Bulletinen

Svenska Matematikersamfundets Bulletin

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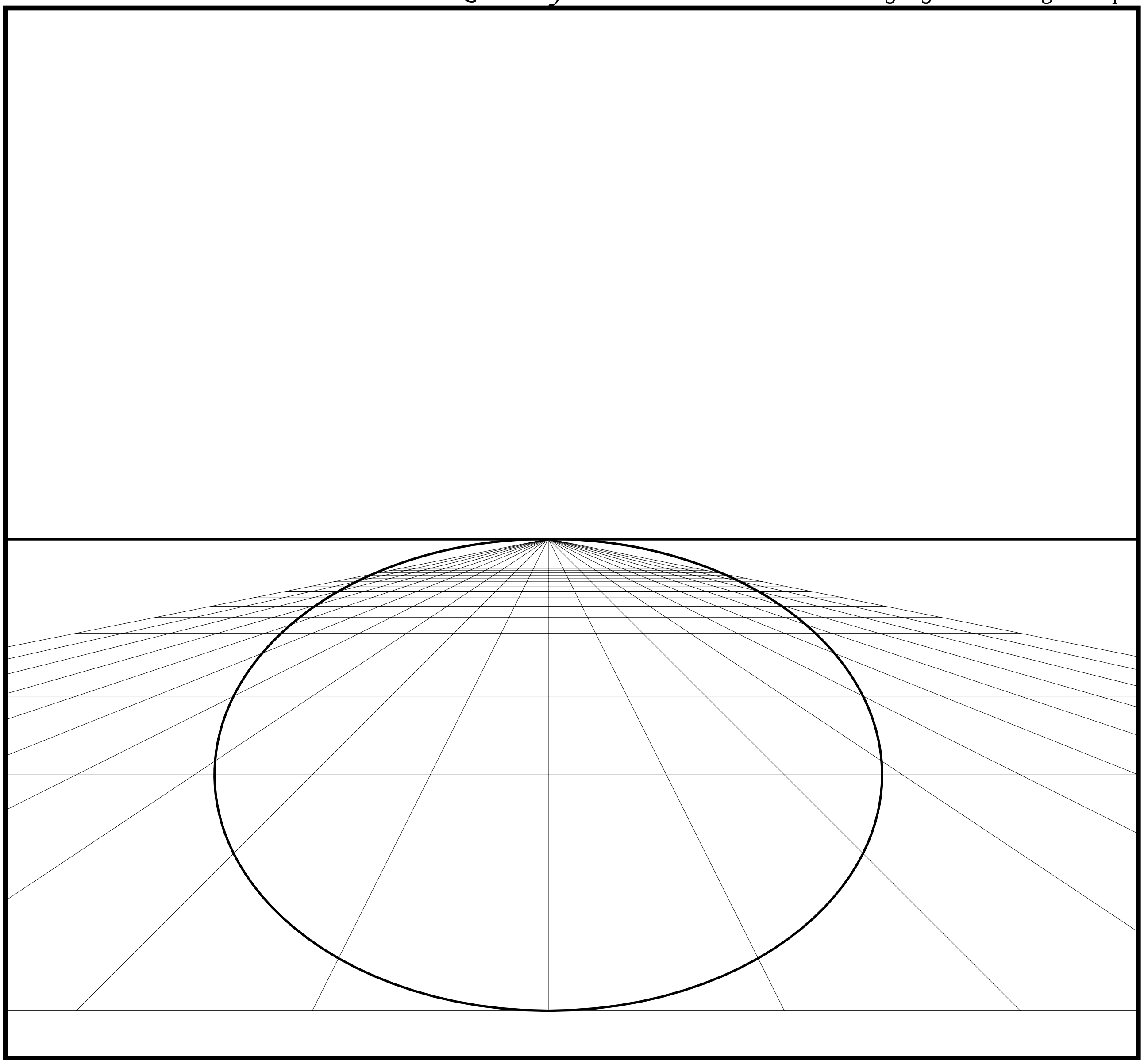
Ansvarig utgivare: Milagros Izquierdo



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Joint Meeting: ***CAT-SP-SW-MATH*** *Umeå , 12-15 juni 2017*

Bulletinen

utkommer tre gånger per år I Januari, Maj och Oktober. Manusstopp är den första i respektive månad

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Detta Nummer

Ulf Persson

Som vanligt har några av våra kolleger gått ur tiden. Håkan Hedenmalm delar med sig av sina personliga minnen av Serguei Shimorin som förolyckades under en bergsvandring i somras. Christer Kiselman låter även publicera minnen av den nyligen bortgångne Gennadi Henkin som presenterades vid Mikael Passaredagen den 5 oktober. Och slutligen den relativt unge Fieldmedaljören Jean-Christophe Yoccoz dog i början av september och Michael Benedicks inkommer med en regelrätt nekrolog. Vidare har Maria Roginskaya uppmärksammat oss på en filosofisk text av Viktor Havin som gavs 1993 i samband med att han förlänades ett hedersdoktorat i Linköping. Jag har även beslutat inkludera en interview jag gjorde med Luc Illusie med tema Grothendieck. Den gjordes i januari 2012 och har ännu inte publicerats (tanken var att den skulle ingå i en samlingsvolym av intervjuer, av vissa en hel del har redan sett dagens ljus i Bulletinen och dess föregångare Utskicket). Vidare presenterar jag en ny kollega vid vår institution - Orsola Tommasi - med anledning av att hon blivit tilldelat naturevetenskapliga fakultetens forskarpris på en kvarts miljon.

Sist min inte minst kommer det att upplysas om den gemensamma mötet med de katalanska och spanska matematiska samfunden som vår ordförande Milagros kommer att anordna uppe i Umeå i juni.

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Mathematics as a Source of Certainty and Uncertainty

Viktor Petrovich Havin

Denna text, som återspeglar ett föredrag som gavs för nästan ett kvarts sekel sedan, uppmärksammades jag av min kollega Maria Roginskaja här vid institutionen. Texten delgavs mig i form av en föråldrad pdf-fil som jag inte kunde kopiera in i en 'editor' och göra en text-fil av den, och eftersom den är ganska lång kände jag inte för att kopiera den för hand. Istället gjorde jag om den till en ps-fil, spaltade upp den gigantiska filen (drygt 110 Mb) i delfiler en för varje sida och införde dessa som illustrationer. Sådana filer kan jag manipulera för hand och därmed finjustera storlek och placering, men kvalitén på texten såsom grafik är något undermålig men, hoppas jag, fullt läsbar. Jag har även funnit det befogat att att lägga till en kommentar (kanske med fortsättning)

Biografiskt data: *Rysk matematiker från St.Petersburg. Född 1933-03-07. Sysslar huviudsakligen med funktionalanalys, reell analys och funktionsteori. Promoverades 1958, habiliterade sig (den ryska doktordgraden) 1969. Han har efter murens fall varit gästprofessor vid ett otal (12 länder) västerländska universitet. Hedersdoktor, som ovan antytts, i Linköping 1993, Onsager-professor i Trondheim 2000, och 2004 erhöll han G.de B.Robinson Award som förlänas av det kanadensiska matematiska sällskapet. Därtill kan läggas utmärkelser från det ryska utbildningsministeriet och hans eget universitet. Bland hans studenter märks Fieldsmedaljören Stanislav Smirnov.*

Redaktören

Ladies and gentlemen,

It is difficult for me to express adequately my appreciation of the honour which has been conferred upon me by inviting me to address this audience. So I immediately start with the topic of this lecture, but before I have to stress that its genre is unusual for me. Unlike in lectures any mathematician is used to deliver, I'm not going to try to increase your store of knowledge. I'll rather express some feelings and a certain mood. For some technical reasons the sources I could use in preparing this lecture were scarce. This explains the excessive use of my personal experience and memory for which I apologize in advance.

"Certainty" seems to be the most appropriate word to express what the non mathematical public thinks of mathematics. This opinion could be supported by quotations of famous mathematicians, though there is no lack of scepticism in what mathematicians think and say about their subject. But the non mathematical public has no doubt that mathematics is a realm of certainty. This feeling is well expressed by the following saying of the French painter Georges Braque: "Art upsets, science reassures" (if we replace the word "science" by "mathematics").

This popular (and completely mistaken) opinion determined my destiny. It is thanks to it that I became a mathematician. The decision was actually taken not by me, but by my father, a philologist whom I fully obeyed at the time, though my mathematical achievements had been nothing more than good highschool marks. According to my natural inclinations I'd rather become a philologist too. But I graduated from the highschool in 1950, the year when Stalin also got interested in philology and wrote a booklet "Marxism and problems of linguistics". This event, memorable for everybody of my generation had been preceded by several ideological campaigns when politicians taught (sometimes with the use of violence) writers, historians, musicians, and literary critics what is good or bad. Impressed by this practice, my father forbade me even to think about the humanities and *ordered* me to become a mathematician. Now, 43 years later I'm grateful to him for this risky decision. But he was completely mistaken in what concerns his main point, namely, search for certainty. Of course, politicians hardly can prescribe to mathematicians which theorems to prove. Nevertheless, mathematics is the worst place to look for certainty and definiteness. It is, to the contrary, a realm of uncertainty. This uncertainty, its malignant and beneficial sides, are the subject of this lecture.

I'll describe how one acquires and then loses the comforting feeling of certainty usually ascribed to mathematics. Then I turn to a kind of uncertainty which I consider as beneficial, and which is supported by mathematical thought in an essential way.

My first impressions from mathematics fully agreed with the opinion of Braque. This science reassured indeed. I was fascinated by the preciseness and expressive power of the language. Mathematicians are much more delicate, cautious, and I'd say, nervous word users than anybody else. Unlike others, they are really bothered with the *meaning* of words they are pronouncing or writing down. I was deeply impressed by the capacity of mathematical language to describe – in an unequivocal and very expressive way – and fix very numerous and heterogeneous ideas, ranging from analysis to probability, from classical to quantum mechanics, from economics to linguistics.

The convincing power of mathematical proofs seemed overwhelming, irresistible to me, exceeding by far any proof in any domain where verbal proofs play an essential role, be it physics, history, or law.

There is one more component of this special certainty insidiously conquering any young mathematician as he gradually opens up his profession. It is something spontaneously felt by any mathematician in spite of the fact that some would deny it and notwithstanding well founded criticism of logicians and philosophers. It is a deeply rooted, visceral belief in (or rather a sensation of) the existence of a special mathematical reality "hard as rock" according to Hardy who praised this transcendental world in his "Mathematician's Apology" claiming that the mathematical reality is more real than the physical one. It is a belief making an active mathematician insensible to remarkable results of logic which warn him against this "naive superstition". He is just unable to question the existence of things whose properties it is his task to conceive and whose very real resistance depriving him of sleep and rest he permanently tries to overcome. He reacts to theorems of Gödel and Cohen, to critical attitudes of intuitivists and constructivists with a mixture of respect and vague feelings of guilt, forgetting all this in the everyday communication with the stubbornly existing mathematical reality. As to me, the loss of certainty came not from logic. Its origin was of lower, much more earthly and practical level. My first doubts can be squeezed to quite silly, childish questions: "What for?" "What is the aims of mathematics?" "What is good and what is bad in it?" "What are the criteria of value?" Now I know that these questions just cannot be answered. But then, in the fifties, I have been really upset, after it became clear to me that nobody can provide me with a satisfactory and comforting answer. Eventually I had to accept this situation and to live on. Now I can say that mathematics, being a beautiful, miraculous science, is, at the same time, subject to fashion and cult of power. Its value criteria (at least those applied in practice) are very often determined by market forces and whimsical, arbitrary and irrational opinions and tastes. I could illustrate this sad assertion by several funny stories and almost every mathematician could add his. Let me only briefly mention a curious fate of the Cantor set which I choose as a symbol of a domain inhabited by species usually called "bad sets" or "bad functions". For the generation of my teachers they symbolized the progress. I was brought up in deep respect of these objects and related ideas and techniques. But soon after I've graduated from the university I knew that these favourite things of my teachers had become obsolete, a mark of backwardness. It became fashionable to say that "bad functions do not exist". The term "Theory of functions of a real variable" acquired an abusive nuance. At that time I often heard from my colleagues that the attention paid in the twenties and thirties to bad sets and functions in Russia and Poland was a kind of decadence and degeneration distracting mathematics from its true destination which is to solve problems of physical origin. But what do we see now? The Cantor set is fashionable again! Masses of people are really obsessed by it (and similar objects) claiming that physics (physics!) just would perish without them. Luxurious volumes of pictures are being printed and successfully sold, the Cantor set and its relatives got a new name, they are "fractals" now; they are not "bad", but "beautiful" (everybody knows the title "The beauty of fractals"). Of course, this boom is related to really deep discoveries in the theory of dynamical systems, new understanding of chaos. Grimaces of vanity and fashion, market tendencies in mathematics coexist with significant development of thought, only masking and distorting it. The above description does not contradict the well-known metaphor comparing mathematics with an orchestra whose participants don't know each other being separated by distances and interdisciplinary barriers, but the orchestra is nevertheless perfectly concerted, producing divine music, as if it were led by an invisible Conductor. This *is* true, but this image can be perceived only from afar, and nobody has ever seen the score. The whereabouts of the Conductor and his plans are obscure, and nobody, no group, no organization can claim his role.

But in the real life a mathematician is often in a situation where he has to judge, to accept or reject. Those who are obliged to accept or reject papers for publication or applications for a job deserve to be pitied. The total lack of formal, algorithmic criteria of selection makes their situation highly unpleasant. If your department got 500 applications, then usually it is not hard to reject 450 according to reasonable and sound considerations. But what if you have only 2 positions, and the rest of applicants consists of good, serious specialists but does not contain, say, a Gauss and a Hilbert? Then you make a clever face and say that a class of spaces, the favourite theme of candidate X, is not worth considering, or that the theorem of Y is good but not a breakthrough, and a theorem of Z *is* a breakthrough, but the number of complex variables is one, and this is old-fashioned.

I'm not criticizing. I have no proposals. I'm describing. In such situations there is no way to escape subjective conclusions conforming with personal tastes. The only thing which could be avoided is to pretend that you possess the objective truth and are motivated by superscientific considerations.

I remember a lecture delivered by P. Aleksandrov, the famous topologist, in Leningrad, somewhere at the end of the sixties. Its title was "Criteria of value in mathematics". He analyzed, one after another, three criteria: applicability, fashion, degree of difficulty – only to reject them all. He proposed instead something very indefinite like "a feeling of a new horizon". This is by far not an algorithmic solution. But I prefer it to the terrible practice when the works of a person are judged according to the journal which published them. This is very algorithmic indeed and frees you from reading mathematics which requires concentration, energy and time.

So far about the criteria of value in mathematics and understanding its own aims and necessity. It is uncertainty in its purest and very unpleasant form.

But, to conclude with an optimistic note, let us turn to a beneficial kind of uncertainty inherent in mathematics and contrasting its malignant forms.

Mathematics is often and deservedly lauded for its applications to other sciences. It is impossible to deny these merits of mathematics whose very existence always was determined (and still is) by a subtle interplay of exterior and interior incentives. But I want to emphasize not the applications, but a capability to create sound and reasonable *doubts* and *uncertainty*, things which are in a short supply, but very necessary nowadays. In this connection we could remember again great achievements of logicians, but I'll dwell on much more elementary, almost highschool matters.

Any mathematician, unlike (unfortunately) other people, knows (not only knows, but has it in his flesh and bones) that not everything which has got a name exists in reality. Mathematicians are *professionally* obsessed by existence problems. And not only by the existence of an *object* (a solution, a function or a set with prescribed properties), but by existence of a solving procedure when the algorithm in question has to satisfy certain requirements.

Normal people, not trained in this school of professional doubt, confronted with any problem, rarely suspect it can be unsolvable. They just start solving it. This is normal. And this is awful. Consider the following series of isomorphic proposals.

Let us trisect an angle using compass and ruler only, let us construct a perpetuum mobile, or "socialism"; let us do away with inflation and unemployment. Mathematics contains a powerful sobering potential suggesting how cautiously you must react to these appeals. Creating and propagating reasonable uncertainty and doubt, mathematics is capable to calm down, to cool away many dangerous and contagious enthusiasms.

In the sixties it became fashionable to jeer and sneer about compass and ruler problems in the highschool ("Why compass and ruler, why not something else?"). The jokers seem to be the same people who produced awful highschool geometry books with axioms of a vector space preceding triangle and circle. There are several strong arguments in defence of the compass-ruler problems, but I emphasize only one: it so happened that just these ancient problems served as the material for discoveries whose contribution to culture is tremendous and whose results need to be inculcated into the mass psyche, to become a commonplace: NOT EVERY PROBLEM CAN BE SOLVED. This is the main reason to include these problems into the highschool teaching. They can be explained to every schoolboy and schoolgirl producing a salutary pedagogical influence. Being acquainted with the procedure of bisection of an angle, it is natural to start thinking about trisection. Why not? These problems are so similar! The non-solvability of the second is highly not obvious. Nevertheless it *is* unsolvable and this can be rigorously *proved*. Mathematics abounds in results of this kind when something seems to be within one's reach but eventually turns out to be impossible. But denying the possibility to find or do something, mathematics yields some consolations in the form of *approximate* solutions, *optimization* algorithms suggesting the ideology of *compromise*. A person brought up in this spirit hardly can join a crowd crying like mad "liberté, égalité, fraternité" only to start mass killings afterwards. An easy reasoning will lead this person to the conclusion that the terms of this triad are not compatible with each other, and it is better to look for something approximate, but feasible.

For ages the general human aspiration was to catch and freeze everything as *notions*, creating all-embracing and all-explaining systems of thought. Isn't it clear now that this is only possible with relatively trivial things? The real complexity of world can be only *approximately* described, and this description cannot manage with *notions*, it needs *images*. Mathematics is a source of a lot of images, not less expressive than images of poetry. Penetrating your heart they are capable to influence your world perception.

Let me use two quotations, one due to a famous sociologist, and another to a humorist. "Many of the greatest things man has achieved are not the result of consciously directed thought, and still less the product of a deliberately coordinated effort of many individuals, but of a process in which the individual plays a part which he can never fully understand" (von Hayek). The second quotation is much shorter: "No snowflake in an avalanche feels responsible" (Jerzy Lec). But in my feeling, vanity and *futility* of individual efforts hardly can be expressed with a greater force than by the following "uncertainty theorem": the value of the Lebesgue integral does not depend on values of the integrand on any prescribed set of zero length.

The theorem is a flagrant expression of senselessness of such notions as "cause", "guilt" or "responsibility" applied to results of sufficiently massive, integral character. Meanwhile every Russian traveller is being asked daily: "What do you think about Gorbachev or Yeltsin" as if these men (or anybody else) can be considered as causing or governing immense, cosmic changes going on in Russia. Returning to the "damned questions"* of mathematics ("what for?" "what is good or bad?") we can again use the above theorem as an instructive image. Results of work of the mathematical community at any moment before the 2nd World War could be expressed as a SUM of individual efforts. By the end of the sixties the set of terms of this sum became practically infinite (though, probably, still countable). But now this sum definitely has become an integral. I think this is a *Lebesgue* integral of personal efforts, though the presence of an infinite set of point masses may be arguable. I'd rather admit a singular *continuous* component with no distinguishable separate points. But let us agree, at least, that this image has a right to exist.

It can cool the incandescence of passions and weaken prohibitive trends. Nobody can or must feel or claim responsibility of mathematics as a whole. Its sense, its message are as inconceivable as life. It is an integral, and preoccupations inspired by fashion are vanitas vanitatum.

Powerful images carrying mighty expressive charge are connected with analytic functions and their antipodes, so called "bad" functions. Creators of Analysis were spontaneously convinced that all functions are analytic (even before this term had been coined). This spirit has been weakened as a result of the "string dispute" at the end of the XVIII century. But in a milder form this frame of mind generally persisted even in XIXth century.

* Literal translation of a Russian expression denoting the most fundamental problems concerning man's essence and existence.

Nature needs analytic functions only. Such was the healthy, elemental view of people believing in God, predeterminacy and predictability of Being. A property drastically opposing an analytic function to a "bad" one is *uniqueness*.

Past and future of a process described by an analytic function are completely determined by its course during a second, or a one millionth part of a second. This mathematical image is apt to create mixed emotions. An analytic function is a symbol of highest perfection as is a favourite melody or line of poetry. Starting first notes or words it is impossible to continue differently from the classical sample. But at the same time the analyticity is a severe verdict, an inflexible prediction, impossible to contest. If an analytic curve $y = f(t)$ coincides with the parabola $y = t^2$ on (0,1), then these two curves are doomed to coincide forever, no choice is possible. There is something very significant in the reluctance of old classics to accept the possibility to represent "an arbitrary" curve as a sum of trigonometrical series, such representation being "a formula". But all functions defined by formulas have to be analytic and cannot change their course in an arbitrary way.

Oscillations of fashion around "bad" and "good" functions mentioned above reproduce, in a sense, the old "string dispute". In spite of its vagueness, abundance of terms not duly defined, absurd claims and personal biases, this dispute includes something really important. Human beings can be divided into two categories. The first one believes (or feels) that world is described by analytic functions. For the second everything is expressed by Lebesgue measurable functions. At any moment their course is absolutely unpredictable and, hence, can (in principle) be changed in any desired way. So, this second attitude implies *certainty*, those people feel they are masters of world. Of course, no argument is thinkable here. We are dealing not with clear statements to be proved or disproved. We are dealing with different psychological approaches to reality, with conflicting world orientations. I dare to express my deeply personal, non-verifiable, non-arguable confidence in the analyticity of the world. Chaotic behaviour results from the interaction of innumerable analytic processes. This irrational feeling is warranted by some rigorously proved mathematical facts. However wild a function of time might look, it is, eventually, the sum of a series of polynomials or a difference of two analytic functions.

I used these elementary examples to show how some images so familiar to any mathematician can suggest the noble habit of doubt and strengthen the feeling of beneficial uncertainty.

This eulogy of uncertainty and doubt I'm finishing to deliver is not something unusual nowadays. The mood I tried to express is gaining more and more room, undermining certainties and selfconfidence of conceited leaders, making it harder to politicians to subdue masses by cheap incantations devoid of any real content. After I've already sent the title of this lecture to professor Hedberg, in a Montreal airport I bought "Le Monde" of the 21st of April with an article of Edgar Morin, French Socialist, "La pensée socialiste en ruine", and was surprised to read the following lines (a newspaper is the last place where I could dream of finding something useful for this lecture): "In the opinion of Marx science is a *source of certainty*.

But today we know that sciences yield ***local certainties***, but theories are scientific insofar as they can be refuted, that is, are ***not certain***. And, in what concerns fundamental questions, the scientific cognition runs into bottomless ***uncertainties***. For Marx, the scientific ***certainty*** eliminated philosophical interrogation. Today we see that scientific progress only animates fundamental philosophical problems."

The attitudes I expressed become more and more banal which is illustrated by their frequent appearance even in the mass media. The more banal, the more commonplace they become, the more is our hope for the eventual improvement of the world, more human relations between human beings. And I hope that the experience accumulated in mathematics, joint with the experience of everyday practice, history, philosophy, positive sciences, religion and art will contribute to making these attitudes of beneficial uncertainty a commonplace indeed.

Platonism and Mathematics

Ulf Persson

One of the fundamental ideas of Western Philosophy (and not necessarily restricted to it) is Plato's conception of forms. He makes a distinction between the apparent reality as transmitted by our senses with all its confusions and temporality and an underlying reality of clarity and permanence. The metaphor he presents is that of men imprisoned in a cave on whose wall the outside world is projected providing their only visual contact with it. To be more literal (risking being a bit silly), we are only served two-dimensional shadows (material manifestations) of three-dimensional objects (the forms). Plato's vision was later watered down by his disciple Aristotle, and what was gained in reasonableness was lost in grandeur. Reasonableness has its undeniable virtues but hardly in bold metaphysical speculation. Understandably platonism, or rather neo-platonism, had a significant impact on the developing Christian religion with its emphasis on other-worldliness, although the heaven of Christianity, with its almost parodical literal interpretation (remember that 'paradise' is a loan from Persian) is rather removed from the abstract heaven of forms of Plato. St. Augustine, of the late fourth and early fifth ventury A.D., although not unaware of Aristotle he was, unlike his later scholastic descendants such as Aquinas, rather unaffected by him, finding Plato a much more congenial thinker, preparing the way for his eventual conversion to Catholicism. But also (more sophisticated) connections between platonism and hinduism are not hard to detect. Now for such reasons (and no doubt others) platonism is in modern academic philosophical circles considered rather naive and thus tended to be caricaturized. Whatever the misgivings, platonism has played a crucial development in the development of science which cannot live on empiricism alone. The great advances of science do not depend on improved observation but on conceptual breakthroughs. The ambition is to go beyond mere appearances and find the hidden underlying reasons, an ambition most fully realized in physics. When it comes to mathematics, platonism is paramount, although mostly conceived in terms of the external reality of mathematical concepts, so called mathematical realism. Mathematicians do not primarily invent but discover, and even invented entities turn ut to have an independent existence and are subject to laws beynd the control of humans. As Karl Popper put it, integers may be an invention of man, but not its laws such as associativity of addition and multiplication. As we all know, inventions always have unintended consequences. Only the most cynical mathematician would tolerate mathematics as a mere social convention ultimately only answerable to the whims of its practioners. For most mathematicians the overwhelming psychological experience is that of grappling with something that kicks back at you. As Yuri Manin put it: Platonism in mathematics is intellectually indefensible, but psychologically inescapable.

But mathematics is done by humans, the argument goes, and does that not make it a human artefact on par with law, art and if you want religion? But physics is also done by humans and no one claims that the physical world out there is but a figment of the physicists imaginations (unless of course you are a die-hard idealist, but even such a die-hard idealist as the Irish bishop Berkeley had a rather nuanced view of those matters). What Havin is concerned with is not, contrary to the the title of his talk, mathematics per se, but the human practice of mathematics, and if you want the sociology of mathematics. As such it is but human, maybe all too human, sharing all the usual defects and short-comings of human activities. The question of mathematical truth is quite different from questions of mathematical beauty and importance. No one really seriously claims that you can work out who is the most distinguished and hence worthy recipient of a prestigious prize in the same way you can deductively work out the truth of a theorem. There are no objective criteria, or better still the objective criteria such as citations of papers are rather irrelevant. The judgement of what is a correct proof is a matter of personal opinion, just as any result in science, but that does not mean that truth is ultimately a personal convention. Theorems that had been thought true may later turn out to be wrong, say by the exhibition of a counterexample. Human judgment, however inescapable, is far from perfect, and so being liable for improvement. Mathematics, like science in general, makes progress.

However, one should not reject the talk of Havin as being totally irrelevant to the platonistic aspects of mathematics. Standards change and when it comes to the more exotic mathematical concepts, more the domains of logicians and philosophers than mathematicians, such as higher cardinalities, it is not entirely clear how to interpret them and what ontological status to give them. Based on what appears as rather arbitrary axioms with no physical tangibility unlike mainstream mathematics, one may be forgiven to regard them as mere figments of human imagination. And as always it is hard to draw lines of demarcation. The most intriguing aspect on the quandaray that presents us was given by Gödel, a die-hard platonist if ever one. He speculated about the existence of natural axioms of set theory, a domain, as suggested above, riddled by a proliferation of different axiomatic interpretaion. Set theory with or without the Axiom oc Choice. The truth of the continuum hypothesis being a question of just choice. Would there be natural axioms, we would recognize them right away, he claimed, true to the claim of Plato that knowledge is something within us but which we have forgotten. Learning is just a question of remembering what we have forgotten[1]. Learning means just recognizing what we have always known, which complies fairly accurately with the psychological experience of encountering a flash of understanding as opposed to working something out by a lengthy computation, be it numerical or deductive. Incidentally deduction is crucial to mathematics, the most convenient way of explaining its realism, but when it comes to understanding and conviction there are other processes at play. Maybe the subject of a forthcoming article.

[1]One may compare with St. Augustine who in his Confessions has an interesting discussion of memory, and how certain facts become known to us, although they cannot have entered the mind through any of the senses, drawing the conclusion that they must already have been present in the mind.

Meeting Grothendieck

Ulf Persson

Luc Illusie *Paris, January 2012*

Följande intervju ägde rum i januari 2012. Jag besökte Paris nedsänd av Bengt Johansson vid NCM för att intervjua Villani och Meyer. Väl nere kom jag på att det inte vore så dumt att intervjua Luc Illusie om Grothendieck. Sagt och gjort jag kontaktade honom och på kort varsel ställde han upp ett par dagar senare. Han tog emot mig i en liten lägenhet i närheten av Place d'Italie som han utnyttjade som arbetslägenhet. Vi hade ett längre samtal om ditt och datt, inte bara om Grothendieck, så den läsare som till äventyrs tycker att de första avsnittet är lite väl tekniskt, skall inte avskräckas. Först och främst är det inte avsett att förstås i matematisk mening utan bara att ge atmosfär, och lättillgängligare teman tas upp senare. Intervjun ägde rum endast ett par månader efter Torsten Ekedahls död och det var naturligt att han skulle dyka upp under konversationen eftersom Illusie som tidigare mentor var mycket tagen av detta.

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?

[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.

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 ?

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

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[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].

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 $Y = \text{Spec } k$.

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 !

[4] see footnote 2

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 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 counterexamples 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

[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.

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

[7] see footnote 2

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 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.

[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.

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 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 ?

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 ?

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

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

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

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

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.

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.

[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.

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]

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

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?

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

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

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: I'm not sure what you mean by "abstract" mathematics. I think there is no such thing as "abstract" versus "concrete" mathematics, it all boils down to how familiar you are with the subject.

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 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 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.

[18] see Weil II, 1.2.11

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

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

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.

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.

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*.

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

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 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.

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[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.

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[26].

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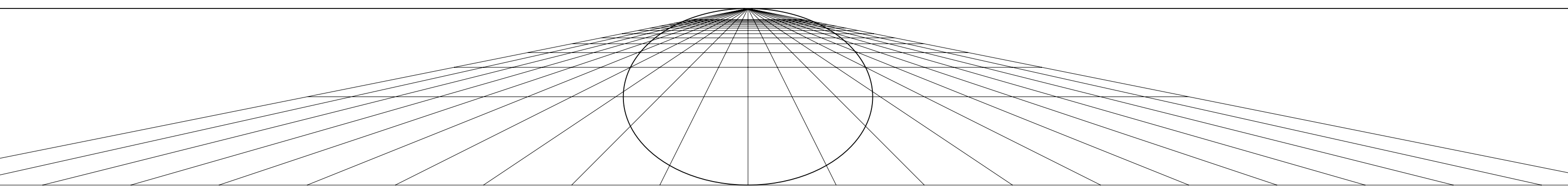
Titelsidans illustration

Ulf Persson



En parabel utgör en oändlig graf, fuktionen $y = ax^2$ är även definierad för godtyckligt stora värden på $|x|$ även om detta är svårt att tänka sig när man ser en graf framför sig. Men inte desto mindre är det möjligt att se hela parabeln i ett enda ögonkast så att säga. Under ICM i Kyoto 1990 råkade jag gå förbi en bild som klargjorde just detta, och på ett så uppenbart sätt att jag förvånades över att jag aldrig, mig medvetet, tänkt på det tidigare. En uppenbarelse av det slag som jag tidigare berört i detta nummer av Bulletinen. Något som man, speciellt i detta fall, redan vetat tidigare men inte varit medveten om det. Amnesia. Bilden ovan visar med all tydlighet att parabeln är ett kägelsnitt som tangerar linjen vid oändligheten. I perspektivläran är den projektiva geometrin förborgad, vilket är klart rent formellt, men tydligen inte helt känslomässigt. Vad vi helt enkelt ser är en projektiv transformation som avbildar en parabel till en ellips. Men det plan vi ser framför oss är hela det euklidiska planet som går mot oändligheten. Punkterna i oändligheten, d.v.s. de på horisonten, finns de rent fysiskt? Grekerna var förtrogna med kägelsnitt, inte bara Apollonius skrev om dem utan även Euklides själv. De koner de hade i tankarna var de som kastades av ljuskällor, eller mera negativt skuggor, de ansåg även att ögat kastade ut strålar när det observerade, inte passivt mottagandes dem, och däri ligger grunden till just perspektivläran. Grekerna var således inte motiverade av solida koner, säg av trä, vilka man av okynne sågade itu längs plan. Kägelsnitten tillhör inte den plana geometrin, dessa kurvor kan ju endast i undantagsfall konstrueras med paare och linjal. Man såg dem som genererade i rymden. Bilden ovan ger helt enkelt två plana snitt av en kon. Det med papperet utgör en ellips, det längs planet mot oändligheten, en parabel. Svårare är det inte.

[26] Alexandre Grothedieck died on November 13, 2014, aged 86

Jean-Christophe Yoccoz död

Michael Benedicks

J.-C. Yoccoz, född 1957 och död i september i år var en av världens mest framstående matematiker och i sitt område, dynamiska system, i nutiden kanske den allra främsta. Han gjorde en traditionell karriär som fransk matematiker. Han studerade vid Lycée Louis-le-Grand och École Normale Supérieure. Han gjorde sin franska militärtjänst[1] vid Instituto Nacional de Matemática Pura e Aplicada i Rio de Janeiro där han fick en stark personlig och professionell relation till Jacob Palis. Han disputerade vid École Polytechnique med Michael Herman som handledare. Han blev professor vid Université Paris Sud 1987 och vid College de France 1997 där han förblev till sin död.

Bland has utmärkelser kan nämnas Salempriset 1988 och Fieldsmedaljen 1994. Han var medlem av Bourbaki och var ledamot av de franska och brasilianska vetenskapsakademierna. Han tilldelades den franska hederslegionen och storkorset av den brasilianska nationella orden för vetenskapliga meriter.

Bland Yoccoz' vetenskapliga genombrott kan nämnas hans fullständiga lösning av två relaterade problem: problemet om linearisering av analytiska cirkeldiffeomorfier och det om linearisering av groddar av analytiska funktioner. Det nödvändiga och tillräckliga villkoret för att en avbilning vars rotationstal har kedjebråkutveckling $\rho = [a_1, a_2, \dots]$, med

$$[a_1, a_2, \dots, a_n] = a_1 + \cfrac{1}{a_2 + \cfrac{1}{a_3 + \ddots \cfrac{1}{a_n}}} = \frac{p_n}{q_n},$$

och nämnarna q_n uppfyller det det sk Brjunovillkoret

$$\sum_{n=1}^{\infty} \frac{\log q_{n+1}}{q_n} < \infty.$$

Arnold löste det lokala problemet (dvs. för analytiska cirkelavbildningar som är tillräckligt nära en stel rotation) i KAM-teorins början. (KAM står för Kolmogorov-Arnold-Moser.) Han antog ett mycket starkare villkor än Brjunovillkoret, att rotationstalet är diofantiskt, dvs att det finns konstanter $C > 0$ och $\tau > 0$ så att

$$\left| \rho - \frac{p_n}{q_n} \right| \geq \frac{C}{q_n^\tau},$$

och ställde också problemet om linearisering i det globala fallet där man inte har något antagande om att avbildningen är nära en stel rotation. Detta problem löstes av Yoccoz' handledare Michael Herman i dennes berömda avhandling. Herman antog att rotationstalet

[1] Det var mycket vanligt att framstående franska matematiker fick göra sin militärtjänst utomlands för att främja fransk kultur. Min kollega Bernard Saint-Donat gjorde på 70-talet sin militärtjänst vid McGill universitetet i Montreal (alltså inte det franska i staden) föreläsande om Weil-hypotesen som nyss hade visats av Deligne. Detta förundrades oss. [red.anm.]

var diofantiskt. I fallet av groddar av analytiska funktioner för diofantiska rotationstal löstes problemet av Siegel. Men den fullständiga lösningen av de båda lineariseringsproblemen gavs alltså av J.-C. Yoccoz.

Andra centrala arbeten av Yoccoz är hans konstruktion av de sk Yoccozpusslen. Han använde dem bl.a. för att bevisa att Mandelbrotmängden är lokalt sammanhängande i alla randpunkter som ej är oändligt renormaliserbara. Om man kunde visa motsvarade resultat för alla randpunkter skulle det följa att Mandelbrotmängden är lokalt sammanhängande vilket bla medför den sk Fatouförmodan: mängden av parametrar c där avbildningen $z \mapsto z^2 + c$ har attraktiva periodiska banor är tät i Riemannsfären $\hat{\mathbb{C}}$. (Här räknas ∞ som en attraktiv fixpunkt.) Fatouförmodan är fortfarande ett öppet problem. Yoccoz publiserade väsentligen inte sina arbeten om Yoccozpussel, kanske för att han inte löste problemet om Mandelbrotmängden fullständigt. Inte desto mindre har Yoccozpusslen visat sig vara en av de viktigaste metoderna i reell och komplex dynamik. Det finns presentationer av metoderna av Milnor och Hubbard, och de genomsyrar viktiga arbeten i området, bla av Lyubich, Graczyk och Światek och den senaste Fieldsmedaljören Avila. I ett Bourbakiseminarium har han presenterat hur metoden kan användas för att bevisa Jakobsons sats om positivt mått för parametrar i den kvadratiska familjen där den motsvarande avbildningen har kaotiskt beteende (absolutkontinuerligt invariant mått).

Ett annat Yoccoz' viktigaste arbeten är arbetet med Carlos Moreira 2001 om stabil skärning av reguljära Kantormängder av stor Hausdorffdimension. Nära relaterat är hans arbeten med Palis om de Kantormängder som uppträder i samband med homoklina bifurkationer.

Under den senaste tiden har han bla ägnat sig åt linearisering av intervallutbytesavbildningar (med Marmi och Moussa) och därmed givet bidrag till ett mycket aktuellt område.

Yoccoz' svenska anknytning

Yoccoz är som framgår en mycket etablerad medlem av den franska matematiska gemenskapen, bla som Bourbakimedlem. Inte desto mindre hade han en smak för konkreta problem som gav en naturlig anknytning till svensk matematik. Han besökte Sverige många gånger. Särskilt uppskattade han Institut Mittag Leffler. Vid ett program i början på 1990-talet lett av Peter Jones och Lennart Carleson gav han tillsammans med de två vetenskapliga ledarna i arbetet "Julia and John", 1993, ett nödvändigt och tillräckligt villkor för att Fatoukomponenerna för ett polynom skall vara sk Johnområden. Han var 2010 års Erlanderprofessor och ledde ett program vid Institut Mittag Leffler tillsammans med Håkan Eliasson, Jörg Schmeling och mig. Våren 2011 var han Erlanderprofessor vid KTH. Han uppskattade svensk natur, särskilt Tyresta Nationalpark, och var en stor vän av Sverige och dess matematik. I år skulle han återkomma till KTH som gästprofessor inom KAW-stiftelsens matematikprogram.

Vi är många som saknar Yoccoz. Han var en utomordentlig matematiker men samtidigt generös med ideer till doktorander, postdocs och samarbetspartners. Han hade en anspråklös attityd även om han givetvis visste sitt värde.

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Gennadi Henkin (1942–2016). Some memories

Christer O. Kiselman

The text is based on a talk at Mikael Passare's Day on 2016 October 05. Every year since 2011 we have honored his memory by organizing a meeting at Stockholm University.

Gennadi Henkin, Mikael Pettersson *1983-10-14*

Introduction

Gennadi Markovič Henkin, Геннадий Маркович Хенкин, was born on 1942 October 26 and died on 2016 January 19. Since 1967 he was employed as a researcher at the Central Economics and Mathematics Institute, CEMI, Центральный Экономо-математический Институт, ЦЭМИ, later as Leading Researcher, главный научный сотрудник.[1] He told me that this position had a very high degree of employment security. The duties were research in economics; no teaching. In 1973 he obtained the degree of Doctor of Sciences in Mathematics at Moscow State University, Московский государственный университет имени М. В. Ломоносова, MGU. Gena moved to Paris and was a professor at Université Pierre-et-Marie-Curie, Paris 6, since 1991. For more about his research see the obituary published by Andrei Iordan (2016).

Gennadi Henkin is best known for his work on complex manifolds and especially for his work on integral formulas in several complex variables. Among his many publications, his two books written with Jürgen Leiterer (1984; 1988) have become standard works of reference.

The results obtained by him are too numerous to be covered even superficially in this little essay. It is easier to list some of his awards. Gena received the Prize of the Moscow Mathematical Society in 1970; the Kondratiev Prize of the Russian Academy of Sciences in 1992 together with Victor Meerovič Polterovič, Виктор Меерович Полтерович, for their work in economics, and the Stefan Bergman Prize (named for Stefan Bergman, 1895–1977) from the American Math Society in 2011 for his work in complex analysis.

[1]Since 1991, CEMI has been reorganized as the Laboratory of Mathematical Economics, CEMI, Russian Academy of Sciences, Лаборатория математической экономики, ЦЭМИ, РАН. However, the acronym ЦЭМИ, CEMI, is still used.

As a person, Gena was always extremely kind, helpful, interested in talking about mathematics and many of its applications, and also very modest and even humble.

MathSciNet

In MathSciNet, the online version of Mathematical Reviews, Gena is the author of 128 items; with addition of the items related to him, there are 139. He has many co-authors, among them Pëtr Polyakov (Петр Поляков, Jürgen Leiterer, and Mikael Passare).

The Mathematics Genealogy Project

In the Mathematics Genealogy Project (indicated by MGP in the list below) five doctors with Gena as advisor are listed. In the English version (CEMI in the list) of the web site at CEMI there are eleven; in the Russian version (ЦЭМИ in the list) also eleven but not the same. They include the five in MGP. Adding them all, we get twelve:

1976 A. V. Romanov, Professor, Moscow (ЦЭМИ, CEMI).
1980 Alexander E. Tumanov, Professor, Illinois (MGP, ЦЭМИ, CEMI).
1981 R. A. Airapetian, Professor, Los Angeles (ЦЭМИ, CEMI).
1994 N. Nivoche, Professor, Nice (ЦЭМИ, CEMI; should be Stéphanie).
1997 Tien Cuong Dinh, Professor, Paris 6, now Professor at National University of Singapore (MGP, ЦЭМИ, CEMI).
1998 P. Dingoyan, MdC, Paris 6 (CEMI).
1999 Stéphane Rigat, MdC, Marseille (MGP, ЦЭМИ, CEMI).
1999 F. Sarkis, MdC, Lille (ЦЭМИ, CEMI).
2000 Bruno Fabre, Post-Doc, Princeton (MGP, ЦЭМИ, CEMI).
2004 Luc Pirio, CR CNRS, Rennes (MGP, ЦЭМИ, CEMI).
2007 A. Irigoyen, Post-Doc, Barcelona (ЦЭМИ, CEMI).
? Mehdi Benchoufi (ЦЭМИ; listed there as unfinished.)

Moscow, 1966

Both Gennadi and I gave short talks at the International Congress of Mathematicians in Moscow in August 1966. His talk had the title

Отсутствие изоморфизма между пространствами гладких функций на отрезке и на квадрате. *(Absence of an isomorphism between spaces of smooth functions on an interval and on a square.)*

He proves that the spaces $C^{(s)}(I)$ and $C^{(p)}(I^n)$ are never isomorphic when $n \geq 2$, $p \geq 0$ and $s \geq 1$. Here I is an interval; I^n the Cartesian product of n copies of I, i.e., an n-dimensional cube. (For $s = p = 0$ it is well known that there exists an isomorphism.)

I do not remember listening to this talk, and I think he was not listening to my talk.

Lev Isaakovič Ronkin (1931–1998) I met and talked with there for the first time. At a lecture I saw Stefan Bergman, who since 1952 had been at Stanford.

Moscow, 1983

I was in Moscow in 1983, thanks to the exchange program between the Soviet Academy of Sciences and the Royal Swedish Academy of Sciences. I visited three institutions in Moscow, and applied also to visit the one in Kharkov where Ronkin was. The last-mentioned proposed visit was not granted.

I arrived on October 04 and visited Gena at CEMI the next day. We had lunch together there and then went to MGU, where Gena gave a talk, accompanied by many comments and a lot of laughter.

On October 06, I visited CEMI again and talked also with Vladimir Lvovič Levin, Владимир Львович Левин (1938–2012).

Natasha Novikova *1983-10-08*

In the evening of October 08 I was invited by Gena to his home. His wife, Natasha Novikova, Наташа Новикова, their son, Roman Novikov, Роман Новиков, and Pётr Polyakov, were also there. Pётr presented his and Gena's results on extending analytic functions from an analytic subset of a polydisk to the whole polydisk. The main point was a discussion of which kind of transversality should be imposed at the boundary.

Natasha, a numerical analyst, was then at an institute making forecasts for earthquakes.

Roman, soon to be 19, studied in the third year at MGU, more precisely at the Chair of Geometry, кафедра геометрии, headed by Sergeĭ Petrovič Novikov, Сергей Петрович Новиков.

Natasha offered me three kinds of berries: калина 'guelder rose', in Swedish 'olvon', *Viburnum opulus*; малина 'raspberry', in Swedish 'hallon', *Rubus idaeus*; and a berry new to me at the time, Облепиха крушиновидная 'common sea-buckthorn', in Swedish 'havtorn', *Hippophaë rhamnoides*. The latter is a real delicacy which I later found in Finland—called *tyrni* in Finnish. It exists also in Sweden, but less often than in Finland.

On October 12 I was again at CEMI and listened to Gena, Pётr and others. Gena talked about q-convex sets. Lunch (обед) with Gena, and then to the Gončar–Šabat Seminar at MGU.

Just after my return to the hotel Mikael phoned me and we went to have evening meal (ужин) at Slavyanskiĭ bazar together with his wife Galina and several other persons.

Mikael, at the time with the original family name Pettersson, had studied at MGU during the whole academic year 1981/82, supported by a scholarship from the Swedish Institute. He had married Galina Lepjosjkina, Галина Лепёшкина, in Moscow on 1982 April 06. After that she had the name Galina Pettersson; in December 1984 both of them changed to the new name Passare. Mikael returned quite often to Moscow.


Pëtr Polyakov, Gennadi Henkin *1983-10-12*

Again on October 14 I was at CEMI, as was Mikael. Gena talked about the relation between q-concave domains and the extension of harmonic functions defined in the real domain into the complex domain. His philosophy was that all partial differential equations in theoretical physics can—or should—be reduced to the Cauchy–Riemann equations together with some algebraic relations.

On October 16 Gena and I visited Vasiliĭ Sergeevič Vladimirov, Василий Сергеевич Владимиров (1923–2012) at the Steklov Institute.

On October 19 I talked at the Vituškin–Gončar–Šabat seminar at MGU. After that Pëtr and I went to listen to Viktor Palamodov, Виктор Паламодов, who talked about Bernstein polynomials. His seminars started at 17:20 and took place in Room 20:17, numbers which reflect the symmetry between time and space, he asserted.

On October 22 I was again invited to Gena's home with Pëtr. In general I talked English with the two, although I knew some Russian since I started to learn the language at the age of thirteen and continued later at Stockholm University College. Pëtr's English was better than Gena's, and it happend a few times that Pëtr had to translate into Russian something I had said.

From all the meetings with Gena and others I have notes.

I gave a talk at MGU (on the definition of the complex Monge–Ampère operator) and one at the Steklov Institute (on the growth of plurisubharmonic functions in infinite-dimensional spaces).

Return to Sweden on October 27.

Invitation to Sweden

In 1984 I planned to invite Gennadi Henkin to Sweden. After careful preparations in cooperation with the Royal Swedish Academy of Sciences and consultations with Swedish researchers, an official letter was sent on 1985 February 18 to the Director of CEMI, Academician Nikolaĭ Prokof′evič Fedorenko, Николай Прокофьевич Федоренко (1917–2006). The inviting institutions were Uppsala University, Institut Mittag-Leffler, the Swedish Mathematical Society, Umeå University, Göteborg University, Linköping University, and Lund University. The visit was to last four weeks in September and October of 1985.

In an earlier letter, dated 1985 January 23 and delivered in person by Håkan Hedenmalm, Gena had thanked me for my efforts and had given me some advice intended to increase the chances of approval.

The Royal Swedish Academy of Sciences sent the invitation with an official recommendation to the USSR Academy of Sciences in a letter of 1985 February 25. Copies were sent to the Swedish Embassy in Moscow and the Soviet Embassy in Stockholm.

In a letter of 1985 June 04 Gena informed me that a visit of one week, September 25 – October 02, had been included in the academy's program. The choice of dates was motivated by the fact that the Swedish Math Society planned a meeting in Göteborg on September 27–28.

Then in a letter dated 1985 July 31, mailed on August 21, and received on August 28, he wrote that the visit had been cancelled:

"The main reason is a big reorganization of our Institute. In particular we have now new director — член-кореспондент АНССР — Валерий Леонович Макаров [Corresponding Member of the Soviet Academy of Sciences, Valeriĭ Leonovič Makarov]. I dream to have opportunity to visit Sweden in future (may be in 1986?)."
I regretted the decision in a letter of August 28, and mentioned that Institut Mittag-Leffler planned to devote the whole academic year 1987/88 to several complex variables.

In a later letter, of 1985 September 12, I mentioned that Mikael planned to go to Moscow on September 20 and that I hoped for Gena's advice concerning themes and invitees for 1987/88.

Institut Mittag-Leffler, 1987/88

Mikael met Gena in Moscow on 1985 September 27 and discussed the Mittag-Leffler year 1987/88, to be organized by John Erik Fornaess and me. Gena put together two long lists of mathematicians he would like to see at the institute: One with 26 names of mathematicians in the Soviet Union (underlining once names of important scientists; twice those of members of the Academy of Sciences), and one with 33 names of mathematicians from other places. Gena himself was to be invited for one or two months.

A year later, on 1986 October 26, he wrote to me that he would be "extremely happy" to visit Institut Mittag-Leffler for one or two months. To improve the chances of approval, he proposed that the invitation be sent to the new president of the Soviet Academy of Sciences, Guriĭ Ivanovič Marčuk, академик Гурий Иванович Марчук (1925–2013), a mathematician. I sent such a letter, signed by John Erik and me, to Marčuk on 1986 November 12, with copies to the Soviet Ambassador to Sweden, Boris Pankin, Борис Дмитриевич Панкин; the Swedish Ambassador to the Soviet Union, Anders Thunborg; the President of the Royal Swedish Academy of Sciences, Sven Johansson; the Director of CEMI, Academician Valeriĭ Leonovič Makarov; and of course to G. M. Henkin.

Gennadi did not come to the Mittag-Leffler year.

Moscow, 1989

I was in Moscow, Ufa and Tashkent in 1989, again thanks to the exchange program between the Soviet Academy of Sciences and the Royal Swedish Academy of Sciences. My itinieray was:

October 16: Uppsala – Moscow; October 25: Moscow – Ufa; October 31: Ufa – Tashkent; November 06: Tashkent – Moscow; November 08: Moscow – Uppsala.
I gave a total of twelve talks in the Soviet Union.

In the home of Gena's mother:
Natasha Novikova, Roman Novikov, Gennadi Henkin, Gennadi's mother *1989-10-17*

On October 17, Gena walked with me to the home of his mother, where we had an evening meal. Gena told me he had plans to go to the US in 1990, and Natasha planned to go to Canada for three months, starting later in 1989.

On October 20 Gena gave a wonderful lecture on the Radon transformation (injectivity, characterization of the range, inversion); on the Fenchel transform of plurisubharmonic functions; on Padé approximation.

In the evening of October 08 I went to Gena, where there were also several other guests, among them Paul Montpetit Gauthier and his wife Sandralee "Sandy" Gauthier with three of their children: Georges, Cathérine and Caroline.

Christer Kiselman, Roman Novikov, Gennadi Henkin *1989-10-17*

Paris, 1992 and 2005

After Gena's move to Paris, I met him several times at various conferences, in particular at the *Colloque P. Dolbeault* in Paris 1992 June 23–26.

In 2005 we were both invited to give talks at the conference in Honor of Henri Skoda, 2005 September 12–16. Gena talked French now, but not with ease.

Trosa, 1997; Saltsjöbaden, 1999; Uppsala, 2006

When Peter Ebenfelt and Mikael Passare organized the first Nordan Conference in 1997 in Trosa, it was only natural that Gena should be invited. His talk on 1997 March 16 had the title *On boundaries of complex analytic varieties.*

Then in 1999, at the Third Nordan Conference held in Saltsjöbaden and organized by Björn Ivarsson, Burglind Juhl-Jöricke, and Maciej Klimek to celebrate my sixtieth birthday, Gena was invited again and talked on *The* $\bar{\partial}$ *equation on singular varieties and projective embeddings of pseudoconcave surfaces.*

In 2006, Gena was invited to give a talk at the Kiselmanfest in Uppsala, May 15–18. His talk on May 16 had the title *Electrical tomography of two-dimensional bordered manifolds and complex analysis.*

At a seminar at MGU *1989-10-18*
Gennadi Henkin, Andreĭ Aleksandrovič Gončar (1931 – 2012)

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Iordan, A[ndrei]. 2016. Gennadi M. Henkin, 1942–2016. *La Gazette des Mathématiciens.* July 2016, N^o 149, 72–73.

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Photos: The photos are taken by the author. The three from 1983 were originally color slides (diapositives) which were printed in 2016. The three from 1989 were printed from color negatives. The prints were all scanned in preparation for this article and made into pdf-files. They were then cropped by the main editor and converted into PostScript files for easier digital manipulation. The original of the Bulletin is converted to a Post Script file, into which those of the pictures are incorporated. Finally the whole thing is converted to pdf.

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Mina minnen av Serguei Shimorin

Håkan Hedenmalm

Serguei Shimorin förolyckades den 18 juli 2016 under en fjällvandring i Abkhasien, en omdisputerad del av Georgien. Han vandrade med två vänner - bröderna Andrei och Roman - och skulle ta sig över forsen Dzhampal som första person i sällskapet. Det hela slutade tyvärr mycket tragiskt. Han blev inte gammal, han var född 1965 i Leningrad. Hans död är en förlust för svensk matematik. Jag vill nu berätta om mina hågkomster relaterade till Serguei och hans vetenskapliga gärning inom matematiken vid både Lunds Universitet och senare KTH.

Hösten 1990 besökte jag Leningrad via akademiernas utbyte, så jag fick ett litet stipendium från Sovjetiska Vetenskapsakademien vilket framförallt ordnade med boende i hotellrum. I Leningrad träffade jag förstås ett antal prominenta deltagare i analysseminariet vid Steklov-institutet som också kallades LOMI (numera POMI), exempelvis Nikolai Nikolski, Nikolai Makarov, Vladimir Peller, Alexei Alexandrov, för att nämna några. Jag minns att detta var en svår tid i Sovjetunionen, och att landet föll samman bara något år senare.

Men gästfriheten var det inget fel på och jag blev hembjuden till både Peller och Nikolski. Vid den här tiden hade precis faktoriseringsmetoden med extremalfunktioner i Bergmanrum utvecklats av mig och andra (jag var stimulerad av Boris Korenblum i SUNY Albany) och jag höll ett par föredrag i ämnet under hösten. Vad jag inte märkte då men förstod senare var att det bland åhörarna fanns en försynt ung man vid namn Serguei Shimorin som lyssnade uppmärksamt. Han var doktorand för Stanislav A. Vinogradov, som själv hade disputerat för Victor P. Havin 1968 i Leningrad. Uppenbarligen gjorde mina framträdanden intryck på Serguei, för något år senare skickade han mig ett preprint med titeln "Factorization of analytic functions in weighted Bergman spaces" vilket arbete senare utkom i tidskriften *Algebra i Analiz* och i engelsk översättning i *St Petersburg Math. J.* år 1994. Arbetet (som väl utgjorde en del av doktorsavhandlingen från 1993 i St-Petersburg) var originellt, speciellt som han hade hittat en sorts pseudodifferentialoperator Δ_α så att motsvarigheten till Greens formel

$$\int_{\mathbb{D}} (h_2 \Delta_\alpha h_1 - h_1 \Delta_\alpha h_2) dA_\alpha = \int_{\partial\mathbb{D}} (h_2 \partial_n h_1 - h_1 \partial_n h_2) ds$$

gäller för enhetsskivan $\mathbb{D}$ och ett intervall av α, där $dA_\alpha(z) = (1 - |z|^2)^\alpha dA(z)$ är det viktade areamåttet. Senare utvecklade Serguei teorin för dessa Δ_α vidare och räknade även ut Greenfunktionen för den viktade biharmoniska operatorn $\Delta(1 - |z|^2)^{-\alpha}\Delta$ på enhetsskivan, med en explicit formel som ger att Greenfunktionen är positiv för $-1 < \alpha < 0$. Frågan om positivitet för biharmoniska Greenfunktioner är delikat med en historia som gå tillbka till exempelvis arbeten av Boggio och Hadamard kring år 1900. Ett annat fascinerande arbete i min uppfattning är "Single-point extremal functions in weighted Bergman spaces", som jag fick mig tillskickat kring år 1996. Där utvecklade Serguei en *ny idé* mellan univalens och divisoregenskaper för enpunktsdivisorer, vilka utgör en motsvarighet till Blaschkefaktorer för Hardyrumsfallet. Vid något tillfälle kring 1994-95 blev jag hembjuden till Serguei och hans familj (hustrun Olga samt barnen Anastasia och Mikhail) och i det sammanhanget undrade jag om han hade deltagit i någon form av matematikolympiad. Den typen av närmast sportslig verksamhet var ju uppmuntrad i Sovjet och Serguei hade tydligen deltagit lyckosamt i något mer lokal sådan olympiadtävling under gymnasietiden. Direkt efter universitetsdiplomet 1987 hade han först arbetat ett par år med programmering och liknande innan han blev doktorand för Vinogradov.

På hösten 1996 var jag senare på konferens i Trondheim orgianiserad av Kristian Seip. Där träffade jag Alexander Borichev, vilken jag tidigare samarbetat med lyckosamt under tiden han var forskarassistent i Uppsala, bland annat har vi gemensamma arbeten i *Acta Math.* och *J. Amer. Math. Soc.* Han hade då lämnat Sverige för Frankrike och nu när jag just hade flyttat själv till Lund från Uppsala föreslog jag att han skulle intressera sig för en kommande utlysning av tjänst i Lund. Han avböjde och föreslog att jag skulle intressera mig för Shimorin som han menade var utmärkt. Så med Borichevs vitsord anställdes Shimorin som forskarassistent i Lund kring 1998 med pengar från NFR (nuvarande VR). Serguei hade läsåret innan varit postdoc i Bordeaux i Frankrike. Lektoratet jag ville att Borichev skulle intressera sig för fick istället senare Alexandru Aleman. När Shimorin var i Lund fick jag honom intresserad av ett projekt att visa att en biharmonisk Greenfunktion var positiv för en allmän vikt som var reproducerande för en punkt och samtidigt logaritmiskt subharmonisk. Detta var förmodat sant men visade sig svårt att visa. Vi lyckades till slut, och Sergueis insats var oumbärlig. Bland annat härledde han en fundamental kärnuppskattning som sedan tillämpades tillsammans med dubbelt applicerat Hele-Shaw-flöde för att nå fram till den önskade positiviteten. Arbetet om Hele-Shaw-flöde gjorde Serguei även viktiga bidrag till. Han var alltid noggrann och sökte eleganta argument när dessa fanns att tillgå. Serguei hade nu kring år 2000 utvecklat en imponerande matematisk förmåga och hans mest citerade arbete "Wold-type decompositions and wandering subspaces for operators close to isometries" i *Crelle* år 2001 tillkom som en spinoff av arbetet med biharmoniska Greenfunktionen. Ett annat arbete, "Approximate spectral synthesis in the Bergman space" publicerat i *Duke Math. J.* år 2000 tillkom i denna produktiva tid. Efter 2002 flyttade både Serguei och jag till KTH och vi fortsatte vårt samarbete, denna gång med fokus på vad som kallas "Brennans förmodan". Serguei hade en avgörande insikt som han först publicerade själv i *IMRN* år 2003 och vi vidareutvecklade tillsammans i ett arbete i *Duke Math. J.* år 2005. År 2004 förärades Serguei med Matematikersamfundets Wallenbergspris (delat med Julius Borcea).

Serguei intresserade sig även för problem som var mer tydligt operatorteoretiska, till exempel "Commutant lifting theorem" och "Complete Nevanlinna-Pick lernels". Jag drar mig till minnes att jag hört från amerikanska kollegor att gruppen kring Donald Sarason i Berkeley gick igenom ett av Sergueis arbeten, och när slutklämmen i beviset kom lär Sarason ha utbristit: "That was smart!"

Som vetenskapsman var Serguei originell och tekniskt duktig. Men som person var han mycket privat och snarast blyg eller ödmjuk. Han intresserade sig inte egentligen för sin karriär utan var snarare en "konstnär inom matematiken", som ibland hittade en vacker blomma och ville visa upp dess skönhet. I vårt universitetssystem har en sådan person uppenbarligen svårt att komma till sin rätt tyvärr. Han var universitetslektor på KTH men sökte mig veterligen aldrig befordran till professor. I universitetsvärlden tenderar vi ju att tänka hierarkiskt och karriärorienterat, och man måste vara beredd att "ta för sig" när tillfälle ges. Serguei lade ned stor möda på alla sina arbetsuppgifter, speciellt föreläsningarna och han var en mycket uppskattad lärare. Hans vetenskapliga förmåga var inte sämre än många professorers men han "tog så lite plats". Jag anser att vi borde kunna ge större plats till så begåvade individer som Serguei. Som en fotnot vill jag även berätta att Serguei utvecklade en passion för fotografi, och hans konstnärliga fotografiska verk kan beskådas på photosight.ru under pseudonymen "Serge de la Mer".

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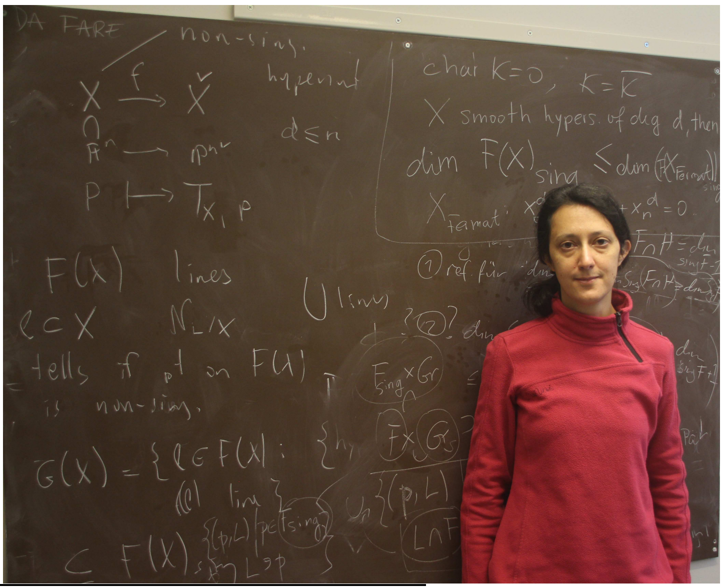
Naturvetenskapliga fakultetens forskningspris

Ulf Persson

Every year the Natural Science Faculty of the University awards a research prize of currently at 250'000 SEK to a young researcher, meaning one who has received his or her Ph.D. not longer ago than twelve years. Rarely is the prize given to a mathematician. However, this year it will be awarded to Orsola Tommasi, who joined the department of mathematics at Chalmers/GU in April this year. Dr. Tommassi, who works in Algebraic Geometry, more specifically on moduli spaces of curves, a very classical subject, grew up in Trieste[1], but moved in 2001 to Nijmegen where she got a Ph.D. in 2005 under Joseph Steenbrink. After that she moved to Mainz, spent some time at Mittag-Leffler but the bulk of her post-doctoral years has been spent in Hannover, followed by a brief stint at Darmstadt before moving to Sweden. As is not unusual for young academics she and her Dutch partner — Remke Kloosterman — have to struggle with the so called two-body problem, he at the moment teaching at Padua.

She has only been in Sweden for six months and her contact with the country has mostly been through academics, who tend to be similar all over the world, so she cannot really make a fair assessment of Sweden compared to the other three countries she has extended experience of. Academically she finds Sweden much closer to the Netherlands (as to local organization) and Italy (national centralized organization) than Germany. In Germany there is a rather pronounced difference between professors and non-professors, which is much less important here.

The prize money is not meant for personal consumption but she can invite people as well as travel herself, which she looks forward to.



Orsola Tommasi in her office
October 20, 2016

[1]City stemming from Roman times. The pre-Roman name Tergeste is supposedly etymologically related to Old Slavonic тьrgъ meaning market (torg, as in Scandinavian languages) cf Turko (Finland) and Torgau (Germany). A traditionally cosmopolitan city (James Joyce lived there) and formerly the main port of the Austrian-Hungarian empire, it was ceded to Italy after the First World War.

Meeting of the Catalan, Spanish and Swedish Mathematical Societies (CAT-SP-SW-MATH)

Det första gemensamma mötet mellan Katalanska, Spanska och Svenska matematikersamfunden (CAT-SP-SW-MATH) äger rum vid Institutionen för matematik och matematisk statistik, Umeå universitet, 12 - 15 juni 2017.

Mötet är tänkt som en bred mötesplats för matematiker från olika teoretiska och tillämpade inriktningar.

Programmet består av tretton plenarieföredrag, som representerar stor matematisk bredd och specialsessioner ägnade åt specialiserade teman.

Programkommitté:
Mats Andersson (Chalmers/ Göteborgs universitet)
María Àngeles Gil (Universidad Oviedo)
Gemma Huguet (Universitat Politècnica de Catalunya)
Ignasi Mundet (Universitat Barcelona)
Joaquín Pérez (Universidad Granada)
Sandra di Rocco (KTH) **Chairperson**
Xavier Tolsa (Universitat Autonoma Barcelona)
Tatyana Turova (Lunds universitet)
Juan Luis Vázquez (Universidad Autónoma Madrid)
Välkomna!
För mer information se `http://liu.se/mai/catspsw.math/`

Proposal of Special Sessions to the Meeting of the Catalan, Spanish, and Swedish Math Societies (CAT-SP-SW-MATH)

We will invite you to organise a Special Session within the Meeting of the Catalan, Spanish and Swedish Math Societies, to be held in UmeÃ¥ (Sweden), 12-15 June 2017. Special Sessions are scheduled to the afternoons of June 13 to 15. Proposals for Special Sessions should be sent, not later than **October 28 2016**, to the meeting's scientific committee.

Each Special Session can take one, two or three afternoons. A Special Session would consist of at least six scheduled twenty-minute talks in a given subject area (with ten-minute breaks in between. However, any talk may be a fifty-minute (equivalent to a duration of two twenty-minute talks).

Each Special Session should be proposed by at least two organizers, members of some of the organizing Societies of the Meeting. We encourage proposal which are likely to be of interest to members of more than one Society.

Proposals should include session's title and organizers (names, affiliations, email addresses, with one organizer designated as the contact person for all communications about the session), a paragraph of description about the topic, a list of potential speakers (these speakers need not have confirmed participation), and the duration of the Special Session. This information should be sent to `catspsw.math@mai.liu.se`

Lokala Nyheter

Umeå.

1. Mathias Norqvist har disputerat den 16 september i matematikdidaktik.

Titel: *On Mathematical Reasoning - Being told or Finding out*

Fakultetsopponent: Kristina Juter, biträdande professor på Högskolan Kristianstad.

Huvudhandledare: Johan Lithner.

2. Lars-Daniel Öhman har hållit sin docentföreläsning den 6 september,

Titel: *Vad jag talar om när jag talar om de naturliga talen*

3. Niklas Lundström har hållit sin docentföreläsning den 20 september.

Titel: *Bifurkationer i dynamiska system*

4. Matematisk statistik som nu är ett ämne inom institutionen firar sitt 50-årsjubileum den 13-14 oktober (det var en egen institution under lång tid [1])

Mälardalens Högskola.

Fredrik Jansson (tidigare på LiU: [2]) anställts som universitetslektor i matematik/tillämpad matematik vid avdelningen för Tillämpad matematik vid Mälardalens högskola.

Linköping

Thomas Karlsson, Natan Kruglyak och Leif Melkersson har gått i pension.

Abubakar Mwasa och Jonas Granholm är nya doktorander i matematik.

Doktorsavhandlingar

Yixin Zhao

Titel: *On the Integration of Heuristics with Column-Oriented Models for Discrete Optimization* (optimeringslära)

Sonja Radosavljevic

Titel: *Permanence of age-structured populations in a spatio-temporal variable environment*

Samira Nikkar

Titel: *Stable high order finite difference methods for wave propagation and flow problems on deforming domains* (beräkningsmatematik)

Licentiat Adson Banda

Titel: *Half Exact Coherent Functors over PIDs and Dedekind Domains*

Göteborg.

Nyanställda

Ksenia Fedosova, post doc, Analys och sannolikhetsteori

Sebastian Herrero, post doc, Algebra och geometri

Magne Nordaas, post doc, Tillämpad matematik och statistik

Jules Lamers, post doc, Analys och sannolikhetsteori

Johan Björklund, gästlärare, Analys och sannolikhetsteori

Lucas Sacchetto, post doc, Algebra och geometri

Qasim Ali, post doc, Tillämpad matematik och statistik

Jakob Björnberg, bitr lektor, Analys och sannolikhetsteori

Disputationer

Matteo Molteni,

Titel: *On numerics for deterministic and stochastic evolution problems*, 2016-05-30

Roza Maghsood,

Titel: *Hidden Markov models for detecting steering events and eva luating fatigue damage*, 2016-09-23

Licentiater

Anna Persson,

Titel: *A generalized finite element method for linear thermoelasticity*, 2016-05-27

Ivar Simonsson,

Titel: Bayesian networks: exact inference and applications in forensic statistics, 2016-06-03

Gustav Kettil,

Titel: *A Novel Fiber Interaction Method for Simulation of Early Paper Forming*, 2016-09-07

Frida Svelander,

Titel: *Robust intersection of hexahedral meshes and triangle meshes with applications in finite volume methods*, 2016-09-08

Tim Cardilin,

Titel: *Data-driven modeling of combination therapy in oncology*, 2016-10-07

Sebastian Jobjörnsson,

[1] http://www.math.umu.se/samverkan/matematisk-statistiks-50-arsjubileum-13-14-oktober-2016/

[2] https://liu.se/ias/medarbetare/jansson-fredrik/presentation?l=sv

Titel: *Optimisation of Clinical Trials using Bayesian Decision Theory*, 2016-10-17

Befordran(till docenter)

Simone Calogero, Julie Rowlett, Martin Westerholt-Raum, Annika Lang, Kin Ceoung Sou

Nya doktorander

Felix Held, FCC

Henrik Imberg, Tillämpad matematik och statistik

João Pedro Paulos, Analys och sannolikhetsteori

Jiacheng Xia, Algebra och geometri

Håkon Strand Bølviken, Algebra och geometri

Efthymios Karatzas, Tillämpad matematik och statistik

Quanjiang Yu, Tillämpad matematik och statistik

Robert Forslund, Arcam

Lund.

Doktorsavhandlingar

19/8, Viktor Nikitin,

Titel: *Fast Radon Transforms and Reconstruction Techniques in Seismology*

9/9, Kerstin Johnsson,

Titel: *Structures in High-Dimensional Data: Intrinsic Dimension and Cluster Analysis*

30/9, Dzmitry Sledneu,

Titel: *Studies in Efficient Discrete Algorithms*

18/10, Simon Burgess,

Titel: *Minimal Problems and Applications in TOA and TDOA Localization*

28/10, Matilda Landgren,

Titel: *Analysis of Medical Images: Registration, Segmentation and Classification*

4/11, Fredrik Ekström,

Titel: *Asymptotics of the Scenery Flow and Properties of the Fourier Dimension*

25/11, Erik Henningsson,

Titel:*Spatial and Physical Splittings of Semilinear Parabolic Problems*

Licentiatavhandlingar.

7/6, Fatemeh Mohammadi,

Titel: *Starters for Multistep Methods in the Solution of Discontinuous ODEs*

29/9, Azahar Monge,

Titel: *The Dirichlet–Neumann Iteration for Unsteady Thermal Fluid Structure Interaction*

Befordran

Umberto Picchini, docent.

Eskil Hansen, professor.

Nyanställning

Umberto Picchini, vikarierande lektor t.o.m. 2017–12–31.

Nya doktorander

Ida Arvidsson, Wafaa Assaad, Samuel Wiqvist, Maria Priisalu, David Gillsjö, Douglas Svensson Seth, Gabrielle Flood

KALENDARIUM

(Till denna sida uppmanas alla, speciellt lokalombuden, att inlämna information)

Författare i detta nummer

Luc Illusie Student till Grothendieck. Förknippad med Krystalin Kohomologi. Verksam i Paris.

Viktor Havin Rysk matematiker verksam i St.Petersburg. Leder sedan 1963 ett med Steklov-institutet gemensamt seminarium om operator-teori.

Michael Benedicks Nyligen pensionerad professor vid KTH. Arbetar inom harmonisk analys, främst dynamiska system.

Håkan Hedenmalm Professorvid KTH.Student till Yngve Domar.

Innehållsförteckning

Notiser