Interview of Luc Illusie by Ulf Persson, January 17, 2012

Meeting Grothendieck

Ulf Persson: Let us go to the heart of the matter. When was the first time you met Grothendieck.

Luc Illusie: You mean eye-to-eye?

UP: Whatever.

LI: It was during a class of Serre at the Collège de France, in his 1962-63 course on Galois cohomology. I was intrigued by someone in the audience who, in a soft voice, raised probing questions. At the end, I asked who it was. "Oh, but it's Grothendieck", I was told.

UP: You met him there?

LI: No. Later on I gave a talk at the Cartan-Schwartz seminar at the ÉNS[1] (Exposé 6: Caractére de Chern. Classe de Todd). Grothendieck was in the audience. I remember that he objected to the fact that I had defined the component of degree zero of the Chern character of a vector bundle as an integer, the rank of the bundle. "It is rather a locally constant function with integral values", he said, which was of course the right definition on general bases. I was struck by the fact that even on seemingly minor points Grothendieck paid so much attention to the accuracy and naturalness of the definitions.

UP: So you were not intimidated by his remarks?

LI: No, as they were constructive comments.

UP: So that was when you finally met him?

LI: Not really, it came about a little bit later. I told the story in another interview[2]. To make it short, let me say that Cartan had proposed to me, as a topic for a thesis, to prove a relative variant of the Atiyah-Singer formula. The so-called analytic index should then be an element of a K-group of the base, instead of an integer. To define it, I had tried several constructions involving complexes of Hilbert bundles, but was stuck. So Cartan encouraged me to consult Grothendieck. I visited him one afternoon at the IHÉS. He patiently listened to me and then gave me a very valuable advice ...

UP: ... which consisted in ?

LI: Working with sheaves instead of bundles. He made me see the flexibility they provided, and in particular, how they gave a natural framework to express the finiteness conditions I was looking for.[3]

UP: And you started to see Grothendieck regularly ?

[1] Séminaire Henri Cartan, 16e annéee (1963/64), dirigé par Henri Cartan et Laurent Schwartz, *Théorème d'Atiyah-Singer sur l'indice d'un opérateur elliptique*, W. A. Benjamin, inc., 1967.

[2] *Reminiscences of Grothendieck and his school*, Luc Illusie, with Alexander Beilinson, Spencer Bloch, Vladimir Drinfeld et al., Notices of the AMS, Vol. 57, no. 9, 1106-1115.

[3] Grothendieck's suggestion is carried out in Exposé II, Appendice II of [SGA 6, *Théorie des Intersections et Théorème de Riemann-Roch*, Séminaire de Géométrie Algébrique du Bois-Marie 1966-67, dirigé par P. Berthelot, A. Grothendieck, L. Illusie, Lecture notes in Mathematics 225, Springer-Verlag, 1971].

LI: Yes, from the fall of 1964. I had to learn a whole new language. Not just scheme theory, but also derived categories, sites, toposes, etc. It was forbidding in the beginning, there were so many complicated concepts and intimidating terminology.

UP: It seems to be a national characteristic of French mathematicians to do abstract things, the more abstract the better. If one would be a bit malicious one would compare them to fashionable French philosophers who have great followings but speak nothing but nonsense. But maliciousness apart there is a tendency for many of a mathematical mind to thrive on abstraction and obtuse language for its own sake.

LI: It was certainly true that some people - including myself - delighted in holding forth on a language for the happy few.

UP: There is of course no point in naming any names.

LI: Of course not. It is a human failing, to which I am no exception, as I have already admitted.

UP: But to Grothendieck this abstract language was not just a formal game to dazzle people with, it had been developed for definite purposes.

LI: Certainly. It was to serve the vision he had.

UP: I take it that it was a revolution in algebraic geometry that had no counterpart in any other field of mathematics.

LI: I believe so, too. And to me it opened up a whole, exciting new world.

UP: Many of the older people could not adopt the new language. I am thinking in particular about André Weil.

LI: True there were many of the old people who just did not make the transition. Yes, even younger ones, like Lang or Néron, did not make the transition.

UP: In the case of Weil he had himself laid new foundations for algebraic geometry ten years earlier, and I guess he was jealous of Grothendieck as he realized that his own efforts had been superseded.

LI: But he had been a pioneer. As to whether he was jealous or not, I can only speculate. He might have been put off with the slightly overbearing manner of Grothendieck in his youth, as once Cartan alluded to to me.

UP: Mathematics is a competitive subject, and as Hardy remarked, a Young Man's game. Grothendieck knew his worth from the start and was probably not shy of exhibiting it.

LI: But he had not to show off to his students for us to see the brilliancy of his technique and the depth of his thought.

UP: I guess that it was Weil who for the first time started to speak about various fields of definitions.

LI: Indeed he did, in particular, the concept of descent with respect to Galois extensions is due to him. But Grothendieck drew the ideas to their logical conclusions...

UP: ...that is in the very spirit of the mathematical temperament, to take lines of reasoning to their extremes. My wife sometimes faults me for doing so in everyday life....

LI: ...such as considering rings instead of just fields and including prime ideals in general and not only maximal ideals in the concept of a spectrum, to make things closed under pullback.

Thinking relative to a base á la Grothendieck

UP: I guess one of the trademarks of the Grothendieck theory was to relativize everything. Hirzebruch proved the Riemann-Roch formula for varieties, and then Grothendieck came up with a relative version, of which Hirzebruch's formula seemed a trivial case.

LI: Trivial only if you do not understand what is involved. Hirzebruch's formula is of course the special case where the base is reduced to a point. True, as you point out, relativization is indeed a key concept in Grothendieck's work. It is especially needed when you want to prove statements by induction on the dimension of a variety by fibering it over one of smaller dimension, using pencils or iterated fibrations into curves. In this respect, curves appear as the crucial geometric input, as somehow everything is built up from them.

UP: You make it sound very simple.

LI: That is deceptive. The real challenge is to find the appropriate relativization. Some conjectures on absolute cases so to speak remain unsolved partly because those relative statements have not so far been found. I'm thinking, for example, on certain problems of independence of ℓ in $\ell-$adic étale cohomology.

UP: How about 'dévissage'?

LI: Right, this is also a key strategy of Grothendieck, which goes closely together with relativization. May I illustrate this by an example, if you don't mind my being slightly technical for a moment ?

UP: No, I love examples.

LI: Thank you. So suppose k is an algebraically closed field, X/k a smooth, projective scheme, ℓ a prime number prime to the characteristic of k, and we want to prove that the étale cohomology groups $H^i(X, \mathrm{F}_\ell)$ are finite dimensional and zero for i big. You are with me?

UP: I guess it would be helpful to have a blackboard or at least a paper napkin.

LI: I have no blackboard, so forget about it, but maybe I could look for a napkin if you insist.

UP: Do not bother. I will close my eyes, listen very carefully, and concentrate.

LI: So let me continue. If X is a curve, this is fine, as we can use Tsen's theorem, the Kummer sequence, and the structure of the Jacobian.

UP: I guess you are right but I cannot see right away how to prove it even then.

LI: It is not a trivial exercise, but all the ingredients are there, and, as a matter of fact, the calculation of the étale cohomology of a curve with constant coefficients had been made, albeit in a different language, by Kawada and Tate, back in 1955. This was known to Grothendieck. However, in higher dimension, the problem looks *a priori* intractable.

UP: Most people would think that it looks intractable from the start.

LI: Maybe. But let us turn to Grothendieck, who comes to our rescue by simply remarking that this problem is not put into the right perspective, not formulated in the relevant generality. The hypotheses are too restrictive

UP: So it is like finding the appropriate formulation to make induction work.

LI: Of course. This is the general strategy, but useless unless you have some inkling on how to modify the formulation, the strategy is only so much hot air. It is here that Grothendieck shows his mettle. He points out

(a) we should allow singularities on X,

(b) instead of limiting ourselves to the constant sheaf $\mathbf{F}_\ell$, we should allow sheaves with singularities, that is constructible $\mathbf{F}_\ell$-sheaves,

and finally most importantly...

UP: ...I am all ears....

LI: (c) instead of considering the absolute cohomology groups $H^i(X, F)$,with coefficients in some constructible sheaf F, say, we should consider relative cohomology groups $R^i f_* F$ for $f : X \to Y$ proper, perhaps not necessarily smooth nor projective, and prove they are constructible, and zero for i large.

UP: Not so quick.

LI: Do you want me to repeat it?

UP: No need. I just need to digest what you have just said.

LI: Take your time.

UP: I think I get the gist, so go on, I can see that you are impatient.

LI: Good. As you no doubt realize this means that the base Spec k can then be safely forgotten, the initial result will just become a corollary for $Y =$ Spec k.

UP: This is just as with the case of Grothendiecks generalization of Hirzebruch's Riemann-Roch. I guess we are in Grothendieck land.

LI: Yes we are. This is the whole point. Yet at first sight, this looks like a formidable challenge. But nevertheless it turns out to be easier than the original problem, as this new problem is so to speak "amenable to dévissage".

UP: A kind of induction in other words.

LI: If you like, but I prefer not to enlarge the notion of induction too far. The rough idea is that f can be, more or less, factored into a succession of relative proper curves, and then we can use Leray spectral sequences to conclude everything by *bona fide* induction, once we have treated the case of relative dimension 1.

UP: I see.

LI: This shifts the problem to the study of cohomology "in families", understanding the $R^q g_* F$ for $g : X \to Y$ a proper relative curve, and in particular, understanding the stalks of these sheaves.

UP: I was never comfortable with those R^q's.

LI: As so much in mathematics it is a matter of habit. You pretty soon get used to them once you see them at work in their proper contexts. So should I continue?

UP: By all means.

LI: So the crucial question to ask: is it true that the stalk of $R^q g_* F$ at some geometric point y of Y is the cohomology of the fiber of g at y with value in the restriction of F?

UP: And?

LI: Supposing this is true, do these stalks vary nicely when g is projective smooth ? Another actor thus enters the picture, namely the proper base change property, and its corollary, specialization, which appears as a prerequisite. Once again a new problem emerges, which again will be amenable to dévissage, reduced to a specialization property of fundamental groups. So eventually, the crucial case of the cohomology of curves over an algebraically closed field with constant coefficients will appear only at the very end of the dévissage.

UP: This is impressive. This tale has a morale I take it.

LI: Very much so. There is often a misconception about Grothendieck's taste for "maximum generality". This taste was not gratuitous : he wanted enough flexibility in the statements in order to be able to prove theorems !

UP: I guess another key concept, namely that of functoriality, also should be brought forward as a unifying theme in his mathematical vision.

LI: Yes, especially his revolutionary way of viewing (and constructing) geometric objects as representing functors. As a matter of fact, this goes hand in hand with "thinking relative to a base", as many geometric properties of, say, schemes or morphisms of schemes, which are not so easy to express in the language of ringed spaces, become apparent on this functorial description.

Working with Grothendieck

UP: So how was it to work with Grothendieck?

LI: Well, I talked about this at length in the interview I mentioned before[4]. Let me just say that he was always patient and friendly with me. He never discouraged me of asking naive, trivial questions. I remember, once - it was at a very early stage of my working with him - I was learning the functorial language, I asked him why a functor is an equivalence if and only if it is fully faithful and essentially surjective, and he took the pain of giving a proof to me on the blackboard !

UP: So you saw him regularly, making appointments with him on, say, a weekly basis ?

LI: No, I saw him when he wanted that we discuss a redaction I had made. I had been assigned to write notes for some exposés of the SGA 5 seminar[5].

UP: That is good exercise.

LI: It is excellent. Because it makes you acquire culture and really learn things as you are forced to consider the nitty-gritty details. Usually I am a poor note-taker, but with Grothendieck it was different. He spoke so well and clearly that it was a delight to listen to him, and to write it all down - which was not a

[4] see footnote 2

[5] SGA 5, *Cohomologie ℓ-adique et Fonctions L*, Séminaire de Géométrie Algébrique du Bois-Marie 1965-66, dirigé par A. Grothendieck, Lecture Notes in Mathematics 589, Springer-Verlag, 1977.

simple affair. I told you that when I started working with Grothendieck, Henri Cartan was my thesis advisor, and I had been working with him for about a year, around his seminar on the Atiyah-Singer formula.

UP: Yes.

LI: I had written up a few exposés. Cartan was extremely demanding on the redaction. All statements had to be justified, and everything expressed in the simplest and most economical way possible. I was helped in these attempts by Adrien Douady, who had been my first "caiman"[6]. Douady, a member of Bourbaki, who was known to have examples and counter-examples up his sleeve for almost anything, was even stricter that Cartan in this respect. Still Cartan's and Douady's demands were somehow modest compared to Grothendieck's. I recalled in the other interview[7] the long afternoons we spent together discussing the innumerable comments he had made on my drafts .

UP: As I understand Grothendieck was not into concrete examples.

LI: That statement has to be nuanced. If you mean in not being a botanist you are right. He had no collectors mania. But he knew the strategic examples and whenever he had occasion to do so he tested everything against them to confirm his abstract intuition.

UP: His head in the sky but his feet firmly on the ground.

LI: This is a good way of putting it.

UP: To return to your editorship of SGA it was a very good way, I guess, of easing you into research. This is usually the hard thing that goes on between an advisor and a student, namely to give a good problem. Often it has to be done before the student really understands what it is about and consequently he or she is in a limbo, not knowing where to turn, more often than not facing a complete blank. That happened to me.

LI: Yes. As I noted, by actively taking notes and writing them up you acquired from the start a general culture and in the writing out the details of the talks you invariably got into snitches you needed to resolve, which could lead into other things. As you put it, easing into the subject. The drawback is that it is time-consuming. But in my days things were much more leisurely than now. You could take your time.

UP: There was a scholarly approach that is no longer present today.

LI: The students of today are in for such time-pressures. They have to complete their thesis in just three years. And that involves learning an awful lot of material, although for those people who succeed in completing a thesis in arithmetical geometry these days, do not really have too much trouble with the necessary prerequisites.

UP: There is a danger with this. It is like with modern civilization when you use all kinds of fancy gadgets the running of which is completely opaque to you. Thus it is often satisfying to do elementary things because you understand everything. It is like the difference between hiking up a mountain by foot or being whisked up by a 'funiculaire'. But I understand that if you want to become

[6]= alligator ; teaching assistant in ENS slang.

[7]see footnote 2

a successful professional mathematician nowadays you simply cannot pass up modern machinery.

LI: Well, certainly, you need to acquire the up-to-date techniques. But when the machinery is heavy, I think there's no harm, in a preliminary stage, to take for granted a few basic results. For example, when I learned étale cohomology, I first admitted the fundamental theorems of SGA 4[8] (proper base change, finiteness, comparison with Betti cohomology, local acyclicity of smooth maps, duality), and played with them formally. Only later on did I study their proofs, which is of course necessary to get a true understanding of the theory. But there are cases where doing this is not so essential. Grothendieck proved deep theorems on abelian varieties using the universal property of Néron models, whose construction he confessed he had not grasped.

UP: So what did you end up doing? Or did Grothendieck bother to formulate a thesis problem for you?

LI: Yes, he did. Cotangent complex and deformation theory, more precisely finding a common generalization of his construction of the truncated cotangent complex and that of Quillen in the affine case, and applying this to a bunch of specific global deformation problems, was indeed a magnificent topic, and I was lucky and happy to be able to work on it. However, it came rather late, at the beginning of 1968. Previously he had asked me rather technical questions with which I had not caught up, such as finding a derived category presentation of the Künneth formulas of EGA III 7[9] (this still remains to be done), or extending to the non noetherian case the proof of the finiteness theorem for higher direct images of coherent sheaves by proper maps, a problem that was solved by Kiehl in the late 60's by reduction to the noetherian case[10]. A method that Grothendieck did not like. He was dreaming of an argument "à la Cartan-Serre" (using a compact operator) for the finiteness theorem of the cohomology of a compact analytic space with values in a coherent sheaf. A dream that was made come true by Faltings much later on[11], using rigid geometry techniques.

UP: Grothendieck was never one for tricks I believe. He wanted proofs to be natural.

LI: Yes, he had a definite vision of how the program should develop, and proofs should comply to its spirit. In fact he was, as you know, even dissatisfied with the way Deligne proved the Weil-conjectures by by-passing the Standard Conjectures that he had formulated. He thought those should have been proved first and then everything should have followed. As it is, it might take time before those are being proved, if ever.

UP: Many times proofs are being treated as nuisances. That is particularly

[8] *Théorie des Topos et Cohomologie étale des Schémas*, Séminaire de Géométrie Algébrique du Bois Marie 1963-64, dirigé par M. Artin, A. Grothendieck, J.-L. Verdier, Lecture Notes in Mathematics 269, 270, 305, Springer-Verlag, 1973.

[9] *Éléments de Géométrie Algébrique*, par A. Grothendieck, rédigés avec la collaboration de J. Dieudonné, III, Étude cohomologique des faisceaux cohérents (Seconde Partie), Pub. Math. IHÉS 17, 1963.

[10] R. Kiehl, *Ein "Descente"-Lemma und Grothendiecks Projektionssatz fur nicht-noethersche Schemata*, Math. Annalen, 198 (1972), pp. 287-316.

[11] G. Faltings, *Finiteness of coherent cohomology for proper FPPF stacks*, Bonn, MPI 2002.

true when lectures are being given. Most people prefer to wave their hands at the blackboard, referring to it as simply getting the ideas across. And the audience is usually relieved by being absolved from being treated to the details. The idea is that a proof is often seen as merely a verification, and thus it is enough to refer to the fact that a verification has been effected and checked by the experts. Grothendieck was surely not of that opinion.

LI: He was not. He wanted everything to be rigorously proven. On the other hand, he was often reluctant to perform what he called "routine verifications", like checking diagram compatibilities (which in fact can turn out to be non trivial, or even false).

UP: Yes, this reminds me of what Oort once told me, with a mixture of pride and embarrassment, how he had spotted and rectified a mistake of Grothendieck dealing with a diagram that turned out no to be commutative.

LI: Yes, I told you, not all diagrams are commutative.

UP: I think that the problem has something to do with the way we normally present mathematics. First there is a clearly formulated theorem, then there is the proof. It gives the impression that the theorem is the important thing, the proof is a kind of an afterthought. You in fact are led to believe that the theorem is all you really need to know, that all the information is readily available from it. This encourages black-box thinking, which is of course very seductive if you are in a hurry. But it is not true. Theorems are not like axioms, in order to properly use them you need to know roughly at least why they are true. Perhaps one should instead start with combining a few natural ideas and see where they lead to, this is usually how mathematical discoveries are made. The point being that the results of those combination of ideas can be formulated in many different ways. Not only can a certain fact have different proofs, but the same proof can result in many different theorems. It is only when you know the ideas behind a theorem that you can use it effectively. Often what you need is perhaps not the theorem as it is exactly formulated but some variant of it.

LI: This is of course an admirable ambition, but can you really adhere to it consistently?

UP: I admit that I never made any attempts to understand the proof of Hironaka's resolution of singularities which was crucial to my thesis. I simply treated it as an axiom. Of course with the years I have had some experience in resolving singularities in specific cases and have thus acquired some modest intuition. But of course some theorems are actually canonical in their formulations and you can use them as points of leverage in a great variety of situations. As an elementary example one can cite the fundamental theorem of algebra to the effect that the complex numbers are algebraically closed. But even here the almost trivial proof using analytic functions is a gem you really could not do without, if for no other reasons than to illustrate the magic of elementary complex analysis.

LI: I would think that a more appropriate example than the fundamental theorem of algebra would be the use Grothendieck made of Néron models. He was able to treat those as black boxes, as I noted before. But, I have to admit that he was nevertheless happy when later Artin and Raynaud provided such a construction in the language of schemes !

UP: I have always vaguely thought that your thesis was on Crystalline Cohomology, But this only illustrates the old adage that you should never work on the topic of your thesis, the danger being that you will never outgrow it.

LI: This is probably true.

UP: This shows the importance to have acquired a wide culture before you write a thesis so you have something to fall back upon. This might be a problem for many people nowadays who are rushed into providing results on the cutting-line.

LI: I would not necessarily agree. The problem was about the same in my days, when you worked perhaps seven years on your thesis, as it is today when its completion takes only three years on an average. The "wide culture" you might have acquired is actually not of so much help. I myself remained unproductive for a few years after I had defended my thesis (in 1971). Other students of Grothendieck chose paths which were quite new to them. For example, Verdier worked on analytic geometry and Whitney stratifications, Giraud worked on resolution of singularities.

UP: You were a Normalien, which meant of course that at that time you had already made it. You would not have to worry about making a living, you belonged to the mandarin class.

LI: This is not true, and it is even less true today. It is true that the French system has evolved in the last fifty years. But even back then the status of being a 'normalien' did not guarantee a permanent job.

Early schooling

UP: Let's change topics. How was your schooling, before you entered the ÉNS[12] ?

LI: At the lycée, both in Nantes up to 1956 and then in Paris, until 1959, where I prepared for the entrance examination to the ÉNS, I had remarkable teachers. I especially remember the teachers of history, French, Latin, Old Greek I had in Nantes. They were such great speakers. The history teacher I had in 55-56 had the talent of a story teller, speaking without any notes, making us live the campaigns of Napoleon as if we had been watching a movie ! And when in a French class we studied a literary text, we did it in depth, for several weeks, sometimes more. In 1954-55, with our French teacher, Henri Lafay, we spent two months on Racine's *Britannicus* ! In fact there is actually a kind of PostScrip to this. As is rather natural you tend to lose contact with your high-school teachers, and I did loose contact with this exceptional teacher at the end of 1955, but, by a strange coincidence, on the occasion of a meeting with friends near Paris on September 25, 2011, I happened to see him again, shortly before he died. A very moving encounter.

UP: It must have been.You said you learnt Old Greek. Did you read Plato ?

[12] École normale supérieure, 45 rue d'Ulm, 75005 Paris.

LI: We studied several texts, in particular *Phedo*, this admirable dialogue in which Plato tells the story of Socrates's death. A great memory, also, is Thucycides. We studied parts of *The Peloponnesian War.* Our teacher excelled in showing us correspondences between Thucydides's analysis and the political problems of our times. I am sorry that I have forgotten all my Greek today.

UP: What about your math teachers ?

LI: In Nantes I was focusing mostly on humanities. My math teachers were good but did not fascinate me as those I just talked about. In the class of *mathématiques élémentaires* and in the *classes préparatoires* at the Lycée Louis-le-Grand in Paris, I had excellent ones. In 58-59, it was André Magnier. I owe him much for my admission to the ÉNS. Incidentally, André Magnier had met the young Grothendieck in 1948 in Montpellier and obtained a fellowship for him to study in Paris at the ÉNS[13]. And then, at the ÉNS, I discovered a whole new world of mathematics in the classes of Henri Cartan. Cartan had a natural authority, and the talent of immediately installing a dialogue between him and the students. His enthusiasm was contagious, making us see difficult, abstract new concepts as just a simple, amusing game. I remember his gestures on the stage, almost dancing at times in front of the blackboard to emphasize his point.

UP: When did you get interested in mathematics ? What about your parents ?

LI: They were both teachers in a lycée. My mother, actually, in mathematics, while my father taught history. When I was at the elementary school - I was perhaps 9 years old - sometimes I had trouble in solving problems involving one or two unknowns. My mother came to rescue, showing me how to call x or y the unknowns, set up equations with them, and eventually solve the problem. I discovered the power of algebra. It was so exciting that you could give names to these unknowns, manipulate them formally until the answer came, without any effort !

UP: What about Euclidian geometry ? It was my first introduction to real mathematics. I was enthralled by the power of thought it opened up.

LI: I was not so impressed at first. I was - with good reason - unsatisfied with the basic definitions. What is a point ? What is a line ? Why these axioms ? And the first "theorems" shown to me - like "equality criteria" for triangles had kind of experimental proofs which aroused my perplexity. But I eventually admitted those few rules, and I got fascinated by the deep results you could derive from them, like those gems in the geometry of triangles (Euler line, Euler circle, Simpson line, etc.).

UP: I suppose you were a math athlete at secondary school ?

LI: Not at all. I could generally solve problems, but I was rather slow. My real interest and involvement in mathematics came later.

Parents and war-time memories

[13]see http://www.math.jussieu.fr/ leila/grothendieckcircle/ikonikoff.pdf

UP: Having brought up your parents in the discussion, I am a bit curious. After all, I met them when you were at the IAS in Princeton, in the spring of 1982.

LI: I was not a member of the IAS at that time. Nick Katz had invited me for one month to the university, but thanks to his recommendation, I had obtained the permission to live in an apartment of the Institute and enjoy some privileges of the members. And, this is true, my parents were with me at the time."

UP: Your parents often travelled with you to conferences I was told.

LI: Yes, they got to see something of the world, and they enjoyed it. In particular that fall in 1982 we all went to Japan. I had been invited to talk in a Japan-French conference held at Tokyo and Kyoto from October 5 to 14, organized by Raynaud and Shioda. It was my first visit to Japan. My parents and I enjoyed it immensely. I made contacts with several Japanese algebraic geometers, which evolved into a lasting co-operation. For example, it is at that time that I first met Kazuya Kato.

UP: You took very good care of your parents. You must have been an only child.

LI: No, I had a brother who was ten years older. He had been a teacher of French at the lycée. He passed away in 2006. My father, who was born in 1905, died in 1986. My mother, who was born in 1901, died in 1997. In 1969 she had a stroke, which left her hemiplegic. She did not recover well, and I helped her during all those years afterwards. The remarkable thing is what changes she witnessed during her lifetime. Not only did she, as did my father, of course, experience the first World War, but even the time before that war. She remembered the streets of Paris with horse drawn carriages. She had vivid memories of the great flood of the Seine in Paris in 1910, with people boating in the streets downtown.

UP: Just as in the days of the late 19th century. And if you compare that with the little we have actually gone through. But you are ten years older than I, so you may have memories of the second world war.

LI: I do in fact, though I was such a little child, having been born in 1940. We lived in Savenay, a tiny village thirty kilometers north-west of Nantes, equally distant from St-Nazaire, an important harbor, where during the war the Germans operated a strategic submarine base. We lived in a house whose three quarters were occupied by the Germans. In the garden they had made an ammunition store. I liked to climb and dance on it, under a walnut tree.

UP: I guess anti-German feeling was actually more virulent after 1870 and up to and including the First World War than it was during the occupation.

LI: My parents were totally anti-German at the time. They admired de Gaulle. They secretly listened to the London Radio. Though the soldiers occupying our house were not Nazis, and some officers were highly educated and spoke excellent French, we avoided to talk to them. Like in Vercors's novel, *The Silence of the Sea*.

UP: Your father was in his thirties, so he must have been called up.

LI: Yes, he was. In May 1940, he was sent to the front, at Diemeringen in the

Vosges. He retreated after the Wehrmacht's breakthrough in the Ardennes, was made prisoner, escaped near Saintes in the south west of France, and eventually returned to Savenay in the late summer of 1940. Later he was approached by a Resistance network, but, perhaps thinking of his two children, he declined to join.

UP: How was everyday life ?

LI: Hard, though in the countryside certainly not so tough as in the cities. We had a rather wide garden, in which we bred chickens and rabbits. I remember eating freshly laid eggs. My parents were teaching at the local school. Lots of rumors, false most of the time, were circulating. Especially about the American landing. When eventually the landing took place, we prepared American flags for the arrival of the Allied Forces. But unfortunately they passed 15 kilometers east of Savenay, and continued their way to Nantes, which was liberated on August 12, 1944. The Germans kept the control of Savenay, as well as that of St Nazaire, where they had a stronghold with about 30000 troops.

UP: Were you yourself and your family ever in danger ?

LI: The allied air force regularly struck targets along the Loire river, between Nantes and St-Nazaire, near the big Donges refinery : bridges, port installations, warehouses, marshaling yards, ammunition stores, etc. The bombing usually occurred at night, and it was not so accurate as it is today. When we heard the planes come, we all rushed to the basement for shelter. Fortunately, Savenay was spared. Our situation became more risky towards the end of 1944. As I said, St-Nazaire was a so-called "pocket of resistance" of the Germans, as was Royan, more to the south, near Bordeaux. De Gaulle decided that these pockets should be re-taken by the FFL[14] in cooperation with the British and American forces. On January 5, 1945, a bombing over Royan resulted in a 1000 civilian casualties. My parents were afraid of a similar attack on St-Nazaire, which would have been much bloodier, because of the considerably stronger position of the Germans there. The Red Cross had negotiated the permission to create what we would now call a "humanitarian corridor", namely organize convoys to evacuate the civilians of the St-Nazaire pocket to Nantes, in the liberated area. My parents immediately seized this opportunity. So, one morning of January 1945, we took such a train. It was snowing. I was carrying a small suitcase in one hand and my teddy bear in the other. We entered a cattle truck, and lay on the straw. I found it exciting. It took us one day to cover the 30 kilometers from Savenay to Nantes, where we got a temporary accommodation at a friends' apartment. The decision of de Gaulle to "reduce" the resistance pockets, which he justifies in his memoirs[15], is controversial. From a mere strategic viewpoint it was certainly unnecessary, as the Allied Forces were already penetrating Germany. Like in a game of go, those pockets were "dead". As for the St-Nazaire pocket, no attempt was made to take it. Its surrender occurred on May 8, 1945, the same day as Germany capitulated.

[14] Forces Françaises Libres = Free French Forces

[15] "Il s'agissait d'en finir avec les enclaves où l'ennemi s'était retranché.[...] je n'admettais pas que des unités allemandes puissent, jusqu'à la fin, rester intactes sur le sol français et nous narguer derrière leurs remparts.", *Mémoires de guerre*, Plon 1989, p. 754.

UP: After the war, times might have been rather tough.

LI: Food was scarce, expensive, and of low quality. I remember the ration cards. Heating was problematic. And the winters of that time were cold. At home we had no refrigerator, no washing machine, everything had to be done by hand. But in 1947, with the advent of the Marshall plan, things took a better turn.

UP: And kept it that way.

LI: Looking back, I think that the amount and pace of material improvement after the war is really remarkable. Of course, at the time I found the pace rather slow. Nantes had suffered a terrible bombing by the Allied Forces in 1943, September 16 and 23, making around 1500 victims. The whole center of the city had been totally destroyed. Reconstruction took more than ten years.

Computers and modern gadgets

UP: I was born in 1950 so this was all in the past to me. When I heard about the war in childhood it seemed so incredibly long ago, as things invariably do that happen before you are born. In fact during my lifetime the real change that has occurred in daily living has been the advent of the personal computer. In fact this is the modern invention I would not like to live without.

LI: It is amazing how it has changed your life. You touch type of course?

UP: No in fact I use two fingers, but I am very fast with them, having had so much practice. Although I cannot at all visualize the key-board my fingers hit the right keys without me really having to look for them.

LI: I actually taught myself touch-typing. It took me two months to learn it. Cartan told me that my handwriting was so bad that I needed to type. With typewriting it was very hard to correct, now with computers it is so easy.

UP: So easy in fact that you become so sloppy.

LI: I think my typing speed has actually gone down due to the many mistakes I have started doing. But just think of e-mail and how that has changed your life. I learned it from Nick Katz, it was in 1985 I believe and he was on his annual visit to the IHÉS when he told me about it. To communicate directly by just typing on a computer, it seemed incredible at the time. Now of course the younger generation is into texting, and they do it just with their thumbs.

UP: That I have never learned. Doing it by the thumbs I mean. But with all the obvious advantages of computers there are also some real dangers. I am thinking of Kindle replacing books.

LI: You mean the inconvenience of reading on a screen?

UP: In fact it goes much deeper than that, because it will entail the abolishment of your personal library. People like us are very attached to our books and our collections of the same. In fact they constitute the main furniture of your home. Here you have a wall of books.

LI: That reminds me of a line of St-John Perse (in *Vents*) : "Les livres tristes, innombrables, sur leur tranche de craie pâle ..."

UP: What will a home be without books? You could as well stay in a hotel room, or camp out in an airport lounge.

LI: Actually, despite St-John Perse's quotation, I have to say that I take pleasure in being surrounded by books. From time to time, I pick one up from the shelf, open it at random, and read a few pages. I like to hold it in my hands, enjoy its particular smell.

UP: I have of course collected books in my library for over forty years. Most of them I have never read I must admit, which means that my library, my mini-universe, still holds untapped treasures. I probably would need another life just to read everything I have so far not sampled. Books become really a part of you. It is something very different from loading them down from internet. The very idea of a home will entirely dissolve. In fact dying is probably not the unmitigated disaster you thought of it as in your youth.

LI: True. And then there is the issue of sustained storage and retrieval. You remember those soft discs we initially used. What are they called again? Oh yes floppies. No one uses them anymore. In fact I'm afraid everything you have stored on them is lost. There are no longer any disc readers.

UP: My son told me many years ago that they were obsolete, and that they leaked data. The back-ups I for many years conscientiously kept, it is all gone now.

LI: Remember the old systems such as Chi-writer of the late 80's.

UP: I had almost forgotten. I wrote my first computer papers on that software.

LI: Those files are useless now. No one can read them.

UP: And all of this has just happened in a few years. A cuneiform tablet you can still make sense of three or four thousand years later. It is so self-contained. Maybe one should try and encode the entire virtual library into clay tablets fired to hold for millennia. But would there be enough man-power to do so, and would the earth supply enough storage space. And more to the point will civilization survive so long as to make the project even worthwhile?

LI: But computer technology develops so fast.

UP: But there surely has to come against a wall. After all the speed of light cannot be superseded, and atoms are of a finite size.

LI: True there are hardware limitations in principle, but on the soft side there is an unpredictable latitude of improvement. Take the case of so called massive memory. The discovery by Albert Fert, in 1988, of the Giant Magnetoresistance Effect (GMR) made it possible to multiply the storage capacity of computers by a factor of a hundred. Predictions always miss the main point. No one imagined the rise of the cell phone."

UP: This might on the global scale have had much more of an impact than the computer. For most people in the world, the cell-phone was their first phone.

LI: On the other hand, despite all the technological improvements brought to cars, trains and planes, transportation has not essentially changed. Except for a few new lines, the metro in Paris is basically the same as it was in the 50's.

UP: Except that there is now only once class.

LI: Also, the appearance of the city has not very much evolved, if we omit the Montparnasse Tower, and a few other high buildings or skyscrapers, like in the *Défense* area.

UP: Stability of your surroundings is a source of security. For your parents generation there must have been much more of an upheaval. The subject is endlessly fascinating, but we should not be digressing too much.

LI: I thought digression was the point of our conversation.

Grothendieck's departure

UP: True. But this does not prevent brutal changes, getting back on track. Why did Grothendieck drop out of mathematics?

LI: This is a question I have pondered for a long time without really coming to a resolution.

UP: Some say that he simply burned himself out, having thought of mathematics continually for almost twenty-four hours every day of the week for years, he was totally exhausted. By the way did you ever talk to Grothendieck on other matters than mathematics?

LI: We did talk on music. Classical music. He was of course very knowledgable as always.

UP: He played the piano?

LI: I know he practised the piano. I regret I have never heard him play.

UP: Let us not digress, at least not for the moment. Why did Grothendieck drop out, if it was not out of pure exhaustion?

LI: You have to keep in mind the spirit of '68. It certainly gripped him..

UP: .. as it did many others...

LI: ..you may in retrospect think of it as naivety, and Grothendieck for all his mathematical sophistication was indeed naive, politically naive I mean. He admired Mao, as many other people, especially academics, did at that time. People had no idea what Maoism and the cultural revolution really entailed.

UP: They did not really want to know. That is understandable and human. Instances of which abound in the past and surely will be with us in the future.

LI: And also one should not forget the concern with ecology that the 60's also had brought about. Ecological concerns had a certain urgency.

UP: They still have today, but now they have been almost entirely focused on the issue of climate change, forgetting perhaps that this is just part of a greater problem.

LI: Before turning to ecology (and eventually thinking of doing mathematics as of an indecent luxury in view of the problems of survival of our species), Grothendieck had been attracted by physics and biology.

UP: In physics and especially biology, his special synthesizing power would not come to the fore. Science does not have the same compelling beauty and logic as does mathematics.

LI: I can't really judge, having had no experience of what you call "science in general". When I entered the ÉNS I hesitated between doing physics or mathematics. We had two professors of physics : Alfred Kastler, who received the Nobel Prize in 1966, and Yves Rocard, the father of Michel Rocard, a former prime minister of France. Yves Rocard had been in charge of the program leading to the construction of the French atomic bomb. Their styles were quite different. Rocard lectured with enthusiasm, but was often confusing and messy. Kastler was clear and clean, making physics look like pretty mathematics. I was leaning toward Kastler. But eventually Cartan made me choose maths !

UP: I guess Rocard was more representative of science as a whole. Let's come back to Grothendieck. Mathematics can be easy, not to say almost trivial, for many of us in the beginning, but eventually everyone of us gets overpowered, Grothendieck being no exception. Mathematics really kicks back at you.

LI: I'm not sure about this "easiness". Anyway, I don't believe in the theory of "exhaustion". In 1970, Grothendieck was actively working on crystals and Barsotti-Tate groups. He had proven fundamental results about them, which he announced in his talk at the ICM in Nice. In 70-71 he gave a beautiful course on this subject at the Collège de France. The problem of the "mysterious functor" that he had formulated, and which eventually resulted in the magnificent theory Fontaine developed in the 70's and 80's, was certainly very attractive to him. On the other hand, I would bet that he was already ruminating about higher homotopy and the anabelian geometry program he was to propose a few years later. Fundamental groups in all their forms had always been a central theme in his thought. There was certainly a life for him outside of the standard conjectures and the construction of motives. It's sad that in the 80's he became so critical and bitter toward the mathematical community, in a kind of paranoid way.

UP: One is also reminded of Perelman, who has totally withdrawn from the mathematical community, and maybe one can speak of paranoia in this case too. Paranoia is of course an expression that is usually employed in a derogative way, to imply that someone is mad and has completely lost touch with reality. But you do not mean it this way I guess.

LI: I certainly do not. I am just trying to qualify the nature of Grothendieck's estrangement.

UP: In fact paranoia is eminently rational. Perhaps it is not surprising that excessively logical minds eventually fall prey to it. But there is surely a distinction between being disenchanted with mathematicians and the mathematical community and falling out of love with mathematics itself.

LI: I think we have said enough of that.

UP: You are right. You mentioned sharing an interest in music with Grothendieck. What is your relation to music?

Mathematics and Music

LI: I myself am an amateur pianist. For a few years I took lessons with the French pianist Jean Micault, who is now 87. I learnt a lot from him, not only on piano playing, but on teaching, from his talent of obtaining the best of his students, letting them fully express their own personality. You often learn things in one discipline that you can somehow carry over to other disciplines.

UP: Even in the case of such disparate disciplines such as mathematics and music? Or are they so disparate after all? At least in the popular mind there is a connection between mathematical ability and musical? What do you think?

LI: I am skeptical. You write somewhere about the symmetries of music and mathematics. I do not believe that this is so relevant.

UP: I agree with you. Are you referring to my review of du Sautoy's book on symmetry in the EMS Newsletter? There are of course many ways you can describe music in terms of mathematics, starting with the Pythagoreans. But I think that this is actually irrelevant as far as music is concerned.

LI: Obviously you can't just explain the power of the music of Bach, Beethoven or Mozart by the symmetries of classical harmony.

UP: du Sautoy in his book on Symmetry makes a rather big thing about them, although wisely abstaining from committing himself. If it would be true, you should in principle be able to generate music, at least Bach type music, algorithmically.

LI: Let me just make the trivial observation that there can't be any rational explanation for the pleasure some music piece can give you. Between two pieces of Bach or Beethoven, with the same degree of elaboration and "symmetries", one can move me sweetly, while the other one will leave me cold, or even irritate me.

UP: The only way I can conceive of a real connection between mathematics and music is on such an abstract level that it would be applicable in a much wider setting, including literature, and there are few mathematicians that display any literary aptitude. Actually Grothendieck might be an exception I have heard.

LI: I told you about his beautiful French, and which is very much in appearance in his memoirs 'Récoltes et semailles'.[16]

UP: So I have been told. But to return to my point. In mathematics as well as in music there are recurring themes. By recurrence of a theme you are both reminded of something well-known as well as seeing it in a different context and hence subtly changed not to say enhanced. It is the same with mathematical concepts, you really only learn them by seeing them in different contexts revealing different aspects. And there are other instances of recurrent themes in mathematics which may not be so easily formalized under a unified heading, and which are best studied case by case, in which the common theme is evoked rather than formulated.

LI: Indeed there are many examples of that in mathematics. Grothendieck's motives being the most famous one. And I would say that the whole Grothendieck program is like a giant symphony with many themes intertwined occurring over and over again.

UP: It makes sense.

Platonism and Mathematics

LI: Let's change topics. I've just started reading your article Platonism in Mathematics - A First Attempt Nov 22, 2006. I find it quite interesting.

UP: So are you a Platonist?

[16] *Récoltes et Semailles : Réflexions et témoignage sur un passé de mathématicien* Université des Sciences et Techniques du Languedoc et CNRS, Montpellier, 1985.

LI: In the sense that I certainly believe that mathematical objects “exist” independently of humans and laws of physics. I remember watching an animated TV debate on this issue between Alain Connes and Jean-Pierre Changeux[17]. Changeux claimed that mathematical objects “existed” only in the brains of mathematicians, the memories of computers, and books, while Connes insisted that he knew pretty well that they “existed” independently of that because he had such a hard time grappling with them. I was, of course, of Connes's opinion.

UP: Exactly, when you really grapple with a problem, as opposed to just reading about it, and when you over a long period of years start to get an overview of a field, how it all fits together almost seamlessly, the tangible reality of it all becomes hard to deny.

LI: But what would philosophers say to such an argument, would they not find it entirely subjective?

UP: But how could philosophical thought be otherwise? Our deeply felt conviction of an outside reality is similarly of a subjective nature. But of course you are right, Yuri Manin writes to the effect that mathematical Platonism is intellectually indefensible but psychologically inescapable.

LI: When you start constructing a mathematical theory, even if you know what the main basic concepts should be, it's quite hard to find the right definitions, the best logical route.

UP: This is where you really need a genius of the calibre of Grothendieck to guide you right.

LI: No, no. This is misleading. Ultimately it's mathematical objects themselves which guide you, by their symmetries, constraints, and interplay. This is of course a commonplace - and, I admit, very much a Platonistic point of view. By the way Grothendieck has written beautifully on this in *Récoltes et Semailles*. Of course I do not mean to deny that some mathematicians see further than others and help you in your search for the "right" constructions.

UP: I would not say that it is a commonplace. At least not to non-mathematicians, to whom I may in fact add many mathematicians. I am thinking of those mathematicians, who may be put off by formidable abstract machinery, and who may be liable to dismiss it as a mere play with definitions. As with abstract mathematics à la Grothendieck....

LI: IÕm not sure what you mean by Òabstract mathematicsÓ. I think there is no such thing as ÒabstractÓ versus ÒconcreteÓ mathematics, it all boils down to how familiar you are with the subject.Ó

UP: As with words in natural languages, they come with many shades and meanings, often contradictory, which often is very handy when you are engaged in argumentation. It is true that in ordinary life, 'abstract' along with 'theoretical' , tends to denote something nebulous and intangible, not to say evasive and ultimately empty, while 'concrete' is down-to-earth, hard and solid, very much tangible and most importantly a vivid stimulus to the imagination. And this view is not seldom employed by mathematicians as well, I am thinking

[17] *Dialogues de savants*, 12/1/1989, in *Apostrophes*, a literary program directed by Bernard Pivot between 1975 and 1990.

of Siegel's notorious dismissal of Hirzebruch's mathematics as being fashionably abstract, with the implications of its ultimate fate. But there is also, especially in the context of mathematics, the notion of abstraction, of probing deeper of seeing behind the surface phenomena, of divesting objects of their accidental and irrelevant properties. This is of course very much in the Platonist vein. Of course once you have familiarized yourself sufficiently with the new viewpoint, it is as strong a stimulus to the imagination as the initial objects, if not even more so, and thus you experience it as very concrete and down to earth. This is what familiarity is all about.

LI: I think you are digressing a bit now.

UP: I know. It is I who should ask the questions, and listen to your answers instead of lecturing you, but I cannot simply help myself. But let us see it this way. I am trying to formulate a question, which I think is very important. Namely that abstraction indicates a history of context, that it is an attempted solution to a problem. In other words it is anchored to a level below. That from a human point of view you need to take a bottom-up approach. I would say that elementary algebra is incomprehensible, except as a formal game, if you have no previous familiarity with dealing with numbers. This is in essence, I think, the criticism which is routinely levied against Bourbaki. My point is that although the process of abstraction may be presented as a ladder, you need to keep in mind all the rungs below. Thus at least from a human point of view there is a definite limit to the level of abstraction which is feasible, just as you cannot keep folding a paper indefinitely doubling its thickness each time.

LI: I am not sure what question you are trying to pose, to me it seems rather as if you are engaged in an abstract soliloquy.

UP: That is actually a very good point of yours, and shows the difference between abstract thought in mathematics and abstract thought in general, in the latter case it so easily dissolves into mere smoke. So let me be more precise. The logicians are fond of abstract principles such as the axiom of choice and also in higher cardinalities posing all kinds of axioms. My question do you think that this is serious mathematics?

LI: Just as I object to the notion of abstract mathematics so I do too, to that of serious mathematics. To me there is only one kind of mathematics. Of course you can pursue it more or less seriously, just as in music there are professionals and amateurs.

UP: So let me rephrase my question. Even if it has all the trappings of mathematics is it really mathematics?

LI: As far as logic is concerned I am a layman, and hence I am reluctant to venture into unknown territories. I donÕt know whether transfinite induction and the axiom of choice are really essential in arithmetic geometry. I know that Deligne is reluctant to use an isomorphism between $\mathbb{C}$ and $\overline{Q_\ell}$, whose existence relies on the axiom of choice[18]. On the other hand it seems to me that sheaf theory, especially the theory of toposes, which is currently used there relies on the axiomatic of universes, which itself uses a hierarchy of cardinals. And

[18] see Weil II, 1.2.11

coming to think of it, already in Grothendieck's famous Tohoku paper the proof of existence of enough injectives in abelian categories satisfying AB 5 and having a generator uses transfinite induction.

UP: This is very interesting. I think that engineers and other so called down-to-earth applied mathematicians would draw the line somewhere here. Can really the safety of flight of an airplane depend on whether the axiom of choice is true or not? Sorry, this was a silly remark, forget about it. Pray continue!

LI: To return to your soliloquy, you seem to say that each new level becomes harder and harder to scale. Is that really so? We certainly need not bring with us everything we have learned. And things we have learned and mastered are good and light companions. The plague is that when we have not been in touch with them for a long time, we have often forgotten them, and it's no so easy to get acquainted with them again.

UP: It may not seem so, I agree, at least not from the logical point of view. But it is easy to concoct logical statements with arbitrarily long nested sequence of quantifiers, but in serious mathematics only those of very limited lengths occur. It may indicate a cognitative limit on human thinking, but that does not exclude the possibility of them being mathematically meaningful even if beyond the human grasp. And as to axiomatics, I am often reminded of Bertrand Russell's quip, that it has the advantage of theft over honest toil.

LI: So once again you refer to serious mathematics. I will let it pass though. Concerning axiomatics, it can sometimes be excellent for extracting the quintessence of a theory, the axiomatics of triangulated categories is quite remarkable in this respect, but it may also have serious limitations. Remember the "axiomatic cohomology theories" of the late 50's and early 60's" with those long exact sequences for pairs of spaces, etc. Grothendieck's duality theories in various contexts, using the formalism of derived categories, made them pointless over night. But although these theories "work the same" in these various contexts, there is no known axiomatics for them all. Motivic duality á la Voevodsky is in its infancy, and a theory of Grothendieck motives is still a dream, or rather an expectation in which some don't even believe.

UP: In other words it is far from straightforward to find the right abstractions. We seem to be almost back to where we started.

Proust and Memory

LI: Yes I think this is a good excuse to terminate this discussion, which I think has been going on too long. Besides we have been talking for so long anyway that I cannot understand how you are going to remember it all.

UP: It is easier than you think, because human memories, unlike those of computers, are linked by strands of associations. One strand leading to another. As you start writing one thing will lead to another, not necessarily in the right chronological order or with the exact choice of words, and details such as names might be lost; but the essence is preserved. In other words: you remember the content if not the form. You can seldom quote but you can often paraphrase.

LI: Human memory is a mystery. It is remarkable what can trigger it, to bring up memories which you never remembered that you had.

UP: A memory is a reconstruction, it is never retrieved wholesale as in a computer, every time you remember something you change it so ever subtly. Thus memories that are precious to you, you are reluctant to bring up too often lest they will wear out and erode. On the other hand memories have to be periodically refreshed not to be irretrievably lost. And there are different kinds of memories. You can vividly imagine a visual scene, but what is it that you really imagine? I believe it is something rather abstract. On the other hand it is almost impossible to imagine a smell, thus when you encounter a smell, the memory associations that are connected with it appear with an almost brutal intensity.

LI: This is what Proust is about. Of course you know this famous passage on the *petite madeleine*[19]. Let me try to recite one sentence of it : "Mais, quand d'un passé ancien rien ne subsiste, après la mort des êtres, après la destruction des choses, seules, plus frêles, mais plus vivaces, plus immatérielles, plus persistantes, plus fidèles, l'odeur et la saveur restent encore longtemps, comme des âmes, à se rappeler, à attendre, à espérer, sur la ruine de tout le reste, à porter sans fléchir, sur leur gouttelette presque impalpable, l'édifice immense du souvenir."

UP: Proustian memory is really about bringing a past moment into the present wholesale. A real piece of the past as opposed to a merely reconstructed one. Whether that is really possible or not is one thing, but the very idea is so poetic so no wonder Proust was inspired to write his suite. The search of the past is truly en elusive one, and as I noted, the very process of trying to catch it changes it.

LI: Also, one thing which fascinates me in Proust is his logical, not to say mathematical, way of thinking.

UP: Really, please elaborate !

LI: Yes, his style combines accuracy, clarity, elegance, logic and nuances all at the same time, in the same way as when we write mathematics we envision examples and counter-examples, extra or superfluous hypotheses, variants and generalizations, but foremost try to express what the crux of the matter is, even if it's not what we had hoped for.

UP: So.

LI: When Proust exercises this kind of mathematical talent on psychological matters and social behavior he usually finds the "key fact", which we had vaguely imagined but not formulated, and then, that discovery makes a twinge in our heart.

UP: It reminds me of Plato's claim that knowledge is simply in the nature of a memory we have temporarily forgotten. When we are told something we understand, it is as if we have known it all along. Just like those key psychological facts that Proust formulates. They vibrate with us.

[19](in *À la recherche du temps perdu*, *Du côté de chez Swann*, Première Partie, Combray)

LI: To lose your memories must be a real tragedy. Alzheimer disease is horrible, and unfortunately there's not much you can do about it.

UP: In particular you cannot avoid dementia by simply being mentally active. It can happen to the most mentally alert, there is no need to mention any names. There are consequently all those kinds of silly advice about doing cross-word puzzles, sudokus, learning a new language, all meant to stave off dementia, as if you could exercise your brain in the same way you exercise your muscles. Many doctors and neurologists lend themselves to that nonsense. Probably the same thing holds with heart attacks. By keeping physical fit you fool yourself into thinking that you are immune. But nevertheless you can be felled anyway.

Torsten Ekedahl

LI: As in the case of Torsten Ekedahl. But where did it happen?

UP: At the department.

LI: It must have been awful. Totally unexpected, I presume ?

UP: Well, the last year of his life he lost some 40 kilos, exercised a lot and seemed to be in a very happy mood. He told one of my colleagues how he had taken up mushroom picking and how he used to take pictures of mushroom with his cellphone and sending them to his mother, who was a mushroom expert, and ask for advice about edibility. I found it very touching.

LI: He was such a strong and original mathematician. Although he was formally a student of mine, I often felt the other way around. He was like Douady, a true Bourbakist, with such a grasp of mathematics and having so many examples up his sleeve. Also, what I admired about him, was that he was not afraid of doing the unconventional in mathematics.

UP: I think that people in Sweden did not really appreciate his greatness. He was also a great personality in addition to being a great mathematician. He might also have a tendency to initially put people off with his somewhat rough manners and overbearing intellect. He could hold forth on mathematics to you unstoppably.

LI: I never found him rough in any sense. He was always so gentle. But it is true he was overflowing with mathematical wisdom. By the way was he not very active in *MathOverflow*?

UP: Yes, I was told that he had checked in just an hour or so before he died.

LI: This is very sad.

UP: It is. Like many mathematicians he had many interests. Non-mathematicians do not usually appreciate that, they think of mathematicians as all wrapped up in mathematics. You have displayed a real interest in literature. I have also been told that you are very interested in movies. Bergman being a favorite.

Movies and other interests

LI: True, I like movies, and Bergman's films are great memories, especially the early ones.

UP: Such as?

LI: *Sourires d'une nuit d'été*, *Le septième sceau*, *Les fraises sauvages*. Of course there are many other film directors, from many countries and different periods, for whom I also have a great admiration. Bergman is not the only favorite. I could talk at length on this.

UP: I think that the old black and white movies had a special appeal absent in more modern color ones. Just as I think that a black and white photograph is superior to one in color. There is something vulgar about color in photography.

LI: Not necessarily, but the fantastic emotional power of *La Grande Illusion*, *Citizen Kane*, *Stagecoach*, *White Heat*, to mention only a few which come to my mind, outside of Bergman's movies we just mentioned, has much to do with the black and white As I said I really could talk at length on this, but maybe it would be more appropriate as a topic for another chat ..

UP: Maybe. I have also been told that you have learned several thousand Chinese characters.

LI: That is an absurd exaggeration. The truth is that I studied 400...

UP: ...that is an impressive number by itself...

LI: ...could be, but the sad fact is that I have by now almost totally forgotten them. For the sake of conversation with Chinese or Japanese colleagues, I learnt a few very complicated characters, like that of melancholy, for example.

UP: To show off?

LI: In a literal sense in that case. At the end of a dinner, this is an excellent topic of conversation : participants try drawing them, compare their writings, correct one another, etc., and the whole group gets very excited.

UP: Sounds wonderful. Calligraphy by the way will be an art form that will disappear if digitalization takes over, as we discussed earlier.

Grothendieck concluded

LI: By the way would you like to see a picture of Grothendieck as a child.

Brings forward a copy of 'Récoltes et semailles' with a personal dedication by the author opposite a childhood picture.

UP: How old could he be?

LI: I do not know. Seven or eight. The character of the grown-up man is already visible in his gaze.

UP: It is amazing how much you can read into a pair of eyes. This is really the only thing you can go by when you try to identify a friend of yours on a group picture from his childhood. The human ability to recognize faces is truly remarkable.

LI: Do you know of GQ - the *Gentlemen's Quarterly*?

UP: I do not read that kind of magazines.

LI: The title may be misleading, this is not a variant of *Playboy*. I was interviewed by Philippe Douroux, a former chief editor of *Libération* and *France-Soir*, for an article on Grothendieck in this magazine[20]. We talked at length, but only a minute part of it actually found its way in the published article.

[20] GQ, n. 44, October 2011.

UP: This is the way usually. With this interview you will have the opposite problem.

Shows a picture of Grothendieck as a young man

LI: It must have been taken in Paris around 1948.

UP: So he was twenty. He looks very sure of himself.

LI: But not in an unpleasant way.

UP: He simply exudes energy and self-confidence.

LI: This is true.

UP: And a full head of hair too. He was not naturally bald I have been told, he shaved himself, way before such things became fashionable. When was the last time you had contact with him?

LI: The last time I saw him was in Montpellier, in 1982, on the occasion of the PhD defense of Daniel Alibert, a student of Verdier. He was friendly. I tried to explain to him the theory of the de Rham-Witt complex, on which I had been working in the past years. He was not so interested. The topic looked to him technical and narrow. We exchanged a few letters in the 80's. From 91 on he remained secluded and he no longer wrote we, until, in January 2010, I received a letter from him containing a handwritten declaration, dated January 3, 2010, that he asked me to made public, and which has been widely circulated since then. In fact, I scanned it and I can retrieve it for you on my laptop. You read French ?

UP: Of course. I simply do not speak it.

LI: As you can see, he has forbidden any publication of his writings and re-publication of his already published work, during his lifetime. He has a lot of unpublished material. When I visited him at his place in the 60's, he would often pick up a handwritten or typed note from a huge filing cabinet behind his chair. I wonder whether all the manuscripts he had accumulated there survived his departure from the IHÉS in 1970. According to Philippe Douroux, in the early 90's he left to Jean Malgoire about 20000 pages of notes and letters stored in five big boxes kept in a secret location.

UP: I have recently been told that contrary to what was always assumed namely that Dieudonné did a lot of the writing of EGA, it was in fact Grothendieck who did almost all of it. His energy must have been amazing.

LI: Who told you that? It is not all true. At least it does not concur with my own understanding of the collaboration. Of course Grothendieck conceived of the whole plan, whose structure he wrote up in detail, and then wrote a first draft for each section. Then Dieudonné got down to work, making corrections and additions, in fact rewriting entire passages. Then Grothendieck stepped in again, making modifications and so on. It was a converging process of wollying back and forth. And in fact certain of the key ideas and techniques are actually due to Dieudonné. Grothendieck was very open about that, pointing especially to EGA IV and the delicate differential calculus in positive and mixed characteristics and its relation with the notion of excellency. And besides I do not see what relevance your remark had to our discussion of his coming *Nachlaß*.

UP: It was just something that crossed my mind in relation to his enormous capacity for work. Forget about it. Could it be that what we have seen so far is only the proverbial tip of the iceberg ?

LI: It's too early to guess about the bottom of this iceberg. Some of his texts have already circulated : *La longue marche à travers la théorie de Galois*, *À la poursuite des champs*, *Dérivateurs*. This last one, despite Grothendieck's declaration, is in the process of being published by M. Künzer, J. Malgoire, and G. Maltsiniotis.

UP: What about his famous memoir to which we have referred to so many times? It must have been published already. So many of my colleagues have read it.

LI: The original text (in French) has not yet been published. But a pdf version has been circulating on the web, and parts have been translated into several languages (English, Spanish, Japanese). Long ago Grothendieck wanted it to be published, and I heard that preliminary contacts had been made with the French publisher *Odile Jacob*, but this attempt aborted. And I'm afraid that now it would be delicate to bypass Grothendieck's interdiction, probably more for ethical reasons than legal ones.

UP: How about the publication (or re-publication) of his mathematical texts (EGA, SGA, for example) ?

LI: It seems that many people in the mathematical community tend to think that despite Grothendieck's interdiction these texts should be published or re-published. As a matter of fact, Parts I and III of SGA 3[21] have been edited by Philippe Gille and Patrick Polo, and re-published by the Société Mathématique de France[22]. Part II should appear soon. SGA 4[23] is undergoing a similar process. EGA[24] was translated into Chinese by Jian Zhou, a professor at Beijing University. It would greatly benefit the Chinese students if it could be published. What do you think one should do?

UP: One should make a difference between personal writing and mathematical. The latter in a sense belongs to the world at large. You cannot patent a theorem.

LI: That is true, and as I have noted your opinion is shared by many mathematicians. But the very contrary opinion has been forwarded by people whom I respect very much.

UP: The opinion that Grothendieck's injunction against publishing should also include mathematics?

LI: Yes. They very much think that his wishes should be respected in their entirety.

UP: I heard that a documentary has been made on Grothendieck.

LI: Yes, by the French director Hervé Nisic[25]. He told me that he sought

[21] *Schémas en groupes*, Séminaire de Géométrie Algébrique du Bois Marie 1962-64, dirigé par A. Grothendieck et M. Demazure, Lecture Notes in Mathematics, 151, 152, 153, Springer-Verlag, 1970.

[22] Documents mathématiques, 7, 8.

[23] *Théorie des Topos et Cohomologie Étale des Schémas*, Séminaire de Géométrie Algébrique du Bois Marie 1963-64, dirigé par M. Artin, A. Grothendieck, J.-L. Verdier, Lecture Notes in Mathematics 269, 270, 305, Springer-Verlag, 1973.

[24] Éléments de Géométrie Algébrique, ...

[25] *L'espace d'un homme*, not yet released.

out Grothendieck's secret residence in the Pyrénées. He actually filmed outside the house, and Grothendieck came out to pick up his mail in the box at the gate.

UP: There must have been a scene.

LI: On the contrary. According to Nisic, Grothendieck was friendly, and apologized for not inviting him inside, explaining that it was too messy.

UP: So Grothendieck is still going strong.

LI: I do not exactly understand what you mean by that.

UP: His reaction struck me as very sane and healthy, belying all speculations of his being a bitter recluse and no longer in full control.

LI: Well, Grothendieck will always surprise us.