REMINISCENCES OF GROTHENDIECK AND HIS SCHOOL

LUC ILLUSIE, WITH SPENCER BLOCH, VLADIMIR DRINFELD, ET AL.

In the afternoon of Tuesday, January 30, 2007 Illusie met with Beilinson, Bloch, Drinfeld and a few other guests at Beilinson's place in Chicago. He chatted by the fireside, recalling memories of his days with Grothendieck. Here is a slightly edited version of a transcript prepared by Thanos Papaïoannou, Keerthi Madapusi Sampath, and Vadim Vologodsky.

At the IHES

—(Illusie) I began attending Grothendieck's seminars at the IHES in '64 for the first part of SGA 5 ('64–'65). The second part was in '65–'66. The seminar was on Tuesdays. It started at 2:15 and lasted one hour and a half. After that we had tea. Most of the talks were given by Grothendieck. Usually, he had pre-notes prepared over the summer or before, and he would give them to the potential speakers. Among his many students he distributed the exposés, and also he asked his students to write down notes. The first time I saw him I was scared. It was in '64. I had been introduced to him through Cartan, who said, *For what you're doing, you should meet Grothendieck.* I was indeed looking for an Atiyah-Singer index formula in a relative situation. A relative situation is of course in Grothendieck's style, so Cartan immediately saw the point. I was doing something with Hilbert bundles, complexes of Hilbert bundles with finite cohomology, and he said, *It reminds me of something done by Grothendieck, you should discuss with him.* I was introduced to him by the Chinese mathematician Shih Weishu. He was in Princeton at the time of the Cartan-Schwartz seminar on the Atiyah-Singer formula; there had been a parallel seminar, directed by Palais. We had worked together a little bit on some characteristic classes. And then he visited the IHES. He was friendly with Grothendieck and proposed to introduce me. So, one day at two o'clock I went to meet Grothendieck at the IHES, at his office, which is now, I think, one of the offices of the secretaries. The meeting was in the sitting room which was adjacent to it. I tried to explain what I was doing. Then Grothendieck abruptly showed me some naive commutative diagram and said, *It's not leading anywhere. Let me explain to you some ideas I have.* Then he made a long speech about finiteness conditions in derived categories. I didn't know anything about derived categories ! *It's not complexes of Hilbert bundles you should consider. Instead, your should work with ringed spaces and pseudocoherent complexes of finite tor-dimension* ... (laughter) ... It looked very complicated. But what he explained to me then eventually proved useful in defining what I wanted. I took notes, but couldn't understand much.

I knew no algebraic geometry at the time. Yet he said *In the fall I am starting a seminar, a continuation of SGA 4*, which was not called "SGA 4," it was "SGAA," the "Séminaire de géométrie algébrique avec Artin." *It will be on local duality. Next year we will reach ℓ-adic cohomology, trace formulas, L-functions.* I said *Well, I will attend, but I don't know if I'll be able to follow.* He said *But in fact I want you to write down the notes of the first exposé.* However, he gave me no pre-notes. I went to the first talk. He spoke with great energy at the board but taking care to recall all the necessary material. He was very precise.The presentation was so neat that even I, who knew nothing of the topic, could understand the formal structure. It was going fast but so clearly that I could take notes. He started by briefly recalling global duality, the formalism of $f^!$ and $f_!$. By that time, I had learned a little bit of the language of derived categories, so I was not so afraid of distinguished triangles and things like that. Then he moved to dualizing complexes, which was much harder. After a month, I wrote down notes. I was very anxious when I gave them to him. They were about fifty pages. For Grothendieck it was a reasonable length. Once, Houzel, who had been my teaching assistant at the École normale, at the end of the seminar, said to Grothendieck : *I have written* *something I'd like to give you.* It was something on analytic geometry, about ten pages. Grothendieck said *when you have written fifty pages, then come back* ... (laughter) ... Anyway, the length was reasonable, but I was still very anxious. One reason is that, meanwhile, I had written some notes about my idea on complexes of Hilbert bundles. I had a final version which seemed to me to be good. Grothendieck said *maybe I'll have a look at that.* So I gave them to him. Not too long afterwards, Grothendieck came to me and said *I have a few comments on your text. Could you please come to my place, I will explain them to you.*

At Grothendieck's place

When I met him, to my surprise, my text was blackened with pencilled annotations. I thought it was in final form, but everything had to be changed. In fact, he was right all the time, even for questions of French language. He proposed modifications in the style, the organization, everything. So, for my exposé on local duality I was very afraid. However, a month later or so, he said *I've read your notes, they are OK but I have a few comments, so could you please come to my place again ?*. That was the beginning of a series of visits to his place. At the time he lived at Bures-sur-Yvette, rue de Moulon, in a little white pavilion, with a ground floor and one storey. His office there was austere, and cold in the winter. He had a portrait of his father in pencil, and also on the table there was the mortuary mask of his mother. Behind his desk he had filing cabinets. When he wanted some document, he would just turn back, and find it in no time. He was well organized. We sat together and discussed his remarks on my redactions. We started at two and worked until maybe four o'clock, then he said *Maybe we could take some break.* Sometimes we took a walk, sometimes we had tea. After that we came back and worked again. Then we had dinner around seven, with his wife, his daughter and his two sons. The dinner didn't last long. Afterwards we met again in his office, and he liked to explained some maths to me. I remember, one day, he gave me a course on the theory of the fundamental group, from several viewpoints, the topological approach, the scheme-theoretic one (with the enlarged fundamental group of SGA 3), the topos-theoretic one. I tried to catch up, but it was hard. He was improvising, in his fast and elegant handwriting. He said that he couldn't think without writing. I, myself, would find it more convenient first to close my eyes and think, or maybe just lie down, but he could not think this way, he had to take a sheet of paper, and he started writing. He wrote $X \to S$, passing the pen several times on it, you see, until the characters and arrow became very thick. He somehow enjoyed the sight of these objects. We usually finished at half past eleven, then he walked with me to the station and I took the last train back to Paris. All afternoons at his place were like that.

Walks in the woods

Among the people coming to the seminar I remember Berthelot, Cartier, Demazure, Dieudonné, Giraud, Jouanolou, Néron, Poitou, Raynaud and his wife, Samuel, Serre, Verdier. Of course we also had foreign visitors, some for long periods (Tits, Deligne, who attended the seminars since 1965, Tate, and later Kleiman, Katz, Quillen, ...). Then we had tea at four in the drawing room of the IHES. That was a place to meet and discuss. Another one was the lunch at the IHES, to which I decided to come after some time. There you could find Grothendieck, Serre, Tate discussing about motives and other topics which passed well over my head. SGA 6, the seminar on Riemann-Roch, started in '66. A little before, Grothendieck said to Berthelot and me *You should give the talks.* He handed me some pre-notes on finiteness conditions in derived categories and on K-groups. So Berthelot and I gave several talks, and we wrote down notes. In this time, we usually met for lunch, and after lunch—that was very nice—Grothendieck would take us for a walk in the woods of the IHES, and just casually explain to us what he had been thinking about, what he'd been reading. I remember, once he said *I'm reading Manin's paper on formal groups* [1] *and I think I understand what he's doing, I think one should introduce the notion of slope, and Newton polygon*, then he explained to us the idea that the Newton polygon should rise under specialization, and for the first time he envisioned the notion of crystal. Then at the same time, maybe, or a little later, he wrote his famous letter to Tate: " ... Un cristal possède deux propriétés caractéristiques : la rigidité, et la faculté de croître, dans un voisinage approprié. Il y a des cristaux de toute espèce de substance : des cristaux de soude, de soufre, de modules, d'anneaux, de

[1] Yu. I. Manin, Theory of commutative formal groups over fields of finite characteristic, Uspehi Mat. Nauk **18** (1963), no. 6 (114), 3–90. (Russian.)

schémas relatifs, etc." "A crystal possesses two characteristic properties : rigidity, and the ability to grow in an appropriate neighborhood. There are crystals of all kinds of substances : sodium, sulfur, modules, rings, relative schemes, etc."

Künneth

—(Bloch) What about you? What about your part? You must have been thinking about your thesis.

—(Illusie) It was not working so well, I must say. Grothendieck had proposed to me some problem, of course. He said *The second part of EGA III is really lousy, there are a dozen spectral sequences abutting to the cohomology of a fiber product, it's a mess so, please, clean this up by introducing derived categories, write the Künneth formula in the general framework of derived categories.* I thought about that and was fairly rapidly stuck. Of course, I could write some formula, but only in the tor-independent situation. I'm not sure that there is even now in the literature a nice general formula in the non-tor-independent situation [2]). For this you need homotopical algebra. You have two rings and you have to take the derived tensor product of the rings, what you get is an object in the derived category of simplicial rings, or you can view it as a differential graded algebra in the characteristic 0 case, but the material was not available at the time. In the tor-independent case, the usual tensor product is good. In the general one I was stuck.

SGA 6

I was therefore happy to work with Grothendieck and Berthelot on SGA 6. At the time you didn't have to finish your thesis in three years. The completion of a thèse d'État could take seven, eight years. So the pressure was not so great. The seminar, SGA 6, went well, we eventually proved a Riemann-Roch theorem in a quite general context, and Berthelot and I were quite happy. I remember that we tried to imitate Grothendieck's style. When Grothendieck handed me his notes on the finiteness conditions in derived categories, I said *This is only over a point. We should do that in a fibered category over some topos ...* (laughter) It was a little naïve, but, anyway, it proved to be the right generalization.

—(Drinfeld) What is written in the final version of SGA 6? Is it in this generality?

—(Illusie) Yes, of course.

—(Drinfeld) So, it was your suggestion, not Grothendieck's.

—(Illusie) Yes.

—(Drinfeld) Did he approve it?

—(Illusie) Of course, he liked it. As for Berthelot, he brought original contributions to the K-theory part. Grothendieck had calculated the K^0 of a projective bundle. We did not call it "K^0" at the time, there were a $K^\bullet$ made with vector bundles and a $K_\bullet$ made with coherent sheaves, which are now denoted K^0 and K'^0. Grothendieck had proved that the K^0 of a projective bundle P over X is generated over $K^0(X)$ by the class of $\mathcal{O}_P(1)$. But he was not happy with that. He said *Sometimes you're not in a quasi-projective situation, you don't have any global resolutions for coherent sheave. Then it's better to work with the K-group defined using perfect complexes.* However, he didn't know how to prove the similar result for this other K group. Berthelot thought about the problem, and, adapting to complexes some constructions of Proj made in EGA II for modules, he solved it. He showed that to Grothendieck and then Grothendieck told me, *"Berthelot est encore plus fonctorisé que moi !"* [3] ... (laughter). Grothendieck had given us detailed notes on lambda operations, which he had written before 60. Berthelot discussed them in his exposés, and solved several questions that Grothendieck had not thought about at the time.

—(Bloch) Why did you choose this topic? There was this earlier paper, by Borel and Serre, based on Grothendieck's ideas about Riemann-Roch. I'm sure he wasn't happy with that!

—(Illusie) Grothendieck wanted a relative formula over a general base and for fairly general morphisms (locally complete intersection morphisms). Also, he didn't want to move cycles. He preferred to do intersection theory using K-groups.

—(Bloch) But he didn't forget his program of trying to prove the Weil conjectures?

SGA 7

—(Illusie) No, but he had several irons in the fire. In '67–'68 and '68–'69, there was another seminar, SGA 7, about monodromy, vanishing cycles, the $R\Psi$ and $R\Phi$ functors, cycle classes, Lefschetz pencils. Certainly

[2]This issue is discussed again in the section under the heading **Cartier, Quillen**

[3]Berthelot is still more functorized than I am!"

he had already thought about the formalism of nearby cycles a few years before. Also, he had read Milnor's book on singularities of hypersurfaces. Milnor had calculated some examples, and observed that for these all the eigenvalues of the monodromy of the cohomology of what we now call the Milnor fiber of an isolated singularity are roots of unity. Milnor conjectured that that was always the case, that the action was quasi-unipotent. Then Grothendieck said *What are the tools at our disposal ? Hironaka's resolution. But then you leave the world of isolated singularities, you can no longer take Milnor fibres, you need a suitable global object.* Then he realized that the complex of vanishing cycles that he had defined was what he wanted. Using resolution of singularities, he calculated, in the case of quasi-semistable reduction (with some multiplicities), the vanishing cycles, and then the solution came out quite easily in characteristic zero. He also obtained an arithmetic proof in the general case : he found this marvelous argument, showing that when the residue field of your local field is not so big, in the sense that no finite extension of it contains all roots of unity of order a power of ℓ, then ℓ-adic representations are quasi-unipotent. He decided to make a seminar on that, and that was this magnificent seminar, SGA 7. It's in it that Deligne gave his beautiful exposés on the Picard-Lefschetz formula (at the request of Grothendieck, who couldn't understand Lefschez's arguments), and Katz his marvelous lectures on Lefschetz pencils.

Cotangent complex and deformations

However my thesis was still empty, I had just attended SGA 7, written up no notes. I had given up long ago this question on Künneth formulas. I had published a little paper in *Topology* on finite group actions and Chern numbers, but that was not much. One day, Grothendieck came to me, and said *I have a few questions for you on deformations.* So we met some afternoon, and he proposed several problems on deformations with similar answers: deformations of modules, groups, schemes, morphisms of schemes, etc. Every time the answer involved an object he had recently constructed, the cotangent complex. In his work with Dieudonné in EGA IV, there appears a differential invariant of a morphism, called the *module of imperfection.* Grothendieck realized that Ω^1 and the module of imperfection were in fact the cohomology objects of a finer invariant in the derived category, a complex of length one, which he called the cotangent complex. He wrote this up in his Lecture Notes, *Catégories cofibrées additives et complexe cotangent relatif* (SLN 79). Grothendieck observed that to get to the obstructions, which involved H^2 groups, his theory was probably insufficient, because a composition of morphisms didn't give rise to a nice distinguished triangle for his cotangent complexes. It happens that at the same time, independently, Quillen had been working on homotopical algebra and, a little later, had constructed, in the affine case, a chain complex of infinite length, which had Grothendieck's complex as a truncation, and which behaved well with respect to composition of morphisms. Independently, too, André had defined similar invariants. I got interested in their work and realized that in André's construction, the classical lemma of Whitehead which played a key role, could easily be sheafified. In a few months, I obtained the main results of my thesis, except for deformation of group schemes, which came much later (the commutative ones required much more work).

After May '68

In May '68 Grothendieck was seduced by the leftist ideology. He admired *Mao's thought*, and the Cultural revolution. He had also started thinking about other topics : physics (he told me he had been reading books by Feynmann), then biology (especially embryology). I have the impression that from that time, mathematics were slowly drifting away from his main focus of interest, though he was still very active (e. g. the second part of SGA 7 was in '68–'69). He had contemplated giving a seminar on abelian schemes after that, but finally decided to go on studying Dieudonné's theory for p-divisible groups, in the continuation of his work on crystalline cohomology. His lectures on this (in '66) had been written up by Coates and Jussila, and he let Berthelot develop a full fledged theory. One can regret he didn't give a seminar on abelian schemes. I'm sure it would have produced a beautiful, unified presentation of the theory, much better than the scattered references we can find in the literature. In 1970 he left the IHES and founded the ecological group *Survivre et Vivre.* At the Nice congress, he was doing propaganda for it, offering documents taken out of a small cardboard suitcase. He was gradually considering mathematics as not being worth of being studied, in view of the more urgent problems of the survival of the human species. He carelessly dispatched around him many of his documents (papers, private notes, etc.). Yet, in '70–'71 he gave a beautiful course (together with a seminar) at the Collège de France on Barsotti-Tate groups, and lectured later in Montreal on the same topic.

Working with Grothendieck

Many people were afraid of discussing with Grothendieck, but, in fact, it was not so difficult. For example, I could call him anytime, provided that it was not before noon, because he would get up at that time. He worked late in the night. I could ask him any question, and he would very kindly explain to me what he knew about the problem. Sometimes, he had afterthoughts. He would then write me a letter with some complements. He was very friendly with me. But some students were not so happy. I remember Lucile Bégueri (now Georges Poitou's widow), who had asked for a topic for her thesis from Grothendieck. It was a bit like with my Künneth formula. I think he proposed to her to write down the theory of coherent morphisms for toposes, finiteness conditions in toposes. That was hard, things didn't go well, and she eventually decided to stop working with him. He was more successful with Mme Raynaud, who produced a beautiful thesis.

I said that when I handed him some notes, he would correct them heavily and suggest many modifications. I liked it because his remarks were almost always quite up too the point, and I was happy to improve my writing. But some didn't like it, some thought that what they had written was good, and there was no need to improve it. Grothendieck gave a series of lectures on motives at the IHES. One part was about the standard conjectures. He asked John Coates to write down notes. Coates did it, but the same thing happened: they were returned to him with many corrections. Coates was discouraged and quit. Eventually, it's Kleiman who wrote down the notes in *Dix exposés sur la cohomologie des schémas.*

—(Drinfeld) But it's not so good for many people, giving a thesis on coherent morphisms of toposes, it's bad for most students.

—(Illusie) I think these were good topics for Grothendieck himself.

—(Drinfeld) Yes, sure.

—(Illusie) But not for students. Similarly with Monique Hakim, *Relative schemes over toposes*, I am afraid this book was not such a success.

—(Unknown) But the logicians like it very much.

—(Illusie) I heard from Deligne that there were problems in some parts. Anyway, she was not so happy with this topic, and she did quite different mathematics afterwards. I think that Raynaud also didn't like the topic that Grothendieck had given him. But he found another one by himself (*Ample line bundles on homogeneous spaces*). That impressed Grothendieck, as well as the fact that Raynaud was able to understand Néron's construction of Néron models. Grothendieck of course had quite brilliantly used the universal property of Néron models in his exposés in SGA 7, but he could not grasp Néron's construction.

Verdier

For Verdier it's a different story. I remember Grothendieck had a great admiration for Verdier. He admired what we now call the Lefschetz-Verdier trace formula, and Verdier's idea of defining $f^!$ first as a formal adjoint, and then calculating it later.

—(Bloch) I thought, maybe, that was Deligne's idea.

—(Illusie) No, it was Verdier's. But Deligne in the context of coherent sheaves used this idea afterwards. Deligne was happy to somehow kill three hundred pages of Hartshorne's seminar in eighteen pages. (laughter)

—(Drinfeld) Which pages do you mean?

—(Illusie) In the appendix to Hartshorne's seminar *Residues and duality.* I say "Hartshorne's seminar," but in fact it was Grothendieck's seminar. Pre-notes had been written up by Grothendieck. Hartshorne gave the seminar from these. Coming back to Verdier, who had written such a nice *Fascicule de résultats* on triangulated and derived categories, one can ask why he did not embark on writing a full account. In the late '60s and early '70s, Verdier got interested in other things, analytic geometry, differential equations, etc. When Verdier died in '89, I gave a talk on his work, at a celebration for him in his memory, and I had to understand this issue, *Why didn't he publish his thesis?* He had written some summary, but not a full text. Probably one of the main reasons is simply that in the redaction of his manuscript he had not yet reached derived functors. He had discussed triangulated categories, the formalism of derived categories, the formalism of localization, but not yet derived functors. At the time he was already too busy with other things. And he did not want to publish a book on derived categories without derived functors. It's certainly a pity.

—(Drinfeld) And the Astérisque volume, how much does it correspond to?

—(Illusie) It corresponds to what Verdier had written, up to derived functors. So it's quite useful, I think, but for derived functors, you have to look at other places.

Filtered derived categories

—(Drinfeld) Did the notion of differential graded category ever appear in Verdier's work? Another potential source of dissatisfaction with derived categories was that the cones were defined only up to isomorphism; there are many natural constructions which do not work naturally in derived categories as defined by Verdier. Then you need differential graded categories or go to "stable categories," but these formally have been developed only recently. In hindsight, the idea of the differential graded category seems very natural. Did you have this idea in the discussion of the derived category?

—(Illusie) Quillen found that differential graded algebras would give you a similar but in general inequivalent category to the derived category defined by simplicial algebras, but this was done in the late '60s or early '70s and did not appear in discussions with Grothendieck. However, I know the story about the filtered derived category. Grothendieck thought that if you have an endomorphism of a triangle of perfect complexes, then the trace of the middle part should be the sum of the traces of the right-hand side and the left-hand side. In SGA 5, when he discussed traces, he explained that on the board. One of the persons attending the seminar was Daniel Ferrand. At the time, nobody saw any problem with that, it was so natural. But then Grothendieck gave Ferrand the task of writing the construction of the determinant of a perfect complex. This is a higher invariant than the trace. Ferrand was stuck at one point. When he looked at the weaker version, he realized that he could not show that the trace of the middle part was the sum of the two extremes, and then he built a simple counterexample. The problem was : *How can we restore that?* The person who at the time could repair anything that went wrong was Deligne. So, we asked Deligne. Deligne came up with the construction of a category of *true triangles*, finer than usual triangles, obtained by a certain process of localization, from pairs of a complex and a subcomplex. In my thesis I wanted to define Chern classes, using an Atiyah extension. I needed some additivity of Chern classes, hence additivity of traces, and algebraic complements; I also needed tensor products, which increase lengths of filtrations. So I thought : why not just take filtered objects and localize with respect to maps inducing quasi-isomorphisms on the associated graded objects. It was very natural. So I wrote it up in my thesis, and everybody was happy. At the time, only finite filtrations were considered.

—(Drinfeld) So it is written in your Springer Lecture Notes volumes on cotangent complex and deformations?

—(Illusie) Yes, in SLN 239, Chap. V. Deligne's category of true triangles was just $DF^{[0,1]}$, the filtered derived category with filtrations of length 1. That was the beginning of the theory. However Grothendieck said *In triangulated categories we have the octahedron axiom, what will replace that in filtered derived categories ?* Maybe the situation is not yet fully understood today. Once, Grothendieck told me, it must have been in '69 : *We have the K-groups defined by vector bundles, but we could take vector bundles with a filtration of length one (with quotient a vector bundle), vector bundles with filtrations of length 2, length n, with associated graded still vector bundles ... Then you have operations such as forgetting a step of the filtration, or taking a quotient by one step. This way you get some simplicial structure which should deserve to be studied and could yield interesting homotopy invariants.* Independently, Quillen had worked out the Q-construction, which is a substitute for the filtration approach. But, I think, if Grothendieck had had more time to think about it, he would have defined the higher K-groups.

—(Drinfeld) But this approach looks more like Waldhausen's one.

—(Illusie) Yes, of course.

—(Drinfeld) Which appeared much later.

—(Illusie) Yes.

Cartier, Quillen

—(Drinfeld) During the SGA 6 seminar, was it known that the λ-operations have something to do with Witt rings?

—(Illusie) Yes. In fact, I think that G. M. Bergman's appendix to Mumford's book on surfaces[4] was already available at the moment.

—(Drinfeld) Are there λ-operations in this appendix?

—(Illusie) No, but I gave a talk in Bures on universal Witt rings and lambda operations. I remember I was going to the Arbeitstagung in Bonn. Having missed the night train I took an early morning train. Surprise : Serre and I were in the same compartment. I told him about the talk I had to prepare, and he generously helped me. During the whole trip, he improvised in a brilliant way, explaining to me several beautiful formulas, involving the Artin-Hasse exponential and other miracles of Witt vectors. This was discussed in the seminar. I wonder, Cartier theory should have existed at the time. *Tapis de Cartier*, I think, existed.

—(Drinfeld) What is *Tapis de Cartier*?

—(Illusie) *Tapis de Cartier* was Cartier's theory of formal groups. *Tapis* (= rug) was an expression used by some Bourbaki members (advocating for a theory being compared to selling rugs).

—(Bloch) But still, if you look back, Cartier made a lot of contributions.

—(Illusie) Yes, but I don't think that Grothendieck used much of the Cartier stuff. On the other hand, Grothendieck was very impressed by Quillen, who had brilliant new ideas on many topics. About the cotangent complex, I don't remember well now, but Quillen had a way of calculating the Ext^i of the cotangent complex and $\mathcal{O}$ as the cohomology of the structural sheaf of a certain site, which looked like the crystalline site, but with the arrows reversed. That surprised Grothendieck.

—(Unknown) Apparently, this idea was rediscovered later by Gaitsgory. [5]

—(Bloch) In Quillen's notes on the cotangent complex it was the first time I'd ever seen a derived tensor product *over* a derived tensor product.

—(Illusie) Yes, in the relation between the (der!ved) self-intersection complex and the cotangent complex.

—(Bloch) It was sort of $A \otimes^L_{B \otimes^L_{C \otimes^L D}} E$ ... I remember studying for days, puzzling over exactly what that meant.

—(Illusie) But when I said I couldn't do my Künneth formula, one reason was that such an object didn't exist at the time.

—(Drinfeld) I am afraid that even now it doesn't exist in the literature (although it may exist in somebody's head). I needed the derived tensor product of algebras over a ring a few years ago when I worked on the article on DG categories. I was unable either to find this notion in the literature or to define it neatly. So I had to write something pretty ugly.

Grothendieck's tastes

—(Illusie) I realize I didn't say much about Grothendieck's tastes. For example, do you know the piece of music he would like most?

—(Bloch) Did he like music at all?

—(Illusie) Grothendieck had a very strong feeling for music. He liked Bach and his most beloved pieces were the last quartets by Beethoven.

Also, do you know what his favorite tree was? He liked nature, and there was one tree he liked more than the others. It was the olive tree, a modest tree, but which lives long, is very sturdy, is full of sun and life. He was very fond of the olive tree.

In fact, he always liked the south very much, long before he went to Montpellier. He had been a member of the Bourbaki group, and he had visited *La Messuguière*, where some congresses were held. He tried to make me come to that place, but it didn't work. It is a beautiful estate on the heights above Cannes. You have Grasse a little higher, and still a little higher you have a small village called Cabris, where there is this estate, with eucalyptus trees, olive trees, pine trees, and a magnificent view. He liked it very much. He had a fancy for this sort of landscape.

—(Drinfeld?) Do you know what Grothendieck's favorite books were? You mentioned his favorite music...

—(Illusie) I don't remember. I think he didn't read much. There are only twenty-four hours in a day ...

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[4]D. Mumford, Lectures on curves on an algebraic surface. With a section by G. M. Bergman. Annals of Mathematics Studies, No. 59, Princeton University Press, Princeton, N.J. 1966.

[5]Gaitsgory, D. Grothendieck topologies and deformation theory. II. Compositio Math. 106 (1997), no. 3, 321–348.

Automorphic forms, stable homotopy, anabelian geometry

In retrospect, I find it strange that representation theory and automorphic forms theory were progressing well in the '60s, but, somehow ignored in Bures-sur-Yvette. Grothendieck knew algebraic groups extremely well.

—(Bloch) Well, as you said, there are only twenty-four hours in a day.

—(Illusie) Yes, but he might have constructed the ℓ-adic representations associated with modular forms like Deligne did, but he didn't. He really was very interested in arithmetic, but maybe the computational aspect of it was not so appealing to him. I don't know.

He liked putting different pieces of mathematics together : geometry, analysis, topology, ... so automorphic forms should have appealed to him. But for some reason he didn't get interested in that at the time. I think the junction between Grothendieck and Langlands was realized only in '71 at Antwerp. Serre had given a course on Weil's theorem in '67–'68. But after '68 Grothendieck had other interests. And before '67 things were not ripe. I'm not sure.

—(Beilinson) What about stable homotopy theory?

—(Illusie) Of course Grothendieck was interested in loop spaces, iterated loop spaces. n-categories, n-stacks were at the back of his mind, but he didn't work it out at the time.

—(Beilinson) When did it actually come about? Picard category is probably about '66.

—(Illusie) Yes, it was related to what he did with the cotangent complex. He conceived the notion of Picard category at that time, and then Deligne sheafified it into Picard stacks.

—(Beilinson) And higher stacks ... ?

—(Illusie) He had thought about the problem, but it's only long afterwards that he wrote his manuscript *Pursuing stacks*. Also, $\pi_1(\mathbb{P}^1 - \{0, 1, \infty\})$ was always at the back of his mind. He was fascinated by the Galois action, and I remember once he had thought about possible connections with that and Fermat's problem. Already in the '60s he had some ideas about anabelian geometry.

Motives

I regret that he was not allowed to speak on motives at the Bourbaki seminar. He asked for six or seven exposés, and the organizers considered it was too much.

—(Bloch) It was kind of unique then; nobody else was lecturing on their own work.

—(Illusie) Yes, but you see, FGA (Fondements de la Géométrie Algébrique) consists of several exposés. He was thinking of doing for motives what he had done for the Picard scheme, the Hilbert scheme, etc. There are also three exposés on the Brauer group which are important and useful, but seven exposés on motives would have been even more interesting. However, I don't think they would have contained things which have not been worked out by now.

Weil and Grothendieck

—(Bloch) I once asked Weil about 19th century number theory and whether he thought that there were any ideas there that had not yet been worked out. He said, 'No.' (*laughter*)

—(Illusie) I discussed with Serre what he thought were the respective merits of Weil and Grothendieck. Serre places Weil higher. But though Weil's contributions are fantastic, I myself think Grothendieck's work is still greater.

—(Drinfeld) But it was Weil who revived the theory of modular forms in his famous article[6]. Probably Grothendieck couldn't have done it.

—(Illusie) Yes, this is certainly a great contribution. As for Weil's books, *Foundations of algebraic geometry*, is hard to read. Serre the other day told me that Weil was unable to prove theorem A for affine varieties in his language. And even Weil's book on Kähler varieties [7], I find it a little heavy.

—(Bloch) That book in particular was very influential.

Grothendieck's style

—(Illusie) Yes, but I'm not so fond of Weil's style. Grothendieck's style had some defects also. One that was barely perceptible at the beginning and became enormous later is his habit of afterthoughts and

[6]Über die Bestimmung Dirichletscher Reihen durch Funktionalgleichungen, Math. Ann. **168** (1967), 149–156.

[7]Introduction à l'étude des variétés kählériennes. Publications de l'Institut de Mathématique de l'Université de Nancago, VI. Actualités Sci. Ind. no. 1267. *Hermann, Paris, 1958.*

footnotes. *Récoltes et Semailles* is incredible in this respect. So many, so long footnotes! Already in his beautiful letter to Atiyah on de Rham cohomology there are many footnotes, which contain some of the most important things.

—(Bloch) Oh, I remember seeing photocopies, early photocopies, when photocopy machines didn't work all that well. He would type a letter, and then add hand-written comments which were *illegible.*

—(Illusie) Well, I was used to his handwriting, so I could understand.

—(Bloch) We would sit around and puzzle...

—(Illusie) To him no statement was ever the best one. He could always find something better, more general or more flexible. Working on a problem, he said he had to sleep with it for some time. He liked mechanisms that had oil in them. For this you had to do scales, exercises (like a pianist), consider special cases, functoriality. At the end you obtained a formalism amenable to dévissage.

I think one reason why Grothendieck, after Serre's talk at the Chevalley seminar in 1958, was confident that étale localization would give the correct H^i's is that once you had the correct cohomology of curves, then by fibration in curves and dévissage you should also reach the higher H^i's.

I think he was the first one to write a map vertically instead of from left to right.

—(Drinfeld) It was he who put the X over S. Before that X was on the left and S was on the right.

—(Illusie) Yes. He was thinking over a base. The base could be a scheme, a topos, anything. The base had no special properties. It's the relative situation that was important. That's why he wanted to get rid of Noetherian assumptions.

—(Bloch) And I remember, in the early days schemes, morphisms were separated, but then they became quasi-separated.

Commutative algebra

—(Illusie) At the time of Weil, you looked at fields, and then valuations, and then valuation rings, and normal rings. Rings were usually supposed to be normal. Grothendieck thought it was ridiculous to make such systematic restrictions from the beginning. When defining $Spec\, A$, A should be any commutative ring.

—(Drinfeld) Sorry, but how did people treat the nodal curve if the rings were supposed to be normal? Non-normal varieties appear...

—(Illusie) Of course, but they often looked at the normalization. Grothendieck was aware of the importance of normality, and I think Serre's criterion of normality was one of the motivations for his theory of depth and local cohomology.

—(Bloch) I wonder whether today such a style of mathematics could exist.

—(Illusie) Voevodsky's work is fairly general. Several people tried to imitate Grothendieck, but I'm afraid that what they did never reached that "oily" character dear to Grothendieck.

But it is not to say that Grothendieck was not happy to study objects having rich structures. As for EGA IV, it is of course a masterpiece of local algebra, a domain in which he was extremely strong. We owe a lot to EGA IV, though maybe some rewriting could be possible now, using the cotangent complex.

Relative statements

Certainly we're now so used to putting some problem into relative form, that we forget how revolutionary it was at the time. Hirzebruch's proof of Riemann-Roch is very complicated, while the proof of the relative version, Grothendieck-Riemann-Roch, is so easy, with the problem shifted to the case of an immersion. This was fantastic.

Grothendieck was the father of K-theory, certainly. But it was I think Serre's idea to look at χ. I think the people in the olden days, they had no idea of the right generalization of Riemann-Roch for curves. For surfaces, both sides of the formula were quite intricate. It's Serre who realized that the Euler-Poincar characteristic, the alternate sum of the dimensions of the $H^i(\mathscr{O})$ or the $H^i(E)$ was the invariant you should look for. That was in the early '50s. And then Grothendieck saw that the universal χ was in the K-group...

The Thèse d'État

—(Drinfeld) So when Grothendieck chose problems for his students he didn't care very much about the problem being solvable.

—(Illusie) Of course, he cared about the problem, and when he didn't know how to solve it, he left it to his students. The *thèses d'état* were like that...

—(Drinfeld) And how many years did it take to write the thesis? For example, how many years did you spend? You had to change the subject once or twice, and then in between you worked on SGA, which had nothing to do with the thesis. It was very helpful for humanity and very good practice for you, but it had nothing to do with your thesis. So how many years did you spend?

—(Illusie) I started working on the cotangent complex in the end of '67, and the whole thing was finished in two years, somehow.

—(Drinfeld) But before this, there were some attempts which were not so successful due to the nature of the problem. When did you begin working on your thesis? As far as I understand, even now the standard amount of time in the US is 5 years.

—(Illusie) In fact, I did it in two years, essentially. In '68 I sent a letter to Quillen sketching what I had done. He said : *It's fine.* And then I wrote up my thesis very quickly.

—(Drinfeld) Were you a graduate student before that (when you began attending Grothendieck's seminar)?

—(Illusie) I was at the CNRS.

—(Drinfeld) Oh, you were already...

—(Illusie) Yes, it was like paradise. You entered the Ecole normale ...

—(Drinfeld) Yes, sure, I understand.

—(Illusie) Then you worked reasonably well, so Cartan spotted you, saying, *Well, this student should go to the CNRS.* Once at the CNRS, you were there for the rest of your life. Which is not true. A position at the CNRS at that time was not permanent. But as I was not idle, my contract was renewed from year to year.

Of course, we were maybe fifteen people at the Ecole normale doing mathematics, and there were not that many positions at the CNRS. Others could get positions as "assistants", which were not so good as the CNRS, but still reasonable.

—(Drinfeld) And did somebody tell you from time to time that it is time to finish your thesis?

—(Illusie) Well, after seven years, it could become a problem. As I had started at the CNRS in '63, and had finished my thesis by '70, I was safe.

—(Drinfeld) And the fact that you spent seven years didn't diminish your chances for future employment?

—(Illusie) No. From '63 to '69 I was attaché de recherche, then, from '69 to '73, chargé de recherche, and promoted maître de recherche in '73 (the equivalent of directeur de deuxième classe today). Nowadays if a student after five years has not defended his thesis, it's a problem.

—(Drinfeld) What has changed...?

—(Illusie) The thèse d'État was suppressed, replaced by the standard thesis, following the American model.

—(Drinfeld) I see.

—(Illusie) Typically, a student has three years to finish his thesis. After three years, the fellowship ends, and he has to find a position somewhere, either a permanent one or a temporary one (like ATER = attaché d'enseignement et de recherche, or a post-doc).

For a few years we had a transitory system with the nouvelle thèse (new thesis), similar to the thesis we have now, followed by the thèse d'État. Now the thèse d'État is replaced by the habilitation. It's not the same kind of thing. It's a set of papers that you present at the defense. You need the habilitation for applying for a position of professor.

Grothendieck today

—(Lady) Maybe you told me, but where is Grothendieck now? Nobody knows?

—(Illusie) Maybe some people know. I myself don't know.

—(Bloch) If we were to go to Google and type in "Grothendieck"...

—(Illusie) We'd find the Grothendieck site.

—(Bloch) Yes, the website. He has a web topos ...

—(Lady) What happened to his son? Did he become a mathematician?

—(Illusie) I think one son studied at Harvard.

Allyn Jackson, who wrote this long article on Grothendieck in the Notices of the AMS, may know a little more, as well as a few other people (Cartier, or Leila Schneps, or Jean Malgoire, a former student of Grothendieck at Montpellier, for example).

EGA

—(Bloch) You can't tell a student now to go to EGA and learn algebraic geometry...

—(Illusie) Actually, students want to read EGA. They understand that for specific questions they have to go to this place, the only place where they can find a satisfactory answer. You have to give them the key to enter there, explain to them the basic language. And then they usually prefer EGA to other expository books. Of course, EGA or SGA are more like dictionaries than books you could read from A to Z.

—(Bloch) One thing that always drove me crazy about EGA was the excessive back referencing. I mean there would be a sentence and then a seven-digit number...

—(Illusie) No... You're exaggerating.

—(Bloch) You never knew whether behind the veiled curtain, was something very interesting that you should search back in a different volume to find; or whether in fact it was just referring to something that was completely obvious and you didn't need to...

—(Illusie) That was one principle of Grothendieck : every assertion should be justified, either by a reference or by a proof. Even a "trivial" one. He hated such phrases as "It's easy to see", "It's easily checked". When he was writing EGA, you see, he was in unknown territory. Though he had a clear general picture, it was easy to go astray. That's partly why he wanted a justification for everything. He also wanted that Dieudonné could understand!

—(Drinfeld) What was Dieudonné's contribution to the EGA ?

—(Illusie) He did re-writing, filling in details, adding complements, polishing the proofs. But already Grothendieck's first drafts (*Etat 000*), some of which I have seen, were already quite elaborate. Nowadays you have such efficient TeX systems, manuscripts look very nice. In Grothendieck's time the presentation was not so beautiful, maybe, but Dieudonné-Grothendieck's manuscripts were still fantastic.

I think Dieudonné's most important contribution was on the part of EGA IV dealing with differential calculus in positive characteristic, with complete local rings, which is basic in the theory of excellent rings.

Also, Grothendieck was not thrifty. He thought that some complements, even if they were not immediately useful, could prove important later, and therefore should not be removed. He wanted to see all the facets of a theory.

—(Unknown) When Grothendieck started working on EGA, did he already have a vision of what would come later, étale cohomology... Did he have in mind some applications?

—(Illusie) The plan he gives for EGA in the first edition of EGA I (in 1960) amply shows the vision he had at that time.