INFOMAT

August 2006

Kjære leser!

Da er vi så vidt i gang med et nytt semester og nye studenter strømmer til våre universiteter og høgskoler. Selv om ikke alle kaster seg over matematikkprogrammene som sine førstevalg, er det opplagt at faget er kommet ut av bølgedalen hva gjelder popularitet blant utdanningssøkerne. Antall ja-svar til programmene tyder på det.

I denne utgaven har vi tatt med hele intervjuet som Christian Skau og Martin Raussen gjorde med Abelprisvinneren Lennart Carleson. Carleson har alltid vært opptatt av matematikkens stilling i samfunnet, noe han også gir uttrykk for i dette intervjuet.

Under sommerens Matematikkolympiade fikk Norge en bronsemedalje ved Jørgen Vold Rennemo. Siden mange av INFOMATs lesere liker en utfordring tar vi med en av oppgavene fra finalens andre dag. Lykke til, og husk at du har bare ca. 90 minutter til disposisjon.

hilsen
Arne B.

ICM 2006

Tirsdag 22. august åpnes den internasjonale matematikerkongressen i Madrid. En av hovedattraksjonene på kongressen er utdelingen av Fields-medaljen. Denne prestisjetunge utmerkelsen ble første gang utdelt i Oslo i 1936 og siden den gang har mange av det forrige århundredets mest betydningsfull matematikere mottatt medaljen. I år nevnes stadig oftere russeren Perelman som storfavoritt ettersom hans bevis for Poincaré-formodningen nå begynner å bli fullstendig godtatt. INFOMAT kommer tilbake med en fyldig reportasje fra kongressen i september-utgaven.

I år skal det også deles ut en ny pris i tillegg til Fields-medaljen og Rolf Nevalinna-prisen (matematiske aspekter ved informasjonsvitenskap). Den nye prisen er oppkalt etter Carl Friedrich Gauss og tildeles for *outstanding mathematical contributions that have found significant applications outside of mathematics.* Etableringen av denne prisen er et godt uttrykk for den økende betydningen matematikk har i stadig større deler av det moderne samfunnet.

IMO 2006, OPPGAVE 5

La $P(x)$ være et polynom av grad $n > 1$ med heltallige koeffisienter, og la k være et positivt heltall. Betrakt polynomet

$$Q(x) = P(P(\ldots P(P(x))\ldots)),$$

med P skrevet k ganger. Vis at det finnes høyst n forskjellige heltall t slik at $Q(t) = t$.

INFOMAT kommer ut med 11 nummer i året og gis ut av Norsk Matematisk Forening. Deadline for september-utgaven er 10. september kl. 2400. Stoff til INFOMAT sendes til

infomat at math.ntnu.no

Foreningen har hjemmeside http://www.matematikkforeningen.no/INFOMAT

Ansvarlig redaktør er Arne B. Sletsjøe, Universitetet i Oslo.

NYTT FRA INSTITUTTENE

Nye doktorgrader:
Xavier Raynaud disputerte 26. juni. Hans avhandling har tittelen *On a shallow water wave equation.*

Nyansettelser:
Instituttet har ansatt følgende undervisningsvikarer høsten 2006:
Halvard Fausk, førsteamanuensis
Pål H. Johansen , universitetslektor
Helge Maakestad, førsteamanuensis
Eirik Mo, universitetslektor
Haaken Annfelt Moe, universitetslektor
Joakim Peterson, førsteamanuensis
Xavier Raynaud, førsteamanuensis
Hans Jakob Rivertz, førsteamanuensis
Bård Skaflestad, førsteamanuensis
Janne Svensson, øvingslærer.

Forskningspermisjoner høst 2006/vår 2007:
Førsteamanuensis **Anne Kværnø**, høst og vår.
Professor **Steinar Engen**, høst og vår
Bo Henry Lindqvist, høst og vår
Professor **Arvid Næss**, høst og vår
Professor **Håvard Rue**, høst og vår
Christian Skau, høst og vår
Førsteamanuensis **Finn F. Knudsen**, høst og vår
Professor **Nils A. Baas,** vår
Førsteamanuensis **Håkon Tjelmeland**, høst.

Nyansettelser:
Frank Proske er ansatt som førsteamanuensis i matematisk finans fra 1. august.
Atle Jensen er ansatt som førsteamanuensis i fluidmekanikk fra 1. august.

Langtidsgjester
Instituttet har tre langtidsgjester på SUPREMA-programmet høsten 2006:
Helmer Aslaksen, Singapore, i perioden 1.8-31.12, **George Hitching**, Hannover, i perioden 1.9-31.12 og **Toke Meier Carlsen,** København, i perioden 1.10-31.12.

Nye doktorgrader:
Stipendiat **Martin Gunnar Gulbrandsen** forsvarte 31. juli sin doktoravhandling *Fibrations on generalized Kummer varieties* for dr. scient-graden.

Nye stillinger:
Instituttet har to ledige stillinger i matematikk, begge med søknadsfrist 15. september 2006. Den ene stillingen er en fast førsteamanuensis og den andre en toårig post.dok med start 1.1.2007.

Nyansettelser:
Stipendiat **Georg Muntingh** er ansatt fra 1. juli.Fagområde er numerisk algebraisk geometri.
Stipendiat **Yeliz Yolcu Okur** er ansatt fra 15. august. Fagområde er stokastisk analyse.
Stipendiat **Trygve K. Karper** er ansatt fra 7. august, tilknyttet Kenneth Karlsens YFF-prosjekt.
Stipendiat **Andrea Barth** er ansatt fra 1. september, delfinansiert av DFG (det tyske forskningsrådet). Fagområde er stokastisk analyse.

Avganger
Postdoc **Mostafa Bendahmane** har fullført sin periode ved CMA, og tiltrådt ny stilling ved Universitetet i Santiago, Chile.
Stipendiat **Pål Hermunn Johansen** har tiltrådt et undervisningsvikariat ved NTNU, Trondheim.

Nye doktorgrader
Mari Anne Killie disputerte 30. juni med avhandlingen *Modeling element abundances in the solar atmosphere with improved transport*

Matematisk kalender

2006
22.-30. ICM 2006, Spania
September:
4.-7. TAG-workshop, Oslo
5.-7. KUL-konferanse, HiA
18.-20. Etterutdanningskonferanse for lærere som underviser matematikk i lærerutdanningen, Dømmesmoen, HiA
2007
Januar:
-. Ski og Matematikk
August:
5.-10. Abelsymposiet, Oslo
2009
Juni:
8.-11. Den Nordiske Matematikerkonferansen, Oslo

DEN INTERNASJONALE MATEMATIKER-KONGRESSEN 2006 22.–30. august 2006

Den internasjonale kongressen for matematikere holdes hvert fjerde år. I år foregår det hele i Madrid med et omfattende program, bl.a. utdeling av Fields-medaljen den 22. august.

ELLIPTISK KOHOMOLOGI OG DERIVERT/HOMOTOPISK ALGEBRAISK GEOMETRI, august-september 2006

Suprema i Oslo organiserer en forelesningsrekke ved Jacob Lurie (Harvard) om elliptisk kohomologi og derivert/homotopisk algebraisk geometri, i august og september 2006. Kurset avsluttes med en konferanse. Se http://www.math.uio.no/~rognes/suprema/lurie.html for nærmere opplysninger.

KUL KONFERANSE, 5. – 7. september 2006

Dette er en konferanse med utgangspunkt i KUL prosjektene vi arbeider med ved Høgskolen i Agder. Prosjektene er LCM – Læringsfellesskap i matematikk (Learning Communities in Mathematics) og IKTML IKT og læring i matematikk, begge støttet av KUL programmet i Norges Forskningsråd.
Konferansen er aktuell for lærere i grunnskole, videregående skole og lærerutdanning. Vi tror også den er av interesse for skoleledere og forskere som arbeider med matematikkdidaktikk. .

ETTERUTDANNINGSKONFERANSE FOR MATEMATIKKLÆRERUTDANNERE, 18.–20. september 2006

HiA vil ta opp igjen tradisjonen med en "årlige" etterutdanningskonferanse for matematikklærerutdannere. Konferansen vil bli holdt på Dømmesmoen ved Grimstad fra 18. til 20. september, arrangert av HiA og HiT i fellesskap. To av hovedtemaene for konferansen vil være begynneropplæring og matematikkvansker.

MER INFORMASJON!

INFOMAT-redaksjonen vil gjerne ha informasjon om hva som skjer rundt omkring på instituttene. Det som har allmenn interesse er:
Nye stillinger, nyansettelser, gjester, avlagte doktorgrader og mastereksamener, undervisningsstatistikk og andre, mer kuriøse nyheter. Send gjerne lenker til ting av interesse.
Nå er det ikke bare slike faktaopplysninger vi er ute etter. Artikler om matematikk, fagpolitikk eller andre emner som kan interessere vår store leserskare blir tatt i mot med åpne armer.

BRONSEMEDALJE I IMO

Jørgen Vold Rennemo fra Lillehammer ble beste norske deltaker i årets Matematikkolympiade i Slovenia. Han fikk bronsemedalje med totalt 18 poeng av 42 mulige. I tillegg til Rennemo fikk Vidar Klungre og Atle Rygg Årdal hederlig omtale av juryen. Sammenlagt endte det norske laget på 66. plass av 90 land med 52 poeng av 252 oppnåelige. Konkurransen ble som vanlig dominert av de asiatiske landene med Kina helt på toppen.

MUMFORD OG WU FÅR SHAWPRISEN FOR 2006

Den 21. juni offentliggjorde Shawpris-stiftelsen navnene på årets vinnere av Shawprisen. Prisen innen matematiske fag deles mellom David Mumford ved Brown University i Providence, USA, og Wentsun Wu ved det kinesiske vitenskaps-akademiet i Beijing. Mumford får prisen for *his contributions to mathematics and to the new interdisciplinary fields of pattern theory and vision research,* og Wu får prisen for *his contributions to the new interdisciplinary field of mathematics and mechanization.* De to prisvinnerne deler prisbeløpet på 1 million US dollar.

MANGE NYE MATEMATIKK-STUDENTER I HØST

Opptakstallene til Bachelor-programmene i matematikk er nå klare. Ved Universitetet i Oslo er det to aktuelle programmer, Matematikk med informatikk-programmet har fått 30 ja-svar (til 52 studieplasser), mens Matematikk, informatikk og teknologi, også kalt MIT-programmet har fått inn 91 ja-svar (120 studieplasser). Ved NTNU er tallene 25 for programmet i matematikk og statistikk, 5 innen biomatematikk og 35 på årsstudiet i matematikk. Gledelig er det at lærerutdanningen i realfag er på stigning og har tatt opp 46 i år, 12 mer enn i fjor. De mest attraktive realfagsprogrammene, når vi legger Oslo-tallene til grunn er fortsatt de mykere informatikkprogrammene, som visualisering og digitale medier, i tillegg til molekylærbiologi.

NY BOK AV NORSKE MATEMATIKERE

Sergey Neshveyev og **Erling Størmer** ved Universitetet i Oslo har kommet med en ny bok med tittelen *Dynamical Entropy in Operator Algebras.* Boka er i Springers Ergebnisse-serie. Forlaget beskriver boka slik:
During the last 30 years there have been several attempts at extending the notion of entropy to noncommutative dynamical systems. The authors present in the book the two most successful approaches to the extensions of measure entropy and topological entropy to the noncommutative setting and analyze in detail the main models in the theory. The book addresses mathematicians and physicists, including graduate students, who are interested in quantum dynamical systems and applications of operator algebras and ergodic theory. Although the authors assume a basic knowledge of operator algebras, they give precise definitions of the notions and in most cases complete proofs of the results which are used.

ABELSYMPOSIET 2004, PROCEEDINGS

Proceedings fra det første Abelsymposiet innen operatoralgebraer foreligger nå i bokform på Springer. Redaktører er Ola Bratteli, Sergey Neshveyev og Christian Skau. Forlaget skriver om boka:
The theme of the first Abel Symposium was operator algebras in a wide sense. In the last 40 years operator algebras have developed from a rather special discipline within functional analysis to become a central field in mathematics often described as "non-commutative geometry". It has branched out in several sub-disciplines and made contact with other subjects. The contributions to this volume give a state-of-the-art account of some of these sub-disciplines and the variety of topics reflect to some extent how the subject has developed. This is the first volume in a prestigious new book series linked to the Abel prize.

CALL FOR NOMINATIONS OF CANDIDATES FOR TEN EMS PRIZES

Principal Guidelines

Any European mathematician who has not reached his/her 35th birthday on 30 June 2008, and who has not previously received the prize, is eligible for an EMS Prize at 5ecm. A total of 10 prizes will be awarded. The maximum age may be increased by up to three years in the case of an individual with a 'broken carreer pattern'. Mathematicians are defined to be 'European' if they are of European nationality or their normal place of work is within Europe. 'Europe' is defined to be the union of any country or part of a country which is geographically within Europe or that has a corporate member of the EMS based in that country. Prizes are to be awarded for work published before 31 December 2007.

Nominations of the Award

The Prize Committee is responsible for solicitation and evaluation of nominations. Nominations can be made by anyone, including members of the Prize Committee and candidates themselves. It is the responsibility of the nominator to provide all relevant information to the Prize Committee, including a résumé and documentation. The nomination for each award must be accompanied by a written justification and a citation of about 100 words that can be read at the award ceremony. The prizes cannot be shared.

Description of the Award

The award comprises a certificate including the citation and a cash prize of 5000 euro.

Award Presentation

The prizes will be presented at the Fifth European Congress of Mathematics by the President of the European Mathematical Society. The recipients will be invited to present their work at the congress. (see www.5ecm.nl) .

Prize Fund

The money for the Prize Fund is offered by the Foundation Compositio Mathematica.

Deadline for Submission

Nominations for the prize must reach the chairman of the Prize Committee at the following address, not later than 1 November 2007:
5ECM Prize Committee, Prof. R. Tijdeman, Mathematical Institute, Leiden University, Postbus 9512, 2300 RA Leiden, The Netherlands.
e-mail: tijdeman@math.leidenuniv.nl
fax: +31715277101, phone: +31715277138

NOMINASJONER TIL ABELPRISEN 2007

Fristen for nominasjoner til Abelprisen for 2007 går ut 15. november 2006. Alle kan nominere hvem de vil, bare ikke seg selv. Se Abelprisens nettsider for mer informasjon.

MOTEKSEMPEL TIL HODGE-FORMODNINGEN

Et e-print på arXiv påstår å ha funnet et moteksempel til Hodge-formodningen. Forfatterne, K.H. Kim og F.W. Roush skriver i abstractet til AG/0608265:

We show that the Hodge conjecture is false in general for products of surfaces. We construct a K3 surface whose transcendental lattice has a self-isomorphism which is not a linear combination of self-isomorphisms which preserve cup products over $\mathbf{Q}$ *up to nonzero multiples. We then find another surface mapping into it in which the transcendental lattice is generated by* H^1 *cup products according to the Kuga-Satake correspondence. For any such surface polarizations of the transcendental lattice arising from* H^1 *cup products into* H^2 *must coincide with the polarizations induced from cup products from* H^2 *into* H^4 *and invariance of polarization gives a contradiction assuming the Hodge conjecture is true. Our example also shows the Tate conjecture is false.*

INTERVIEW WITH LENNART CARLESON

Interviewers: Martin Raussen, Aalborg and Christian Skau, NTNU

The interview was conducted in Oslo on May 22nd prior to the Abel prize celebration and was later shown on Norwegian TV. The first two questions and their answers were originally phrased in the three Scandinavian languages: Norwegian, Danish and Swedish. They are reproduced here translated into English. The interview will also be published in the Newsletter of the EMS.

Introduction

On behalf of the Norwegian and Danish mathematical societies, we want to congratulate you on winning the Abel prize for 2006.

This year we commemorate the 100th centenary of the death of the Norwegian dramatist and poet Henrik Ibsen. He passed away on the 23rd of May just a stone's throw away from this place. The longest poem he ever wrote is called "Balloon letter to a Swedish lady" and it contains a verse which reads as follows:

"--- aldri svulmer der en løftning
av et regnestykkes drøftning
--- ti mot skjønnhet hungrer tiden ---"

Without drawing too far-reaching conclusions, Ibsen seems to express a feeling shared by many people, i.e. that mathematics and beauty or art are opposed to each other, that they belong to different spheres. What are your comments to this view?

I do not think that Ibsen was very well-oriented about beauty in mathematics, which you certainly can find and enjoy. And I would even maintain that the beauty of many mathematical arguments can be easier to comprehend than many modern paintings. But a lot of mathematics is void of beauty. Maybe particularly in modern mathematics, where problem areas have often gotten extremely complex and complicated, with the result that the solution can only be formulated on several hundreds of pages. And that can scarcely be called beautiful. But in classical mathematics you find many striking theorems and arguments that hit you as something really original. It is reasonable to use the term beauty for those.

Mathematicians all over Scandinavia are proud of counting one of their own among the very first recipients of the Abel Prize. How would you characterize and evaluate Scandinavian, and particularly Swedish, mathematics in an international perspective?

I think that Scandinavia does quite well in this respect. In Sweden, we have a fine new generation of young mathematicians. And I think it looks very much alike in the other Scandinavian countries. It is difficult to perceive a new Abel on the horizon, but that is probably too much to hope for.

Could you please characterize the unique contribution that the Finnish/Swedish school of Lindelöf, M.Riesz, Carleman, R.Nevanlinna, Phragmen, Beurling and Ahlfors brought to analysis in the first half of the 20th century, which was formative and decisive for your own contribution to hard analysis?

In your list, you miss another Scandinavian mathematician: J. L. Jensen. The importance of "Jensen's inequality" can hardly be exaggerated. He and Lindelöf started the Scandinavian school, building of course on Riemann's approach to complex analysis rather than that of Cauchy-Weierstrass; Nevanlinna and Carleman continued, followed by Ahlfors and Beurling, a remarkable concentration of talent in Scandinavia. My lecture tomorrow will give more details.

Mathematical achievements in context

Abel first thought that he had solved the general quintic by radicals. Then he found a mistake and subsequently he proved that it was impossible to solve the quintic algebraically. The famous and notoriously difficult problem about the pointwise convergence almost everywhere of L^2-functions, that Lusin formulated in 1913 and actually goes back to Fourier in 1807, was solved by you in the mid-1960's. We understand that the prehistory of that result was converse to that of Abel's, in the sense that you first tried to disprove it. Could you comment on that story?

Yes, of course. I met the problem already as a student when I bought Zygmund's book on trigonometric series. Then I had the opportunity to meet Zygmund. He was at Harvard in '50 or '51. I was at that time working on Blaschke products and I said maybe one could use those to produce a counterexample. Zygmund was very positive and said "of course, you should do that". I tried for some years and then I forgot about it before it again came back to me. Then, in the beginning of the '60's, I suddenly realized that I knew exactly why there had to be a counterexample and how one should construct one. Somehow, the trigonometric system is the type of system where it is easiest to provide counterexamples. Then I could prove that my approach was impossible. I found out that this idea would never work; I mean that it couldn't work. If there were a counterexample for the trigonometric system, it would be an exception to the rule.

Then I decided that maybe no one had really tried to prove the converse. From then on it only took two years or so. But it is an interesting example of 'to prove something hard, it is extremely important to be convinced of what is right and what is wrong'. You could never do it by alternating between the one and the other because the conviction somehow has to be there.

Could we move to another problem, the so-called Corona problem that you solved in 1962? In this connection, you introduced the so-called Carleson measure, which was used extensively by other mathematicians afterwards. Could you try to explain why the notion of the Carleson measure is such a fruitful and useful notion?

Well, I guess because it occurs in problems related to the general theory of BMO and H^1-spaces. I wish this class of measures had been given a more neutral name. In my original proof of the Corona problem, the measures were arc lengths on the special curves needed there. Beurling suggested that I should formulate the inequality for general measures. The proof was the same and quite awkward. Stein soon gave a natural and simple proof and only then the class deserved a special name.

I'll move to another one of your achievements. Hardy once said that mathematics is a young mans game. But you seem to be a counterexample; after you passed sixty years of age, you and Michael Benedicks managed to prove that the so called Hénon map has strange attractors exhibiting chaotic behaviour. The proof is extremely complicated. It's a tour de force that took many years to do. With this as a background, what is your comment on mathematical creativity and age?

I guess and hope that you don't get more stupid when you get older. But I think your stamina is less, your perseverance weakens (keeping lots of facts in your mind at the same time). Probably this has to do with the circulation of the blood or something like that. So I find it now much harder to concentrate for a long period. And if you really want to solve complicated problems, you have to keep many facts available at the same time.

Mathematical Problems

You seem to have focused exclusively on the most difficult and profound problems of mathematical analysis. As soon as you have solved any one of these, you leave the further exploration and elaboration to others, while you move on to other difficult and seemingly intractable problems. Is this a fair assessment of your mathematical career and of your mathematical driving urge?

Yes, I think so. Problem solving is my game, rather than to develop theories. Certainly the development of mathematical theories and systems is very important but it is of a very different character. I enjoy starting on something new, where the background is not so complicated. If you take the Hénon case, any schoolboy can understand the problem. The tools also are not really sophisticated in any way; we do not use a lot of theory.

The Fourier series problem of course used more machinery that you had to know. But that was somehow my background. In the circles of dynamical systems people, I always consider myself an amateur. I am not educated as an expert on dynamical systems.

Have there been mathematical problems in analysis that you have worked on seriously, but at which you have not been able to succeed? Or are there any particular problems in analysis that you especially would have liked to solve?

Yes, definitely. There is one in dynamical systems, which is called the standard map. This is like the Hénon map but in the area preserving case. I spent several years working on it, collaborating with Spencer for example, but we never got anywhere. If you want to survive as a mathematician, you have to know when to give up also. And I am sure that there have been many other cases also. But I haven't spent any time on the Riemann hypothesis… and it wouldn't have worked either.

Characterization of great mathematicians

What are the most important features, besides having a good intellectual capacity of course, that characterize a great mathematician?

I don't think they are the same for everybody. They are not well defined really. If you want to solve problems, as in my case, the most important property is to be very, very stubborn. And also to select problems which are within reach. That needs some kind of intuition, I believe, which is a little closer to what we talked about initially, about beauty. You must somehow have a feeling for mathematics: What is right, what is wrong and what is feasible. But, of course, there are many other mathematicians who create theories and they combine results into new buildings and keep other people working. It is a different kind of a mathematician. I don't think you should try to find a simple formula for people.

For several decades, you have worked hard on problems that were known to be exceptionally difficult. What drove you and what kept you going for years, with no success guaranteed? What drives a person to devote so much energy to an arcane subject that may only be appreciated by a handful of other mathematicians?

Yes, that's a big issue. Stubbornness is important; you don't want to give up. But as I said before, you have to know when to give up also. If you want to succeed you have to be very persistent. And I think it's a drive not to be beaten by stupid problems.

Your main research contribution has been within mathematical analysis. What about your interest in algebra and topology/geometry?

Geometry is of course very much part of the analysis. But I have no feeling for algebra or topology, I would say. I have never tried to... I should have learned more!

Mathematics of the future

What do you consider to be the most challenging and exciting area of mathematics that will be explored in the 21st century? Do you have any thoughts on the future development of mathematics?

Yes, of course I have had thoughts. Most of the influence comes from the outside. I think we are still lacking a good understanding of which kind of methods we should use in relation to computers and computer science. And also in relation to problems depending on a medium sized number of variables. We have the machinery for a small number of variables and we have probability for a large number of variables. But we don't even know which questions to ask, much less which methods to use, when we have ten variables or twenty variables.

This leads to the next question. What is the significance of computers in mathematics? Is it mainly checking experimentally certain conjectures? Or is it completing proofs by checking an enormous amount of special cases? What are your thoughts on computers in mathematics?

There are a few instances that I have been involved with. I had a student, Warwick Tucker, who proved that the Lorenz attractor exists. The proof was based on explicit computations of orbits. And in that case you could get away with a finite number of orbits. This is very different from the Hénon map, where you could never succeed in that way. You could never decide whether a parameter was good

or bad. But for the Lorenz attractor he actually proved it for the specific values that Lorenz had prescribed. Because it is uniformly expanding, there is room for small changes in the parameter. So this is an example of an actual proof by computer.
Of course then you could insist on interval arithmetics. That's the fine part of the game so to say, in order to make it rigorous for the people who have very formal requirements.

But what about computers used, for instance, for the four colour problem, checking all these cases?

Probably unavoidable, but that's okay. I wouldn't like to do it myself. But it's the same with group structures, the classification of simple groups, I guess. We have to accept that.

The solution of the 350 year old Fermat conjecture, by Andrew Wiles in 1994, uses deep results from algebraic number theory. Do you think that this will be a trend in the future, that proofs of results which are simple to state will require a strong dose of theory and machinery?

I don't know.
The striking part in the proof of the Fermat theorem is the connection between the number theory problem and the modular functions. And once you have been able to prove that, you have moved the problem away from what looked like an impossible question about integers, into an area where there exists machinery.

Career. Teachers.

Your CV shows that you started your university education already at the age of 17 and that you took your PhD at Uppsala University when you were 22 years old. Were you sort of a wunderkind?

No, I didn't feel like a wunderkind.

Can you elaborate about what aroused your mathematical interests? And when did you become aware that you had an exceptional mathematical talent?

During high school I inherited some books on calculus from my sister. I read those but otherwise I didn't really study mathematics in any systematic way. When I went to university it was natural for me to start with mathematics. Then it just kept going somehow. But I was not born a mathematician.

You already told us about your PhD advisor, Arne Beurling, an exceptional Swedish mathematician, who is probably not as well known as he deserves. Could you characterize him as a person and as a researcher in a few sentences? Did he have a lasting influence on your own work?

Yes, definitely. He was the one who set me on track. We worked on the same type of problems but we had a different attitude towards mathematics. He was one of the few people about whom I would use the word genius. Mathematics was part of his personality somehow. He looked at mathematics as a piece of art. Ibsen would have profited from meeting him. He also considered his papers as pieces of art. They were not used for education and they were not used to guide future researches. But they were used as you would use a painting. He liked to hide how he found his ideas. If you would ask him how he found his result, he would say a wizard doesn't explain his tricks.
So that was a rather unusual education. But of course I learned a lot from him. As you said, he has never been really recognized in a way which he deserves.

Apart from Arne Beurling, which other mathematicians have played an important part in your development as a mathematician?

I have learnt from many others, in particular from the people I collaborated with and in particular from Peter Jones. I feel a special dept to Michel Herman. His thesis, where he proved the global Arnold conjecture on diffeomorphisms of the circle, gave me a new aspect on analysis and was my introduction to dynamical systems.

You have concentrated your research efforts mainly on topics in hard analysis, with some spices from geometry and combinatorics. Is there a specific background for this choice of area?

I don't think so. There is a combinatorial part in all of the three problems we have discussed here. And all of them are based on stopping time arguments. You make some construction and then you stop the construction, and you start all over again.

This is what is called renormalization?

Yes, renormalization. That was something I didn't learn. Probability was not a part of the Uppsala school. And similarly for coverings, which is also part of the combinatorics.

Which mathematical area and what kind of mathematical problems are you currently the most interested in?

Well, I like to think about complexity. I would like to prove that it's harder to multiply than to add.

That seems to be notoriously difficult, I understand.

Well, I am not so sure. It's too hard for me so far.

You have a reputation as a particularly skilful advisor and mentor for young mathematicians; 26 mathematicians were granted a PhD under your supervision. Do you have particular secrets on how to encourage, to advise and to educate young promising mathematicians?

The crucial point, I think, is to suggest an interesting topic for the thesis. This is quite hard since you have to be reasonably sure that the topic fits the student and that it leads to results. And you should do this without actually solving the problem! A good strategy is to have several layers of the problem. But then many students have their own ideas. I remember one student who wanted to work on orthogonal polynomials. I suggested that he could start by reading Szegö's book. "Oh, no!" he said, "I don't want to have any preconceived ideas".

Publishing mathematics

I would like to move to the organization of research. Let's start with the journal Acta Mathematica. It is a world famous journal founded by Gösta Mittag-Leffler back in 1882 in Stockholm as a one-man enterprise at that time. It rose very quickly to be one of the most important mathematical journals. You were its editor in chief for a long period of time. Is there a particular recipe for maintaining Acta as a top mathematical journal? Is very arduous refereeing most important?

It is the initial period that is crucial, when you build up a reputation so that people find it attractive to have a paper published there. Then you have to be very serious in your refereeing and in your decisions. You have to reject a lot of papers. You have to accept being unpopular.

Scientific publication at large is about to undergo big changes. The number of scientific journals is exploding and many papers and research results are sometimes available on the internet many years before they are published in print. How will the organization of scientific publication develop in the future? Will printed journals survive? Will peer review survive as today for the next decades?

I've been predicting the death of the system of mathematical journals within ten years for at least 25 years. And it dies slowly, but it will only die in the form we know it today. If I can have a wish for the future, I would wish that we had, say, 100 journals or so in mathematics, which would be very selective in what they publish and which wouldn't accept anything that isn't really finalized, somehow. In the current situation, people tend to publish half-baked results in order to get better promotions or to get a raise in their salary.

The printing press was invented by Gutenberg 500 years ago in order to let information spread from one person to many others. But we have completely different systems today which are much more efficient than going through the printing process and we haven't really used that enough.

I think that refereeing is exaggerated. Let people publish wrong results and let other people criticize. As long as it's available on the net it won't be any great problem. Moreover, referees aren't very reliable; it doesn't really work anyway. I am predicting a great change, but it's extremely slow in coming. And in the meantime the printers make lots of money.

Research Institutions

I've just returned from a nice stay at the Institute Mittag-Leffler, which is situated in Djursholm, north of Stockholm; one of the leading research institutes of our times. This institute was, when you stepped in as its director in 1968, something that I would characterize as a sleeping beauty. But you turned it into something very much different, very active within a few years. By now around thirty mathematicians work together there at any given time but there is almost no permanent staff. What was the inspiration for the concept of the Institute Mittag-Leffler as it looks like today? And how was it possible to get the necessary funds for this institute? Finally, how would you judge the present activities of the institute?

To answer the last question first, I have to be satisfied with the way it worked out and the way it continues also. I just hope that it can stay on the same course. In the '60's, there was a period when the Swedish government (and maybe also other governments) was willing to invest in science. There was a discussion about people moving to the United States. Hörmander had already moved and the question was whether I was going to move as well. In this situation, you could make a bargain with them. So we got some money, which was of course the important part. But there was a rather amusing connection with the Acta, which is not so well known. From Mittag-Leffler's days, there was almost no money in the funds of the academy for the Mittag-Leffler institute. But we were able to accumulate rather large sums of money by selling old volumes of the Acta. Mittag-Leffler had printed large stocks of the old Acta journals which he never sold at the time. They were stored in the basement of the institute. During the 50's and early 60's one could sell the complete set of volumes. I don't remember what a set could be sold for, maybe 1000 dollars or so. He had printed several hundred extra copies, and there were several hundred new universities. If you multiply these figures together you get a large amount of money. And that is still the foundation of the economy of the institute.

A bit later, you became the president of the International Mathematical Union, an organization that promotes international cooperation within mathematics. This happened during the cold war and I know that you were specifically concerned with integrating Chinese mathematics at the time. Could you share some of your memories from your presidency?

Well, I considered my main concern to be the relation to the Soviet Union. The Chinese question had only started. I went to China and talked to people in Taiwan, and to people in mainland China. But it didn't work out until the next presidential period and it simply ripened. The main issue was always whether there was to be a comma in a certain place, or not, in the statutes.

It was somehow much more serious with the Russians. You know, they threatened to withdraw from international cooperation altogether. The IMU committee and I considered that the relation between the West and the East was the most important issue of the International Mathematical Union. So that was exciting. Negotiations with Pontryagin and Vinogradov were kind of special.

Did these two express some anti-semitic views also?

No, not officially. Well they did, of course, in private conversation. I remember Vinogradov being very upset about a certain Fields Medal being given to somebody, probably Jewish, and he didn't like that. He said this is going to ruin the Fields Prize forever. Then I asked him if he knew who received the first Nobel Prize in literature. Do you? It was a French poet called Sully Prudhomme; and that was during a period when Tolstoy, Ibsen and Strindberg were available to get the prize. Well, the Nobel Prize survived.

Mathematics for our times

You wrote a book, "Matematik för vår tid" or "Mathematics for our times", which was published *in Sweden in 1968. In that book, you took part in the debate on so-called New Mathematics, but you also described concrete mathematical problems and their solutions. Among other things you talked about the separation between pure and applied mathematics. You described it as being harmful for mathematics and harmful for contact with other scientists. How do you see recent developments in this direction? What are the chances of cross-fertilization between mathematics on the one side and, say, physics, biology or computer science on the other side? Isn't computer science somehow presently drifting away from mathematics?*

Yes, but I think we should blame ourselves; mathematics hasn't really produced what we should, i.e. enough new tools. I think this is, as we talked about before, really one of the challenges. We still have lots of input from physics, statistical physics, string theory, and I don't know what. I stand by my statement from the sixties.

But that book was written mostly as a way to encourage the teachers to stay with established values. That was during the Bourbaki and New Math period and mathematics was really going to pieces, I think. The teachers were very worried and they had very little backing. And that was somehow the main reason for the book.

If you compare the sixties with today, mathematics at a relatively elevated level is taught to many more people and other parts of the subject are emphasized. For example the use of computers is now at a much higher state then at that time, where it almost didn't exist. What are your main points of view concerning the curriculum of mathematics at, say, high school level and the early years of university? Are we at the right terms? Are we teaching in the right way?

No, I don't think so. Again, something predictable happens very slowly. How do you incorporate the fact that you can do many computations with these hand-computers into mathematics teaching?

But in the meantime, one has also expelled many things from the classroom which are related to the very basis of mathematics, for example proofs and definitions and logical thinking in general. I think it is dangerous to throw out all computational aspects; one needs to be able to do calculations in order to have any feeling for mathematics.

You have to find a new balance somehow. I don't think anybody has seriously gotten there. They talk a lot about didactics, but I've never understood that there is any progress here.

There is a very strong feeling in school, certainly, that mathematics is a God-given subject. That it is once and for all fixed. And of course that gets boring.

Public awareness

Let us move to public awareness of mathematics: It seems very hard to explain your own mathematics to the man on the street; we experience that right now. In general pure mathematicians have a hard time when they try to justify their business. Today there is an emphasis on immediate relevance and it's quite hard to explain what mathematicians do to the public, to people in politics, and even to our colleagues from other sciences. Do you have any particular hints on how mathematicians should convey what they are doing in a better way?

Well, we should at least work on it; it's important. But it is also very difficult. A comment which may sound kind of stupid is that physicists have been able to sell their terms much more effectively. I mean who knows what an electron is? And who knows what a quark is? But they have been able to sell these words. The first thing we should try to do is to sell the words so that people get used to the idea of a derivative, or an integral, or whatever.

As something mysterious and interesting, right?

Yes, it should be something mysterious and interesting. And that could be one step in that direction, because once you start to talk about something you have a feeling about what it is. But we haven't been able to really sell these terms. Which I think is too bad.

Thank you very much for this interview on behalf of the Norwegian, the Danish and the European Mathematical Societies!