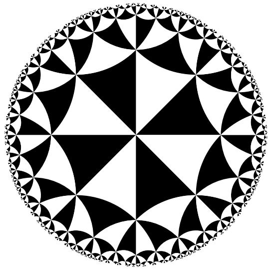
INFOMAT



Utgitt av
Norsk Matematisk Forening

August 2003

Augustnummeret av INFOMAT er litt anderledes enn vanlig. Dette fordi vi trykker et lengre intervju med Abelprisvinneren Jean-Pierre Serre. Det har medført litt plassmangel, og vi har utsatt noe til septembernummeret. Det er flere ledige stillinger ved NTNU med søknadsfrist 21. august, så for de av dere som leser det i tide: ta kontakt med eventuelle kandidater med én gang!

Nytt fra instituttene

Innholdet baserer seg på innsendt informasjon fra enkeltmedlemmer og fra instituttene. Dersom du har stoff som du mener passer for INFOMAT, send et brev til

`infomat@math.ntnu.no`

Matematisk institutt, Universitetet i Bergen

Pris til Dag Tjøstheim Sammen med to medforfattere er professor Tjøstheim tildelt Tjalling C. Koopmans Econometric Theory Prize 2003. Prisen deles ut hvert tredje år. Se

`http://korora.econ.yale.edu/et/award/tck.htm` og
`http://korora.econ.yale.edu/et/award/tck-past.htm#2000`

Vi gratulerer med prisen!

Institutt for matematiske fag (IMF), NTNU

Ledige stipendiatstillinger. Det er lyst ut 4 stipendiatstillinger ved IMF. Én er knyttet til NTNUs satsingsområde medisinsk teknologi, to er knyttet til satsingsområdet IKT og én ligger innenfor matematikk/statistikk generelt. Søknadsfrist for samtlige er 21. august 2003. Utlysingstekstene finnes på disse nettsidene:

Stillingen knyttet til medisinsk teknologi:

`http://innsida.ntnu.no/getfile.php/vedlegg/3f28c6afd4ba35.69914074/293+fulltekst+norsk.doc`

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`http://innsida.ntnu.no/getfile.php/vedlegg/3f2a1cfdb50f45.69914074/294+fulltekst+norsk.doc` og
`http://innsida.ntnu.no/getfile.php/vedlegg/3f2a215276a905.69914074/301+fulltekst+norsk.doc`

Stillingen innenfor matematikk/statistikk generelt:

`http://innsida.ntnu.no/nettopp_lesmer.php?kategori=nyheter&dokid=3f2a1dd8c9f7e4.40201086`

Ledig postdoktorstilling. Det er også lyst ledig postdoktorstilling i innenfor satsingsområdet medisinsk teknologi. Fullstendig utlysingstekst finnes på:

`http://innsida.ntnu.no/getfile.php/vedlegg/3f2a224eaf8455.69914074/302+fulltekst+norsk.doc`

Søknadsfrist: 21. august 2003.

Gjesteforelesere ved instituttet i august er N. Christopher Phillips, University of Oregon, professor Roger J-B Wets, University California, Davis og dr. Juan José Moreno Balcazar, Universidad de Almería.

Matematisk institutt, Universitetet i Tromsø

Avlagt dr. scient. eksamen. Hugues Verdure. Veileder, Loren Olsen: *Factorization patterns of division polynomials of elliptic curves defined over a finite fields.*

Sammen med EMS Newsletter og Matilde trykker INFOMAT intervjuet med Jean-Pierre Serre som Martin Raussen og Christian Skau gjorde i juni.

Interview with

Jean-Pierre Serre

during the Abel Prize Celebrations,
Oslo, June 2, 2003

Martin Raussen, Aalborg University, Denmark

Christian Skau, NTNU, Trondheim

Topology

First of all, we would like to congratulate you on winning the first Abel Prize.

You started your career with a thesis which was centered in algebraic topology. At that time this was – at least in France – a very new discipline and not one of the major areas. What made you choose this topic?

I was participating in the Cartan Seminar, on Algebraic Topology. But Cartan did not suggest research topics to his students: they had to find one themselves; after that he would help them. This is what happened to me. I found that Leray's theory (about fibre spaces and their spectral sequence) could be applied to many more situations than was thought possible, and that such an extension could be used to compute homotopy groups.

I think it is fair to say that the methods and results that you created in your thesis revolutionized homotopy theory and shaped it in its modern look.

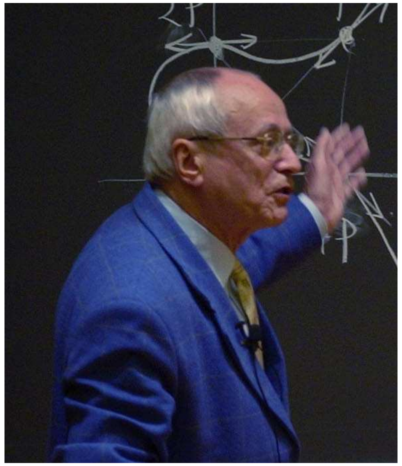
They certainly opened up lots of possibilities. Before my thesis, homotopy groups of spheres were almost entirely terra incognita; one did not even know that they are finitely generated!

One interesting aspect of the method I introduced was its algebraic character. In particular, one could make "local" computations, where the word "local" here is taken as in number theory: relative to a given prime number.

I have heard that one of the crucial points in this story was to identify something that looks like a fibre space without being it on the nose?

Indeed, to apply Leray's theory I needed to construct fibre spaces which did not exist if one used the standard definition. Namely, for every space X, I needed a fibre space E with base X and with trivial homotopy (for instance contractible). But how to get such a space?

One night in 1950, on the train bringing me back from our summer vacation, I saw it in a flash: just take for E the space of paths on X (with fixed origin a), the projection $E \to X$ being the evaluation map: path $\to$ extremity of the path. The fibre is then the loop space of (X, a). I had no doubt: this was it! So much so that I even waked up my wife to tell her... (Of course, I still had to show that $E \to X$ deserves to be called a "fibration", and that Leray's theory applies to it. This was purely technical, but not completely easy.) It is strange that such a simple construction had so many consequences.



(Foto: Abelprisen)

Work Themes and Work Style

This story about your sudden observation is reminiscent of an episode on Poincaré's flash of insight when stepping into a tramway which is told in Hadamard's booklet "The Psychology of Invention in the Mathematical Field". Do you often rely on sudden inspirations or would you rather characterise your work style as systematic? Or is it a mixture?

There are topics on which I come back from time to time (ℓ-adic representations, for instance), but I do not do this in a really systematic way. I rather follow my nose.

As for flashes, like the one Hadamard described, I have had only two or three in more than 50 years. They are wonderful... but much too rare!

These flashes come after a long effort, I guess?

I would not use the word "effort" in that case. Maybe a lot of thinking. It is not the conscious part of the mind which does the job. This is very well explained in Littlewood's charming book "A Mathematician's Miscellany".

Most of your work – since the "topology years" – has been devoted to number theory and to algebraic geometry.

You see, I work in several apparently different topics, but in fact they are all related to each other. I do not feel that I am really changing. For instance, in number theory, group theory or algebraic geometry, I use ideas from topology, such as cohomology, sheaves and obstructions.

From that point of view, I especially enjoyed working on ℓ-adic representations and modular forms: one needs number theory, algebraic geometry, Lie groups (both real and ℓ-adic), q-expansions (combinatorics style)... A wonderful mélange.

Do you have a geometric or rather an algebraic intuition and way of thinking – or both?

I would say algebraic, but I understand the geometric language better than the purely algebraic one: if I have to choose between a Lie group and a bi-algebra, I choose the Lie group! Still, I don't feel I am a true geometer, such as Bott, or Gromov.

I also like analysis, but I can't pretend to be a true analyst either. The true analyst knows at first sight what is "large", "small", "probably small" and "provably small" (not the same thing). I lack that intuitive feeling: I need to write down pedestrian estimates.

You have had a long career and you have been working on a lot of different subjects. Which of the theories you have created or of the results you have obtained do you like most? Which are most important to you?

A delicate question. Would you ask a mother which of her children she prefers?

All I can say is that some of my papers were very easy to write, and some others were truly difficult. In the first category, there is FAC ("faisceaux algébriques cohérents"). When I wrote it, I felt that I was merely copying a text which already existed; there was almost no effort on my part. In the "difficult" category, I remember a paper on open subgroups of profinite groups, which gave me so much trouble that, until the very end, I was not sure whether I was proving the theorem or making a counter-example! Another difficult one was the paper dedicated to Manin where I made some very precise (and very daring) conjectures on "modular" Galois representations (mod p); this one was even painful; after I had finished it, I was so exhausted that I stopped publishing for several years.

On the pleasure side, I should mention a paper dedicated to Borel, on tensor products of group representations in characteristic p. I had been a group theory lover since my early twenties, and I had used groups a lot, and even proved a few theorems on them. But the theorem on tensor products, obtained when I was in my late sixties, was the first one I really enjoyed. I had the feeling that Group Theory, after a 40 years courtship, had consented to give me a kiss.

You have been active in the mathematical frontline for more than 50 years. Hardy has made the often quoted remark that "Mathematics is a young man's game". Is that altogether wrong – aren't you a perfect counterexample?

Not a perfect one: have you noticed that most of the quotations of the Abel Prize are relative to things I had done before I was 30?

What is true is that people of my generation (such as Atiyah, Borel, Bott, Shimura...) keep working longer than our predecessors did (with a few remarkable exceptions such as Elie Cartan, Siegel, Zariski). I hope we shall continue.

Relations to mathematical history

Since you won the Abel Prize, I would like to ask some questions drawing the line back to Abel's time. The algebraic equations that Abel and Galois studied coming from the transformation theory of elliptic functions turned out to be very important much later for the arithmetic theory of elliptic curves. What are your comments on this remarkable fact, especially in connection with your own contribution to this theory?

Yes, elliptic curves are very much in fashion (with good reasons, ranging from Langlands' program to cryptography). In the 60s and 70s I spent a lot of time studying their division points (a.k.a. Tate modules) and their Galois groups. A very entertaining game: one has to combine information coming from several different sources: Hodge–Tate decompositions, tame inertia, Frobenius elements, finiteness theorems à la Siegel,... I like that.

Hermite once said that Abel had given mathematicians something to work upon for the next 150 *years. Do you agree?*

I dislike such grand statements as Hermite's. They imply that the person who speaks knows what will happen in the next century. This is hubris.

Abel writes in the introduction of one of his papers that one should strive to give a problem a form such that it is always possible to solve it. Something which he claims is always possible. And he goes on saying that by presenting a problem in a well-chosen form the statement itself will contain the seeds of its solution.

An optimistic point of view! Grothendieck would certainly share it. As for myself, I am afraid it applies only to algebraic questions, not to arithmetic ones. For instance, what would Abel have said about the Riemann hypothesis? That the form in which it is stated is not the good one?

The role of proofs

When you are doing mathematics, does it happen that you know something is true even before you have the proof?

Of course, this is very common. But one should distinguish between the genuine goal (say, the modularity of elliptic curves, in the case of Wiles), which one feels is surely true, and the auxiliary statements (lemmas, etc), which may well be untractable (as happened to Wiles in his first attempt) or even downright false (as happened similarly to Lafforgue).

Do proofs always have a value in

themselves? I am thinking of, e.g., the proof of the four colour theorem.

We are entering a grey area: computer-aided proofs. They are not proofs in the standard sense that they can be checked by a line by line verification. They are especially unreliable when they claim to make a complete list of something or other.

[I remember receiving in the 90s such a list for the subgroups of given index of some discrete group. The computer had found, let us say, 20 of them. I was familiar with these groups, and I easily found "by hand" about 30 such. I wrote to the authors. They explained their mistake: they had made part of the computation in Japan, and another part in Germany, but they had forgotten to do some intermediate part... Typical!]

On the other hand, computer-aided proofs are often more convincing than many standard proofs based on diagrams which are claimed to commute, arrows which are supposed to be the same, and arguments which are left to the reader.

What about the proof of the classification of the finite simple groups?

You are pushing the right button. For years, I have been arguing with group theorists who claimed that the "Classification Theorem" was a "theorem", i.e. had been proved. It had indeed been announced as such in 1980 by Gorenstein, but it was found later that there was a gap (the classification of "quasithin" groups). Whenever I asked the specialists, they replied something like: "Oh no, it is not a gap, it is just something which has not been written, but there is an incomplete unpublished 800 pages manuscript on it".

(Foto: Abelprisen)

For me, it was just the same as a "gap", and I could not understand why it was not acknowledged as such. Fortunately, Aschbacher and Smith have now written a long manuscript (more than 1200 pages) in order to fill in the gap. When this will have been checked by other experts, it will be the right moment to celebrate.

But if the proof is 1200 *pages long, what use is it for?*

As a matter of fact, the total length of the proof of the classification is much more than 1200 pages: about 10 times more. But that is not surprising: the mere statement of the theorem is itself extremely long, since, in order to be useful, it has to include the detailed description, not only of the Chevalley groups, but also of the 26 sporadic groups.

It is a beautiful theorem. It has many very surprising applications. I don't think that using it raises a real problem for mathematicians in other fields: they just have to make clear what part of their proof depends on it.

Important mathematical problems

Do you feel that there are core or mainstream areas in mathematics – are some topics more important than others?

A delicate question. Clearly, there are branches of mathematics which are less important; those where people just play around with a few axioms and their logical dependences. But it is not possible to be dogmatic about this. Sometimes, a neglected area becomes interesting, and develops new connections with other branches of mathematics.

On the other hand, there are questions which are clearly central for our understanding of the mathematical world: the Riemann hypothesis and the Langlands program are two obvious cases. There is also the Poincaré conjecture – which may well stop being a conjecture, thanks to Perelman!

Do you have more information or a hunch about the correctness of the proof?

Hunch? Who cares about hunches?

Information? Not really, but I have heard that people at IHES and MIT are very excited about this sketch of proof. An interesting aspect of Perelman's method is that it uses Analysis, for what is a purely topological problem. Very satisfying.

We moved already a little into the future with this discussion of the Poincaré conjecture. Which important mathematical problems would you like to see attacked and solved in the near future? In particular, do you agree with the primary importance of the Clay Millenium Prize Problems?

Ah, the million dollars Clay problems! A strange idea: giving so much money for one problem... but how can I criticize it, just after having received the Abel prize? Still, I feel there is some risk involved, namely that people would shy from discussing their partial results, as already happened ten years ago with Fermat's theorem.

As for the choice of questions made by the Clay Institute, I feel it is very good. The Riemann hypothesis and the Birch & Swinnerton–Dyer conjecture are rightly there. The Hodge conjecture, too; but for a different reason: it is not clear at all whether the answer will be yes or no: what will be very important will be to decide which (I am hoping, of course, that it will not turn out to be undecidable...). The $P = NP$ question belongs to the same category as Hodge, except that there would be many more applications if the answer turned out to be "yes".

Could you think of any other problems of the same stature?

I already told you that the Langlands program is one of the major questions in mathematics nowadays. It was probably not included in the Clay list because it is very hard to formulate with the required precision.

Besides your scientific merits, you are also known as a master expositor, as we could witness during your lecture today.

Thanks. I come from the South of France, where people like to speak; not only with their mouth, but with their hands, and in my case with a piece of chalk.

When I have understood something, I have the feeling that anybody else can understand it too, and it gives me great pleasure to explain it to other mathematicians, be they students or colleagues.

Another side of the coin is that wrong statements make me almost physically sick. I can't bear them. When I hear one in a lecture I usually interrupt the speaker, and when I find one in a preprint, a paper or in a book I write to the author (or, if the author happens to be myself, I make a note in view of a next edition). I am not sure this habit of mine has made me very popular among lecturers and authors...

Accessibility and importance of mathematics

Mathematics witnesses an explosion of subjects and disciplines making it difficult to master even the minor disciplines. On the other hand – as you have demonstrated today in your lecture – it is very important that disciplines cross-fertilize each other. How can young mathematicians, in particular, cope with this explosion of knowledge and come up with something new?

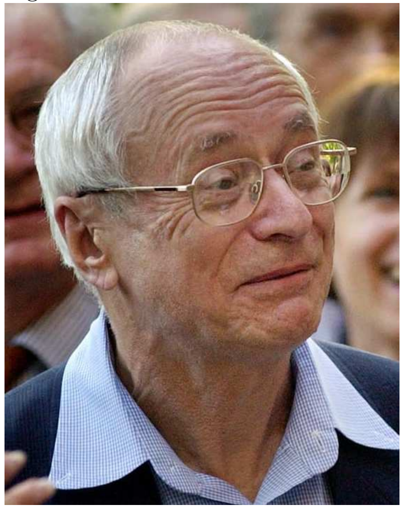
Oh yes, I have already been asked that question in my Singapore interview, reproduced by Intelligencer. My answer is that, when one is truly interested in a specific question, there is usually very little in the existing literature which is relevant. This means you are on your own.

As for the feeling of "explosion" of mathematics, I am convinced that Abel felt the same way when he started working, after Euler, Lagrange, Legendre and Gauss. But he found new questions and new solutions. It has been the same ever since. There is no need to worry.

Another current problem is that many young and talented people – and also public opinion leaders – don't think that mathematics is very exciting.

Yes. Sadly enough, there are many such examples. A few years ago, there was even a French minister of Research who was quoted as saying that mathematicians are not useful any more, since now it is enough to know how to punch a key on a computer. (He probably believed that keys and computer programs grow on trees...)

Still, I am optimistic about young people discovering, and being attracted by, mathematics. One good aspect of the Abel festivities is the Norwegian Abel competitions, for high school students.



(Foto: Abelprisen)

Sports and literature

Could you tell us a little about your interests besides mathematics?

Sports! More precisely: skiing, ping-pong, and rock climbing. I was never really good at any of them (e.g. when I skied, I did not know how to slalom, so that I would rather go "schuss" than trying to turn); but I enjoyed them a lot.

As luck has it, a consequence of old age is that my knees are not working any more (one of them is even replaced by a metal-plastic contraption), so that I had to stop doing any sport. The only type of rock-climbing I can do now is a vicarious one: taking friends to Fontainebleau and coaxing them into climbing the rocks I would have done ten years ago. It is still fun; but much less so than the real thing.

Other interests:

- movies ("Pulp Fiction" is one of my favourites – I am also a fan of Altman, Truffaut, Rohmer, the Coen brothers...);
- chess;
- books (of all kinds, from Giono to Böll and to Kawabata, including fairy tales and the "Harry Potter" series).

Prof. Serre, we thank you for this interview on behalf of the Danish and the Norwegian Mathematical Societies.