristic which could not be lifted to characteristic zero (see [44], page 704). Here is the idea.

Let  $E$  be a supersingular elliptic curve, say over  $\mathbb{F} = \overline{\mathbb{F}}_p$  (it can be defined over  $\mathbb{F}_{p^2}$ ). The group scheme  $E[F]$ , the kernel of the Frobenius map  $F : E \rightarrow E^{(p)}$ , is called  $\alpha_p$ ; this is a finite group scheme of rank  $p$  which is neither isomorphic with  $(\mathbb{Z}/p)$  nor with  $\mu_p$ . Choose an embedding

$$i = (i_1, i_2) : \alpha_p \hookrightarrow E \times E; \quad \text{define } X_i := (E \times E)/i(\alpha_p).$$

It is easy to see that  $X_i$  is a product of elliptic curves if and only if  $i_1/i_2 \in \mathbb{F}_{p^2}$ . Moreover,  $X_i$  is a CM abelian variety. If  $i$  cannot be defined over  $\mathbb{F}$ , then the CM abelian variety  $X_i$  (defined over a transcendental extension of  $\mathbb{F}$ ) cannot be CM lifted to characteristic zero.

It might be that any  $X_i$  defined over  $\mathbb{F}$  which is *not isomorphic to a product of elliptic curves* cannot be lifted to characteristic zero (and this was the example Grothendieck and Serre had in mind). This abelian surface does not admit a principal polarization, and the deformation problem might be non-smooth. However it can be shown that any such  $X_i$  can be lifted to characteristic zero. Moreover, in general (i.e. in case  $i$  generates a “large” finite field), it cannot be CM lifted to characteristic zero, as we will see in [20].

**CM liftings.** One can ask for even more. An abelian variety defined over a finite field is always a CM abelian variety, as was proved by Tate (see [81]). Does it admit a CM lifting to characteristic zero? Complete answers can be found in [20].

**(8.4). A conjecture by Grothendieck.** This line of thought, this partly non-canonical approach as sketched in (8.3), was also used to prove a conjecture of Grothendieck from 1970 about deformations of a  $p$ -divisible groups. He asked the following question:

*Let  $X_0$  be a  $p$ -divisible group over a field of characteristic  $p$ ; let  $\zeta$  be a Newton polygon under the Newton polygon of  $X_0$ ; does there exist a deformation in equal characteristic where the generic fiber has Newton polygon equal to  $\zeta$ ? (see [9], page 150 of the appendix, a letter of Grothendieck to Barsotti).*

This question remained unanswered for almost thirty years. The problem shows the same kind of difficulty as we saw above: one can describe a deformation space of a (quasi-polarized)  $p$ -divisible group. By a theorem of Grothendieck and Katz, a given Newton polygon describes a closed set in that space (see [9], pp. 149/150; see [34], Th. 2.3.1 on page 143). However, in general that locus is highly singular; the corresponding deformation problem is (formally) non-smooth in most of the interesting cases. A locus where we require the generic fiber to have Newton polygon equal to  $\zeta$  may even be

empty as can be seen on some examples (for certain non-principally quasi-polarized  $p$ -divisible groups and given  $\zeta$ ); for a complete description of all such examples, see [59], Section 6. But also for a principally quasi-polarized  $(X_0, \lambda_0)$ , the general approach does not give a straightforward proof for this conjecture by Grothendieck. An analog of Mumford’s approach, however, proved to be successful (it was only much later that I realized this analogy between my approach to this question, and the method as described in (8.3)). A general theory was developed where for certain cases (technically speaking the case  $a(X_0) = 1$ ) objects known as “displays” and easy linear algebra showed the Grothendieck conjecture to be true (see [56]). The proof was finished (the most difficult step) by showing that a deformation exists with the same Newton Polygon and with  $a = 1$  in the generic fiber (a non-canonical, non-unique choice is needed). For details and references see [60], especially § 8, and the discussion in § 9.

We note that the developments described in (8.3) and the methods described here not only use ideas and structures developed by Grothendieck, but also show the necessity (sometimes) of supplementing these by new insight and non-canonical constructions. For the tricky step (deformation to  $a = 1$ ) in this approach to this conjecture by Grothendieck, we still do not have an “easy” proof; we do not have a structure or a general method which avoids any computation and study of special cases. This aspect of mathematics, considered as not very elegant by some people, has an appealing beauty to me, “cela me ravit” (I find this exciting).

**Theorem (8.4).1.** *Let  $X_0$  be a  $p$ -divisible group over a field  $\kappa$  of characteristic  $p$ . Let  $\gamma := \mathcal{N}(X_0)$  be its Newton polygon. Assume that  $\beta$  is a Newton polygon such that all points of  $\beta$  lie on or below  $\gamma$ . Then there exists an integral domain of characteristic  $p$ , a residue class map  $R \rightarrow \kappa$  and a  $p$ -divisible group  $X \rightarrow \text{Spec}(R)$  with  $X \otimes_R \kappa \cong X_0$  such that the Newton polygon of its generic fiber equals  $\mathcal{N}(X_\eta) = \beta$ .*

An analogous theorem holds for principally quasi-polarized  $p$ -divisible groups, and for principally polarized abelian varieties. An analogous statement for quasi-polarized  $p$ -divisible groups and for polarized abelian varieties admits many counterexamples. For references see [60] or [59].

We remark that in (8.3), reduction to the case  $a = 1$  (the case of monogenic Dieudonné modules for the local-local component of the  $p$ -divisible group) was done by an appropriate Hecke correspondence (see [43], page 141, see [53], Lemma 3.4). However, this is of little help for a proof of this conjecture by Grothendieck : a Hecke correspondence might drastically change the local deformation space. Moreover, the non-principally polarized analogue of this conjecture by Grothendieck does not hold in general. Hence a new method, deformation to  $a = 1$  keeping the Newton polygon fixed, had to be developed for this case.

**(8.5). Extending homomorphisms between  $p$ -divisible groups.** *Let  $X$  and  $Y$  be  $p$ -divisible groups over a discrete valuation ring  $R$  with field of fractions  $K$ . Suppose a homomorphism  $\beta_K : X_K \rightarrow Y_K$  is given. Does this extend to a homomorphism  $\beta : X \rightarrow Y$ ?*

In case the characteristic of  $K$  equals zero, this question was answered in a positive way by Tate in 1966 (see [79], Theorem 4). For a long time, any answer to this question in the remaining cases was unknown. On page V of the introduction of Exp. IX by Grothendieck in [3] 7I (page 317 in that volume) we find this question in the general setting.

Once someone said to me that Grothendieck tried to prove that indeed such an extension should exist in general, that he did not succeed, and that this was his reason for leaving algebraic geometry; this seems unlikely to me, but I do not know. Johan de Jong solved this affirmatively for all cases in 1998 in [33]. Also here, we see that at least as far as we know at present, no general theory, no “general machinery” can decide for us what the answer should be (also see [39]).

**Theorem** (Tate, A. J. de Jong) **(8.5).1.** *Suppose we are given a discrete valuation ring  $R$  with field of fractions  $K$ , and  $p$ -divisible groups  $X, Y \rightarrow \text{Spec}(R)$ . Then any homomorphism  $\beta_K : X_K \rightarrow Y_K$  extends to a homomorphism  $\beta : X \rightarrow Y$ . (See [79], Th. 4 on page 180; [33], Coroll. 1.2.)*

**(8.6). Truncations of  $p$ -divisible groups.** On several occasions Grothendieck considered Barsotti-Tate groups, also called  $p$ -divisible groups. Such a group, or rather ind- $p$ -group scheme, is an inductive limit, a union,  $\{G_i\}$  of group schemes:

$$G = \cup_i G_i = \text{limind } G_i, \quad \text{with } G[p^i] = G_i;$$

we refer to [30] for definitions and certain properties; also see [21], 1.15. On 5.1.1970 (see [44], p. 745) Grothendieck wrote to Mumford:

“I wonder if the following might be true: assume  $k$  algebraically closed, let  $G$  and  $H$  be BT groups, and assume  $G(1)$  and  $H(1)$  are isomorphic. Are  $G$  and  $H$  isomorphic? This is true, according to Lazard, if  $G$  is a formal group of dimension 1.”

Here Grothendieck writes  $G(i)$ , which we can also denote by  $G_i$  or by  $G[p^i]$ . Note that  $G_{i+1}/G_i \cong G_1$ , i.e.  $G$  is a “tower of which all building blocks all isomorphic to the same  $G_1$ ”. Mumford answers right away that the answer to this question is negative, as already is shown by 2-parameter formal BT groups. In [60], Section 12, we find an explicit infinite set of mutually different isomorphism classes of BT groups over  $\overline{\mathbb{F}}_p$  which all have, up to an isomorphism, the same  $p$ -kernel.

This exchange of ideas shows that Grothendieck could ask a question that could be answered by giving an easy example, and reveals that Grothendieck had an expectation that mathematical reality would show a simple and



beautiful structure (understanding BT groups would be elegant if this were true). But, it also shows that Grothendieck could lose interest as soon as the pattern could be more intricate (or less elegant) than he expected at first. I think this little episode is quite characteristic of his way of thinking and working: test by an easy question (e.g. to Serre or to Mumford), and only proceed when the original idea shows that mathematics indeed is simple.

As far as we know, Grothendieck dropped this idea. Was this topic not as beautiful and elegant as he wished? One could, however, proceed by asking: for which  $G(1)$  can we conclude  $G(1) \cong H(1) \Rightarrow G \cong H$ ? The answer is not obvious, not simple and elegant, but the technique developed in this way is very useful. Not knowing anything about this correspondence between Grothendieck and Mumford until 2010, I myself considered this problem; a complete answer can be found in [57]; also see [60], Section 12.

Here is an elegant and simple answer to this question (although the proof I know is neither obvious nor trivial). For any Newton polygon  $\zeta$ , define over  $\mathbb{F}_p$  a  $p$ -divisible group  $H(\zeta)$  which we call *the minimal  $p$ -divisible group attached to  $\zeta$* . Such a  $p$ -divisible group can easily be described explicitly (e.g. in terms of Dieudonné modules), but we will not do that here. A minimal  $p$ -divisible group  $H$  can be characterized over  $\mathbb{F} = \overline{\mathbb{F}}_p$  by requiring that  $H$  be a direct sum of its simple factors, and that for any simple summand  $H_i$  over  $\mathbb{F}$  we have that  $\text{End}(H_i)$  is the maximal order in  $\text{End}^0(H_i) := \text{End}(H_i) \otimes_{\mathbb{Z}_p} \mathbb{Q}_p$ .

**Theorem (8.6).1.** *Let  $k \supset \mathbb{F}_p$  be an algebraically closed field. Let  $X$  be a  $p$ -divisible group over  $k$ . Then*

$$(\forall Y, X[p] \cong Y[p] \implies X \cong Y) \iff (X \text{ is minimal}).$$

See [57].

Another characterization that can be found in [58] gives the following elegant result.

**Theorem (8.6).2.** *Let  $k \supset \mathbb{F}_p$  be an algebraically closed field. Let  $X$  be a simple  $p$ -divisible group over  $k$ . Then  $X$  is minimal if and only if  $X[p]$  is BT1 simple (i.e. there is no smaller, non-zero BT1 group scheme contained in  $X[p]$ ).*

**(8.7). Conclusion of this section.** Grothendieck constructed an impressive theory, a foundation for a new way of doing algebraic geometry, and handed down to us new tools. In many cases, all this leads directly to results and proofs. However, in some cases general theory can only be applied if special choices and non-canonical constructions are also supplied. Although this seems to contradict what Grothendieck taught us, sometimes such roads have to be taken. In fact, it is very often the combination of methods constructed by Grothendieck and insight we owe to him together with the study of special cases and the use of examples and “tricks” that lead us to new results.

In 1966 Grothendieck wrote to Mumford:

“ .. I found it kind of astonishing that you should be obliged  
to dive so deep and so far in order  
to prove a theorem whose statement looks so simple-minded.”

See [44], p. 717. Of course, we should always look for a simple proof, a proof which uses more structure and less tricks. But the beautiful reality, and the real beauty (I think) of mathematics is that you sometimes really do have to “dive so deep and so far”.

### References

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