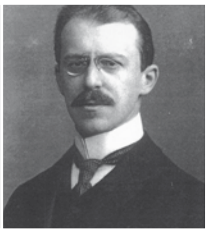
NEWSLETTER

OF THE EUROPEAN MATHEMATICAL SOCIETY



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June 2012
Issue 84
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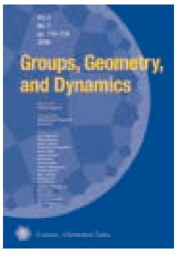
European
Mathematical
Society

Journals published by the European Mathematical Society



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2012. Vol. 6. 4 issues
Approx. 800 pages. 17.0 x 24.0 cm
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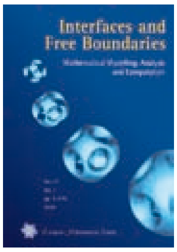
Aims and Scope
Groups, Geometry, and Dynamics is devoted to publication of research articles that focus on groups or group actions as well as articles in other areas of mathematics in which groups or group actions are used as a main tool. The journal covers all topics of modern group theory with preference given to geometric, asymptotic and combinatorial group theory, dynamics of group actions, probabilistic and analytical methods, interaction with ergodic theory and operator algebras, and other related fields.



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390 € online only
430 € print+online

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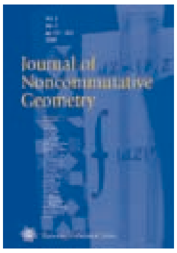
Aims and Scope
Interfaces and Free Boundaries is dedicated to the mathematical modelling, analysis and computation of interfaces and free boundary problems in all areas where such phenomena are pertinent. The journal aims to be a forum where mathematical analysis, partial differential equations, modelling, scientific computing and the various applications which involve mathematical modelling meet.



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Editor-in-Chief:
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Aims and Scope
The *Journal of Noncommutative Geometry* covers the noncommutative world in all its aspects. It is devoted to publication of research articles which represent major advances in the area of noncommutative geometry and its applications to other fields of mathematics and theoretical physics. Topics covered include in particular: Hochschild and cyclic cohomology; K-theory and index theory; measure theory and topology of noncommutative spaces, operator algebras; spectral geometry of noncommutative spaces; noncommutative algebraic geometry; Hopf algebras and quantum groups; foliations, groupoids, stacks, gerbes.

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ISSN online 1664-0403
2012. Vol. 2. 4 issues
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198 € online only
238 € print+online

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Aims and Scope
The *Journal of Spectral Theory* is devoted to the publication of research articles that focus on spectral theory and its many areas of application.

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2012. Vol. 14. 6 issues
Approx. 1800 pages. 17.0 x 24.0 cm
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548 € online only
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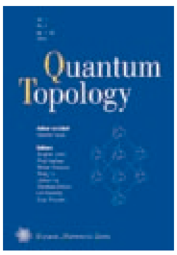
Aims and Scope
The *Journal of the European Mathematical Society* is the official journal of the EMS. The Society, founded in 1990, works at promoting joint scientific efforts between the many different structures that characterize European mathematics. JEMS publishes research articles in all active areas of pure and applied mathematics.



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Editor-in-Chief:
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Aims and Scope
Quantum Topology is dedicated to publishing original research articles, short communications, and surveys in quantum topology and related areas of mathematics. Topics covered include: low-dimensional topology; knot theory; Jones polynomial and Khovanov homology; topological quantum field theory; quantum groups and hopf algebras; mapping class groups and Teichmüller space categorification; braid groups and braided categories; fusion categories; subfactors and planar algebras; contact and symplectic topology; topological methods in physics.

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EMS Agenda

2012

30 June–1 July
Council Meeting of European Mathematical Society, Kraków, Poland
www.euro-math-soc.eu

1 July
Meeting of the RPA Committee of the EMS, Kraków, Poland

2 July
Meeting of the Editorial Board of the EMS Newsletter, Kraków, Poland
Vicente Muñoz: vicente.munoz@mat.ucm.es

2–7 July
6th European Mathematical Congress, Kraków, Poland
www.6ecm.pl

6–8 July
Pre-world-congress Meeting of Young Researchers in Probability and Statistics 2012 (PWCYPS 2012), Istanbul, Turkey
http://pwc2012.ku.edu.tr/

23–27 July
17th Conference for Mathematics in Industry, ECMI 2012, Lund, Sweden
www.maths.lth.se/ecmi/ecmi2012.org

30 July–3 August
EMS-IAMP Summer School on Quantum Chaos, Erwin Schödinger Institute, Vienna

6–11 August
International Congress on Mathematical Physics, ICMP12 Aalborg, Denmark
www.icmp12.com

19–26 August
The Helsinki Summer School on Mathematical Ecology and Evolution 2012
wiki.helsinki.fi/display/huippu/mathbio2012

27–31 August
5th European Summer School in Financial Mathematics, Paris
www.cmap.polytechnique.fr/~euroschoolmathfi12

26–28 October
Executive Committee Meeting, Helsinki
Stephen Huggett: s.huggett@plymouth.ac.uk

4–8 November
Kristian Seip, EMS Lectures, Tel Aviv University, Israel
"Selected problems in operator-related function theory and harmonic analysis"
www.euro-math-soc.eu/node/2471

3–7 December
Kristian Seip, EMS Lectures, Saint Petersburg, Russia
"Selected problems in operator-related function theory and harmonic analysis"
www.euro-math-soc.eu/node/2471

2013

20–25 July
29th European Meeting of Statisticians, Budapest
www.ems2013.eu

"Mathematics of Planet Earth 2013": An Invitation

Ehrhard Behrends (Berlin), José Francisco Rodrigues (Lisbon)

In 2000 the World Mathematical Year offered the occasion for a collective reflection on the great challenges of the 21st Century, on the role of mathematics as a key for development and on the importance of the image of mathematics in the public understanding. The countlessly repeated phrase "the Universe is written in the language of mathematics", written by Galileo in 1614, is now truer than ever but it raises new challenges in the current age of data-intensive science driven, in particular, by the information and communication technologies, as identified in a recent report to the European Commission [1].

The "rising tide of scientific data" created by the digital revolution provides new possibilities of facing some of society's great challenges of energy and water supply, global warming and healthcare. Over the last few centuries, mathematics has developed a "universal method for the study of the systems". In particular, for the Planet Earth System the mathematician Jacques-Louis Lions has synthesised in his book *El planeta Tierra* that universal method in three parts: the mathematical modelling; the analysis and the simulation; and the control of the systems [2].

In 2007 a scientific workshop on "Climate Change: From Global Models to Local Action", organised by the Mathematical Sciences Research Institute in Berkeley, identified several mathematical research topics that might contribute to resolving problems whose solutions would have a large societal impact [3]: from high dimensional systems to model reduction, from multiscale computations to data assimilation and from uncertainty quantification to economics and societal aspects. The areas of mathematics that might have a significant role in those problems vary from dynamical systems and nonlinear differential equations to asymptotic and numerical analysis, from computational science to statistics and operations research and from stochastic processes to game and control theories.

"Mathematics of Planet Earth 2013" (MPE2013) is an initiative proposed by the North American (Canadian and U.S.) Mathematical Institutes that now has many partners in Europe and around the world. MPE2013 aims to increase the engagement of mathematicians (researchers, teachers and students), as well as the public, with the role of mathematics in issues affecting our Planet Earth and its future.

It is expected to be a year full of scientific research programmes and activities for the public, media and schools. The *mission statement* of this worldwide endeavour consists of:

- Increase the engagement of mathematicians – researchers, teachers, students – as well as the public, with the role of mathematics in issues affecting our Planet Earth and its future.
- Encourage research to identify and address fundamental questions that have to do with our planet to which mathematics can contribute to a solution, including understanding its climate and environment, and addressing its sustainability.
- Encourage mathematics teachers at all levels to communicate issues related to our Planet Earth through their instruction and their curriculum development.
- Encourage mathematics students and beginning researchers to pursue research areas related to our Planet Earth.
- Inform the public about roles that mathematics can play in addressing questions related to our Planet Earth.

A special aspect of MPE2013: a competition for modules of a virtual exhibition, was described in some detail in the last issue of this newsletter [4]. Last month the UNESCO granted its patronage for the international launching of the MPE Open Source Exhibition, proposed to take place in February 2013. With the present article we hope to motivate you to realise your MPE2013 project in *your* city and in *your* country.

A list of MPE topics

It is not hard to identify a number of topics that are important when we try to master the problems of our contemporary world where mathematics plays a crucial role (see for instance [5]). Here are some examples:

- Network Science in Ecology, Environment, Society and Finance
- Climate Change
- Finance and Sustainability
- Biological Processes
- Environmental Management (nuclear waste disposal, contaminant transports and water quality, transportation emissions)
- Uncertainty Quantification (geostatics and stochastic modelling)
- Renewable and Sustainable Energy (batteries, biofuels, nuclear, natural gas)
- Disease
- Genetics

- Catastrophic Events (seismic modelling, storm surge modelling, tsunami modelling, severe weather prediction)
- Internet and Communications
- Computational and Theoretical Fluid Dynamics
- Materials Sciences (polymers, microstructure and interfacial phenomena, phase transitions, optical and photonic materials)
- Imaging (compression, inverse problems, applications in biomedicine, geophysics, etc.)
- Celestial mechanics

It is very likely that your special subject is close or at least related to one of these topics. Then you are the right person to realise an MPE2013 project!

Of course, if you are an applied mathematician, you may have already written a research paper concerned with one of these topics. But you may wish to write an expository version of a popular mathematical lecture on the global change, as in [6], or a survey article, as, for instance, in [7], that deals with different analytical and numerical models for climate dynamics and presents the interesting contention "that the greatest challenges as well as the greatest promise for novel and innovative mathematical thinking is at this interface between data and models". Or else you may wish to discuss and develop concrete models concerning any topic on human wellbeing and the natural or societal environment, as suggested in [8], for instance. But even if you are not a mathematician directly involved with any of these topics you may well find other ways to relate mathematics to the MPE2013 project.

Concrete projects

In Europe several European Research Centres of Mathematics belonging to ERCOM have already prepared and/or announced initiatives associated explicitly with MPE2013 (see http://www.ercom.org/centres.htm). For instance, the Mathematisches Forschungsinstitut Oberwolfach (this institute also hosts the open source platform for the competition of modules for a virtual exhibition [4]) announced at least two workshops directly related with MPE2013, one on "Geophysical Fluid Dynamics" (W#1308) and another on "Design and Analysis of Infectious Disease Studies" (W#1346). Some centres have already associated their initiatives to a topic, like the Centre de Recerca Matemàtica in Spain, which has a research programme on "The Mathematics of Biodiversity" and has announced for 2013 a conference on "New Trends in Regularization Theory and Methods for Geomathematical Problems".

The Institut Henri Poincaré in Paris will host a trimester at the Center Emile Borel on "Mathematics of Bio-Economics" from January till April 2013 and the Centro Internacional de Matemática is organising in Lisbon, Portugal, two international conferences, one on "Mathematics of Energy and Climate Change" in March 2013 and another one on "Dynamics, Games and Science" in September 2013.

The Newton Institute in Cambridge, UK, has in 2012 a programme on "Multiscale Numerics for the Atmosphere and Ocean" with three workshops. For 2013 their announcements use the MPE2013 logo and three programmes have relations with it, namely "Mathematical Modelling and Analysis of Complex Fluids and Active Media in Evolving Domains", "Infectious Disease Dynamics" and "Infectious Disease Dynamics".

At the individual or group level, there are many additional possibilities for being active in 2013. Here are some examples:

- Write a research or survey article!
- Initiate a research project!
- Organise a workshop!
- Present a contribution to the competition of virtual modules!
- Write an article for the general public to be published in a newspaper in your city/country! (In Germany, for example, there will be a series of MPE2013 articles in the nationwide newspaper WELT. Each month there will appear a contribution written by a specialist of one of the MPE2013 topics.)
- Invite a speaker to give a talk for the general public!
- Organise an exhibition!
- Prepare a summer school for the students of your department!

A list of MPE topics already announced in various institutions around the world can be found at http://www.crm.umontreal.ca/Math2013/en/theme.php.

Also, you are invited to be an active partner for this worldwide project. If you make up your mind to realise something then don't forget to send an email to the MPE2013 organisers: info@mpe2013.org. They are very interested to learn what's going on in the world.

References

[1] "Riding the wave. How Europe can gain from the rising tide of scientific data", final report of the HLG on Scientific Data to the European Commission, 2010.

[2] J.-L. Lions, *El planeta Tierra. El papel de las matemáticas y de los superordenadores*, Instituto de España, Madrid, 1990.

[3] *Mathematics of Climate Change, A new discipline for an uncertain century*, Mathematical Sciences Research Institute, Berkeley, CA, 2008.

[4] E. Behrends, A. Matt, J. F. Rodrigues, "A Competition in Connection with Mathematics of Planet Earth 2013: Modules for a Virtual Exhibition", *EMS Newsletter* 83, March 2012, 12–13.

[5] C. Rousseau, "Four themes with potential examples of modules for a virtual exhibition on the "Mathematics of Planet Earth", *Centro Internacional de Matemática Bulletin* #30 July 2011, 31–32.

[6] R. Klein, "Mathematics in the Climate of Global Change", Chap. 15 of *Mathematics Everywhere*", edited by M. Aigner, E. Behrends, American Mathematical Society, Providence, R.I. 2010, 197–216.

[7] C. K. T. Jones, "Will climate change mathematics (?)" *IMA J. Appl. Math.* 76 (2011), no. 3, 353–370.

[8] A. Friedman, "Human Well-being and the Natural Environment: Research Challenges in Mathematical Sciences", Report to a Workshop on Mathematical Challenges for Sustainability held at DIMACS, Rutgers University, 15–17 November 2010. Available online at http://dimacs.rutgers.edu/SustainabilityReport/friedman8-26-10.pdf.

New Editor-in-Chief of the EMS Newsletter appointed

The Executive Committee of the EMS has appointed **Lucia Di Vizio** as the next Editor-in-Chief of the Newsletter of the EMS, for the period 2013–2016. Lucia joins the Editorial Board of the Newsletter during 2012 for a smooth transitional period, during which the current Editor-in-Chief Vicente Muñoz and she will be co-editing the Newsletter.

Lucia Di Vizio graduated in 2000 from University of Paris 6 and, after a post-doc at IAS, has become a researcher in CNRS, France, in 2001, and 'directeur de recherche' last year, when she rejoined the department of mathematics of the University of Versailles Saint-Quentin. Her fields of interest are: the algebraic theory of functional equations; difference and differential Galois theory; and p-adic differential equations. She was Vice-President of the Société Mathématiques de France from 2004 to 2009.

The Meetings Committee of the EMS seeks

1. Nominations for EMS Distinguished Speakers for 2012.
An EMS Distinguished Speaker is a prestigious appointment, awarded to an internationally renowned researcher. An EMS Distinguished Speaker is asked to deliver a plenary lecture at a large regional or international European conference.

2. Proposals for EMS Weekends for 2012 and 2013.
An EMS Weekend is a regional European conference – interdisciplinary and covering several mathematical fields.

The Executive Committee of the EMS, via its Meetings Committee, is willing to provide support that would cover the cost of Distinguished Speakers and support (partially at times) the organisation of EMS Weekends. Since resources are scarce, only a limited number of events can be supported; alternatively, we shall be able, if so desired, to provide the stamp "endorsed by the EMS" and allow the use of the EMS logo for worthy meetings.

Proposals should include the name of the intended lecturer or speaker and enough relevant details about the person, as well as details about the proposed meetings and where the talks would take place.

The deadline is 30 April for Distinguished Speakers and EMS Weekends for 2012. Distinguished speakers for 2013 can be proposed at any time during 2012.

Please address your suggestions, as well as any questions you may have, to Joan Porti, Head of the Meetings Committee, via email: porti@mat.uab.cat.

The Abel Prize Laureate 2012

The Norwegian Academy of Science and Letters has decided to award the Abel Prize for 2012 to

Endre Szemerédi
(Alfréd Rényi Institute of Mathematics, Hungarian Academy of Sciences, Budapest, and Department of Computer Science, Rutgers, The State University of New Jersey, USA)

for his fundamental contributions to discrete mathematics and theoretical computer science, and in recognition of the profound and lasting impact of these contributions on additive number theory and ergodic theory.

The Ferran Sunyer i Balaguer Prize 2012

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Institut d'Estudis Catalans

The Ferran Sunyer i Balaguer Prize 2012 was awarded to

Angel Cano (Universidad Nacional Autónoma de México),
Juan Pablo Navarrete (Universidad Autónoma de Yucatán) and
José Seade (Universidad Nacional Autónoma de México), for their work

Complex Kleinian Groups

Abstract: Kleinian groups were introduced by Henri Poincaré in the 1880s as the monodromy groups of certain 2nd order differential equations. These are discrete groups of automorphisms of the complex projective line CP^1; they can be regarded also as groups of isometries of the real hyperbolic 3-space. These groups have played for decades a major role in several fields of mathematics, as for example in the theory of Riemann surfaces, in holomorphic dynamics and in the geometrization conjecture for 3-manifolds.

In higher dimensions, various authors have studied Kleinian groups regarded as groups of isometries of the real hyperbolic (n+1)-space. In this monograph we study complex Kleinian groups, a concept introduced by Seade and Verjovsky in the late 1990s, though its origin traces back to the work of E. Picard and others. These are discrete groups of automorphisms of the complex projective n-space. This theory includes the groups appearing in real hyperbolic geometry and also those appearing in complex hyperbolic and in complex affine geometry. In fact there are many other ways how complex Kleinian groups naturally appear in mathematics, as for instance via the celebrated twistor construction. This monograph lays down the foundations of the theory of complex Kleinian groups.

This monograph will be published by Birkhäuser Verlag in the series *Progress in Mathematics.*

EUROMATH 2012

Gregory Makrides (Cyprus)

The 4th European Student Conference in Mathematics (EUROMATH 2012) took place in Sofia, Bulgaria, 21–25 March 2012. This annual event is organised by the Cyprus Mathematical Society and the Thales Foundation of Cyprus. EUROMATH aims to provide a forum for students aged 12-18 to present and exchange or develop their ideas or creations in mathematics in an international context. The conference consists of presentations, workshops and other sessions covering multiple themes and mathematical activities. Through these the participants have the opportunity of becoming accustomed to the ideas of research and invention and to discuss and present their findings or other mathematical content to their colleagues.

EUROMATH 2012 was organised in cooperation with the European Mathematical Society, the VUZF University and the Union of Bulgarian Mathematicians. Their contribution to the success of the conference was extremely important and proved once again the worth of international cooperation in such events.

EUROMATH 2012 was put under the auspices of the Bulgarian Ministry of Education, Youth and Science and the opening ceremony was enriched with cultural activities by the Mathematical High School of Sofia.

The conference's activities included a large number of presentations by students from countries in Europe and other continents. These presentations covered a broad spectrum of mathematical areas, ranging from the history of mathematics up to issues that are of current research interest. There were also a number of workshops, led by eminent teachers.

This year's activities also included two enriching events: the MathFactor Competition and a Mathematics Poster Design Competition. The MathFactor Competition consisted of short (3-minute) oral presentations aimed at explaining in simple words, to non-experts, a mathematical concept or idea. The presentations were assessed by a committee, taking into account content, clarity and charisma, the marking using factors multiplied for the total score. In the Poster Competition each participating individual or group of students prepared a poster based on a mathematical idea. The posters were assessed by a group of experts, taking into account content, design and originality. The Munich RE sponsored the prizes for these competitions together with the European Mathematical Society.

Results of Competitions:

MATH-Factor Competition

1st Prize: "More honey, please" – Ljubica Vujovic, The first Grammar School in Kragujevac, Serbia

2nd Prize: "A brief overview and some useful applications of multivariable calculus" – Yue Wang, Malmo Borgarskola, Sweden

3rd Prize: "Is Fibonacci still alive?" – Aleksandar Hrusanov, High School of Mathematics and Science, Bulgaria

MATH Poster Design Competition

1st Prize: Jungic Branimir, XV High School, Zagreb, Croatia

2nd Prize: Veronika Vrhorec, XV High School, Zagreb, Croatia

3rd Prize Shared: Kyriaki Ioannou, Constantina Mikeou, The G. C. School of Careers, Cyprus

3rd Prize Shared: Tomas Sura, Matus Zeman, Leo Cunderlik, Samo Lihotsky, 1st Independent High School, Slovakia

Gregory Makrides
Chair of EUROMATH 2012
President, Cyprus Mathematical Society
President, THALES Foundation
www.euromath.org

MathFactor Finalists

MathFactor 1st Prize Winner

Math Poster 1st Prize Winner

Mathematics School Education Provides Answers – To Which Questions?

Günter M. Ziegler (Berlin)

If mathematics education at school is the answer, what was the question? What is the primary goal of mathematics education at schools? My claim will be that: (a) It is not one goal but at least three; and (b) These goals are moving targets. To name three primary goals:

1. To present mathematics as a part of our culture and as a basis for modern key technologies, presenting answers to very basic, very natural questions, in history, in the present and in the future.
2. To present mathematics as a field that equips everyone with the ability to give answers to important problems and questions that occur in daily life.
3. To introduce mathematics as a field of study – and to lay the foundations for possible (university or vocational) studies in science, engineering or mathematics itself.

All these goals change over time – so in shaping and designing the mathematics school curricula we must be careful that the questions haven't changed fundamentally by the time our answers are being implemented…

1. My perspective

If mathematics education at school is the answer, what was the question? What is the primary goal of mathematics education at schools?

I was invited to present here[1] my view on mathematics school education. In order to make my view plausible, I should perhaps first explain my *perspective*.

I am a research mathematician and I have been a university professor of mathematics for more than 15 years now. I have received prizes for my research but I have not worked on education or didactics. Thus, I am looking at mathematics school education from a university perspective. And I will be talking about the *contents*, not primarily about the *mechanisms*, of school education. However, you will see that I believe that in the great panorama of reasons why mathematics school education fails so often, and to such a large extent (and probably in many countries), the *contents* may be a major component.

As a mathematics university professor, teaching (as I did last year, again) basic courses for beginning students, I am confronted with the results of mathematics school education that our students are equipped with. My summary is: *we are not content*. I assume that you are not surprised by this. However, in my view the fact that the students who try to study mathematics at university know too little mathematics as a result of their high school education is only one component of my and our dissatisfaction. We are not content in multiple ways. Here are *four complaints*.

Complaint I: Insufficient knowledge

The students we get from school have insufficient knowledge of mathematics.

To exemplify this, at the beginning of my first semester "Linear Algebra" course at TU Berlin last year we did a simple entrance test. Only about 50% of the students, all of whom major in mathematics, correctly solved a simple exercise with fractions or could produce the formulas for the area and the circumference of a circle of radius r. 84% gave the value of π to two digits after the decimal point. (The correct answer is 3.14.) This clearly shows that many of our mathematics students are not prepared for studying mathematics – or any other scientific subject. And this not only means Berlin high schools don't work; our students come from all over Germany and also from abroad.

Yes, I know you have heard this complaint before. It is sad, it can be compensated but it is not the main problem. Here is problem number two.

Complaint II: Insufficient knowledge status

The students we get from school have a badly inadequate and insufficient idea about their own state of knowledge.

Many students that I see in exams, oral or written, don't know whether they are good or bad. And they get it wrong in both directions: there are students who think they have mastered it all and basically haven't understood anything (in particular, I observe this in self-confident male students) and I see many students who believe they don't know anything and really have a firm grasp of all the material (this does not only occur with female students).

This is a serious handicap. The students don't know what they know. They do not know how to find out whether they have understood something. They do not know how much they know. As a result they have much too little or much too much self-confidence.

And it appears that this problem, my complaint number two, has become much more serious over the last few decades.

Here comes my most serious and fundamental complaint.

1 This text is based on a plenary lecture at the Fibonacci Project Conference, Bayreuth, September 2010 (http://www.fibonacci-project.eu/). A German translation by the author appeared in Mitteilungen der DMV, (3) 19 (2011), 174–178.

Complaint III: Insufficient knowledge framework

The students we get from school have insufficient knowledge about "What is mathematics?".

Indeed, let me for this point quote from a 2008 study by three British sociologists Heather Mendick, Debbie Epstein and Marie-Pierre Moreau at the "Institute for Policy Studies in Education" at London Metropolitan University. The study was entitled "Maths Images & Identities: Education, Entertainment, Social Justice". It was based on a survey among British students. The authors of the study summarised it as follows:

> *Many students and undergraduates seem to think of mathematicians as old, white, middle-class men who are obsessed with their subject, lack social skills and have no personal life outside maths.*
> *The student's views of maths itself included narrow and inaccurate images that are often limited to numbers and basic arithmetic.*

The first and the second diagnosis belong together. The mathematicians are part of what is Mathematics!

What is mathematics? What do you think? Today's schoolchildren may ask *Wikipedia* for help – and be disappointed. Indeed, Wikipedia won't help you on that:

> *Mathematics is the study of quantity, structure, space, and change. Mathematicians seek out patterns, formulate new conjectures, and establish truth by rigorous deduction from appropriately chosen axioms and definitions.*

Indeed, the German version of Wikipedia goes one step beyond this and, as part of the definition of mathematics, stresses that there is no commonly accepted definition. I translate:

> *Mathematics is the science that developed from the investigation of figures and computing with numbers.*
> *For mathematics, there is no commonly accepted definition; today it is usually described as a science that investigates structures that it created itself for their properties and patterns.*

Is this a good answer?

I believe that if you ask education bureaucrats, you will often find the belief that the question "What is Mathematics?" is answered by high school curricula. But what answers do these give?

If you ask the same question to university mathematicians, they might point you to a very successful book by Richard Courant and Herbert Robbins that has the title "What is Mathematics". However, this is a question – what is the answer? Indeed, the book called "What is mathematics" by Courant & Robbins was first supposed to be called something like "Mathematical discussions of some basic elementary problems for the general public" – before Thomas Mann convinced Richard Courant that "What is Mathematics" is the title that would sell more copies.

Such investigations could give one an idea about what mathematics is – but is that all?

What is Mathematics? It is at least three things at the same time, which we should consider separately and to a certain extent also *teach* separately:

1. A collection of *basic tools*, part of everyone's survival kit for modern-day life – or to put it differently, quoting Dirk Nowitzky's handball coach from the latest issue of the *Mitteilungen*, the *Notices* of the German Mathematical Society: "Mathe ist einfach ein saugutes Werkzeug" – "Maths is a helluva good tool".
2. A *field of knowledge* with a long history, which is part of our culture and an art but also a very productive basis (indeed a *production factor*) of all modern key technologies.
3. A highly developed, active, huge *research subject*.

Complaint IV: Insufficient knowledge of the activity

The students we get from school have insufficient knowledge about what it means to "do mathematics".

To do mathematics does not mean to compute a number. To do mathematics does not mean to apply a formula. To do mathematics does not mean to find a formula. What does a mathematician *do*? This is a nice question as a basis of enquiry-based mathematics education in school!

How can mathematics solve problems?

Let me remind you how mathematicians do *not* solve problems.

1. Typically mathematicians do not *compute a number* as an answer to your problem or to any problem. (Indeed, the rumour that the number "42" is the answer to all questions is British humour, which Germans tend to misunderstand.)
2. Typically mathematicians do not just *apply a formula* or *discover a formula* that solves the problem. (Newspaper stories that start with "Mathematicians have discovered a formula for…" are always nonsense.)
3. Typically mathematicians do not solve a problem in a single passage "from reality to a model that can be solved by mathematics". Indeed, in any practical, industrial or even physics situation the process of creating models, adapting parameters, adding constraints, discovering hidden conditions, etc. is long, and has to go through many cycles until anything useful will be found. In this process, typically a large amount of paper, pencils, erasers, chalk, computer time and coffee is used, with little visible effect.

Nevertheless, mathematics does solve problems, and it contributes knowledge, and it contributes key technologies to virtually all parts of modern high technology. Indeed, there are large parts of our industry that may be understood as "Mathematical Industries" – industries where mathematical tools are essential for design, optimisation and production. This is not only the case for financial industries, telecommunication industries and logistics but nearly all the others as well. Think about it – and tell the children at school. They have to learn that this is a fact and that that's what mathematicians do, and that this is the result of doing mathematics, as part of their view on "What is Mathematics?".

As a result of these four deficits, let me formulate my agenda for mathematics school education.

2. One subject called mathematics is not enough – we need three.

Mathematics I: Basic tools

Of course, a primary goal of mathematics education at school must be to equip all pupils with basic mathematics knowledge and abilities. If we are honest, it is not so much mathematics that we really need and use in everyday life; instead what we need includes numbers, geometric shapes, probabilities, percentages and little more than that. When did you last solve a quadratic equation in real life? When did you last differentiate a function? My impression is that this part of mathematics is the only one that gets a reasonable fraction of space on the school curricula in many countries – but teaching fails miserably for many different reasons. One of them is lack of motivation, which stems from the fact that children are not interested in the topic, which is Mathematics I without Mathematics II–III.

Mathematics II: Field of knowledge

Where does the subject come from? There are 6,000 years of mathematics (or even 22,000 years) full of stories, of history, of developments and of motivation. Indeed, this part of mathematics should probably be taught in school in close cooperation or even jointly with physics and astronomy, as they are so deeply linked.

The fact that mathematics is not only a set of rules and a finished product but that it has history is most important for the view of "What is mathematics?". Meet the heroes and hear the stories about Archimedes, Euler, Gauss, Sonja Kovalevskaya, Andrew Wiles, Grigorij Perelman, Terry Tao and Lisa Sauermann that can shape the image of what mathematics is about!

Still no woman has received a Fields Medal but four women will be the presidents of the four most important world mathematics associations in 2011, among them Ingrid Daubechies, the first woman president of the International Mathematical Union, Marta Sanz Solé from Barcelona, who is the first woman president of the European Mathematical Society, and Barbara Lee Keyfitz, Ohio State University, who will be the first female president of the International Council for Industrial and Applied Mathematics (ICIAM).

This is also the subject where we can and should connect mathematics with the other arts! This is where students can *experience* and *feel* mathematics. The summary is that mathematics as a subject is alive!

Part of mathematics as a field of knowledge has to be a multitude of *answers* to the question: *what is mathematics good for?* Indeed, many students need these answers as part of their motivation for studying mathematics. Perhaps you are aware of the fact that mathematics is a key component of virtually all modern key technologies. All students have to hear about this. They should also get a chance to get in touch with this, as concretely as possible. Try it out, if possible on real problems and real data!

Mathematics III: Research subject

Tell all of them about it! You cannot teach "mathematics research" to all children in school but you have to show them that it exists – that mathematics is alive and that it is constantly changing, that it is a huge subject and that it is ever expanding! You have to show them that it encompasses dozens of fantastic areas of studies that you will never hear about at school, such as topology, ergodic theory, measure theory, group theory, Galois theory, Lie theory, etc.

Also a part of Mathematics III is: *prepare for university!* That is, provide the basics, namely all you need to *know* and to *be able to do* if you want to study (maths or any science or medicine or any other advanced subject). Clearly this should include the basic *concepts* that will be needed for a successful start in university studies – concepts such as *logic*, *functions* and *basic calculus* but perhaps more importantly *proofs*!

Indeed, Mathematics III needs to provide skills for mathematics as a research subject – this heading should thus also contain *proofs*, *problem solving strategies* and preparation and possibly training for mathematics competitions – on all different levels, from kangaroo (for *all* the children) to the International Mathematical Olympiads (for only a very few).

Summary: Many subjects, moving targets

To summarise, if we for a moment try to put together mathematics as a school subject anew, with a fresh start, then we would find that there are a great number of topics – mathematics school education must present a *kaleidoscope of mathematics*:

- Basic tools.
- Field of knowledge, with applications.
- Research subject.

In the end, questions like "which parts of mathematics, facts, components and skills should we teach to which students – and why?" have to be answered.

- Basic tools are needed by every pupil.
- Field of knowledge – history, stories, applications and the overview, important for *motivation* and *education* – is also for everyone!
- Research subject – tell all of them but preparation for university and other career paths as far as possible/necessary should be adapted to respective levels, talents and ambitions.

Certainly this must be done in a *multitude of ways*. At school there has to be time to:

1. Explain, practise and memorise.
2. Ask questions, search for answers and discover stories.
3. Explore, play and compete.

And as we are talking about a *dynamic* subject, we are indeed talking about *moving targets*:
- Mathematics is constantly changing.
- Mathematics school education has to reflect that.

You won't rewrite mathematics school education from scratch but instead look to reshape it in view of the picture/answer to "What is Mathematics?".

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Mikael Passare 1959–2011

Christer O. Kiselman (Uppsala, Sweden)

Mikael Passare at the Nordan meeting in Mariehamn, Åland, 2008 (Photo: Ragnar Sigurðsson)

Mikael Passare died from a sudden cardiac arrest in Oman in the evening of 15 September 2011. His next of kin are his wife Galina Passare, his son Max and his daughter Märta.

Mikael was born in Västerås, Sweden, on 1 January 1959 and pursued a rapid and brilliant career as a mathematician. He started his studies at Uppsala University in the Autumn of 1976 while still a high-school student, merely seventeen and a half. He finished high school in June 1978 in Västerås and gave his first seminar talk in November 1978 at Uppsala University, where he got his Bachelor's degree in 1979 and where he also worked as an assistant. He was then a PhD student with me as his advisor and he presented his thesis on 15 December 1984. He was appointed full professor at Stockholm University on 1 October 1994.

He spent four academic years in four different countries: 1980–81 at Stanford University; 1981–82 at Lomonosov University in Moscow; 1986–87 at Université Pierre et Marie Curie, Paris VI (he was also often at Orsay, Université Paris-Sud 11); and 1992–93 at Humboldt-Universität zu Berlin. He was a guest professor in France on several occasions: at Toulouse (June 1988), Grenoble (April 1992), Bordeaux (May 1992), Paris VII (March 1993), Lille (April 1999) and Bordeaux again (June 2000).

Mikael was awarded the Lundström–Åman Scholarship during the Autumn Semester of 1984 and the Spring Semester of 1985, the Marcus and Marianne Wallenberg Prize in 1988, the Lilly and Sven Thuréus Prize in 1991 and the Göran Gustafsson Prize in 2001.

Mikael was much appreciated as a researcher and teacher and was very active outside the university. He was Head of the Department of Mathematics at Stockholm University from January 2005 through August 2010 and then Director of the newly created Stockholm Mathematics Center, which is a collaboration between Stockholm University and the Royal Institute of Technology. When Burglind Juhl-Jöricke and Oleg Viro resigned from Uppsala University on 8 February 2007, he arranged for a guest professorship for Burglind at Stockholm University and was one of the organisers of a big conference in honour of Oleg, *Perspectives in Analysis, Geometry and Topology*, at Stockholm University over seven days, 19–25 May 2008.

As Chair of the Swedish National Committee for Mathematics, he led the Swedish delegation to the General Assembly of the International Mathematical Union in Bangalore, Karnataka, India, in August 2010.

Mikael Passare was Deputy Director for Institut Mittag-Leffler, Djursholm, Sweden, from 2010. He was very much appreciated for his activity there, which included organising the Felix Klein Days for teachers and a research school for high-school students.

At the time of his death, Mikael was President of the Swedish Mathematical Society and also a member of the Committee for Developing Countries (CDC) of the European Mathematical Society. His activity for mathematics in Africa is described in a later section.

Mikael's nine PhD students

Mikael served as advisor of nine PhD students who successfully completed their degrees. They are registered in the *Mathematics Genealogy Project* and are: Yang Xing, PhD 1992, Mikael Forsberg 1998, Lars Filipsson 1999, Timur Sadykov 2002, Hans Rullgård 2003, Johan Andersson 2006, Alexey Shchuplev 2007, David Jacquet 2008 and Lisa Nilsson 2009.

Mikael's mathematics

Mikael soon became known as an eminent researcher in complex analysis in several variables and his thesis was an important breakthrough with new results in residue theory. Its title was *Residues, Currents, and Their Relation to Ideals of Holomorphic Functions* and it was later published in the *Mathematica Scandinavica*.

Residue theory in several variables is a notoriously difficult part of complex analysis. Mikael's work was inspired by that of Miguel E. M. Herrera (1938–1984). Miguel and I were together at the Institute for Advanced Study in Princeton during 1965–66 and it was there that I learned about residues from him. His results, which culminated in a paper by Herrera and Lieberman and a much quoted book by Coleff and Herrera, published in 1971 and 1978, respectively, were well known long before these publications. I could somehow serve as mediator to Mikael for this interest without doing much research on residues myself.

Also, Alicia Dickenstein, who was a student of Miguel and got her PhD at Buenos Aires in 1982, knew this theory very well and soon came into contact with Mikael. As for integral formulas, Mikael took advice from Bo Berndtsson, already then a renowned expert in that field.

Mikael Passare (age 22), Jean François Colombeau, Leif Abrahamsson, and Urban Cegrell in March or April 1981 (Photo: Christer Kiselman)

Another important person for Mikael's mathematical development was Gennadi Henkin. They met in Moscow during 1981-82 and several times in the period 1984–1990 and then in Paris and Stockholm in 1991–2010.

While residues in one complex variable have been well understood for a long time, the situation is quite different in several variables. There were pioneers like Henri Poincaré (1854–1912) and Jean Leray (1906–1998), and Alexandre Grothendieck developed a residue theory in higher dimensions but it was quite abstract. Through the work of Miguel Herrera, François Norguet and Pierre Dolbeault the theory could be linked to distribution theory, which had been developed by Laurent Schwartz (1915–2002), and that was the road that Mikael continued to follow. He worked intensively with August Tsikh, both on residue theory and amoebas.

Residues in several variables

Let f and g be holomorphic functions of n complex variables. The *principal value* $\mathrm{PV}(f/g)$ of f/g is a distribution defined by the formula

$$\left\langle \mathrm{PV}\Big(\frac{f}{g}\Big), \varphi \right\rangle = \lim_{\epsilon\to 0} \int_{|g|>\epsilon} \frac{f\varphi}{g} = \lim_{\epsilon\to 0} \int \frac{\chi f \varphi}{g}, \qquad \varphi \in \mathcal{D}(\mathbf{C}^n),$$

where $\chi = \chi(|g|/\epsilon)$ and χ is a smooth function on the real axis satisfying $0 \le \chi \le 1$ and $\chi(t) = 0$ for $t \le 1$, $\chi(t) = 1$ for $t \ge 2$.

The *residue current* is $\bar\partial\, \mathrm{PV}(f/g)$. Can the products

$$(\mathrm{PV}(f_1/g_1))(\mathrm{PV}(f_2/g_2)), \quad \Big(\bar\partial(\mathrm{PV}(f_1/g_1))\Big)(\mathrm{PV}(f_2/g_2))$$

and other similar products be defined?

Mikael's construction of residue currents goes as follows. Take $f = (f_1, \ldots, f_{p+q})$, $g = (g_1, \ldots, g_{p+q})$, two $(p+q)$-tuples of holomorphic functions, and consider the limit

$$\lim_{\epsilon_j \to 0} \frac{f_1}{g_1} \cdots \frac{f_{p+q}}{g_{p+q}} \bar\partial\chi_1 \wedge \cdots \wedge \bar\partial\chi_p \cdot \chi_{p+1} \cdots \chi_{p+q},$$

where $\chi_j = \chi(|g_j|/\epsilon_j)$ and the ϵ_j tend to zero in some way.

Coleff and Herrera took $q = 0$ or 1 and assumed that ϵ_j tends to zero much faster than ϵ_{j+1}, which in this context means that $\epsilon_j/\epsilon_{j+1}^m \to 0$ for all $m \in \mathbf{N}$ and $j = 1, \ldots, p+q-1$; thus it is almost an iterated limit. This gives rise to the strange situation that, in general, the limit depends on the order of the functions (and is not just an alternating product).

Mikael took instead $\epsilon_j = \epsilon^{s_j}$ for fixed $s_1, \ldots, s_{p+q}$. The limit, which will be written as $R^pP^q[f/g](s)$, where we now write $[\ldots]$ for the principal value, does not exist for arbitrary s_j. But he proved that if we remove finitely many hyperplanes then $R^pP^q[f/g](s)$ is locally constant in a finite subdivision of the simplex

$$\Sigma = \Big\{ s \in \mathbf{R}^{p+q};\ s_j > 0, \quad \sum s_j = 1 \Big\},$$

so that the mean value

$$R^pP^q\Big[\frac{f}{g}\Big] = \fint_\Sigma R^pP^q\Big[\frac{f}{g}\Big](s)$$
$$= \bar\partial\Big[\frac{f_1}{g_1}\Big] \wedge \cdots \wedge \bar\partial\Big[\frac{f_p}{g_p}\Big] \cdot \Big[\frac{f_{p+1}}{g_{p+1}}\Big] \cdots \Big[\frac{f_{p+q}}{g_{p+q}}\Big]$$

exists. This is the product of p residue currents and q principal-value distributions.

For complete intersections, i.e., when the set of common zeros of $f_1, f_2, \ldots, f_p$ has maximal codimension, Mikael established a division formula with remainder term:

$$h = \sum_1^p g_j f_j + h \cdot \mathrm{Res},$$

where Res is the residue current, which is a factor in the remainder term $h \cdot \mathrm{Res}$ and has the property that $f_j \cdot \mathrm{Res} = 0$ for all j. This implies that h belongs to the ideal generated by $f_1, \ldots, f_p$ if and only if $h \cdot \mathrm{Res}$ vanishes. This is a beautiful characterization of the ideals of holomorphic functions and explains the choice of title in several of his papers. The characterization of the ideals with the help of residues was proved independently and at about the same time by Alicia Dickenstein and Carmen Sessa.

This characterization of ideals enabled Mikael and Bo to formulate an elegant and explicit variant of Leon Ehrenpreis' Fundamental Principle; it was published in a joint paper with Bo in 1989. Later, in 2007, Mats Andersson and Elizabeth Wulcan could define a residue without the assumption of a complete intersection. In this work, a paper by Mikael, August and Alain Yger played an important role.

Mikael showed that his original definition of residues and the definition which uses meromorphic extension agree.

Lineal convexity

André Martineau (1930–1972) gave a couple of seminars on lineal convexity (*convexité linéelle*) in Nice during the academic year 1967–68 when I was there. This is a kind of complex convexity which is stronger than pseudoconvexity and weaker than convexity. Since I was of the opinion that the results for this convexity property were too scattered in the literature and did not always have optimal proofs, I suggested that Mikael write a survey article on the topic.

On the one hand, this piece of advice was certainly very good, for he found a lot of results in cooperation with his friends Mats Andersson and Ragnar Sigurðsson (Mikael's mathematical uncle). On the other hand, it was perhaps not such a good suggestion, for the survey just kept growing; two preprints started circulating in 1991 and by then they had been busy writing for a long time already. The article became a book and it did not appear until 2004. Anyway, it is thanks to André Martineau that lineal convexity came to be studied in the Nordic countries – and the book has become a standard reference.

Amoebas and tropical geometry

Mikael's later work is concerned with amoebas and coamoebas – the first publications in this field were Mikael Forsberg's thesis of 1998 and a joint paper published in 2000. The spine of an amoeba – in mathematical zoology, amoebas are vertebrates – is a tropical hypersurface. Tropical mathematics is a rather new branch of mathematics where addition and multiplication are replaced by the maximum operation and addition, somewhat similar to taking the logarithm of a sum and a product. His interest in tropical mathematics was a break with his earlier work on complex analysis, which he once compared with my switching to digital geometry.

An amoeba is a set in $\mathbf{R}^n$ defined as follows. We define a mapping

$$\mathrm{Log}\colon (\mathbf{C} \setminus \{0\})^n \to \mathbf{R}^n \text{ by}$$
$$\mathrm{Log}(z) = (\log|z_1|, \log|z_2|, \ldots, \log|z_n|).$$

If f is a function defined in $(\mathbf{C} \setminus \{0\})^n$ then its *amoeba* is the image under Log of its set of zeros. The term was introduced by I. M. Gelfand, M. M. Kapranov and A. V. Zelevinsky in 1994.

One can of course study the image in $\mathbf{R}^n$ of any set but zero sets of certain functions have interesting properties. An amoeba is typically a closed semianalytic subset of $\mathbf{R}^n$ with tentacles which go out to infinity and separate the components of the complement of the amoeba. The number of such components is at most equal to the number of integer points in the Newton polytope for f if f is a Laurent polynomial, in certain cases equal to the latter number.

An easy example, which Mikael himself used in his lectures, is the zero set of the polynomial $P(z, w) = 1 + z + w$ of degree one. A zero $(z, w) \in \mathbf{C}^2$ must satisfy $1 \leq |z| + |w|$, $|z| \leq |w| + 1$ and $|w| \leq 1 + |z|$. It is easy to see that any point $(p, q) \in \mathbf{R}^2$ which satisfies the inequalities $1 \leq p+q$, $p \leq q+1$ and $q \leq 1 + p$ is equal to $(|z|, |w|)$ for some zero (z, w) of P. (A useful observation here is the fact that the corresponding strict inequalities are the exact conditions under which there exists a triangle with side lengths 1, p and q.) The amoeba of P is then given by the three inequalities $1 \leq e^x + e^y$, $e^x \leq e^y + 1$ and $e^y \leq 1 + e^x$.

A *coamoeba* is defined analogously but with the mapping Log replaced by the mapping $\mathrm{Arg}(z) = (\arg z_1, \arg z_2, \ldots, \arg z_n)$. Mikael wanted to establish formally the duality between amoebas and coamoebas and he started to write a paper with Mounir Nisse, which Mounir is now finishing.

In a little paper published in the *Monthly* in 2008, which is indeed a gem, Mikael shows how the concept of an amoeba can be used to show the well known formula $\zeta(2) = \sum_1^\infty 1/n^2 = \pi^2/6 \approx 1.644934$ (the so-called Basel problem).

The Pluricomplex Seminar

I started a seminar series in Uppsala in the 1970s, later to become known as *The Pluricomplex Seminar* – a name I borrowed from Jean-Pierre Ramis. Mikael gave his first lecture in the seminar during the Autumn Semester of 1978. He reported on chosen sections of the little book by Lev Isaakovič Ronkin (1931–1998), *The Elements of the Theory of Analytic Functions of Several Variables*, which had been published in Russian (in 2,700 copies) in Kiev the year before and cost 93

Håkan Samuelsson, Elin Götmark, Elizabeth Wulcan, Mikael Passare and Liz Vivas at Institut Mittag-Leffler, Spring 2008 (Photo: Ragnar Sigurðsson)

kopecks. The task was a part of the examination for the course *Mathematics D*. He gave a total of 29 seminar talks over the period 1978–2010.

Originally, the seminars took place at Uppsala with a lecture almost every week. From the Spring Semester of 1999 onwards, when Mikael had become well established as a professor at Stockholm, they became a joint activity for Uppsala University, Stockholm University and the Royal Institute of Technology (KTH). From 2007, when I had switched to digital geometry, mathematical morphology and discrete optimization, and Burglind Juhl-Jöricke had left Uppsala University, it became an activity exclusively in Stockholm.

The *Nordan* Meetings

Together with Mats Andersson and Peter Ebenfelt, Mikael Passare initiated a series of encounters on complex analysis in the five Nordic countries. Mikael and Peter organised the first conference, which took place in Trosa, Sweden, 14–16 March 1997, and Mats organised the second, in Marstrand, Sweden, 24–26 April 1998. Following a voting procedure at the end of the first meeting, these annual meetings were named *Nordan*[1] – a clear reference to *Les Journées complexes du Sud*, which over a long period have taken place in the south of France.

Nordic meetings like these were something that Mikael and Mats had discussed and planned for many years. And the initiative turned out to be a long lasting success: the 15th encounter took place in Röstånga in southern Sweden, 6–8 May 2011; the 16th in Kiruna in northern Sweden, 11–13 May 2012.

Africa

Mikael Passare was a Member of the Board of the International Science Programme (ISP), Uppsala, and a Member of the Board of the Pan-African Centre for Mathematics (PACM) in Dar es-Salaam, Tanzania. He was a driving force in the creation of this Pan-African Centre, which is a collaborative project between Stockholm University and the University of Dar es-Salaam.

Mohamed E. A. El Tom, Chairman of the Board of PACM and a member of the Reference Group for Mathematics of ISP, says he is confident that had it not been for Mikael PACM would have remained a mere idea in the head of its initiator, i.e., in Mohamed's head.

Mikael's last assignment was to chair and constitute a search committee for the Director of the Centre. He accepted the charge and promised to respond with detailed ideas upon his return from his trip to Dubai, Oman and Iran.

Mikael's commitment and enthusiasm for the Centre was unsurpassed. He was confident that the grand objective of establishing a world-class Centre of Mathematics in Africa is attainable.

Sonja Kovalevsky

The chair which Mikael Passare held was the one which was created for Sonja Kovalevsky (03/15 January 1850 – 10 February 1891). An earlier incumbent for seven years (1957–1964) was Lars Hörmander, Mikael's mathematical grandfather. Mikael was proud of having been given Sonja's chair. He is buried not far from her grave.

Exactly 150 years after Sonja's birth, on 15 January 2000, Mikael organised a symposium to her memory. It was held in the *Aula Magna* of Stockholm University. Among the invited speakers were Agneta Pleijel, Roger Cooke and Ragni Piene.

Languages and music

Mikael knew many languages. His Russian was "really perfect!" according to Timur Sadykov. "He spoke Russian perfectly, so it was totally impossible to recognise his Swedish origin," said Andrei Khrennikov. He took a course in French corresponding to 30 ECTS credits at Stockholm University before going to Paris in 1986–87. He learned some Fijian when he visited the Republic of Fiji.

His knowledge of German was very good, although he had not studied that language in high school. He also studied Finnish and spoke the language so well that he was interviewed on the Finnish-language *Sisuradio* in Sweden.

Spanish and Italian he knew enough to get along. He was recently in Italy and Spain with Anders Wändahl and never talked English when visiting a restaurant or when asking for directions in the street. He could also speak some Polish and Bulgarian.

Finally, he studied Arabic and could at least read that language. Maybe Arabic would have been his next project.

Mikael loved classical music; in his teens he sold his bicycle in order to buy a piano. He played clarinet and flute. He composed a piece for clarinet, which was played in a theatre in Stockholm. His last love was an instrument called theremin.[2] He dreamed about being able to play it.[3]

A "Swedish Classic"

Mikael performed what is known as a "Swedish Classic" in 1989. It consists of four parts, which have to be completed within a 12-month period: (1) One of the ski runs, the Engelbrekt Run (60 km) and the Vasa Run / Open Track (90 km); (2) Going around Lake Vättern on bicycle (300 km); (3) The Vansbro Swim (3 km); (4) The Lidingö Run, running (30 km). Mats Andersson remembers that he claimed the cycling to be the most painful of the four, noting the chafing after so many hours on the saddle.

A passionate traveller

Mikael was a passionate traveller. He visited 152 countries. When he and I, together with several other Swedish mathematicians, were invited in September 2006 to celebrate the 20th anniversary of the *Groupe Inter-Africain de Recherche en Analyse, Géométrie et Applications* (GIRAGA) and after that to participate in the *First African-Swedish Conference on Mathematics*, both in Yaoundé, Cameroon, he first visited the Central African Republic and continued afterwards to Equatorial Guinea and Gabon; thus he got four new countries on his list – assuming that he had not been to any of these before – while I got only one.

The United Arab Emirates and Oman turned out to be the last ones. Land number 153 should have been Iran: he planned to arrive at Tehran Imam Khomeini International Airport at 21:25 on 17 September, as he wrote on 15 September 2011, the last day of his life, to mathematicians in Tehran. Siamak Yassemi, Head of the School of Mathematics, University of Tehran, was ready to meet him there.

Finally

Mikael's significance goes much beyond his own research. Many people have testified to his positive view of life, his humour and to his genuine interest in people he met. He was an unusually stimulating partner in discussions: listening, inspiring and supportive, in professional situations as well as private ones.

For Mikael's friends and colleagues around the world his unexpected departure is a severe loss.

For an unabridged obituary and a manuscript entitled "Questions inspired by Mikael Passare's mathematics" see the webpage www.math.uu.se/~kiselman/passareinmemoriam.html.

Notes

1. This is the name in Swedish of a chilly wind from the north but also reminds us of the original purpose: to promote <u>Nord</u>ic <u>An</u>alysis.
2. Терменвокс, which was invented by Лев Сергеевич Термен, Léon Theremin (1896–1993).
3. At his funeral on 28 October 2011, *Dance in the Moon* was played on CD; the performer was Lydia Kavina, a leading thereminist.

Torsten Ekedahl: some recollections

Luc Illusie

At the end of the summer of 1980 I received an astonishing letter. Its author, a certain Torsten Ekedahl, wrote to me: "I have obtained some results on the slope spectral sequence, some of which are perhaps unknown to you. (…)"[1] At that time I was busy together with Michel Raynaud in preparation of an article on the de Rham–Witt complex.[2] Some of the results presented by Ekedhal were known to me but were proved in a shorter and more elegant way. Others, which I had expected to prove but which had resisted all my attempts, were proved with the same ease. A little later, in a subsequent letter, Ekedahl explained to me the solution of a problem whose formulation had even appeared intractable to me: duality in the theory of de Rham-Witt complexes. Once again, the method was very natural and the proof that he sketched very convincing. That was the starting point of an intense correspondence. It was not until later, when Ekedahl went to Orsay in order to finish the preparation of his thesis, that I became privy to how this young student – he was 25 at the time – had taken an interest in this sophisticated theory, which at the time was quite mysterious (and, I fear, remains so today). In July 1978, he was on vacation in Brittany. He had heard of a meeting which was taking place in Rennes, the Journées de géométrie algébrique.[3] Out of curiosity, he went into the lecture hall and listened to the talk I was giving on the de Rham-Witt complex and its relations to crystalline cohomology. Thrilled, he decided to work on the subject. But he made no contact with me so I had no inkling of that.

The year of his stay at Orsay, 1981–82, was one of the most rewarding in my career. I helped him in writing up his thesis and asked him questions. We would see each other practically every day. He resolved all my questions one by one, constantly introducing new ideas. In principle I was his advisor but I often had the impression that I was actually his student. The Künneth formulas in de Rham–Witt theory seemed even more inaccessible than those on duality. Child's play to him. In the Autumn of 1982 there was a conference on algebraic geometry in Japan.[4] I gave a two-hour survey on his work. And thereafter there was the memorable defence of his thesis in Gothenburg in 1983. I played the role of the opponent. In order to trick him, I asked him questions about signs and commutativity of diagrams. Wasted effort. That evening at his home with his family we celebrated the occasion in style, downing a raw, homemade aquavit.

Afterwards we ran into each other frequently, especially in the 80s and 90s. He quickly turned to other subjects, such as surfaces, foliations and moduli spaces, each of which received the spark of his genius. The qualities that first come to my mind were his gentleness, his modesty and his generosity, and his sharp, Bourbaki-like way of tackling problems, coupled with his ability to think in an unconventional fashion, 'penser à côté' (to go against the grain) in the words of Hadamard. How many times were we not on the phone! I can still hear his voice when he picked up the receiver: 'Torsten'. And then began, in French, a language he mastered to perfection, a rich and stimulating conversation.

Adieu, Torsten

Paris, 27 November 2011

Translated from the French by Ulf Persson. The article was initially solicited by the Swedish Mathematical Society, delivered with commendable promptness and published in a Swedish translation by the editor Per-Anders Ivert in the first issue of its new newsletter – SMS Bulletinen – in December 2011. The original will be published in the French journal Gazette des Mathématiciens.

1 In English in the original. Translator's remark

2 L. Illusie et M. Raynaud, *Les suites spectrales associées au complexe de de Rham–Witt*, Publ. math. I.H.E.S. 57 (1983), 71–219.

3 *Journées de Géométrie Algebrique de Rennes, I, II, III*, Eds. P. Berthelot, L. Breen, Astérisque 63, 64, 65 SMF, 1979.

4 *Algebraic Geometry,* Proceedings Tokyo/Kyoto 1982, Eds. M. Raynaud, T. Shioda, Lecture Notes in Mathematics 1016, Springer-Verlag.

Torsten Ekedahl

Jan-Erik Roos

Torsten Ekedahl is dead. He collapsed at the Department of Mathematics of Stockholm University on the morning of 23 November 2011. Attempts at resuscitation by colleagues and paramedics were to no avail. The cause was in most likelihood a massive heart-attack. He was active until the very end. Just a few hours before, he had been logged in on the site "mathoverflow" to which he was a much appreciated contributor. He was 56 years old. It is a very big loss to Swedish mathematics, and many of us have not only lost a good friend but also a passionate and exceedingly knowledgeable discussion partner.

My first real contact with Torsten Ekedahl occurred when I was President of the Swedish Mathematical Society (1980–82). An anonymous benefactor had for many years donated money to the prizes given out to the winners in the Swedish High-school competition in mathematics. I thought (inspired by the AMS) that it would also be a good thing to give stipends to young, promising mathematicians who had just written their dissertation, thus enabling them for the next few years to continue their research without any material worries. At that time this was not so easy to arrange in Sweden. The donator liked the idea and provided the necessary means. It was decided that the first stipend would be awarded in 82/83 after I had been succeeded by Lars-Inge Hedberg as president. Professors in mathematics and adjacent subjects in Sweden were invited in the Autumn of 1982 to suggest candidates, and a committee consisting of myself, Björn Dahlberg and Hedberg was set up as a jury to make a final selection out of the 13 candidates submitted. Torsten Ekedahl was submitted from Gothenburg. It was natural for me to contact Heinz Jacobinski in Gothenburg, whom I knew well from our time in Lund. His reaction was astounding to me. He compared Ekedahl with Hörmander.

The referee reports we requested of the candidates were all positive but in the end it became clear that Ekedahl was in a class all by himself. Torsten received the stipend in January 1983 and a few years later the anonymous donator would be replaced by the Wallenberg Foundation after which the stipend would be renamed. Torsten defended his thesis in Gothenburg on 28 May 1983. His advisor was Juliusz Brzezinski who had played an important supporting role in Torsten's mathematical development, although he was more specialised in his interests. How could Torsten make such an important contribution to a subject so very far away from results that had been obtained earlier in Swedish mathematics? The answer, of course, is to be found in his inherent ability as a mathematician but also in his passionate interest and his ultimate goal to understand all of mathematics.

Torsten was born in Lund in 1955. In the Autumn of 1974 he shared first prize in the mathematical high-school competition referred to above and graduated from high-school the following Spring in Helsingborg. He took his basic university degree (fil.kand) in Gothenburg in 1977. But before he got a stipend for graduate study he had hit upon unconventional ways of furthering his studies and research. The following story is typical (he told it to me himself). When Torsten was on an inter-rail vacation in Brittany he learned coincidentally that an international conference on algebraic geometry was taking place in Rennes (3-7 July 1978). Torsten went there by train and attended the lectures on the first day, no doubt already making comments in the style of which he would later excel, and returned in the evening to his sleeping bag in the train station. This was repeated four times until the conference came to an end. He made valuable contacts, notably with Nicholas Katz from Princeton, but above all he became inspired by the lectures given by Luc Illusie from Paris. Later Torsten was given a doctoral stipend which enabled him to study algebraic topology at Århus (1980/81) while concomitantly developing the threads he had picked up in Rennes. On the strength of the results he achieved in the Summer and Autumn of 1980 he was invited to IHES (1981/82). His crucial move was to seek out Illusie, who later wrote: "He asked me to give him some guidance. I did my best, but quickly the opposite occurred: he was the one who guided me!"

His dissertation dealt with cohomology of algebraic varieties defined over a field of finite characteristics. At the time there were at least three different cohomological theories: the Witt-vector cohomology of Serre, the so-called crystalline cohomology due to Grothendieck and Berthelot and, finally, the Hodge groups, which each, in its way, gave important information about the structure of the varieties. Spencer Bloch and others had introduced a so-called Rham–Witt complex which was intended to connect those various theories and led to a unified theory (de Rham–Witt (hyper)cohomology). This had been studied by several distinguished mathematicians and yet many problems remained, such as duality and multiplicative structure. Ekedahl solved these problems in a natural and elegant way, which had occurred to neither Deligne nor Illusie and which harnessed all the modern algebraic tools available. An excellent survey of all of this is to be found in an article by Illusie in SLN 1016. But, in spite of this predilection for abstraction, Torsten was not a stranger to very concrete applications, which are to be found in his thesis. When he applied for a professorship at Stockholm in 1988, Atiyah was very impressed by his ability to combine abstract theory with concrete results.

After his dissertation Torsten explored many other mathematical avenues with a more classical flavour. One may as an example mention that he showed in an elegant article that the results of Deligne–Griffiths–Morgan–Sullivan on rational homotopy theory of complex projective manifolds were the best possible, in the sense that the Massey-products modulo p could be non-zero for those manifolds. He also showed in a longer work, subsequently to be published in the IHES-series, that many of the standard results on surfaces in characteristic 0 could be extended to finite characteristics, although Kodaira's vanishing theorem did not apply. Miyaoka judged it as "[…] a fundamental contribution to the theory of algebraic surfaces in positive characteristics".

Furthermore, he proved a generalization of Hilbert's irreducibility theorem, which has often been cited, and he had tentative ideas about extending rational homotopy theory to a theory over the integers. He often spent time at IHES but did not have a permanent position of any kind. In 1984, due to a successful evaluation of mathematics, a further research position, a so-called docent position, was created at Stockholm University. Many worthy candidates applied and an expert committee headed by Deligne and Yves Meyer awarded Torsten the position. In that way he became attached to Stockholm University and when a new professorship was created there by the Government in 1988 it was his for the taking. (Incidentally, three Fields Medallists served on the expert committee: Faltings and Hörmander in addition to the aforementioned Atiyah.)

After Torsten became a professor he developed his research interests in all kinds of directions and it is impossible to describe everything he did.

He wrote monographs and collaborated with many mathematicians. He was very happy about a recent collaboration with the combinatorialist Anders Björner applying "étale intersection cohomology" to derive unexpected results about the Bruhat order (*Annals of*

Torsten Ekedahl and Kathryn Hess acting in the play "Fermat's riddle", November 2000

Math. 170, 2009). But he also had joint publications with Gerard van der Geer, the brothers Boris and Michael Shapiro, Nick Shepherd-Barron, Dan Laksov, Trygve Johnsen, Dag Einar Sommervoll, Pelle Salomonsson and not to forget Jean-Pierre Serre. The last joint publication has an interesting story. When Torsten in 1989 attended a conference on the Dutch Frisian island of Texel he got involved in a "competition" with Serre to find examples of the Jacobian of a curve essentially decomposing as a product of elliptic curves. (In classical language, when can certain abelian integrals on a complex curve be written in terms of elliptic integrals?) It is unknown if you can find examples for arbitrarily high genera. It has never been clear to me who "won" but, according to Torsten, Serre thought after a while that they would stop competing (it had by then been brought up to genus 1297). (See the joint note in *Comptes Rendus* 317 (1993), 509–513, as well as comments in the collected works of Serre volume 4.) But Torsten worked with many others, e.g. Carel Faber (they arranged a year on moduli spaces at Mittag-Leffler), Roy Skjelnes, Sergei Merkulov, Sandra di Rocco, Wojciech Chacholski, Richard Bøgvad, Ralf Fröberg, Leif Johansson, Lennart Börjeson, Tomas Ericsson and me. He has also been a very active advisor of many graduate students, out of whom Alexander Berglund can be mentioned as the recipient of several awards.

Torsten was elected a member of the mathematical section of the Swedish Royal Academy of Sciences in 1990. His wide culture was invaluable and his academy lectures to a general educated public on such varied subjects as the Riemann Hypothesis and the results of Perelman on 3-manifolds testifies that he took the obligation to reach the general public seriously. The last lecture will be available until 2015 on http://urplay.se/162252 and it is, incidentally, the best of its kind I have heard and seen of him. Torsten managed by computer animation to show the flow of Perelman's proof without any unnecessary formalism. This really impressed the audience including the attending non-mathematicians.

In addition, Torsten got the Göran Gustafsson Prize in mathematics in 1994, he was a member of the board of the Mittag-Leffler Institute, he worked for the National Swedish Science Foundation and he was also dean for a section of the science faculty at the university.

Torsten was a natural talent. Whatever he touched he always contributed a new thought or a different perspective. This applied not only to mathematics, theoretical physics and computer science but also to other fields of human endeavour. His encyclopaedic erudition and sound judgement made him a much sought-after general lecturer, a member of various expert committees and a referee. One may forgive him if his generosity paired with kindness and a general inability to say no saddled him with too many refereeing assignments that were invariably delayed. But his opinions on important questions were very well thought through and were enormously appreciated.

When a mathematician dies it is common to say he will survive through his publications. This is also true for Torsten but in his case with the addendum that many of his ideas remain half-complete. He had many pending projects, on his own as well as with collaborators, which he would have had the capacity to successfully bring to fruition. He also had inspiring ideas about future research projects which he had not had time to make more precise. In connection to being awarded the Gustafsson Prize (referred to above), one of the referees wrote about one of his "visionary ideas": "I would almost apply for a second life as a mathematician in order to be able to go into this direction myself."

Sweden has lost one of its foremost mathematicians and we miss him very much.

Acknowledgment: I would like to thank Juliusz Brzezinski, Richard Bøgvad and Per Salberger for important information about Torsten as a young man and also Ulf Persson for some technical information and rendering an English version of a text which was an extension of a memorial talk given at the Department of Mathematics of Stockholm University on 30 November 2011.

Jan-Erik Roos, professor emeritus of mathematics at University of Stockholm

The Swedish original, of which this is a slight reworking, was published in December 2011 in the first issue of the newly started newsletter – SMS Bulletinen – of the Swedish Mathematical Society.

Henri Poincaré, 1854–1912

Jeremy Gray (Open University and University of Warwick, UK)

Henri Poincaré. © 2002 Henri Poincaré Archives (CNRS)

The year 2012 will be marked by commemorations of the life and work of Jules Henri Poincaré, who died on 17 July 1912. Meetings held in Paris, London, Utrecht, Rio de Janeiro and elsewhere will recall diverse aspects of his achievements and their present-day implications, and there is a lot to choose from.

Poincaré's achievements

Poincaré first emerged on the mathematical scene in 1879 and 1880 with a number of small papers on number theory after the manner of Hermite, who was pleased with them, and a couple on differential equations. He turned 26 in 1880 so he was no prodigy. But in 1881 he began to publish the work that placed him second in a prize competition of the Académie des Sciences and led to a stream of new ideas that transformed the study of three areas of mathematics: complex function theory, differential equations in the complex domain and non-Euclidean geometry. By 1884, when Poincaré's interests began to embrace yet more fields, he had re-written the theory of Riemann surfaces, created new classes of functions that solve a large class of hitherto intractable differential equations and placed at the centre of it all the topic of non-Euclidean geometry that had previously been merely exotic. The new functions he defined, variously called Fuchsian or Kleinian functions after other investigators or, more generally, automorphic functions, were a generalisation of elliptic functions, a subject of considerable importance in its own right although one that did not detain Poincaré.

It is possible to trace Poincaré's progress quite closely in these years and we do not see a sudden flash that illuminates the whole. Rather, we see the gradual emergence of a governing family of ideas, built around the deepening appreciation of the group idea. Once Poincaré saw how the isometry group of non-Euclidean geometry entered the story he had a programme that he could pursue. It raised questions that he could mostly solve and which, in what was both a cooperation and a competition with the German mathematician Felix Klein, led eventually to a brilliant insight – the uniformisation theorem – that had to remain an unproved conjecture for 25 years.

Thereafter, his work displayed no particular pattern. Unlike most of his contemporaries he did not stay in one field and deepen his understanding of it. Nor, like some more restless souls, did he simply switch fields from time to time. He took up new interests but seldom dropped any. His earliest interests remained his last – his very last paper, as fate was to determine it, was on Fuchsian functions and number theory. But a significant shift came when in the mid-1880s he began to develop theories that applied to planetary astronomy: the shape of planets, their orbits and the long-term stability of the solar system.

These were traditional questions going back at least as far as Newton, and they were central to a French establishment that revered Laplace, but Poincaré reinvigorated them. Once again he soon reached a governing idea, in this case that for such problems the long-term behaviour of the solution curves was what had to be understood. This marked a complete contrast with the astronomers' incremental tradition in which prodigious amounts of calculation were deployed to calculate the ephemerides for only a few years ahead. Poincaré succeeded to a remarkable degree with a preliminary study of differential equations and their solution curves on surfaces – which was a further way for him to appreciate their topology – and then embarked on what became a lifelong involvement with planetary motion. What remains one of his most celebrated discoveries is his demonstration that there is a deep reason for the failure of traditional methods to resolve even the simplest non-trivial problem, the three body problem: even three bodies moving under their mutual gravitational attraction can display chaotic motion and have orbits extremely sensitive to the initial conditions, thus making long-term predictions almost impossible.

When Poincaré became a professor of mathematical physics and probability in 1886 his interest in physics deepened, and no topic was more important and exciting than the theory of electricity, magnetism and optics. To the British this meant the theory presented by James Clerk Maxwell, who had died in 1879, but this theory was distasteful to French scientists who found it lacking the elegant mathematical sophistication they were used to in their own tradition. Poincaré even found it inconsistent but he also admired it for its depth, its mathematics and its appreciation of the fact that there will not be a unique explanation of nature if there is any explanation at all. In the 1890s Poincaré became the French expert on the theory, the man who could indeed provide an elegant exposition of the ideas of Maxwell, Helmholtz and Hertz, point out their strengths and weaknesses and, in due course, do the same for Lorentz's contributions. He also became the adjudicator of a number of disputes in the subject, contributed to the technological exploitation of the new ideas and, in 1905, became the author of one of the lasting ideas in what is now the subject of special relativity: what he modestly called the Lorentz group. Finally, in 1911, his grasp of Max Planck's new theory of quanta was influential in the acceptance of the new ideas with a speed that Planck had feared impossible.

From 1890 until his death Poincaré retained an interest in the theory of real and complex functions in one and several variables and worked successfully on a number of outstanding problems. He made lasting contributions to Sophus Lie's theory of transformation groups and to algebraic geometry. But his major contribution to mathematics in those years was undoubtedly that of topology. It was one of his abiding beliefs that a qualitative analysis of a problem ought to precede a quantitative one; the pioneer of qualitative methods in mathematical analysis was Bernard Riemann, who had died in 1866 leaving behind a profound reorganisation of the subject that would take at least a generation to assimilate. Poincaré's involvement with Riemann surfaces early in his career educated him in the power of Riemann's ideas – in many ways Riemann and Poincaré were kindred spirits – and the three body problem had led Poincaré to contemplate problems in extending Riemann's topological ideas to three dimensions. What he accomplished here essentially created a new branch of modern mathematics: algebraic topology. It may have done so in part because his methods were so visionary that they had more-or-less to be done again and differently in order to be rigorous, but he also set out an attractive topic and ways of approaching its problems. He outlined several ways of defining 3-dimensional manifolds, sketched what would later be called a Morse-theoretic decomposition of them and described the two natural algebraic objects that are associated to a manifold, their first homotopy and homology groups, with enough precision to establish a profound problem, one that grew in successive interpretations to become the Poincaré Conjecture.

Poincaré the person

We have some evidence of how Poincaré actually worked on a daily basis. Like all really good mathematicians, Poincaré kept a structured account or story of mathematics in his mind, one that placed the key concepts, methods and theorems in a coherent way. He read in the fashion of some of the best mathematicians, as his nephew Pierre Boutroux observed (Boutroux, P. 1914/1921. Lettre de M. Pierre Boutroux à M. Mittag-Leffler, *Acta Mathematica* 38, 197–201, rep. in Poincaré, *Oeuvres* 11, 146–151).

HENRI POINCARÉ DANS SON CABINET DE TRAVAIL. — PHOT. DORNAC.

Henri Poincaré in his office. © 2002 Henri Poincaré Archives (CNRS)

> *He did not force himself to follow long chains of deductions, the closely-woven net of definitions and theorems that one usually finds in mathematical memoirs. But going straight away to the result that lay at the centre of the memoir, he interpreted it and reconstructed it in his own way; he took control of it in his own way and then, taking the book up in his hands once again he looked rapidly through the propositions, lemmas, and corollaries, that furnished the memoir … Instead of following a linear route his mind radiated from the centre of the question he was studying to the periphery. As a result, in his teaching and even in ordinary conversation he was often difficult to follow and could even seem obscure. When he expounded a scientific theory, or even told a story, he almost never began at the beginning but, ex abrupto, he set forth at once the salient fact, the characteristic event or the central person, someone he had absolutely not taken time to introduce and whose name his interlocutor did not even know.*

He added: "All his discoveries my uncle made in his head, most often without the need to check his calculations in writing or setting his proofs down on paper. He waited for the truth to strike him like thunder, and counted on his excellent memory to remember it."

This way of working helps explain why Poincaré had rather distant relationships with his contemporaries and no real students. As Boutroux explained to Mittag-Leffler, Poincaré was willing to be very patient with students but when it came to expressing an opinion his standards were very high: either they had really grasped the idea or they had not. Add to that the fact that the French system was much more closely tied to the old model of young independent inventors making their way in the world than the German system of graduate seminars, and the fact that most mathematicians in the 19th century worked on their own anyway, and his isolation is less surprising. But it did not spring from any reluctance to express himself or from an 'ivory tower' mentality; he served energetically on numerous committees and editorial boards. Even those who strayed into his territory, like Jacques Hadamard and Paul Painlevé, do not seem to have become mathematical confidantes.

Another measure of the man is afforded by the work of others that excited and impressed him. The first of these seems to have been Georg Cantor's work on point-set topology, which he applied to his own work in the 1880s. He was impressed by Lie's theory of transformation groups when he met Lie in Paris but he did not work on the subject until 1900, after Lie was dead. Hill's new approach to the study of the motion of the Moon he regarded as an insight into dynamical systems that was likely to be very useful in numerous ways. Among the physicists, the ideas first of Hertz and then Lorentz impressed him and drew him to the frontier of electro-magnetic theory. Hilbert's *Foundations of geometry* he recognised as presenting a profound and radical challenge to his own ideas, and this seems to have impressed him more than Hilbert's work on integral equations, where Poincaré always gave the palm to Ivar Fredholm's contributions. He appreciated Hermann Minkowski's *Geometry of numbers* as a breakthrough in number theory, a topic Poincaré regarded as particularly difficult, and he seems to have appreciated the work of Italian geometers on the theory of algebraic surfaces sufficiently to produce his own, complex analytic version of one of their most incisive results. His last enthusiasm was for Planck's insight into the quantum nature of radiation.

His blind spots and negative judgements are also revealing. He never learned much from Einstein's theory of special relativity and seems not to have fully grasped it, despite coming up with the Lorentz group at the same time. One reason for that seems to be that Poincaré expected a dynamical theory to resolve the fundamental problems but Einstein's solution was entirely kinematic. He did not get involved with the younger generations of French analysts – Emile Borel, Maurice Fréchet, Henri Lebesgue and Paul Montel – and did not take up the idea of measure theory, despite his interest in probability theory and thermodynamics. Famously, he disliked what he saw of the attempt to reduce mathematics to logic and while he remained polite to Zermelo he was doubtful that any attempt to reduce mathematics to axiomatic set theory would succeed. This derived from his feeling that the foundations for mathematics had to be self-evident because they could not rest on anything else and, in Poincaré's opinion, that imposed limitations on how sets could be defined that required them to be no larger than the first uncountable set.

Another side to Poincaré is so well known it is often not appreciated but it may be of particular value to mathematicians today. Throughout his working life Poincaré was preoccupied with what it is to understand a topic and so be able to add to it. Quite apart from the successes this led him to in various fields of mathematics and physics, it also accounts for the lasting interest in his many popular essays. He was not much interested in conveying news of the latest discoveries but passionate about conveying a sense of what it is to do mathematics and physics. Many of his contributions are far from exhausting their value and these essays are among them.

Note. This essay appears in a modified form as part of the introduction in *Henri Poincaré: a scientific biography* by Jeremy Gray, to be published by Princeton University Press in 2012.

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H. Weyl's and E. Cartan's proposals for infinitesimal geometry in the early 1920s

Erhard Scholz

1 Introduction[1]

Einstein's theory of general relativity triggered a multiplicity of new ideas in differential geometry. In 1917, Levi-Civita discovered that Einstein's interpretation of the Christoffel symbols in Riemannian geometry as components of the gravitational field could be given a geometrical meaning by the concept of parallel displacement. That was the starting point for investigating a whole range of generalized differential geometric structures. J. A. Schouten and his student D. Struik studied symbolic methods for establishing an "absolute calculus" in Amsterdam. In Zürich, H. Weyl formed the generalized concept of an affine connection, no longer necessarily derived from a Riemannian metric, and generalized the concept of metrical structure with the idea of a gauge metric and a non-integrable scale connection. A. Eddington investigated affine and linear connections at Cambridge. In Paris, E. Cartan started his programme of bringing Klein's view of geometry to bear upon differential geometry, and at Princeton the group around O. Veblen, L. P Eisenhart and T. Y. Thomas looked for projective structures in differential geometry. Most of these geometrical research programmes were closely related to attempts to create a unified field theory of matter, interactions and geometry.[2]

The upsurge of new ideas made the 1920s and 1930s a happy time for differential geometry. In this contribution we look at the proposals of H. Weyl and E. Cartan from the early 1920s. The question of how the Kleinian view of transformation groups could be imported into a differential geometric setting played a crucial role for both of them. They gave different answers, although with a certain overlap. Only after further steps of generalization could their views be subsumed into an even wider frame, that of connections in principal fibre bundles. This was an achievement of the second half of the century, with C. Ehresmann as one of the principal players. It will not be discussed here; here we concentrate on Weyl's and Cartan's respective views in the 1920s.

2 Weyl

Weyl's papers of 1918 and STM

In April 1918, A. Einstein presented Weyl's paper *Gravitation and electricity* (Gravitation und Elektizität) (Weyl 1918*a*) to the Berlin Academy of Sciences. He added a short critical comment explaining why he doubted the reliability of the physical interpretation Weyl gave. The paper contained a scale gauge generalization of Riemannian geometry, with a *length connection* expressed with a differential form $\varphi = \sum_i \varphi_i dx^i$ as a crucial ingredient. Weyl wanted to identify the scale connection with the potential of the electromagnetic field and built the first geometrically unified theory (UFT) of gravity and electromagnetism on this idea (Vizgin 1994, O'Raifeartaigh 1997). The unification built crucially on the property of φ being a *gauge field*. This idea turned out to be of long-lasting importance, although not in its original form. A few weeks later, a second paper of Weyl followed in *Mathematische Zeitschrift* (Weyl 1918*b*). It presented the same topic to a mathematical audience and put the Weylian metric in the perspective of a broader view of differential geometry. Here Weyl generalized Levi-Civita's idea of parallel displacement in a Riemannian manifold to that of an *affine connection* $\Gamma = (\Gamma^i_{jk})$ (logically) independent of any metric.

The manuscript of Weyl's first book on mathematical physics, *Space – Time – Matter* (STM) (Raum – Zeit – Materie), delivered to the publishing house (Springer) Easter 1918, did *not* contain Weyl's new geometry and proposal for a UFT. It was prepared from the lecture notes of a course given in the Summer semester of 1917 at the Polytechnical Institute (ETH) Zürich. Weyl included his recent findings only in the 3rd edition (1919) of the book. The English and French versions (Weyl 1922*b*, Weyl 1922*a*), translated from the fourth revised edition (1921), contained a short exposition of Weyl's generalized metric and the idea for a scale gauge theory of electromagnetism. E. Cartan read it and referred to it immediately.

Weyl's basic ideas for the generalization of Riemannian metrics in his papers of 1918 and in STM (3rd edition ff.) may be summarised as follows:

(1) Generalize Levi-Civita's concept of parallel displacement for Riemannian manifolds to an abstract kind of "parallel displacement", not a priori linked to a metrical structure, $\Gamma = (\Gamma^i_{jk})$, called an *affine connection* (or *torsion free linear* connection in Cartanian's terminology).

(2) Build up geometry from the purely infinitesimal point of view ("local" in today's physicists language, i.e. using essentially the tangent structure of the manifold), with *similarities* as the basic transformations of space structure because *no natural unit* should be assumed in geometry a priori.

(3) The possibility to directly compare metrical quantities (physical observables) at different points of the spacetime manifold M ought to be considered a *defect of Rie-*

mannian geometry which is due to its historical origin in Gaussian surface theory. It presupposes a kind of "distant geometry" counter to modern field physics.

In Weyl's view it should be possible to choose a *scale* (Maßstab) freely and independently at every point of the spacetime M, to *gauge* the manifold. Then one arrives at a Riemannian (or Lorentzian, etc.) metric $g := (g_{\mu\nu})$ with the squared line element

$$ds^2 = \sum g_{\mu\nu} dx^\mu dx^\nu .$$

Let us call it the *Riemannian component* of a gauged *Weylian metric*. Comparison of quantities (observables) at different points was then possible only by integrating a *length* or *scale connection*, given by a differential 1-form,

$$\varphi = (\varphi_\mu) \qquad \varphi = \sum \varphi_\mu dx^\mu = \varphi_i dx^\mu ,$$

which expresses the infinitesimal change of measuring standards (relative to the gauge). Both components together (g, φ) specify the metric in the chosen gauge.

To secure consistency, a different choice of the scale $\tilde{g} = \Omega^2 g$ has to be accompanied by a transformation

$$\tilde{\varphi} = \varphi - d(\log \Omega) = \varphi - \frac{d\Omega}{\Omega} , \qquad (1)$$

a *gauge transformation (Eichtransformation)* in the literal sense of the word. In late 1918, this word appeared in correspondence with Einstein (Einstein 1987ff., VIII, 661), maybe after their oral discussion in the months before. In 1919, Weyl started to use it in his publications.

In moderately modernized language, we may consider a *Weylian metric* $[(g, \varphi)]$ to be defined by an equivalence class of pairs (g, φ). Equivalence is given by gauge transformations.

With this generalization of Riemannian geometry, Weyl looked for gauge covariant descriptions of properties and in particular for *gauge invariant* objects, among which the *scale curvature* (curvature of the scale connection) $f := d\varphi$ was the first to be found. He discovered that a Weylian metric uniquely determines a compatible affine connection, the Weyl–Levi-Civita connection Γ. It leads to scale *invariant* Riemann and Ricci curvatures, *Riem*, *Ric*, and scale *invariant* geodesics. A Weylian metric turned out to be reducible to a Riemannian one if and only if $f = d\varphi = 0$ (integrable Weyl geometry). Finally, Weyl derived a tensor $C = (C_{ijkl})$ depending only on the conformal class $[g]$ of the metric, with $C = 0$ a necessary condition for conformal flatness (but not sufficient) if $dim\, M = n > 3$. Later it was called *conformal curvature* or the *Weyl tensor* (Weyl 1918*b*, 21).

As has already been mentioned, Weyl originally identified the scale connection φ with the potential of the *electromagnetic* field. That led to a *gauge field* theory for electromagnetism with group $(\mathbb{R}^+, \cdot)$. He thus thought that the Weylian metric $[(g, \varphi)]$ was able to *unify* gravity and electromagnetic interaction. In this frame the Mie-Hilbert theory of matter with its combined Lagrangian for gravity and electromagnetism could be placed in a geometrically unified scheme. This would, so Weyl hoped for roughly two years, lead to a success for a purely field theoretic, *dynamistic* theory of matter.

Einstein did not trust Weyl's new theory physically, although he admired it from a mathematical point of view. He praised the "beautiful consequence (wunderbare Geschlossenheit)" of Weyl's thought "… *apart from its agreement with reality* …" (emphasis, ES) (Einstein 1987ff., vol. VIII, letter 499). For Einstein the path dependence of the scale transfer function for the measurement units

$$\lambda(p_0, p_1) = e^{\int_0^1 \varphi(\gamma')} d\tau, \quad \gamma \text{ path from } p_0 \text{ to } p_1 \qquad (2)$$

gave reason for serious concern. In his view, no stable frequency of atomic clocks could be expected in Weyl's theory. But Weyl was not convinced. He countered with the assumption that there seems to be a *natural* gauge for atomic clocks because they adapt to the local field constellation of scalar curvature (Weyl gauge).

Other physicists, among them A. Sommerfeld, W. Pauli and A. Eddington, reacted differently and at first positively. But after a period of reconsideration they also adopted a more critical position. That did not remain without influence on Weyl. In particular, Pauli's critique formulated in his article on general relativity in the *Enzyklopädie Mathematischer Wissenschaften* (Pauli 1921), known to Weyl in draft already in Summer 1920, and during discussions at Bad Nauheim in September the same year, left traces on Weyl's position.

In late 1920, Weyl withdrew from defending his programme of a purely field theoretical explanation of matter and relativised the role of his unified field theory. But he did not give up his programme of *purely infinitesimal geometry*.

What remained?

Weyl's ideas contained two germs of insight which turned out to be of long-lasting importance:

- The enlargement of the automorphism group of classical differential geometry by the scale gauge group resulted in a *new invariance principle*. Weyl identified it as "the law of the conservation of electricity" (Weyl 1918*a*, 38).
- Moreover, scale gauge geometry was conceptually basic and structurally well founded. Weyl showed this in an investigation which he called the *analysis of the problem of space (APOS)*

The first point was later identified as a special case of *E. Noether's theorems* (Noether 1918).[3] With Yang/Mills and Utiyama's generalization, it became an important structural feature of non-abelian gauge theory in the second half of the century. With regard to the second point, Weyl took up motifs of the 19th century discussion of the problem of space in the sense of *Helmholtz – Lie – Klein* and adapted the mode of questioning to the constellation of field theoretic geometry after the rise of GRT. That made Weyl's enterprise compatible to Élie Cartan's broader programme of an infinitesimal implementation of the Kleinian viewpoint.

Analysis of the problem of space (APOS)

Between 1921 and 1923, Weyl looked for deeper conceptual foundations of his purely infinitesimal geometry in a manifold M (the "extensive medium of the external world") as an a priori characterization of the "possible nature of space". In a clear allusion to Kant's distinction of different kinds of statements a priori, Weyl distinguished an "analytic" part and a "synthetic" part of his investigation. In the first step, Weyl analysed what he considered the necessary features of any meaningful transfer of congruence considerations to purely infinitesimal geometry. In the second step he enriched the properties of the resulting structure by postulates he considered basic for a coherent geometric theory.

His basic idea was that a group of generalized "rotations", a (connected) Lie subgroup $G \subset SL_n\mathbb{R}$, had to be considered similarly to Kleinian geometry. In the new framework of purely infinitesimal geometry, the group could no longer be assumed to operate on the manifold M itself but had only "infinitesimal" ranges of operation. In slightly modernised terminology, G operates on every tangent space of M separately.

Conceptually necessary features ("Analytic part" of APOS):

– At each point $p \in M$ point *congruences* ("rotations") $G_p \subset SL_n\mathbb{R}$ are given. They operate on the infinitesimal neighbourhood of the point (in T_pM). All G_p are isomorphic to some $G \subset SL_n\mathbb{R}$.
– The G_p differ by conjugations from point to point

$$G_p = h_p^{-1} G h_p ,$$

where h_p lies in the normalizer $\tilde{G}$ of G and depends on the point p. Weyl called $\tilde{G}$ the "similarity group" of G.

The G_p allowed one to speak of point congruences ("rotations") inside each infinitesimal neighbourhood T_pM only. In order to allow for a "metrical comparison" between two neighbourhoods of p and p', even for infinitesimally close points p and p', another gadget was necessary. Weyl argued that the most general conceptual possibility for such a comparison was given by a linear connection.

– In addition to the G_p, a linear connection $\Lambda = (\Lambda^i_{jk})$ is given (in general with torsion in the later terminology of Cartan). Weyl called Λ an *infinitesimal congruence transfer*, or even simply a (generalized) *metrical connection*.

An infinitesimal congruent transfer need not be "parallel". Thus an *affine connection* Γ (without torsion)[4] continued to play a different role from a general metrical connection. Moreover, two connections Λ^i_{jk} and $\tilde{\Lambda}^i_{jk}$ may characterize the same infinitesimal congruence structure. This is the case if they differ (point dependently) by "infinitesimal rotations" from the Lie group of G. In more modern language that meant:

$$\Lambda \sim \tilde{\Lambda} \Longleftrightarrow \Lambda - \tilde{\Lambda} = A, \qquad A \text{ diff. form with values in } \mathfrak{g} = Lie\, G . \quad (3)$$

Rotations in the infinitesimal neighbourhoods and metrical connections were, according to Weyl, minimal conditions necessary for talking about infinitesimal geometry in a (generalized) metrical sense. He did not yet consider these conditions sufficient but established two additional postulates.

Complementary conceptual features ("synthetic part" of APOS): In order that an infinitesimal congruence structure in the sense of the analytic postulates may characterize the "nature of space", Weyl postulated that the following conditions are satisfied.

– *Principle of freedom.*
In a specified sense (not discussed here in detail) G allows the "widest conceivable range of possible congruence transfers" at one point.

With this postulate Weyl wanted to establish an infinitesimal geometric analogue to the Helmholtz postulate of free mobility in the classical analysis of space. Of course, it had to be formulated in a completely different way. Weyl argued that the "widest conceivable" range of possibilities for congruence transfers has to be kept open by the geometric structure, in order not to put restrictions on the distribution and motion of matter. In place of free mobility of rigid bodies Weyl put the idea of a free distribution of matter.

The widest possible range for congruence transfer given, Weyl demanded from the group G that it took care of a certain coherence of the infinitesimal geometric structure. For him such a coherence condition was best expressed by the existence of a uniquely determined affine connection among all the metrical connections which could be generated from one of them by arbitrary infinitesimal rotations at every point (compare with equation (3)).

– *Principle of coherence.*
To each congruent transfer $\Lambda = (\Lambda^i_{jk})$ exists *exactly one equivalent affine* connection.

In his Barcelona lectures (Weyl 1923) Weyl gave an interesting argument by analogy to the constitution of "a state" in which a postulate of freedom (for citizens, rather than for matter in general) is combined with a postulate of coherence. He expected from the constitution of a liberal republic that the free activity of the citizens is restricted only by the demand that it does not contradict the "general wellbeing" of the community (the "state"). So Weyl saw a structural analogy between the constitution of a liberal state and the "nature of space" and used it to motivate the choice of the postulates of the "synthetic" part of his analysis of the space.

After a translation of the geometrical postulates into conditions for the Lie algebra of the groups which are able to serve as "rotations" of an infinitesimal congruence geometry in the sense of the APOS (analytical and synthetic part), Weyl managed, in an involved case by case argument, to prove the following.

Theorem. *The only groups satisfying the conditions for "rotation" groups in the APOS (analytic and synthetic part) are the special orthogonal groups of any signature* $G \cong SO(p,q)$ *with "similarities"* $\tilde{G} \cong SO(p,q) \times \mathbb{R}^+$.

That was a pleasing result for Weyl's generalization of Riemannian metrics. It indicated that the structure of Weyl geometry was not just one among many more or less arbitrary generalizations of Riemannian geometry but of basic conceptual importance.[5] Note that, in modernized language, the "similarities" $\tilde{G}$, i.e., the normalizer in $GL(n)$ of the "congruences" G, plays the role of the structure group, not the "rotations" themselves. Weyl implemented a (normal) extension of the congruence group as the structure group of his generalized "metrical" infinitesimal geometry. That gave place to the gauge structure characteristic for his approach.

According to the 4th edition of STM, Weyl proudly declared that the analysis of the problem of space ought to be considered "... a good example of the essential analysis [Wesensanalyse] striven for by phenomenological philosophy

(Husserl), an example that is typical for such cases where a non-immanent essence is dealt with" (Weyl 1922*b*), translation from (Ryckman 2005, 157).

Weyl on conformal and projective structure in 1921

Shortly after having arrived at the main theorem of APOS, Weyl wrote a short paper on the "placement of projective and conformal view" in infinitesimal geometry (Weyl 1921). It was triggered by a paper of Schouten which he had to review for F. Klein. In his paper Weyl investigated classes of affine connections with the same geodesics. These defined a *projective structure* ("projektive Beschaffenheit") on a differentiable manifold. Weyl derived an invariant of the projective path structure, the *projective curvature* tensor Π of M. Vanishing of Π was a condition for the manifold to be projectively flat. In this case it is locally isomorphic to a linear projective space.

In addition, Weyl found a highly interesting relationship between conformal and projective differential geometry and a Weylian metric.

Theorem. *If two Weylian manifolds* $(M, [(g, \varphi)])$, $(M', [(g', \varphi')])$ *have identical conformal curvature* $C = C'$ *and identical projective curvature* $\Pi = \Pi'$*, they are locally isometric in the Weyl metric sense (Weyl 1921).*

This theorem, so Weyl explained, seemed to be of deep physical import. The conformal structure was the mathematical expression for the causal structure in a general relativistic spacetime. Physically interpreted, the projective structure characterized the inertial fall of mass points, independent of parametrization, i.e., independent of conventions for measuring local time. Thus Weyl's theorem showed that the *causal and inertial structure of spacetime* uniquely determine its *Weylian* – not Riemannian – *metric*. This observation was taken up by Ehlers/Pirani/Schild half a century later in their famous paper *The geometry of free fall and light propagation* (Ehlers 1972). It made the community of researchers in gravitation theory aware of the fundamental character of Weyl metric structures for gravity.

Outlook on Weyl in the later 1920s

In the following years (1923–1925) Weyl started his extensive research programme in the representation theory of Lie groups (Hawkins 2000, Eckes 2011). After an intermezzo of intense studies in the philosophy of mathematical sciences in late 1925 and 1926 (Weyl 1927), he turned toward the new quantum mechanics. He published his book on *Group Theory and Quantum Mechanics* (Weyl 1928) and, a little later, on the general relativistic theory of the Dirac equation with a $U(1)$ version of the gauge idea. This idea had been proposed, in different contexts, by E. Schrödinger, F. London, O. Klein and V. Fock.[6] In the early 1920s, he started a correspondence with E. Cartan, interrupted for some years but taken up again in 1930. In a later phase of the correspondence the two mathematicians tried to find out how far they could agree on the basic principles of infinitesimal geometry in the area dominated by the ideas of general relativity. We come back to this point at the end of this paper.

3 Cartan

Towards an infinitesimal version of Kleinian spaces

In 1921–1922 Cartan studied the new questions arising from the theory of general relativity (GRT) for differential geometry. At that time he could already build upon a huge expertise in the theory of *infinitesimal Lie groups* (now *Lie algebras*),[7] which he had collected over a period of roughly 30 years. Among others, he had classified the simple complex Lie groups in (Cartan 1894), and 20 years later the real ones (Cartan 1914). Moreover, he had brought to perfection the usage of *differential forms* ("Pfaffian forms") in differential geometry (Katz 1985). In 1910 he had started to describe the differential geometry of classical motions by generalizing Darboux's method of "trièdres mobiles" (moving frames) (Cartan 1910).[8]

In the early 1920s Cartan turned towards reshaping the Kleinian programme of geometry from an infinitesimal geometric point of view. In several notes in the *Comptes rendus* he first announced his ideas of how to use *infinitesimal group structures* for studying the foundations of GRT. Different from Weyl and most other authors, he did *not* rely on the "absolute calculus" of Ricci/Levi-Civita. He rather built, as much as possible, on his calculus of differential forms. Starting from Levi-Civita's *parallel displacement* like Weyl, he generalized this idea to *connections* with respect to various groups and devised a general method for differential geometry, which transferred Klein's ideas of the Erlangen programme to the infinitesimal neighbourhood in a differentiable manifold. These were "glued" together by the generalized connection in such a ("deformed") way that the whole collection did not, in general, reduce to a classical Kleinian geometry. The arising structures were later to be called *Cartan geometries* (Sharpe 1997).

Deforming Euclidean space

Before Cartan could "deform" Euclidean space $\mathbb{E}^3$, the latter had to be *analysed* in the literal sense of the word. That is, the homogeneous space $\mathbb{E}^3 \cong Isom\,\mathbb{E}^3/SO(3,\mathbb{R})$ was thought to be disassembled into infinitesimal neighbourhoods bound together by a connection, such that from an integral point of view classical Euclidean geometry was recovered. In a second step, the arising structure could be deformed to a more general infinitesimal geometry.

In order to analyse Euclidean space with coordinates $x = (x_1, x_2, x_3)$ Cartan postulated that:

- orthogonal 3-*frames* ("trièdres" – triads) $(e_1(x), e_2(x), e_3(x))$ be given at every point A;
- frames in an "infinitesimally close point" A' (described in old-fashioned notation by coordinates $x + dx$) may be related back to the one in A by (classical) parallel transport. Cartan expressed that by differential 1-forms

$$\omega_1, \omega_2, \omega_3, \quad \omega_{ij} = -\omega_{ji} \quad (1 \le i, j \le 3)\,.$$

In total, $\omega = (\omega_1, \omega_2, \omega_3, \omega_{12}, \omega_{13}, \omega_{23})$ obtained values in the infinitesimal *inhomogeneous Euclidean group* $\mathbb{R}^3 \oplus so(3)$.[9]

Cartan knew that in Euclidean space the ωs had to satisfy a compatibility condition

$$\omega_i' = \sum_k [\omega_k \omega_{ki}]\,; \quad \omega_{ij}' = \sum_k [\omega_{ik}\omega_{kj}]\,.$$

He called this the *structure equation* of Euclidean space (later the *Maurer-Cartan* equation). Here ω_i' denoted the exterior derivative of the differential form and square brackets the alternating product of differential forms.

Using upper and lower index notation ω^i and ω_j^k for the differential forms and Einstein's summation convention, the equation may be rewritten as

$$d\omega^i = \omega^k \wedge \omega_k^i\,, \qquad d\omega_i^j = \omega_i^k \wedge \omega_k^j\,. \tag{1}$$

Passing to "deformed Euclidean space", Cartan allowed for the possibility that parallel transport of the triads around an infinitesimal closed curve may result in an "infinitesimal small translation" and/or an infinitesimal "rotation" (Cartan 1922*d*, 593f.). Then the *structure equations* were generalized and became, denoted in moderately modernized symbolism,

$$d\omega^i = \omega^k \wedge \omega_k^i + \Omega^i \tag{2}$$

$$d\omega_i^j = \omega_i^k \wedge \omega_k^j + \Omega_i^j\,, \tag{3}$$

with differential 2-forms Ω^i (values in the translation part of the Euclidean group) and Ω_i^j (rotational part), which describe the deviation from Euclidean space. Cartan called them the *torsion* (2) and *curvature* form (3) respectively.

Cartan spaces in general

A little later, Cartan went a step further and generalized his approach of deforming Euclidean spaces to other homogeneous spaces. The underlying idea was:

> One notices that what one has done for the Euclidean group, the structural equations of which [(1) in our notation, E.S.] have been deformed into [(2, 3)], can be repeated for any finite [dimensional] or infinite [dimensional] group.[10] (Cartan 1922*a*, 627)

As announced in this programmatic statement, Cartan studied diverse "spaces with connections" or "non-holonomous spaces" (later terminology: *Cartan spaces*) over the following years.[11] Cartan's spaces M arose from "deforming" a classical homogeneous space S with Lie group L acting transitively and with isotropy group G, such that

$$S \approx L/G\,.$$

He directed his interest on the infinitesimal neighbourhoods in S, described, in modernized symbolism, by

$$\mathfrak{l}/\mathfrak{g} \cong \mathfrak{k} \quad \text{with} \quad \mathfrak{l} = \mathrm{Lie}\,L,\ \mathfrak{g} = \mathrm{Lie}\,G\,,$$

and $\mathfrak{k}$ an infinitesimal sub-"group" (i.e., subalgebra of $\mathfrak{l}$), invariant under the adjoint action of G.[12]

The "deformation" of a Kleinian geometry in $S \approx L/G$ presupposed identifications of a typical infinitesimal neighbourhood of S with the infinitesimal neighbourhoods of any point of a manifold M (Cartan: "continuum") that was used to parametrize the deformed space. Cartan thought about such identification in terms of smoothly gluing homogeneous spaces S to any point $p \in M$. More precisely, $\mathfrak{k}$ had to be "identified" with T_xM for all points $x \in M$ in such a manner that the transition to an infinitesimally close point p' could be related to the T_pM sufficiently smoothly. Such an identification was not always without difficulties, although in general Cartan presented the transformation group L as operating on a (properly chosen) class of "reference systems" ("répères") and could derive such an identification from the infinitesimal elements in the "translational" part of L.[13] These intricacies aside, a connection 1-form ω on M with values in $\mathfrak{l}$ could be used to define a connection in the infinitesimalized Kleinian geometry. Then the structural equations (2), (3) defined torsion and curvature of the respective "non-holonomous" (Cartan) space.

In particular, Cartan studied non-holonomous spaces of the:

- Poincaré group in papers on the geometrical foundation of general relativity (Cartan 1922*a*, Cartan 1923*a*, Cartan 1924*b*) (for torsion $\Omega^i = 0$ such a Cartan space reduced to a Lorentz manifold and could be used for treating Einstein's theory in Cartan geometric terms).
- Inhomogeneous similarity group (for torsion = 0, this case reduced to Weylian manifolds).
- Conformal group (Cartan 1922*b*).
- Projective group (Cartan 1924*c*).

In the last case, Cartan introduced barycentric reference systems in infinitesimal neighbourhoods of a manifold (tangent spaces T_pM) ("répères attachés aux differentes point de la variété") and considered projective transformations of them. He remarked that this is possible in "… infinitely many different ways according to the choice of the reference systems".[14] That came down to considering the projective closure of all tangent space.

In this way, Cartan developed an impressive conceptual frame for studying different types of differential geometries: Riemannian, Lorentzian, Weylian, affine, conformal, projective, … All of them were not only characterized by connections and curvature but enriched with the possibility of allowing for the new phenomenon of torsion. And all of them arose from Cartan's unified method of adapting the Kleinian viewpoint to infinitesimal geometry.

Cartan's space problem

Cartan learned about Weyl's problem of space from the French translation of STM (Weyl 1922*a*) and gave it his own twist (Cartan 1922*c*, Cartan 1923*b*). He tried to make sense of Weyl's descriptions of how the "nature of space" ought to be characterized by "rotations" operating in infinitesimal neighbourhoods in terms of his own concepts. He interpreted Weyl's vague description of the "nature of space" to mean a class of non-holonomous spaces with isotropy group $G \subset SL_n\mathbb{R}$ and the corresponding inhomogeneous group $L \cong G \ltimes \mathbb{R}^n$.

Cartan understood Weyl's "metrical connection" in the sense of a class of (Cartan) connections $[\omega]$ with regard to G, and L, where two exemplars of the class $\omega,\ \bar{\omega} \in [\omega]$ differed by a 1-form with values in $\mathfrak{g}$ only. That was a plausible restatement of the "analytical part" of Weyl's discussion; but Cartan passed without notice over Weyl's distinction between "congruences" (G) and "similarities" ($\tilde{G}$). So he suppressed the specific group extension (basically $\tilde{G} = G \times \mathbb{R}^+$) which led to Weyl's scale gauge structure.

On that background Cartan reinterpreted Weyl's "synthetic" part of the analysis and stated:

- "le premier axiome de M. H. Weyl". In any class $[\omega]$ defining a ("metrical") connection with values in L, one can find one connection with *torsion* = 0.

– "le second axiome de M. H. Weyl". Every class $[\omega]$ gives rise to only one torsion free connection.

Cartan's rephrased "premier axiome" had, in fact, not much to do with Weyl's postulate of freedom but at least it was an attempt to make mathematical sense of it. Using his knowledge in classification of infinitesimal Lie groups, he could argue that the "first axiom" is satisfied not only by the generalized special orthogonal groups $SO(p, q)$ but also by the special linear group itself, the symplectic group (if n is even) and the largest subgroup of $SL_n\mathbb{R}$ with an invariant 1-dimensional subspace (Cartan 1923*b*, 174). If the second axiom was added, only the special orthogonal groups remained (Cartan 1923*b*, 192).

Cartan's simplification avoided the subtleties and vagueness of Weyl's "postulate of freedom". Together with the streamlining of the analytical part of the analysis, he arrived at a slightly modified characterization of the problem of space. In this form it was transmitted to the next generation of differential geometers and entered the literature as *Cartan's problem of space* (S. S. Chern, H. Freudenthal, W. Klingenberg, Kobayashi/Nomizu).

In the 1950/60s, Cartan's space problem was translated into fibre bundle language of modern differential geometry without the use of Cartan spaces. In these terms, an n-frame bundle over a differentiable manifold M, with group reducible to $G \subset SL_n\mathbb{R}$, was called a *G-structure* on M. In G-structures, linear connections with and without torsion could be investigated. The central question of the *Cartan–Weyl space problem* (i.e., the Weylian space problem in Cartan's reduced form) turned into the following. Which groups $G \subset SL_n\mathbb{R}$ have the property that every G-structure carries exactly one torsion free connection?

It turned out that the answer was essentially the one given by Weyl and Cartan, i.e. the generalized special orthogonal groups of any signature, with some additional other special cases (Kobayashi 1963, vol. II). From the group theoretical point of view these considerations were still closely related to Weyl's problem of space, while the geometrical question had now been modified twice, first by Cartan then by the differential geometers of the next generation. Only a minority of authors were still aware of the difference between Weyl's and Cartan's problem of space (Scheibe 1988, Laugwitz 1958). These authors insisted that it ought not to be neglected from a geometrical point of view.

Toronto talk: Erlangen, Riemann and GRT

At the International Congress of Mathematicians 1924 in Toronto, Cartan found an occasion to explain his view of differential geometry in a clear and intuitive way to a broader mathematical audience. He started from a reference to the classical problem of space in the sense of the late 19th century:

> From M. F. Klein (Erlangen programme) and S. Lie one knows the important role of group theory in geometry. H. Poincaré popularized this fundamental idea among the wider scientific public [...]
>
> [...] In each geometry one attributes the properties [of figures] to the corresponding group, or *fundamental* group [Hauptgruppe] [...]

It was clear, however, that Riemann's "Mémoire célèbre: Über die Hypothesen, welche der Geometrie zu Grunde liegen" stood in stark contrast to such a perspective.

> At first look, the notion of group seems alien to the geometry of Riemannian spaces, as they do not possess the homogeneity of any space with [Hauptgruppe]. In spite of this, even though a Riemannian space has no absolute homogeneity, it does, however, possess a kind of infinitesimal homogeneity; in the immediate neighbourhood it can be assimilated to a [Kleinian space]. [...]

Such an "assimilation", as understood by him, stood in close connection to frames of references or, in the language of physics, to observer systems in relativity. Cartan observed:

> [T]he theory of relativity faces the paradoxical task of interpreting, in a non-homogeneous universe, all the results of so many experiences by observers who believe in homogeneity of the universe. This development has partially filled the gap which separated Riemannian spaces from Euclidean space ("qui permit de combler en partie la fosse qu séparait les espace de Riemann de l'espace euclidien") [...]. (Cartan 1924*a*)

Thus he did not hide the important role of general relativity for posing the question of how to relate the homogeneous spaces of the classical problem of space to the inhomogeneous spaces of Riemann. But while in physics and philosophy of physics the debate on the changing role of "rigid" measuring rods or even "rigid" bodies was still going on, Cartan himself had been able to "fill the gap which separated Riemannian spaces from Euclidean space" in his own work – building upon the work of Levi-Civita and his own expertise in Lie group theory and differential forms. That was similar to what Weyl had intended; but Cartan devised a quite general method for constructing finitely and globally inhomogeneous spaces from infinitesimally homogeneous ones. In the result, Cartan achieved a reconciliation of the Erlangen programme and Riemann's differential geometry on an even higher level than Weyl had perceived.

4 Discussion Cartan – Weyl (1930)

Weyl's Princeton talk 1929

In June 1929, Weyl visited the United States and used the occasion to make Cartan's method known among the Princeton group of differential geometers. Veblen and T. Y. Thomas had started to study projective differential geometry from the point of view of path structures (Ritter 2011). To bring both viewpoints together, Weyl outlined Cartan's approach of infinitesimalized Kleinian geometries. He discussed, in particular, how to identify Cartan's generalized "tangent plane", the infinitesimal homogeneous space $\mathfrak{k}$ in the notation above, with the tangent spaces T_pM ("infinitesimal neighbourhood" of p) of the differentiable manifold M. To make the Princeton view comparable with Cartan's, one needed not only that an isomorphism $\mathfrak{k} \longrightarrow T_pM$ be given for every point $p \in M$ but also a contact condition of higher order ("semi-osculating") (Weyl 1929, 211). In this case, a torsion free projective connection, in the sense of Cartan, was uniquely characterized by a projective path structure studied by the Princeton group (leaving another technical condition aside).

Cartan's disagreement

Cartan was not content with Weyl's presentation of his point of view. He protested in a letter to Weyl, written in early 1920:

> Je prend connaissance de votre article recent [...] paru dan le Bulletin of the Amer. Math. Society. Je ne crois pas fondée les critiques que vous addressez à ma théorie des espace á connexion projective [...] L'exposition que vous faites de ma théorie ne répond pas tout à faites à mon point de vue. [...] (Cartan to Weyl, 5 Jan 1930)

A correspondence of three letters between January and December 1930 followed.[15]

Cartan did not agree that an infinitesimal Kleinian space had to be linked to the tangent spaces T_pM of the manifold as strictly as Weyl had demanded. He defended a much more general point of view.[16] He even went so far as to admit a homogeneous space of different dimension from the base manifold.[17] Thus Cartan tended toward what later would become fibre bundles over the manifold, here a projective bundle with fibres of dimension n over a manifold of dimension m. On the other hand, he had also studied the conditions under which the integral curves of second order differential equations could be considered as geodesics of a ("normal") projective connection (Cartan 1924*c*, 28ff.).

Weyl insisted even more on the necessity of a ("semi-osculating") identification of the infinitesimal homogeneous space with the tangent spaces of the manifold, in order to get a differential geometric structure that would be truly *intrinsic* to M. He reminded his correspondent that they had discussed this question already in 1927, after a talk of E. Cartan at Bern:[18]

> I remember that we discussed this question already at Bern, and that I was unable to make my point of view understood by you. (Weyl to Cartan, 24 Nov 1930)

In particular, for the conformal and projective structures Weyl now saw great advantages of the studies of the Princeton group (Veblen, Eisenhart, Thomas). Apparently he came to the conclusion that they could be connected to the Cartan approach only after such a smooth (semi-osculating) identification.

Although he did not mention it in the discussion, it seems quite likely that the physical import of conformal (causal) and projective (inertial) structures for GRT played an important background role for Weyl's insistence on the "intrinsic" study of conformal and projective structures. In 1922, Weyl had realised that inertial/projective and causal/conformal structure together determine a Weylian metric uniquely (compare with the end of Section 2). Such considerations make sense, of course, only if conformal and projective structures are understood as intrinsic to the manifold.

Trying to find a compromise

Although Cartan at first defended his more abstract point of view, he agreed that he might better have chosen a different terminology avoiding the intuitive language of a "projective tangent space", which he applied even in the more abstract case of fibre dimension different from *dim M*.

After Weyl had explained why he insisted on the closer identification, Cartan became more reconciliatory:

> [...] je vous accorde très volontiers. [...] C'est un problème important et naturel de chercher comment l'espace linéaire tangent est 'eingebettet' dans l'espace non-holonome donné. (Cartan to Weyl, 19 Dec 1930)

At the end of the year, after the initial problems of understanding each other had been resolved, Cartan admitted that Weyl's question was not just any kind of specification inside his more general approach. Cartan's general view was neither withdrawn nor devalued; it later found its extension in the theory of fibre bundles. But for the more intrinsic questions of differential geometry the identification of infinitesimal Kleinian geometry with the tangent space of the base manifold has become part of the standard definition of *Cartan geometry*.[19]

5 In place of a résumé

Weyl and Cartan started from quite different vantage points for the study of generalized differential geometric structures motivated by the rise of general relativity. Both put infinitesimal group structures in the centre of their considerations. In the early 1920s, Cartan had a lead over Weyl in this regard and it was exactly such geometrical considerations that led Weyl into his own research programme in Lie group representations (Hawkins 2000). After he came into contact with Einstein's theory, Cartan immediately started to work out a general framework for how differential geometry could be linked to an infinitesimalized generalization of Klein's Erlangen programme.

Weyl, on the other hand, started from a natural, philosophically motivated generalization of Riemannian geometry which, as he hoped for about two years, might be helpful for unifying gravity and electromagnetism and might help to solve the riddle of a field theoretic understanding of basic matter structures. After he began to doubt the feasibility of such an approach, he turned towards a more general conceptual-philosophical underpinning of his geometry. That led him to take up the analysis of the problem of space from the point of view of infinitesimal geometry.

Both authors agreed upon the importance of using infinitesimal group structures for a generalization of differential geometry in the early 1920s. They read each other's work and managed to come to grips with it, even though sometimes with difficulties and with certain breaks. Still, at the end of the 1930s Weyl admitted, in an otherwise very positive and detailed review of Cartan's recent book (Cartan 1937), the problems he had had with reading Cartan.[20] But in spite of differences with regard to technical tools and emphasis of research guidelines, they came to basically agree on the way that connections in various groups could be implemented as basic conceptual structural tools in the rising "modern" differential geometry of the second third of the new century.

Notes

1. First appeared in *Boletim da Sociedada Portuguesa de Matemática* (special issue Proceedings of Mathematical Relativity in Lisbon, International Conference in honour of Aureliano de Mira

Fernandes (1884–1958), Lisbon, 2009). Reprinted with permission.

2. Accordingly much of the historical literature is directed at the unified field theory side of the story (Vizgin 1994, Goenner 2004, Goldstein 2003); others look at the geometrical side (Reich 1992, Gray 1999, Bourgignon 1992, Scholz 1999, Chorlay 2009).
3. Noether's paper *Invariante Variationsprobleme* was presented 26 July 1918 to the Göttingen Academy of Science by F. Klein; the final version appeared in September 1918. Weyl could not know it in his publications (Weyl 1918*a*, Weyl 1918*b*). He referred to variational considerations of Hilbert, Lorentz, Einstein, Klein and himself. This remained so even in his later publications (Kosman-Schwarzbach 2011, Rowe 1999).
4. Weyl continued to call Γ a "parallel transfer", in distinction to the "metrical" transfer.
5. In this sense, the analysis of the problem of space may also be read as a belated answer to another of Einstein's objections to accepting Weyl geometry as a conceptual basis for gravitation theory: why should there not appear a "Weyl II" who proposes to make angle measurement dependent on the local choice of units? (Einstein 1987ff., VIII, 777)
6. (Vizgin 1994, Goenner 2004, Scholz 2005).
7. Here we shall switch between the historical and the present terminology without discrimination.
8. For a more detailed discussion of the following see (Nabonnand 2009).
9. The infinitesimal displacement $dx = (dx_1, dx_2, dx_3)$ from A to A' is described by a tangent vector $\sum \omega^i e_i$. The ω^i are differential 1-forms dual to the e_i (they depend linearly on the dx_j). The change of orthogonal frames in A to frames e'_1, e'_2, e'_3 in A' is described by an infinitesimal rotation $e_i = \sum \omega_i^j e_j$ ((ω_i^j) element of the Lie algebra $\mathfrak{so}(3)$), the entries of which not only depend linearly on dx_k but also on the parameters of the rotation group (written by Cartan as x_3, x_4, x_6).
10. "On conçoit que ce qui a été fait poir le groupe euclidien, dont les équations de structure (1) sont déformées en (1'), peut se répéter poir n'importe quel groupe, fini ou infini."
11. The terminology "non-holonomous" was taken over from the specification of constraints in classical mechanics, see (Nabonnand 2009).
12. Compare the modern presentation of Cartan geometry in (Sharpe 1997).
13. Later the *repére mobiles* were substituted by introducing *Cartan gauges*, locally defined by certain $\mathfrak{L}$-valued forms on the manifold. The whole collection of possible *repéres* can be described in modern terms by a principal G-bundle endowed with a $\mathfrak{l}$-valued connection, the *Cartan connection*. The identification of tangent spaces of the base manifold with $\mathfrak{k}$ can then be expressed by the translational part of the Cartan connection. In the physics literature one often speaks of a *solder form* (Sharpe 1997, 174, 181, 235). Compare also the discussion with Weyl discussed below.
14. "… une infinité des manières different suivant le choix de répères". Translated into much later language, Cartan hinted here at the possibility of different trivializations of the projective tangent bundle.
15. The correspondence is preserved at ETH Zürich, Handschriftenabteilung (Cartan 1930). I thank P. Nabonnand for giving me access to a transcription.
16. "En tous cas le problème d'établir une correspondance ponctuelle entre l'espace à connexion projective et l'espace projectif tangent ne se pose ici pour moi: c'est un problème intéressant mais qui, dans ma théorie, est hors de question" (Cartan 1930, Cartan to Weyl, 5 Jan 1930).
17. "On pourrait même généraliser la géométrie différentielle projective à n dimensions sur un continuum à $m \neq n$ dimension [...] " (Cartan to Weyl, 5 Jan 1930).
18. (Cartan 1927)
19. Cf. footnote 12.
20. "Does the reason lie only in the great French geometric tradition on which Cartan draws, and the style and contents of which he takes more or less for granted as a common ground for all geometers, while we, born and educated in other countries, do not share it?"(Weyl 1938, 595)

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Landau and Schur – Documents of a Friendship until Death in an Age of Inhumanity[1]

Reinhard Siegmund-Schultze (Norway)

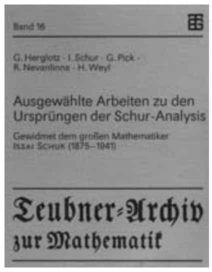
In the following we want to bring to mind two important German-Jewish mathematicians, Edmund Landau (1877–1938) and Issai Schur (1875–1941).[2] They were close friends of about the same age and they were both students of Georg Frobenius (1849–1917) in Berlin around 1900.

During their careers they went into quite distinct mathematical topics and worked at the two leading German (Prussian) universities, in Göttingen and Berlin. Together with their students such as Carl Ludwig Siegel and Hans Heilbronn (of Landau) and Richard Brauer (of Schur),[3] many of whom were persecuted by National Socialism, they have shaped a considerable part of world mathematics, particularly in algebra, number theory and function theory. The two mathematicians have been honoured with the publication of Collected Works. While Landau focused on analytic number theory, and Schur on group representations, they both contributed specifically to complex function theory. In Schur's case his contribution has become known as "Schur Analysis".

Landau and Schur remained friends their entire lives in spite of personal competition and institutional rivalry existing between their universities; their friendship was even reinvigorated in the dark days of the Nazi regime. Too old for a new career abroad and exposed to daily humiliation like all Jewish Germans, only death and emigration prevented them from becoming victims of the ultimate crime, the holocaust.

Their lives mirror many of the problems and prejudices of German mathematics in the first third of the 20th century, not least because their careers and prospects were partly determined by the actions of leading non-Jewish mathematicians such as Frobenius, Felix Klein, David Hilbert, Ludwig Bieberbach and Erhard Schmidt.

Many of the documents cited below speak for themselves and give information about the mathematical research done by these two mathematicians. It is probably due to the extreme conditions of their last years of life that no correspondence between Landau and Schur, which undoubtedly must have existed, seems to have survived.



Left: Edmund Landau (Acta Mathematica 1882–1912, Table générale des tomes 1–35, S. 154)
Middle: Issai Schur (portrait collection Mathematical Research Institute Oberwolfach)
Right: Schur Analysis, edited by B. Fritzsche and B. Kirstein with Teubner in Stuttgart and Leipzig, 1991

I start with the end, with Landau's death in Berlin in 1938 after he had escaped a racist boycott of his lectures in Göttingen and fled to his birthplace Berlin. In an obituary of Landau his friend Schur wrote in the *Jüdische Rundschau (Jewish Review)* in Berlin on 1 March 1938:

> *"On the night of 18 February Edmund Landau died from a heart attack. Science loses one of the* most important mathematicians of our time*, a man of strong vigour and individuality. The main features of his magnificent work were his immense working capacity and a never resting creative energy, connected with a sharp sense of precision and critical judgment. His life's work comprises a considerable number of major monographs, as well as over 300 articles. Analytical number theory recognises him as one of its leading masters; it is largely thanks to him that this field has attained the status of a wide-ranging mathematical discipline over the last four decades. Function theory owes him for a wealth of important innovations and new research methods too.*
>
> [… The omitted passage here will be quoted below.]


JÜDISCHE RUNDSCHAU

BERLIN

Nr. 17, 1. III. 1938

Prof. Edmund Landau

In der Nacht zum 19. Februar ist Edmund Landau im Alter von 61 Jahren einem Herzschlag erlegen. In ihm verliert die Wissenschaft einen der bedeutendsten Mathematiker unserer Zeit, eine Persönlichkeit von stärkster Kraft und Eigenart. Die Hauptmerkmale seines großartigen Schaffens waren eine gewaltige Arbeitskraft, eine nie ruhende Energie in schöpferischem Vorwärtsdringen und Aufbauen, verbunden mit schärfstem Sinn für Präzision und mit kritischem Urteil. Sein Lebenswerk umfaßt eine stattliche Reihe von größeren Werken, wozu noch gegen 300 Einzelabhandlungen hinzukommen. Die analytische Zahlentheorie sieht in ihm einen ihrer führenden Meister; es ist zu großem Teil sein Verdienst, daß sie in den letzten vier Jahrzehnten den Rang einer weitverzweigten mathematischen Disziplin erlangt hat. Ebenso verdankt ihm die Funktionentheorie eine Fülle wichtiger Neuschöpfungen und neuartiger Forschungsmethoden. Er war ein Lehrer großen Stils, ein Künstler im konzisen Darstellen eigener und fremder Ergebnisse. Am glanzvollsten hat er dies in seinen beiden Hauptwerken bewiesen, der zweibändigen „Lehre von der Verteilung der Primzahlen" (1909) und den dreibändigen „Vorlesungen über Zahlentheorie" (1927).

Prof. Landau Privatfoto

Edmund Landau wurde am 14. Februar 1877 als Sohn des bedeutenden Mediziners Leopold Landau in Berlin geboren. Er studierte in München und Berlin, 1901—1909 wirkte er bereits mit großem Erfolg als Privatdozent an der Berliner Universität. Im Jahre 1909 wurde er nach dem Tode Hermann Minkowskis als ordentlicher Professor nach Göttingen berufen, wo er bis 1933 die bedeutungsvollste Zeit seines Lebens verbracht hat. Seitdem lebte er in intensivem Weiterschaffen in Berlin. Jahrzehntelang empfing er seinem wachsenden Ruhm entsprechend hohe wissenschaftliche Ehrungen, er war Mitglied zahlreicher Akademien des In- und Auslands und Ehrendoktor der altberühmten Universität in Oslo.

Einem alten Geschlecht entstammend, fühlte sich Landau aufs stärkste im Judentum verwurzelt. Es erfüllte ihn mit größtem Stolz, Rabbi Jecheskel Landau in Prag zu seinen Vorfahren zu zählen; ihm zu Ehren pflegte er sich mit Vorliebe „Edmund Ezechiel Landau" nennen zu lassen. Mit leidenschaftlicher Liebe hing er am Palästinawerk, insbesondere am Schicksal der Universität in Jerusalem. Er war Mitglied des Kuratoriums der Universität, und es ist seiner Initiative zu verdanken, daß die mathematischen Lehrstühle an der Universität so trefflich besetzt sind. Mit bewundernswerter Energie wußte er noch im reiferem Alter der Schwierigkeiten der hebräischen Sprache Herr zu werden. Nachdem er im Jahre 1925 bei der Abfassung einer hebräisch zu veröffentlichenden Abhandlung zum Teil noch fremder Hilfe bedurft hatte, konnte er schon 1927 monatelang in Jerusalem eine längere Vorlesung in fließendem Hebräisch halten.

J. Sch.

Imprint Jüdische Rundschau and Obituary of Landau

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[1] Translated from the German original in *Mitteilungen der DMV* 19 (2011), 164–173. This publication was an extended version of a short talk given at the annual Euler lecture in Potsdam Sanssouci, 20 May 2011. I thank Heinrich Wefelscheid (Essen), Günter Ziegler (Berlin) and Norbert Schappacher (Strasbourg) for advice.

[2] Some information concerning Schur has already been published on pages 23–28 of the catalogue to an exhibit: J. Brüning, D. Ferus and R. Siegmund-Schultze, *Terror and Exile: Persecution and Expulsion of Mathematicians from Berlin between 1933 and 1945*, Berlin: DMV 1998.

[3] See Walter Ledermann: Issai Schur and his school in Berlin; *Bulletin London Mathematical Society* 15 (1983), 97–106.

Edmund Landau was born in Berlin, 14 February 1877, as the son of the noted physician Leopold Landau. He studied in Munich and Berlin and 1901–1909 he was already working successfully as a private lecturer [Privatdozent] at the University of Berlin. In 1909, following the death of Hermann Minkowski, he was appointed full professor at Göttingen, where he spent the most important part of his life until 1933. Since then he has been living in Berlin, intensively continuing his work. […]"[4]

Over his entire lifetime Landau strove for a full professorship in Berlin but it was denied him. It was only on his expulsion from Göttingen as a 56-year-old that he would return to his birthplace, which he then often left for lectures abroad seemingly without considering permanent emigration.

When in 1912 Landau received a call to Heidelberg in the German state of Baden, we wrote a letter to an official of the Prussian ministry of education, which was responsible for the universities of Göttingen and Berlin. In his letter of 3 December he suggested the prospect of rejecting the appointment in Heidelberg:

"If [your reply] would cause me to stay in Göttingen for now, a decisive point in this decision would be the hope of being able – at a later point of time – to work at the University of Berlin and there – at the place of my parents whose only child I am – to use everything I have learned in Göttingen."[5]

Although Landau was two years Schur's junior, he had already taken his habilitation in 1901, i.e. as a 24-year-old, one and a half years prior to Schur. Therefore he was the primary candidate to become extraordinary professor, although Schur was much closer mathematically to Frobenius and had published with him two fundamental papers on group representation theory in 1906. Applications in 1904 and 1908 by the Philosophical Faculty of Berlin University to promote Landau were, however, turned down by the ministry. Instead, Landau, as mentioned in Schur's obituary, was appointed one year later in 1909 as ordinary (full) professor in Göttingen. This was quite unusual, with respect to skipping an extraordinary professorship but also because there were only a few religious Jews appointed as full professors in the years of the German monarchy.

The historian of mathematics Kurt-R. Biermann, to whom we owe most of our knowledge about the more recent history of Berlin mathematics, argues that both the rejection of Landau's promotion and his final appointment in Göttingen showed that the ministry deliberately privileged Göttingen as a mathematical centre over Berlin.[6] This was not without psychological effect on the established Berlin professors. Frobenius in particular developed as early as the 1890s a kind of persecution mania, directed mostly against the influential Göttingen mathematician and science organiser Felix Klein (1849–1925). In 1914, when the other leading mathematician at Göttingen David Hilbert (1862–1943) refused an appointment in Berlin again, to Frobenius – at least – the bridges between the two mathematical centres were finally burned. This had negative consequences too on the prospects of returning to Berlin for the younger mathematician of Göttingen Landau. In 1917, when refilling the chair of Hermann Amandus Schwarz (1843–1921), the ministry asked the Berlin faculty directly – somewhat against convention – "to comment on Landau in Göttingen too".[7] A hostile reaction from the Berlin mathematicians followed immediately. The faculty sent a proposal, drafted in Frobenius' own hand, for candidates to succeed Schwarz. Erhard Schmidt (1876–1959), who would finally be nominated, was mentioned first. While Schur appeared second on the list, the faculty said about Landau:

"Landau is an extraordinarily diligent and talented man but somewhat one-sidedly oriented towards analytic number theory. Landau and Schur are the best scholars that have originated from the school of Mr Frobenius over the past 25 years. […] But the versatile Schur compares to Landau like a genius to a talent. Among Schur's works are many of highest value. The majority of Landau's works, as interesting as they are now, would lose their value on the day when a certain conjecture of Riemann is fully proven. […] In consideration of all these circumstances the Faculty cannot think about proposing Landau as ordinary professor at a university where Schur […] is unfortunately still merely an extraordinary professor."[8]

Given the fact that the "certain conjecture of Riemann" is even today, in 2012, unproven and given that Landau was highly appreciated by foreigners such as the Englishman G. H. Hardy and the Dane H. Bohr and later would have influential students as well, the faculty's proposal is clearly recognisable as an emotional statement, strongly coloured by Frobenius' self-interest.

We do not know whether Landau learned about this unjust comparison with Schur although we deem it highly probable, a suggestion that is supported by the following citation. In any case, it is remarkable that Landau did not show any sign of jealousy with respect to his old friend Schur. This follows convincingly from Landau's detailed, seven-page proposal, dated 27 February 1919, to elect Schur as a member of the Göttingen Society of Sciences. Here we read among other things:

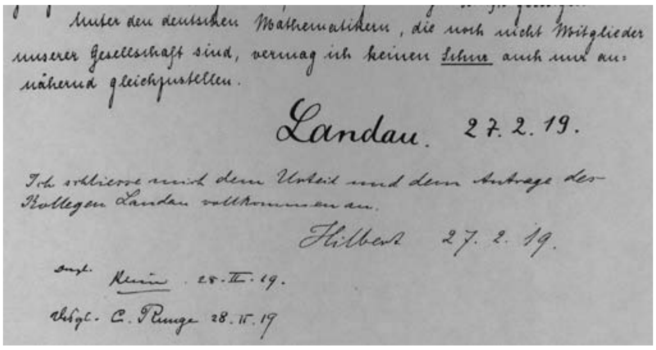
Frobenius (Source: Biermann loc. cit. portrait appendix)

"I deem Schur one of the most important mathematicians of my generation

[4] *Jüdische Rundschau* 43 (1938), 1 March 1938, p. 7. Accessible online through www.compactmemory.de. Emphasis by Schur.
[5] Geheimes Staatsarchiv Berlin, Rep. 76 Va, Sekt. 6, Tit. IV, Nr. 1, Bd. 23, Bl. 265.
[6] K.-R. Biermann: *Die Mathematik und ihre Dozenten an der Berliner Universität 1810–1933*; Berlin: Akademie-Verlag 1988, p. 177.
[7] Biermann l.c. p. 182.
[8] Biermann l.c. p. 328.

[…]. Even though Schur has an established name based on his work in number theory, function theory, integral equations, differential equations […] his main accomplishments are barely known in our Göttingen mathematical circle, even though they fall in the realm of our interests […]. Schur is supposed to be the only student of Frobenius. However, the word student has to be very much understood in a transferred sense. In the mathematical school at Berlin of the 90s there did not exist much of a stimulus for independent research or even for doctoral theses. Frobenius learned about his 'student' only at the moment when Schur delivered his finished doctoral dissertation, a work by which he joined – according to F.'s verdict – the ranks of the 'masters of algebraic research' […]. Particularly important for group theory are the two treatises in the Berlin Reports of 1906, the only ones where he worked together with Frobenius. They contain some of the most beautiful and most peculiar theorems of the entire theory; for instance 'each finite group of linear substitutions with real coefficients can be transferred by a linear transformation of variables into a group of orthogonal substitutions.' […]

Following what has been said so far one could deem Schur's interests and output one-sided (as was actually and unfortunately the case with his important teacher Frobenius). […] But Schur was from his days as a student the first in our Berlin circle to go into all modern disciplines of mathematics (except for geometry, which unfortunately was not offered to us in Berlin), for example set theory, newer function theory, axiomatics, integral equations and Lebesgue's integral, and he enlightened even the specialists among us. His output shows that he was able to apply his algebraic vigour to many problems of analysis proper. […]"[9]



Unter den deutschen Mathematikern, die noch nicht Mitglieder unserer Gesellschaft sind, vermag ich keinen Schur auch nur annähernd gleichzustellen.

Landau. 27.2.19.

Ich schliesse mich dem Urteil und dem Antrage des Kollegen Landau vollkommen an.

Hilbert 27.2.19.

Klein 28.II.19.

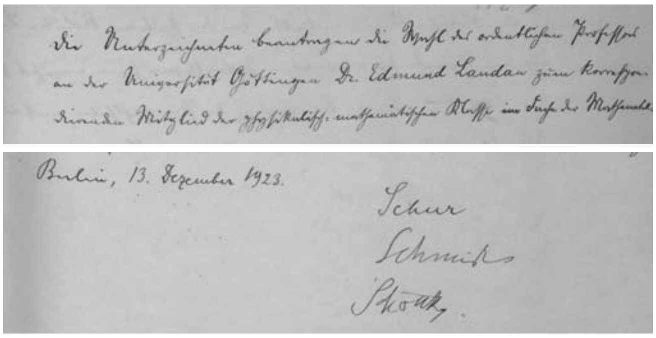
C. Runge 28.II.19

Signatures of Landau, Hilbert, Klein and Runge under the election proposal for Schur (courtesy of the archives of the Göttingen Academy of Sciences, archived there under Pers. 20, 988)

It is very interesting that Landau admits that the Göttingen mathematicians still had something to learn from their colleagues in Berlin. Like Frobenius, Landau stresses Schur's versatility. He denies Frobenius, however, any accolades for instilling this quality in Schur, calling Frobenius one-sided instead. This accusation is undoubtedly a kind of revenge on Frobenius and a sign of wounded pride.

Schur himself fended off Frobenius' accusation against Landau's alleged one-sidedness, supporting Landau's election as corresponding member of the Prussian Academy of Sciences in Berlin, 13 December 1923. In a proposal in Schur's handwriting which was co-signed by Schmidt and Friedrich Schottky (Bieberbach stood himself for election at the same time) one reads:

"Many of the works mentioned show that Landau is not just an acute number theorist but also a powerful analyst. He has worked with great success in many fields of function theory proper. The most important result which function theorists owe to Landau is his generalization in 1904 of Picard's theorem which shows that a transcendental entire function f(z) assumes one of the values 0 or 1 in a circle, which is defined by f(0) and f'(0) ≠ 0 alone."[10]



Die Unterzeichneten beantragen die Wahl des ordentlichen Professors an der Universität Göttingen Dr. Edmund Landau zum korrespondierenden Mitglied der physikalisch-mathematischen Klasse im Fache der Mathematik

Berlin, 13. Dezember 1923.

Schur

Schmidt

Schottky.

Reproduction with permission of the archives of the Berlin-Brandenburg Academy of Sciences Berlin, PAW (1812–1945), II-III-140, p. 172–173

Landau's cordial relationship with Schur did not suffer as a result of Schmidt's appointment in Berlin in 1917 or Schur's promotion to "personal ordinarius" in late 1919 at the same place. However, these appointments must have made it clear to Landau that the opportunities for him to move to Berlin practically did not exist anymore. His friend Schur knew about his wish and tried to help. When in 1921 Schur's promotion to a regular ordinary professorship as successor to retiring Friedrich Schottky (1851–1935) was imminent, Schur saw for a short time a chance for Landau. On 4 April 1921 he wrote a letter to Ludwig Bieberbach (1886–1982), the noted function theorist who had just been appointed in Berlin:

"I now want to come back to a remark which you made when you were here. When I said that I wanted to propose Landau as my successor, you replied that you did

[9] Archives Akademie der Wissenschaften zu Göttingen, Pers. 20, 988. Emphasis by Landau. A facsimile of this document can also be found on pages 31–37 in the unpublished work by Wolfgang Kluge: *Edmund Landau: Sein Werk und sein Einfluss auf die Entwicklung der Mathematik*, 154 pp. written homework, University of Duisburg, advisor Heinrich Wefelscheid. The text of the proposal is clearly not in Landau's handwriting; only the signature is. Mr Wefelscheid tells me that Landau used to commission students to copy his drafts, Lotti von Baranov among them.

[10] Archives Berlin-Brandenburg Academy of Sciences Berlin PAW, II–III, 140, fol. 172–173. Landau's student Konrad Knopp, in his obituary of Landau in 1951 to be quoted below, recalls with regard to the theorem cited by Schur the "beaming pride with which he [Landau] communicated to us his discovery". (p. 60)

not like the idea and would prefer younger scholars. Nevertheless I would like to win you around to my proposal. We barely had a chance to discuss Landau's importance as a researcher and teacher but I assume that you too consider him one of our foremost scholars. I can say of myself that I have always admired the unusual strength of his talent and his extraordinary breadth of knowledge. In his recent works he shows some bad habits, which I regret too, but I consider this merely an external issue; many of these works are still full of power and important results. Also as a teacher he goes his own way but you know, as much as myself, how successfully he has introduced his students to his methods and how much they have learned from him, even if they went along different directions later.

If we succeed in winning a personality like L. for Berlin, the interests of the younger have to take second place. Together with him we could all accomplish a collaboration in great style and this goal is definitely the most important we should aim at.

I have reason to believe that Landau would not refuse a call to Berlin and would not make excessive demands. He feels close to Berlin and thrives best here."[11]

Schur's proposal could not be realised, most of all because his previous position was kept only at the level of an extraordinary professorship. Moreover it seems clear that Schur did not meet with much approval from his colleagues in the faculty: Bieberbach, Schmidt and the applied mathematician Richard von Mises (1883–1953). The latter wrote in his personal diaries with the date of 29 May 1921:

"In the afternoon discussion with Schmidt, Schur and Bieb. re successor. Joint refusal of Schur's effort to bring Landau here."[12]

This opposition may well have had its cause in Landau's "bad habits" mentioned in Schur's letter and which are usually described as the "Landau style" in mathematics. Probably the best description of this mathematical style can be found in Landau's obituary by Konrad Knopp (1882–1957), the first student of Landau's.[13] In this obituary in the *Jahresbericht of the DMV*, which could only appear in 1951 after the fall of the Nazi regime, one reads:

"His first major work (Leipzig, 1909), the 'Handbook on the theory of the distribution of prime numbers', is still written in the old style of the young Landau: a most detailed explanation of motivations, careful discussion of all details, overview of the various proof methods, almost no unexplained notions or facts […].

However, a few years later in 1918 (Leipzig, 2. Edition, 1927), his 'Introduction into the elementary and analytical theory of algebraic numbers' reveals the new style of the older Landau, who has now matured to the final way of thought and creation. It is this way of presentation, which as 'Landau style' has become exemplary for many, that is rejected as exaggerated by some. Avoiding any superfluous, even any not strictly necessary word, it instantaneously presents definition 1, definition 2, theorem 1, proof, theorem 2 […] and leaves it to the reader to understand for themselves the general ideas behind the argument. […] This 'Landau style' is on the one hand very impersonal and objective. It lets the facts speak for themselves; the inner experience has to recede. On the other hand, however, the Landau style is so closely connected to the person of its originator that it cannot be 'imitated,' as much as it has served as an example for English mathematical literature and will certainly still have future influence everywhere."[14]

To my knowledge there is no investigation in the existing literature asking which biographical circumstances may have caused Landau to change his mathematical style in the way described by Knopp. It is remarkable, though, that World War I lies between the "old" and the "new" style. In the opinion of this author it cannot be ruled out that this political earthquake may have caused a certain disillusionment in Landau with respect to the honesty and the effect of human motivations and subjectivity.[15] A purely negative interpretation of this style, which places the responsibility squarely on Landau's personality and which is ultimately racist was given by Ludwig Bieberbach in his infamous writings on "styles of mathematical creation". These racist theories, which were often summarised under the name of 'Deutsche Mathematik' (German mathematics), served to 'justify' the mass expulsions of Jewish scientists after 1933. Also, 'modern algebra' in the sense of Emmy Noether was attacked by Bieberbach and his accomplices, the 'structural method' of which, by the way, had little in common with the 'Landau style'.

Bieberbach (Source: Catalogue, Terror and Exile, loc. cit., p. 9

In April 1934 Bieberbach wrote the following in an article about the student boycott in Göttingen that led to the 'voluntary' resignation on the part of Landau:

"A few months ago differences with the Göttingen student body put an end to the teaching activities of Herr Landau. ... This should be seen as a prime example of the fact that representatives of overly different races do not mix as students and teachers. ... The instinct of the

[11] Handwritten, 2 folios. Bieberbach's partial holdings kept by Menso Folkerts, Munich, who is hereby thanked for allowing this citation from the letter.

[12] Harvard University Archives, Richard von Mises Papers, HUG 4574.2 Diaries 1903–1952. Thanks to the Archives for allowing this citation, translated from German shorthand.

[13] Knopp took his PhD in 1907 in Berlin. The official judgment was written by Schottky and Frobenius, since Landau was not yet a professor and not yet entitled to officially act as advisor.

[14] K. Knopp: Edmund Landau, *Jahresbericht DMV* 54 (1951), 55–62, pp. 56–57.

[15] On the effect of World War I on Landau see my recent "Opposition to the Boycott of German Mathematics in the Early 1920s: Letters by Edmund Landau (1877–1938) and Edwin Bidwell Wilson (1879–1964)"; *Revue d'histoire des mathématiques* 17 (2011), 139–165.

Göttingen students was that Landau was a type who handled things in an un-German manner."[16]

Bieberbach also helped the new rulers in throwing out his colleague Issai Schur. This he did on the one hand as dean of the faculty forcing Schur into premature retirement in August 1935, even before the "Nuremberg laws" of September 1935 came into effect. On the other hand, Bieberbach was an influential member of the Prussian Academy of Sciences to which belonged almost all the full professors of the University of Berlin and in this capacity he forced Schur out of the Academy and its commissions.

In March and early April 1938 mathematicians and physicists of the Academy who belonged to the academic commission for the publication of Karl Weierstrass' works signed a circular, beginning with the signatures of Erhard Schmidt and Issai Schur, who both wrote: "read" [gesehen]. The following signatures were [see facsimile below]:

29 March, Bieberbach: "I find it surprising that Jews are still members of academic commissions."
30 March, Th. Vahlen: "I propose modification."
3 April, M. Planck, who was Secretary of the Academy: "I will take care of it."


PREUSSISCHE AKADEMIE DER WISSENSCHAFTEN

Eingegangen 1. APR 1938

Zirkular

An die Mitglieder der Akademie

Source: Catalog, Terror and Exile, loc. cit., p. 26.

In the relevant file of the Academy, Schur's resignation from the academic commissions follows immediately. Half a year later Schur had to resign from the Academy altogether. In 1928 Bieberbach and Schur published, all the same, a well-known joint article in the Proceedings of the Academy.[17]

About the harsh times which Schur had to live through after his dismissal from the university in 1935 we have a report by his student Alfred Brauer (1894–1985) from a speech he gave in East Berlin on the 150 year jubilee of Humboldt University. Brauer was the older brother of the better known group theorist Richard Brauer (1901–1977), who emigrated to the United States even earlier than Alfred. Both were students of Schur. Alfred writes:

"The enforced end of his teaching at the age of 61 was a terrible blow to Schur. During the short time in which Rohrbach stayed on as assistant at the Mathematical Institute of Berlin University it was still possible to indirectly see books from the institute's library. But after Rohrbach lost his position and left for Göttingen as an assistant, we were increasingly cut off from the world of mathematics. One example will illustrate this. When Landau died in February 1938, Schur was supposed to give a speech at his funeral. For this he needed some mathematical facts, which had, however, escaped him. He asked me to find these facts in the literature. Of course, I was forbidden to use the library of the Mathematical Institute which I myself had built up over many years. I sent an application to the Prussian State Library. I was permitted, at a fee, to use the reading room of this library for a week. I was not allowed to borrow books, though. Thus I could at least answer some of Schur's questions. In those years I visited Schur quite often. The ever new stipulations which made the life of all German Jews increasingly difficult led to deep depression in Schur. He obeyed the laws punctiliously. Nevertheless it happened several times, when he opened the door after I had rung, that he would shout out, relieved: 'Oh, it is you and not the Gestapo.'"[18]

From an indirect source, namely Schur's student Max Schiffer (1911–1997), who was in Palestine from 1933 where he met Schur in 1939, we have another report about Schur's time in Berlin after 1935. Following Schur's information Schiffer says that the physicist Max von Laue and the mathematician Erhard Schmidt visited Schur after his dismissal. In the same report Schiffer quotes Schur's memories of one of these visits, a quotation which has to be evaluated with caution:

"When he complained bitterly to Schmidt about the Nazi actions and Hitler, Schmidt defended the latter. He said, suppose we had to fight a war to rearm Germany, unite with Austria, liberate the Saar and the German part of Czechoslovakia. Such a war would have cost us half a million young men. [...] Now Hitler has sacrificed half a million Jews and has achieved great things for Germany. I hope some day you will be recompensed but I am still grateful to Hitler."[19]


Erhard Schmidt, 1876–1959 (Source: Biermann loc. cit., portrait appendix)

It is clear also from other sources that Schmidt – unlike Laue – only rarely had the courage of resisting the regime openly. However, his behaviour towards Schur and Landau was quite different from Bieberbach's. In an internal report on Schmidt to the Berlin NS organisation of docents, written by the Nazi and mathematician Werner Weber, one reads:

[16] L. Bieberbach: Persönlichkeitsstruktur und mathematisches Schaffen; *Unterrichtsblätter für Mathematik und Naturwissenschaften* 40 (1934), 236–243, p. 236.

[17] "Über die Minkowskische Reduktionstheorie der positiven quadratischen Formen", *Sitzungsberichte der Preussischen Akademie der Wissenschaften* 1928, Physikalisch-mathematische Klasse, pp. 510–535

[18] A. Brauer: Gedenkrede auf Issai Schur, in A. Brauer and H. Rohrbach (eds.), *Issai Schur: Gesammelte Abhandlungen*, Berlin: Springer 1973, volume I, v–xiv, p. vii.

[19] Menahem Max Schiffer: Issai Schur: Some personal reminiscences; in H. Begehr (Hrg.): *Mathematik in Berlin: Geschichte und Dokumentation*; Aachen: Shaker, 1998, volume 2, pp. 177–181, p. 180.

"I think that Schmidt shows little or no understanding of the Jewish question. It was striking that a few days after the death of the Jew Landau (February 1938) Schmidt was the only one in the entire mathematical faculty who knew about it at all. He was also informed in detail about the circumstances: which disease Landau had had, when he was buried, where the various relatives stayed […]. Where he had gathered all that information from (contact with I. Schur??), I do not know."[20]

Landau's tombstone at the Jewish cemetery in Berlin-Weißensee before and after the restoration in the nineties upon initiative by Heinrich Wefelscheid, Essen. The vanished D of the given name even after restoration seems to remind us of keeping Landau's name alive.

When Landau was buried on 22 February 1938 at the Jewish Cemetery in Berlin-Weißensee, Schur, based on the support of his student Alfred Brauer, gave the funeral address. The only non-Jew taking part in the funeral was apparently Theodor Heuß (1884–1963), who after the war became President of the Federal Republic of Germany.[21] In the obituary of his friend Edmund Landau in the Jüdische Rundschau of 1 March 1938, already quoted above, Schur says among other things:

"He was a teacher of grand style, an artist in the concise presentation of his own and other people's [fremder; R.S.] results. Most brilliantly he showed this in his two main works, the two-volume 'Theory of the Distribution of the Prime Numbers' (1909) and the three-volume 'Lectures on Number Theory' (1927)."

I believe that in view of Bieberbach's racist concoctions within "Deutsche Mathematik" Schur's choice of the words "grand style" and "own and other people's results" is not coincidental, bearing also in mind that in the German language the word "fremde Ergebnisse" (other people's results) has the double meaning "foreign" or "alien". That Schur – unlike Knopp in his obituary – would not put the works of the young and the old Landau in contrast to each other apparently aims at stressing the complexity of Landau's oeuvre and the equal value of various mathematical approaches. Schur ends his obituary with the following words:

"Descended from ancient lineage, Landau felt deeply rooted in Judaism. He prided himself immensely in counting Rabbi Jecheskel Landau in Prague among his forbears; in his honour he preferred to be named 'Edmund Ezechiel Landau'. With passionate love he adhered to the Zionist project of Palestine [Palästinawerk], in particular to the fate of the University in Jerusalem. He was a member of the Advisory Board [Kuratorium] of the university, and the university owes it to him that the mathematical chairs are in so competent hands today. With admirable energy he mastered even in mature age the difficulties of the Hebrew language. While in 1925 he still had to rely on the help of others when writing an article in Hebrew, in 1927 he was able to give a long lecture in fluent Hebrew."[22]

One realises that Schur, who was less traditional in religious matters than Landau, under anti-Semitic persecution stressed the Jewish traditions, a quite well-known effect of the conditions of the Third Reich.

Before Schur emigrated in January 1939 from Berlin, under permanent threat from the Gestapo and afflicted by health problems, he tried – though without success – to mediate the transfer of Landau's library to the university in Jerusalem.[23]

Schur himself fled to Tel Aviv but he did not have strength left to resume his mathematical work before his death there in 1941.

Left: Landau's tombstone at the Jewish cemetery in Berlin-Weißensee (Photo: Christoph Eyrich)
Right: Tombstone of Schur and his wife Regina in Tel Aviv, Israel (courtesy of Leo Corry, Tel Aviv)

Reinhard Siegmund-Schultze [Reinhard.Siegmund-Schultze@uia.no] was born in 1953 and is currently professor at University of Agder (Norway). He studied mathematics in Halle and has been an historian of mathematics for three decades, since 2000 in Norway. In recent years he has been working particularly on the emigration of mathematicians from Hitler's Germany and on the life and work of Richard von Mises.

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[20] Archives Humboldt University Berlin, NS-Dozentenschaft no. 222, fol, 8-10v, fol. 9v-10. Handwritten, emphasis and signed by Weber, 11 May 1938.

[21] This according to Landau's son Matthias, as reported by Wolfgang Kluge, l.c., p. 100.

[22] *Jüdische Rundschau* 43 (1938), 1 March 1938, p. 7. On Landau's relations to the university in Jerusalem and in particular on his talk in Hebrew at the opening ceremony for the university in 1925. See L. Corry and N. Schappacher: Zionist Internationalism through Number Theory: Edmund Landau at the Opening of the Hebrew University in 1925; *Science in Context* 23 (2010), 427–471.

[23] The library apparently went in 1940 – at least temporarily – to Colby College in the USA, mediated by Landau's son-in-law I. Schoenberg, who was also a mathematician and had been in the U.S. since 1930. Thanks for this information go to Navaah Levin, librarian at the Hebrew University of Jerusalem.

Interview with Bodil Branner

Poul G. Hjorth (Denmark)

Poul G. Hjorth and Bodil Branner during the interview. Photo taken by Sven Branner

When and how did you become interested in mathematics?

This is not easy to answer because I think I have really always been, but perhaps without being conscious about it. My father would often play number games with me. He was so important to me and was a great support for me. Actually, upon entering high school I had to make a choice about a science curriculum or a more classics-and-languages curriculum and I was much in doubt. My Latin teacher at the time was very inspiring. Finally, I chose mathematics. Many in my family were academics but none in the exact sciences. I soon found out that I had a flair for maths and physics. My physics teacher in particular was very inspiring. In the second year, there was an arrangement at the city hall, where representatives of tertiary education were present and you could ask them questions. I had booked time for speaking with a theologian, a librarian and a mathematician. The mathematician was Professor Svend Bundgaard. I spoke with him for nearly an hour. When I left, there was no doubt left in my mind: I had to go into mathematics. And I have never regretted that decision. My mother disapproved somewhat. My father was very supportive.

So you entered the University of Aarhus majoring in mathematics. Were there any subjects or teachers in particular that caught your attention?

I was immediately faced with another decision. It was possible to major in mathematics only or to major in mathematics combined with another science topic. Bundgaard was to play a decisive role in my education. Bundgaard lectured in analysis; I asked him and he told me to combine maths with physics so I would get a broad view of science. He was such an authoritative but also charismatic figure at the institute in those years. So I followed both mathematics and physics courses. I remember in particular quantum mechanics, which was incredibly interesting. In the long run, though, mathematics won out. Those years were years of rapid growth, energy and internationalization for the new mathematics department. Algebraic topology was a strong subject and I did my thesis with Leif Kristensen. Master's students had their own environment on the top floor with shared offices. We had a good and productive working situation. After I graduated, I stayed with the department as a teaching assistant for two years. At the time I had married Sven and we already had two children. It was in the late 1960s.

What was the employment situation, particularly with respect to female mathematicians, when you got your first university employment?

At the time, there was no PhD in the Danish system. For that, one would have to go abroad. Many of my friends and colleagues did that, and Sven and I planned to go to the U.S. But rather suddenly, Sven, who is a chemist, decided to seek employment in industry, in Copenhagen. We needed to move to Copenhagen. I consequently had to find some sort of employment in Copenhagen. I considered becoming a high school teacher but the high schools (gymnasiums) would not even talk to me because I had not taken a teaching competency exam. Bundgaard, however, intervened. He had found out that there was a vacancy at the mathematics department at the Technical University (now DTU) in Lyngby, near Copenhagen. He made the introductions for me and recommended me so enthusiastically that they called me and asked me to apply – which I did, and got the job as amanuensis, a sort of professor's assistant (not to be interpreted as assistant professor!). This was August 1969. I worked for Professor Fabricius-Bjerre, who was a kind and thoughtful employer.

There was among the faculty at the time a range of attitudes towards what the professor's assistants should do. Some regarded assistants as simply graders. Fabricius-Bjerre, fortunately for me, strongly encouraged us to continue our research. Along with teaching, of course. It was enlightening for me to teach, for instance, classes in differential geometry; in addition to the formal and very abstract differential geometry that I knew, here was also the geometry of the engineers, with details of curve geometry, curvature and torsion, kinematical relations and so forth. My own research, as a consequence, turned a bit from algebraic topology towards differential geometry.

What came as a real surprise to me was the attitude towards women as staff that some of the senior faculty members had. I had come from a department in Aarhus with a pioneering spirit and a very informal and collegial atmosphere. At the Technical University, the tone was much more formal and conservative. It was a very male environment. Not till some time into the 1970s was the old system of the 'Professor dictatorship' abolished. About this time, I got tenure.

Where and when did your interest for holomorphic dynamical systems arise?

In the late 1970s, I was supervisor for a Master's student who wanted to write about new developments in geometry and dynamical systems. Talking to Peter Leth Christiansen about the subject, I was pointed to very recent work of Robert May, and others, about iterative systems, period doubling and these things, just emerging at the time. I read some of the papers. There was a remark in one about how it 'would be interesting' to look at cubic polynomials. I decided to give that a try. At the time, computers were punch-card fed! We found some structures, a cusp catastrophe. The student graduated. I read more. I wrote a paper about some special cases of cubic polynomials, the associated kneading sequences. I started going to conferences about iterative systems. The first person to show me a picture (crude at the time, in the early 1980s) was Predrag Cvitanovic, then at the Niels Bohr Institute. In June 1983, Cvitanovic organised an international conference on the new science of chaos. The conference was attended by physicists, biologists, chemists and some mathematicians. Among them were Adrian Douady and John Hubbard. I was in a bit of a hurry because I was also going to "Dynamic Days" in Twente, Holland. I organised Douady to give a lecture at my department. We had discussions. They asked what I was doing and when I told them: cubic real polynomials, they immediately pointed out that this made much more sense to study in the complex plane.

My background in holomorphic functions was limited but I was convinced that this would be a fruitful direction. Looking back at those days, I feel lucky and privileged to have been at the right place, with the right questions, and then on top of this have people who encouraged me and believed in me. I said to Hubbard, I have heard that you have generalised the concept of kneading sequences to the complex domain - can you explain to me how this is done? And he did. He explained about what is now called the 'Hubbard-tree'. Then he said: now it is your turn to work the same thing out in another example. To this day I wonder how I managed to do it, but I did. He was suitably impressed. Douady and Hubbard suggested that after Twente, I should come to Paris and we could collaborate on this. In addition to this, they suggested that I attend an upcoming Summer School where Thurston was to lecture and that I should then visit Cornell University. I did, and we managed to formulate and convince ourselves of a new theorem in just a week. We uncovered the classical Cantor set in the parameter space for cubic polynomials.

Eventually, you spent a year at Cornell?

Hubbard invited me to spend a year as visiting professor, September 1984–September 1985. This I had to negotiate with my family and the department at DTU. My children were hesitant; in the end, we managed to all go. Sven's company negotiated with him and he found a position as a visiting scientist at Cornell. Our son Kim had just graduated from high school and could attend classes at Cornell, and our daughter Eva got into a local high school. My department at DTU gave me leave without pay. We all travelled to Ithaca. I taught three courses, one of them a course in dynamical systems, based on Hubbard's notes, that later became a textbook.

Hubbard and I worked on two papers (or, more precisely, a paper in two parts), the second of which was only finished three years later. Both were published in "Acta Mathematicae".

We returned to Denmark after a year and a half.

Back in Denmark I began to supervise more students in holomorphic dynamics, among them Carsten Lunde Petersen. He was a graduate from the University of Aarhus but he wanted to work further on holomorphic dynamical systems. I advised him on his PhD thesis. I also supervised projects by DTU students and one semester I gave a course in holomorphic dynamics at the University of Copenhagen. I travelled a lot and made contacts with a large international group of people. The field was in rapid expansion. Yoccoz found in the Mandelbrot set combinatorial structures very similar to the ones that Hubbard and I had worked with in the cubic polynomials. This was further developed in a paper by Douady and me from 1987.

The international group of people, the 'crowd', working on holomorphic dynamics, is known to be a very close-knit group of mathematicians, with famously good relations and a strong sense of community and collegiality. What do you see as the reason for that?

There are actually several schools, some centered on topics and some centered on people. My own background in topology fitted naturally with the approaches taken by Douady and Hubbard. We asked topological questions and used techniques from topology. Other people came from a more measure-theoretical direction. Around me, we were much influenced by Douady's attitude of openness, sharing of ideas and support for young researchers.

In 1993, there was a fairly large international conference that you organised?

Yes. Eventually, the field had begun to grow so much that we felt it was natural to conduct an international conference. I applied for money from various sources and, in June 1993, we had the meeting in Hillerød, Denmark. It was a "NATO Advanced Study Institute". It was a seminal event, more perhaps than we realised at the time. We had 110 registered participants and about 70% were PhD students or postdocs, so it was very much a meeting of young researchers. Many young researchers came together here for the first time, met informally with senior people and also made contacts between themselves that are still in place. In the Scientific Committee we had, among others, Sebastian van Strien and I believe it was he who suggested the 'free-for-all' sessions where people could ask any questions about any topic and whoever knew about it would get up and explain at a blackboard. Everyone took notes. Many stood up for the first time then and there and spoke in an international setting. In among that we had of course more formal, prepared lectures. It was a magical two weeks.

Another topic that you have been affiliated with is "European Women in Mathematics". How did this begin?
It began when I attended the ICM conference in Berkeley in 1986. I was invited to a panel discussion organised by the "Association for Women in Mathematics" (AWM). This was partly because during my year at Cornell, Hubbard had introduced me to a female colleague Linda Keen. Linda and I became friends and she was involved with AWM, in 1986 as president. I was asked to bring a Scandinavian perspective to the discussion and I did as best I could. We were four women from Europe and we decided to form as a subset of, or sister organisation to, AWM, a "European Women in Mathematics" committee. We were Caroline Series, Marie-Françoise Roy, Gudmund Kalmbach and me. Meeting Caroline Series then was also a wonderful new connection within holomorphic dynamics. But it began with "European Women in Mathematics".

How did your work with the European Mathematical Society begin and what do you remember from the period?
I was an individual member of the EMS almost from the beginning and very early on became a delegate of individual members to council meetings. I was inspired to be a candidate for that through the network of European Women in Mathematics and later also for the executive committee of the EMS. I served for eight years on the executive committee, from 1997 to 2004, the last four years as one of the vice-presidents. In 1997 the EMS was still a very young society but it developed rapidly. I remember especially the cluster of activities preparing for 2000 as a "World Mathematical Year", expanding the interfaces of the EMS to society and also the many activities to make it a broader mathematical organisation, including the different mathematical branches. The EMS Publishing House was established during that period, due to an initiative of Rolf Jeltsch.

You remained during your EMS period and are still active with European Women in Mathematics?
It was not possible to continue being active in "European Women in Mathematics" during that time. Since 2010 I have been a member of the EMS committee "Women in Mathematics". We see our task to be an umbrella organisation for the various national initiatives around women in mathematics. We work closely together with "European Women in Mathematics". For the upcoming ECM in Krakow in 2012, our committee is organising a panel discussion, chaired by Caroline Series, who has been the chair of the committee since 2012.

You have also worked with the history of mathematics, in particular the Danish-Norwegian surveyor and mathematician Caspar Wessel, who described the geometry of complex numbers in the late 1700s?
In 1995 Douady had a 60th birthday conference at the Poincare Institute and I wanted to find a unique present. Douady, being multi-talented, had taught himself quite a bit of Danish. I thought Wessel's original paper on complex numbers, written in Danish, would make a nice present. I contacted the Royal Danish Academy of Science and Letters to obtain a copy of the original article. This turned out to be quite involved. In the end, they decided to give me one of the original folios from 1797. Suddenly I stood with one of these original, unbound stacks and Douady's birthday was approaching. At the very least I had to have it hardbound. I contacted the book binder at DTU, who was immediately interested in this unique assignment. He made a beautiful bound volume. I read, in the weeks before the conference, a lot about the history of Wessel and his work and I realised that I couldn't just hand Douady the book without saying at least a few words about the significance and the history behind. At the conference, there was quite a stir. Not very many people had heard about Wessel; most people believed that Argand had been the first to describe the geometry of complex numbers. When I came home, I wrote a letter of thanks to the Danish Academy and reminded them of the significance and that we were actually near a bi-centennial of its first publication. They ought to have a complete English translation made. They then arranged, with Professor Jesper Lützen at the University of Copenhagen as chairman, a Wessel Symposium. Discussing with Lützen, I said that there ought to be a Wessel biography and that perhaps I would like to write it. He agreed. I began to dig further into historical sources and was surprised to find so much help and enthusiasm from various sources. Wessel is quite known in the land surveying business, and people there were so forthcoming. Also from mathematical colleagues in Norway, I received help and support. I met Niels Voje Johanson, who was also at work on a biography and we decided to join forces. He supplied much valuable material from the national archives. The biography was published in time for the Academy Symposium, which lasted a full week.

You were elected Chair of the Danish Mathematical Society and greatly boosted the activities of the society. What do you see as your biggest accomplishment?
The work as a member of the executive committee of the EMS strengthened my views on the important role played by the national mathematical societies. I had earlier been a member of the board of the Danish Mathematical Society, in fact during a period where several initiatives were taken that involved mathematicians from the different mathematics departments in Denmark, both in so-called pure and applied mathematics. Originally mathematics at university level was concentrated around the Copenhagen area. In the late 1950s the mathematics department at Aarhus University was founded and since then several other mathematics departments have been established at newer universities. The Danish Mathematical Society needed, in my mind, to reflect this change in diversity more directly. Having expressed

that, I was asked if I was willing to become president of the society. During my presidency, we started a newsletter, Matilde. The second editor was Martin Raussen, who later became editor of the EMS Newsletter and is now one of the vice-presidents of the EMS. I am grateful for the support from many individual mathematicians and also from the different mathematics departments during my presidency.

You are now retired but seem as busy as ever. You even have time for beekeeping?

Four years ago I retired. In the beginning it mainly meant that I stopped teaching mathematics courses but otherwise continued as before. But gradually I am changing priorities. I still enjoy being involved with mathematics, although it is not as intensive as before. I have taken up other interests, such as singing in a choir. However, beekeeping is the main activity. Ten years ago Sven decided to become a beekeeper. The life of the bees immediately fascinated me too, although I did not have the time to be much involved. To retire earlier than I had to was partly motivated by obtaining more time for beekeeping. I enjoy the outdoor activity. Bees in cities are popular these days. It is a way to get closer to nature. It is a gift to have a background in natural science, since mathematics, physics, chemistry and biology need to come together when one tries to understand how the bees function as social insects.

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A Discussion Between a Researcher and an Educator in Mathematics

Preda Mihăilescu (Göttingen), and Stefan Halverscheid (Göttingen)

1. Introduction

***SH:** Preda; after reading the contribution in the EMS notices by Sweller, Clark and Kirschner on "Teaching general problem solving does not lead to mathematical skills or knowledge", we have discussed intensively on the role of teaching mathematics and problem solving in mathematics at school.*

Before going into our discussion, I am – especially as a mathematics educator – very interested in knowing how professional problem solvers in mathematics discovered this subject. What was your way to mathematics?

PM: I teach mathematics at a German university; in my family they have been teaching for generations and it is more than earning a living. In discussions over generations and teachers of various disciplines, I have become accustomed to the fact that the art of captivating the interest of the young and of motivating a maximum investment of work with a minimal sense of effort are invariants of the teacher's craftsmanship. Add to this the humility of being continuously curious to discover new ways of understanding and perceiving the "old ceremony", and also by learning from the ones that are taught, together with the openness to acknowledge errors and incorporate them in the didactic body.

***SH:** For me, classical problem solving following Pòlya's types of tasks was quite an important step towards my decision to study mathematics. My mathematics teacher Ulf Treibel encouraged his students to participate in competitions and he invested quite some of his spare time in criticising my first solutions and the way I wrote them up – without giving the solution himself. This was a pretty individual approach, for which I am very grateful.*

Of course, this does not imply that this approach, which worked out well in my case, is the best way of introducing problem solving for the majority of students. What would you suggest as to how to integrate problem solving at school?

PM: My high school teaching experience is minimal but I count some very good teachers among my friends – and I was always fascinated by the way such a teacher manages over decades to keep up the motivation, teaching more or less the same subjects, to control classes of pupils with less and less social and motivational cohesion and still keep the dedication for the children. A friend teaching high school mathematics in Switzerland gave me if not an answer, the most beautiful succinct image instead: "a class of pupils is a living being on its own, and when you meet a new class you have three to four weeks to understand its metabolism and find the way to address it, in order to propagate motivation and overdo resistance". If it sounds like systems theory, it probably is – but theory was not the source of the insight; it came from experience. Pupils interact and one gains a lot by finding a way to use this interaction in favour of teaching and one loses by only considering the skills and problems of the individual young people. Bringing such a reasonable concept, which may also be derived from deep theories, into practice remains a matter of personal skills of the teacher. I think this is a good example when it comes to discussing the wealth and limitations of the help that didactical sciences and philosophies can provide to teaching in class. A further secret of his is that instead of giving up his interest for developments in mathematical research, he spends a solid part of his time in following them as much as possible, and draws from this a rich imagination about new and surprising topics he may try explaining in class, along with the compulsories. Last but not least, he does not get frustrated by the repetitive component of teaching. Altogether, this keeps the spirits alert, both for the teacher and his pupils.

Different subjects require, of course, different methods of teaching. When it comes to mathematics, I am convinced that there is a list of skills, knowledge and aptitudes which are a minima moralia and need to be consolidated by training, like keeping in good physical shape too.

***SH:** Erich C. Wittmann [Wi] suggests regarding mathematics education as a design science. Similarly to engineers, teachers acquire a repertoire of knowledge and methods – pure mathematical, applied mathematical, pedagogical, didactical, psychological – and react on the specific learning situation with a suitably designed class session. What can you make of this or, to put it differently, what do you teach and how do you teach mathematics?*

PM: It makes sense – but careful, it is a restricted metaphor: take it too seriously and one should necessarily dislike the mechanical aspect of it! When I went to school, there were the basic problem books for learning the tricks of the trade in various subjects: algebra and linear systems, geometry, trigonometry and calculus. They contained a bulk of semi-dull repetitive problems, which maintained a certain increase of difficulty and led to some beautiful, unexpected results, in order to keep an interest alive. And then there were some collections of jewels of mathematics, written by authors like Hadamard, Pòlya and Szegö, Sierpinsky, Țițeica or Yaglom – there, almost every problem was an experience in itself and an opening to an unexpected garden. I would say: choose your tools well, set up a design target in your mind … and then forget about it and jump into the adventure of human interaction! When that begins, engineering metaphors should be forgotten.

SH: *Your concept of mathematics courses requires very well educated teachers. They have to be strong and self-confident in mathematics. They plan their courses individually and they are able to evaluate their results and to make adjustments in the next lessons. Standards for achievements in mathematics, text books and – in the way I understand them – worked-out examples are meant to support teachers. In mathematics education, we hope that with their knowledge on mathematics, on diagnosing learning difficulties and with their ability to work out suitable problems, they are able to cope with most of the situations which arise at school. How do you regard the impact of mathematics education for the teaching and learning of mathematics?*

PM: The teacher is out there, the *lonesome hero*, called to lead a good maximum of young people to this valuable experience of personal daring and humility. And the way to do that they will ultimately find in the interaction with the being called "class". Didactics and additional secondary sciences can stand him by, with precious insights and evaluation of long time experiences and wisdom – but they cannot give him a recipe of success and no curriculum should ever be conceived in this unrealistic intention. You may learn what flowers to bring to your beloved but when it comes to opening her heart, it is an individual experience!

Your questions touch the concern that we might need some lower standards with which the average teacher can well cope. Let me tell you a secret: this is a timeless misconception; it is the very mind-perception for which we have in all languages the saying "the road to hell is paved with good intentions". No! Raising the standards high, by pointing out how many different aspects can collaborate in a pleasant and successful teaching, while being honest about the fact that possibly no single person can achieve simultaneously all these goals – nothing can be more tolerant and fruitful than that! The reason is that every teacher is a human with his or her gifts and will find in a high standard attitude the confirmation that they might have done well. And then forget about the rest or try to improve it – this is part of the message: no-one can do it all! But try to reduce things to a minimum, of which one expects, however, that everyone can and must make it – then not only do you have a problem of choice but no matter how "scientific" the criteria of choice, you will get annoyed and frustrated teachers. Can the pupils then be better?

But I think it is time to get closer to the subject of the paper we wish to discuss.

2. The message of "cognitive science"

SH: *Why has the contribution "Teaching general problem solving does not lead to mathematical skills or knowledge", which you encountered recently in the EMS journal, attracted your attention?*

PM: It is with this interest in teaching and these perceptions of its significance that I got interested by the title "Teaching general problem solving does not lead to mathematical skills or knowledge". Already the very self-confident and affirmatively negating title raises some questions: what does "teaching problem solving" mean? One does not teach problem solving but introduce students to problems and then let them work on other problems, learning alone by the process of solving. But certainly, the title suggests that a quantity of people have lived for a long time in some illusion about something related to problem solving, and now a new light has dissipated the illusion. This is enough in order to become curious. The paper is short; I read it three times. First, I was seeking for an appropriate definition of "problem solving" versus the second actor of the drama "worked examples", which appears only in the text. Since to me the two are just *sides of one single medal* or the polar forces by which we draw students on the spiral of learning, the suggested *exclusive complementarity* remains striking – I found no single explanation of the terms so the reader is left to guess why one would be tempted to choose which of the two is better. If you allow, to me it is like forcing upon one the decision of which of a fork and a knife is really important for eating at the table?

SH: *On the other hand, introducing problem solving to, say, seventh-graders is not at all an easy endeavour. The selection and formulation of problems, embedding them in the context of a course, supporting students during the solution, discussing strategies to solve them, working on the mathematical writing to present a solution. This learning situation is very demanding for teachers indeed. And this fact often leads to the obvious teacher's choice, namely to neglect problem solving.*

PM: I try to imagine under what circumstances I would be led to a similar decision against problem solving. The only possible answer is the image of a dull and annoyed teacher who overwhelms his pupils with hundreds of repetitive problems and is unwilling to explain or communicate with them.[1] Then one would certainly step in and shout: "This is hopeless; they won't understand more from five than from a hundred problems, unless you do your part in motivating and explaining." And I guess I might accept calling that part "worked out examples", if someone wishes so. But such people have a statistical chance of occurring, which is totally independent of the curriculum they are in; no science can help avoid them, so there is no need of a scientific debate on the possible abuse of one-sided, dull problem solving. It is evident! On the other hand we shall not make up curricula so that the dull have even less to do: they won't even do as much as they did; it's called Thermodynamics! What holds for pupils holds for teachers too: treat them as impossible to motivate, lazy and dull, and that is what you will earn. But invest trust in them and you may be surprised!

Back to the paper, having not understood how the terms under debate should relate to each other or where a sensible delimitation between the one and the other should be put, I read it a second time, looking for the arguments. Here one reads, in relation to Pòlya, the fol-

1 Remember how Gauss discovered the summation formula $\Sigma_{k=1}^{n} k = \frac{n\,(n+1)}{2}$?

It is purported this happened because of such a dull teacher asking what the sum of the first 100 integers was.

lowing astonishing phrase: "no systematic body of evidence demonstrating the effectiveness of any general problem-solving strategy has emerged". If anyone is so naive as to stipulate the existence of some "general problem-solving strategy" then they never worked more than ten problems of mathematics in life (certainly not Pòlya!). It is only in the IT of banks that I ever encountered such a naive expectation of a universal panacea: some program or programming language that would do all the things for you. But even there, it was called "the egg laying, wool and milk producing pig-trout", taking into derision those who were seeking this kind of perpetual motion. It is a deep misunderstanding of Pòlya – to the best of my knowledge, the problems and inductive methods he spoke about firstly assumed some well assimilated basic knowledge and then provided a wealth of intelligent problems that were indeed well suited for expanding the mathematical horizon and understanding. This is miles away from a mechanical problem solving strategy and I found the assertion quite insulting for the intelligence both of Pòlya and the readers of the EMS.

SH: *A variant of this misunderstanding of Pòlya can also be encountered in the teaching of problem solving. In extra-curricular activities, in particular in the maths Olympiad movement, Pòlya's problem solving strategies have enormous influence. Sometimes, tasks get single-sided because the commissions build up suitable tasks around one of the strategies instead of using natural contexts in which they appear. There are indications from the development of the number of participants that the students lose interest in mathematics. Some years ago, I carried out a little experiment with students who were very successful in the Kangaroo contest, prize winners of various European countries. We made an experiment with two sessions on stochastic processes: the first with an organised scheme of many problems in the sense of maths Olympiads and the other on more complex problems where stochastic processes could be helpful but there was no definite correct or wrong answer. The two sessions were given by different teachers who changed their groups. They were rated as very competent in the first session but as poorly competent in the second.*

PM: I do not know what to say. My first reaction is: the maths Olympiads have created an immense wealth of interested pupils and then valuable mathematicians, worldwide. It may be possible that they reach a critical point, where optimising the outcome starts working against the long-term goal of solid mathematical training. One could compare with sports.

Back to the paper again. After Pòlya was discarded decently, with the regret that his claims (which are the author's and not his) could not be confirmed by cognitive science in 50 years, we receive maybe the best piece of evidence of the whole paper. It is a detailed description of an interesting experiment of the Dutch psychologist De Groot. In this experiment, a group of high performance chess professionals were investigated against a group of common players and both were confronted for several seconds with a chess situation on a board. After a short break, they had to reconstruct the situation: the professionals led by 70% – 30 % when the situations were drawn from actual games but had no measurable advantage when it came to totally random situations, which were thus, from the point of view of a player, absurd, since there was no way they could occur in some meaningful game. To me this shows that professionals did not expand their memory but their connections and capacity of sensing the qualities and tension of valuable situations. The authors seem to draw the opposite conclusion, claiming that this shows the worked out examples had been at work and not problem solving. *Very confusing and unconvincing*. Personally I do not play much chess but I have seen some semi-professionals studying historical games. Before reading what happened, they try themselves seven ways of completing the game and only then confront the historical completion: exactly like in problem solving.

SH: *In education in general, and in mathematics education in particular, experiments like this are important to build up knowledge on how learning works, and what are its obstacles and possible measures to improve it. I strongly support research in this area because education is a very recent scientific area. And it is certainly reasonable to find out more on how to apply worked-out examples in teaching mathematics.*

The tiny amount of what we know so far, of course, implies that one should be very careful deducing from a certain number of experiments that a certain way of teaching is particularly successful. As you point out correctly, it is not a long-term experiment. For instance, a "longitudinal" study with students confronted with "worked-out examples" would be necessary. I would also prefer a suggestion like this: "Dear mathematicians: there is an interesting study by De Groot which indicates that worked-out examples might be helpful for getting acquainted to problem solving. What about trying to incorporate this in your teaching from time to time and reporting about your experiences?"

PM: When the same experiment leads to diametrically opposed interpretations, it is either the end of science or a deep problem with the definition of the underlying concepts – or both. I may agree with the value of similar experiments but the discipline displays adolescent problems: the concepts are not ripe, the relationship between what experiments can be done and to what extent they may lead to defendable conclusions is not well established yet. *Such a discipline should not be allowed to infer serious things like changing curricula!* Turning back to mathematics, there is a noteworthy remark, which to me characterizes the spirit of the whole paper. The authors pose the question: "How do people solve mathematical problems that they have not previously encountered." For answering, they give an abstract description of the process which can only be true by its evasivity – or, of course, false, if you prefer. In this, they assert the possibility of some approach and then conclude by saying: "*There is no evidence that this strategy is teachable or learnable because we use it automatically.*" This is unbelievable for scientists! In traffic, the unexpected situations are those in which one has to react quickly and it is true that the reactions are to a large extent automatic, and should better be correct. The argument of the authors would suggest in this case that driving lessons are a useless time investment: the most important automatic reactions are not a *teachable strategy.* Can you believe that? Any driver understands that the value of driving lessons consists of providing a basic system of experience and compulsory reactions in traffic which allows the brain to be free of doubts in extreme situations so it can concentrate on the specific challenge. It is no different with mathematics: teaching does not create the automatisms; this comes about by work and it serves when this work is guided and focused in school time. But teaching avoids losing time with reinventing the wheel when it comes to solving real problems.

The paper goes on like that and after a statement like the above, the reader has already developed the impression that the two pages were written between breakfast and lunch without much reflection of the statements made. However, this impression is strongly challenged by the unambiguous conclusions: *mathematicians and mathematics should (give up problem solving and) work together to develop a sound K-12 curriculum … based on carefully selected and sequenced worked examples.* If they cannot make clear and sensible statements, they are nevertheless out to *set guidelines for teaching mathematics worldwide* in the future! I can do nothing but humbly suggest that it might be worthwhile starting with some sound and widely understandable definitions of the way the authors intend to distinguish "worked-examples" from "problem-solving". Then we meet again! Maybe by trying to do that, they shall eventually realise by themselves from the difficulty in setting the demarcation line that they address a false or ill-posed problem. As it stands, their claims seem to suggest that mathematics can be taught more efficiently if the pupils or students do not need to work but just assimilate some examples. A scary illusion!

SH: *In my work as a mathematics educator, I know how convincing a certain concept may seem if you have developed it or if you just like it. Research on phenomena of learning and teaching are important, in my opinion, to find out which impacts such concepts, of "worked-out examples" say, have on learning. However, the teaching work tells me that the role of the teacher as an individual is very important. Learning mathematics cannot work if the teacher is not authentic in what they do.*

This is why I consider learning concepts and research on their use as offers to independently teaching teachers or, preferably, "teachers' researchers" who employ scientific methods to shape their courses. But they build up their own experience and decide independently. There is no such thing as the best coursework. This would be my conclusion from our discussion. What about yours?

PM: The discussion of various approaches to teaching is certainly important and may provide the "hero in the field" – the teacher – with valuable suggestions and insights. Very probably, not more. The present example was useful in showing how contradictory the language of empirical field tests is and thus how unreliable the outcomes, when it comes to the wish of deriving curricular changes on the basis of such tests. *This should simply be avoided*. Most of all, the kind of tests that pretend to decide on some exclusion or alternative are hard, since the terms on which the decision is made are far away from being suited for such alternatives. Reality will always be a sane mixture of the … apparent opposites, which in fact are only complements within a whole. Teachers should know this!

I guess I made it clear that I put the accent on human interaction and development not only of skills – which depend on aptitudes – but of the individual self-confidence of pupils, in their ability to produce reliable, logical, mathematical reasoning. No matter how modest, this is an enrichment – we should not measure there, just foster. To conclude, I would mention that I went personally behind the books, searching for modern trends in teaching and encountered the paper [H] by Gerhard Hüter, a well known German neurobiologist and psychiatrist, addressing a wide scientific public. The message could not be clearer: *curiosity, enthusiasm and creativity (Gestaltungslust)* are recognised by specialists as fundamental triggers for the capacity of the brain to assimilate knowledge. The terminus technicus is "experience dependent neuroplasticity". The paper is worth reading and yields a good balance to the one under discussion. Bored pupils cannot assimilate even the little which one requires from them; motivated ones may become astonishing!

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The European Set Theory Society

Mirna Dzamonja

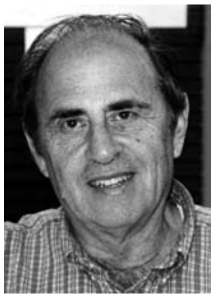
"Nobody will expel us from the paradise that Cantor has created." D. Hilbert, 1900

Most people have seen a picture of an older version of the gentleman on the left. But he was actually quite young when he discovered the fundamental notion of an infinite set and when he proved that the reals are uncountable. Georg Cantor, born in 1845 in St. Petersburg was only 29 when that proof was published in the Crelle Journal. Cantor was solving a conjecture of Heine on the uniqueness of the presentation of functions as sums of trigonometric series. Thus was created *set theory* as a way to solve a problem in analysis. Since that time, set theory has lived many lives, some of them parallel and some serial. It has been used for foundations of mathematics and as a way to get closer to the ideas of the Hilbert programme, yet it was finally the Cantor diagonal argument that was at the heart of Gödel's celebrated Incompleteness Theorems [2] which spelled the end of that programme. Set theory has many faces.

1st European Set Theory Conference, Będlewo, Poland 2007

Intradisciplinarity and interdisciplinarity

Set theory was taken by Hilbert as a paradigm for the foundations of mathematics and it is in this context that the axioms of set theory were developed. The search for such axioms was long and there have been several interesting candidates. The scheme considered as mainstream today is ZFC, the Zermelo-Fraenkel axioms with the axiom of choice. These axioms can express most modern mathematical objects and therefore it is natural that an investigation of such axioms would have consequences on mathematics in general. It is also natural that research into the foundations of mathematics would necessarily lead to logical questions, making set theory part of mathematical logic and giving it connections to computer sciences and philosophy. On the other hand, set theory started as a mathematical investigation of infinite sets and in fact is a subject of mathematics, interested in everything that has to do with infinite sets. From the combinatorics of the infinite to graph theory, topology, measure theory, Banach spaces, C*-algebras, ergodic theory and group theory, set theorists have over the years not only collaborated and contributed to all these branches of mathematics but have often been instrumental in developing whole subfields of other fields of mathematics. A recent example is the flourishing subject of nonseparable Banach spaces where major questions have been solved using set theory. Set theory has its own ground as well: large cardinals, inner model theory, forcing axioms and the theory of forcing. If there is a simple phrase to describe set theory, it is the study of the infinite.



***Theorem*: The value of $2^{\aleph_0}$ cannot be calculated in ZFC. (P. Cohen 1963, Fields Medal)**

***Theorem*: If $2^{\aleph_n} < \aleph_\omega$ for all *n*, then $2^{\aleph_\omega} < \aleph_{\omega_4}$. (S. Shelah 1984, Israel Prize, Bolyai Prize, Wolf Prize)**

Set theory and Europe

There were good times, there were bad times. From the beginning, set theory was loved by some (Mittag-Leffler, Hilbert) and hated by some others (Kronecker). Questions were asked in set theory that literally drove people mad (such as the famous Continuum Hypothesis of Cantor, that stated that every infinite subset of the reals is either bijective with $\mathbb{N}$ or with $\mathbb{R}$, hence $2^{\aleph_0} = \aleph_1$). Set theory was being developed fast in Germany first of all and then Czechoslovakia, France, Italy, Hungary Poland, Russia, Yugoslavia and elsewhere in Europe. In some countries this was a strategic development, such as in Poland where the young Polish state between the two wars actively helped build the Polish School of Mathematics, led by a mixture of set theory, topology and measure theory published in the celebrated *Fundamenta Mathematicae*. As is the case with the rest of the mathematical community, the events leading to and during the second world war damaged the set-theoretic community to a terrible extent, with many of our colleagues dying in concentration camps or choosing to end their own life. Escaping the ruins of what had once been, new, strong set theoretic communities emerged in Israel and in the United States. Fraenkel of ZFC moved to Israel (then British Palestine) from Germany and started the celebrated Israeli School of Set Theory. John Von Neuman, Stanislaw Ulam and many others moved to the United States and started the equally celebrated USA School. Many set theorists did remain


Abraham Fraenkel

in Europe and continued developing their subject. The post-war years were marked by the unavailability of personal contact or even published work between mathematicians in the Eastern and in the Western block. In set theory we were lucky to have that flying mathematician and enthusiast Paul Erdös, who was the messenger between these two worlds.

European Set Theory Society

In good times and in bad times, set theory has always existed in Europe and has remained active. Now are the good times. Since the end of the cold war there has been a tremendous increase in the set theoretic activities in Europe, with new centres developing in various countries and old centres flourishing.

To give an indication of the image of set theory among young people, a yearly conference called "Young Set Theory" attracts every year about 70 top Ph.D. students. European Science Foundation has funded a large research network called INFTY (2009–2014), led by set theorists in collaboration with other logicians and philosophers. Whole research centres are focused on set theory, such as the Kurt Goedel Research Centre in Vienna.

The INFTY logo

Nevertheless, we feel that the set theoretic community could become even stronger if we build on the strengths of the community in Europe. Set theory has a very multicultural and diverse history and ambitions and it was in the view of this that we founded the European Set Theory Society. This organisation aims to represent all set theorists (not only European) and any set theorist is welcome to join. We have a modest joining fee of 20 euros a year (free for students, unemployed and retired members of the community). Our society has actions and it has dreams. We are called the European Set Theory Society because our actions take place in Europe. Our actions are the organisation of conferences, scientific support of the members of the community and, importantly, raising the level of awareness and understanding of research in set theory among other mathematicians. We have just started officially; in 2011 we became a registered charity in the UK. Our financial ability is still quite small but we intend to build it up and to use that money for prizes, grants and other ways of promoting our subject. Our dreams are very simple; we would like set theory to gain its deserved and historical place as a mainstream subject of mathematics in Europe.

The European Set Theory Society has a website: http://ests.wordpress.com/.


Young Set Theory, Bonn 2011

Kavli IPMU, the University of Tokyo

T. Kohno (Tokyo)

1. A brief history

The Institute for the Physics and Mathematics of the Universe (IPMU) was established at the University of Tokyo in October 2007 and has been supported by the World Premier International Research Center Initiative (WPI) of the Japanese government. The WPI programme is designed to promote world-class science in Japan and its international visibility. There are six institutions in Japan supported by the WPI programme and the IPMU is one of them. The IPMU is situated on the Kashiwa Campus of the University of Tokyo and is the first of the Todai Institutes of Advanced Study (TODIAS) of the University of Tokyo. Early in 2012 the IPMU received a major endowment from the Kavli Foundation and joined the family of Kavli Institutes. It is now called the Kavli Institute for the Physics and Mathematics of the Universe (Kavli IPMU).

Research building of the Kavli IPMU (Courtesy: Kavli IPMU)

2. Interdisciplinary and international research environment

Galileo once remarked that the universe is written in the language of mathematics and this is the firm belief at the Kavli IPMU. The institute regards mathematics research as fundamental to its quest to understand the universe. The Kavli IPMU brings together a wide range of researchers from pure mathematicians and string theorists to experimental particle physicists and observational astronomers in a truly interdisciplinary and collaborative environment. Of the institute's nearly 200 principal investigators, faculty members, postdoctoral researchers, joint appointments and graduate students, more than half are from outside Japan. The official language of the institute is English. The Kavli IPMU hosts international workshops and meetings on these and other topics of interest, and over the years it has strengthened relationships with other prominent research programmes at U.C. Berkeley, Princeton University, IHES and other institutions.

Every day at 3 o'clock in the afternoon, members of the institute make their way down a sloped corridor and gather in the Piazza Fujiwara, an open space of the Kavli IPMU research building, for afternoon tea. Such informal daily gatherings promote mutual understanding of researchers of different disciplines. Sometimes new research projects stem from these daily conversations.

Tea in the Piazza Fujiwara – a large open space that occupies the centre of the building of the Kavli IPMU from the 3rd floor up (Courtesy: Kavli IPMU)

3. Research in mathematics at the Kavli IPMU

Although the Kavli IPMU covers a wide range of research subjects including mathematics, theoretical and experimental physics and astronomy, let us focus on research related to mathematics.

We first state some background of the research in mathematics at the Kavli IPMU. Gauge theory, quantum field theory, general relativity, superstring theory and the theory of integrable systems in physics have provided major influences in the development of mathematics such as algebraic geometry, differential geometry, topology, representation theory, algebraic analysis and number theory. For the past 20 years, methods of quantum field theory have had a major influence on mathematics. Since quantum field theory treats the differential and integral calculus of an infinite number of degrees of freedom, the rigorous development of quantum field theory in mathematics has yet to be established. Nevertheless, in these 20 years, a lot of concepts arising from quantum field theory such as quantum groups have had a major influence on modern mathematics and physics.

Mathematicians and physicists inspire each other at the Kavli IPMU. Let us give an overview of geometric aspects of research at the Kavli IPMU. Geometric objects studied in mathematics include several kinds of spaces, such as topological spaces, differentiable manifolds, symplectic manifolds, complex manifolds and algebraic varieties. These various branches of geometries are deeply connected and influence each other. For instance, mirror symmetry is a conjectural duality between symplectic manifolds and algebraic varieties, which was found from the duality between different types of string theories. One of the research objectives of the geometry group at the Kavli IPMU is to invent and investigate the mathematical notions describing mirror symmetry and to give some applications to the geometric problems we are interested in.

In the theory of mirror symmetry, a Calabi-Yau 3-fold plays an important role. A Calabi-Yau 3-fold is a complex manifold of real dimension 6 with a Ricci flat metric. In string theory, the spacetime is expected to be 10-dimensional and the extra 6-dimensional space is expected to take the form of a Calabi-Yau 3-fold. On a Calabi-Yau 3-fold, we can define the quantum invariant counting Riemann surfaces on it, called the Gromov-Witten (GW) invariant. One of the ways to describe mirror symmetry is to establish the relationship between GW invariants and the period maps on the mirror manifold. At the Kavli IPMU, Principal Investigator Kyoji Saito is studying the period maps and developing the theory of primitive integrals.

Another way to describe mirror symmetry is to use the homological algebra proposed by Maxim Kontsevich, who has been a visitor at the Kavli IPMU several times. It is stated as an equivalence of triangulated categories between the derived category of coherent sheaves and the derived Fukaya category on the mirror manifold. At the Kavli IPMU, Principal Investigator Alexey Bondal is developing the theory of triangulated categories, describing the structure of several triangulated categories to show the existence of the exceptional collections. The development of this theory is relevant to the understanding of mirror symmetry.

On a Calabi-Yau 3-fold we can define another quantum invariant, called the Donaldson-Thomas (DT) invariant. It counts D-branes in terms of string theory and is expected to be equivalent to the GW invariant. DT theory depends on the choice of stability condition on the derived category and the set of stability conditions form a complex manifold, which is expected to be a stringy Kähler moduli space. Understanding DT invariants and the structure of the space of stability conditions is important in connection to string theory. At the Kavli IPMU, Associate Professor Yukinobu Toda is studying these theories. Also, the theory of quantum invariants of low dimensional manifolds has begun with the study of quantum theory, such as integrable systems, soliton equations and conformal field theory. These quantum invariants turn out to have a deep connection with GW theory. There is also a new development in conformal field theory at the Kavli IPMU from the point of view of vertex operator algebras.

Now let us describe algebraic aspects of research at the Kavli IPMU. The subjects include homological algebra and category theory. Homological algebra began as a study of homology groups of topological spaces. K-theory is an example of the cohomology theories. Let us recall that in connection to string theory, an interesting and basic example is that an element of a K-group of a certain topological space has a physical interpretation. This enables us to use the powerful machinery of homological algebra to study string theory.

Nowadays, a basic algebraic invariant associated with a geometric object is a triangulated category. For example, this appears from an algebraic variety as the derived category of coherent sheaves. The notion of triangulated category is so abstract that they appear everywhere in mathematics. We know that some non-commutative geometry is better described in this language. Recent research is focused on finding a more complicated structure than that of a triangulated category. Differential graded categories and model categories are examples of objects that are equipped with more structure than a triangulated category. The researchers in mathematics at the Kavli IPMU are seeking to reveal the algebraic structure common to various phenomena occurring in mathematics and physics.

Among the seminars regularly held at the Kavli IPMU are the MS seminar (Mathematics – String theory), the DMM seminar and the Math-Astro seminar. The MS seminar gathers both mathematicians and string theorists. The DMM seminar focuses on derived categories, mirror symmetry and McKay correspondence. The Math-Astro seminar deals with topics such as the relationship between gravitational lensing and the theory of singularities.

Here are some of the conferences and workshops held at the Kavli IPMU related to mathematics.

- Asian mathematicians and theoretical physicists, 20–22 March 2008.
- Moonshiney conference in Kashiwa, 22–24 May 2008.
- Exceptional collections and degenerations of varieties, 1–5 September 2008.
- Workshop on micro-local analysis on symplectic manifolds, 16–18 September 2008.
- Mini workshop at IPMU on a new recursion from random matrices and topological string theory, 11–13 December 2008.
- Supersymmetry in complex geometry, 4–9 January 2009.
- Focus week on new invariants and wall crossing, 18–22 May 2009.

Workshop in the lecture hall (Courtesy: Kavli IPMU)

- Workshop on quantizations, integrable systems and representation theory, 5–6 November 2009.
- Workshop on elliptic fibrations and F-theory, 4–8 January 2010.
- Workshop on geometry of lattices and infinite dimensional Lie algebras, 17–19 March 2010.
- Workshop on geometry and analysis of discriminants, 7–8 February 2011.
- Log Hodge theory and elliptic flat invariants, 24 February 2011.
- IPMU workshop "Extended root systems and fundamental groups", 13–17 February 2012.
- Workshop on geometry and physics of the Landau-Ginzburg model, 25–29 June 2012.

4. Job opportunities at the Kavli IPMU

Each year the Kavli IPMU appoints approximately 15 postdocs for three-year terms. The subject of research of the postdocs ranges over mathematics, statistics, theoretical and experimental physics and astronomy. Applications can be made from October to December every year. There are also recruitments for distinguished, five-year term postdocs and faculty members. The Kavli IPMU is particularly interested in candidates with broad interests to interact with people from other subfields.

The Kavli IPMU has generous travel support for postdocs and encourages full-time members to be away from the institute for between one and three months every year.

http://www.ipmu.jp/

Toshitake Kohno [kohno@ms.u-tokyo.ac.jp] is Principal Investigator at Kavli Institute for the Physics and Mathematics of the Universe, and Professor at Graduate School of Mathematical Sciences, University of Tokyo.

ICMI Column

Mariolina Bartolini Bussi

In this column I summarise some current ICMI activities. 2012 is a very busy year for the ICMI, with the International Congress on Mathematical Education (ICME 12) in Seoul, the General Assembly in Seoul where the new EC for 2013-2016 is elected and some studies in progress.

The Felix Klein Medal for 2011 goes to Alan H. Schoenfeld, University of California at Berkeley, USA

It is with great pleasure that the ICMI Awards Committee hereby announces that the Felix Klein Medal for 2011 is given to the Elizabeth and Edward Connor Professor of Education and Affiliated Professor of Mathematics, Alan H. Schoenfeld, University of California at Berkeley, USA, in recognition of his more than 30 years of sustained, consistent and outstanding lifetime achievements in mathematics education research and development. Alan Schoenfeld, a research mathematician by training, developed his keen interest in mathematics education early on in his career. He quickly emerged as a pioneer and leader in research on mathematical problem solving and, more broadly, on mathematical thinking, teaching and learning. His scholarly work shows a remarkable lifelong pursuit of deeper understanding of the nature and development of mathematical learning and teaching at different educational levels. Starting with work on mathematical problem solving in the late 1970s, he broadened his interests in the mid-1980s to focus on mathematical teaching and teachers' proficiency. His work has helped to shape research and theory development in these areas, making a seminal impact on subsequent research. Alan Schoenfeld has also done fundamental theoretical and applied work that connects research and practice in assessment, mathematical curriculum, diversity in mathematics education, research methodology and teacher education. His work is internationally acclaimed across disciplines with more than 200 highly-cited publications in mathematics education, mathematics, educational research and educational psychology. His scholarship is of the highest quality, reflected in esteemed recognition from mathematical, scientific, teaching and educational organisations over the years. Another significant component of Alan Schoenfeld's achievements is the mentoring he has provided to graduate students and scholars; he has nurtured a generation of new scholars who are having an increasing impact on the field of mathematics education research, both nationally and internationally. Alan Schoenfeld's achievements also include a remarkable amount of outstanding work for national, regional and international communities in education, mathematics and mathematics education. He has provided important leadership in prestigious professional associations and joint research endeavours, both nationally and internationally, and has been an invited keynote speaker at numerous conferences around the globe.

The Hans Freudenthal Medal for 2011 goes to Luis Radford, Université Laurentienne, Sudbury, Canada

It is with great pleasure that the ICMI Awards Committee hereby announces that the Hans Freudenthal Medal for 2011 is given to Professor Luis Radford, Université Laurentienne, Canada, in recognition of the theoretically well-conceived and highly coherent research programme that he initiated and has brought to fruition over the past two decades, which has had a significant impact on the community. His development of a semiotic-cultural theory of learning, rooted in his interest in the history of mathematics, has drawn on epistemology, semiotics, anthropology, psychology and philosophy, and has been anchored in detailed observations of students' algebraic activity in class. His research, which has already garnered several awards, has been documented extensively in a vast number of highly renowned scientific journals and specialised books and handbooks, as well as in numerous invited keynote presentations at international conferences. The impact of Luis Radford's programme of research has been felt especially by the community of research in algebra teaching and learning, where his theoretical and empirical work has led to significant new insights in this domain and more broadly by the entire community of mathematics education research, with his development of a groundbreaking, widely applicable theory of learning. Further evidence of the impact of Luis Radford's work can be found in the many mentoring workshops for graduate students he has been invited to give in several countries, including Italy, Spain, Denmark, Colombia, Mexico and Brazil. Moreover, he has influenced teachers, teacher educators, curriculum developers and representatives of ministries of education at the regional and national levels by his seminars on the implications of his research. His scholarly work has also led to prestigious invitations at the international level, such as his participation in the scientific programme of the Symposium for the ICMI Centennial "The First Century of the International Commission on Mathematical Instruction (1908-2008): Reflecting and Shaping the World of Mathematics Education" in Rome in 2008. In addition, he has served as associate editor of *For the Learning of Mathematics* and is currently an associate editor of *Educational Studies in Mathematics*'.

Alan Schoenfeld, Luis Radford, Gilah Leder (Felix Klein Medal for 2009) and Yves Chevallard (Hans Freudenthal Medal for 2009) will be present at the ICME12 in Seoul. The medals and certificates of the awards given will be presented at the opening ceremony. Furthermore, the awardees will be invited to present special lectures at the congress.

More information is available on the ICMI website: http://www.mathunion.org/icmi.

A new ICMI study is in progress: task design in mathematics education

ICMI Study 22

This study aims to produce a state-of-the-art summary of relevant research and to go beyond that summary to develop new insights and new areas of knowledge and study about task design. In particular, we aim to develop a more explicit understanding of the difficulties involved in designing and implementing tasks, and of the interfaces between teaching, researching and designing roles – recognising that these might be undertaken by the same person or by completely separate teams.

Convenors: Anne Watson, University of Oxford, UK, & Minoru Ohtani, Kanazawa University, Japan

Plenary speakers: Marianna Bosch, Toshiakira Fujii, Jan de Lange, Michal Yerushalmy

IPC

Janet Ainley, School of Education, University of Leicester, UK

Janete Bolite Frant, LOVEME Lab, UNIBAN, Brazil

Michiel Doorman, Utrecht University, Netherlands

Carolyn Kieran, Université du Québec à Montréal, Canada

Allen Leung, Hong Kong Baptist University, Hong Kong

Claire Margolinas, Laboratoire ACTé, Université Blaise Pascal, Clermont Université, France

Peter Sullivan, Monash University, Australia

Denisse Thompson, University of South Florida, USA

Yudong Yang, Shanghai Academy of Educational Sciences, China

Conference administrator: Ellie Darlington

Study conference: The study conference will take place at the Department of Education, University of Oxford, 22–26 July 2013 (inclusive). Places are limited to 80 and only those whose papers are accepted will be invited to attend. The study conference will be organised so that most work takes place in Theme Working Groups. For more information about these please read the discussion document. Conference proceedings will be online.

Call for papers: Papers are invited from designers, researchers, teacher educators, teachers and textbook authors and we are especially interested in co-authored papers that cross these communities. Deadline: 1 August 2012.

Outline of the discussion document

There has been a recent increase in interest in task design as a focus for research and development in mathematics education. Task design is core to effective teaching. This is well-illustrated by the success of theoretically-based long-term design-research projects in which design and research over time have combined to develop materials and approaches that have appealed to teachers.

One area of investigation is how published tasks are appropriated by teachers for complex purposes and hence how task design influences mathematics teaching. Such tasks are often complex and multi-stage, addressing complex purposes. We encourage an interest also in tasks that have more limited but valid intentions, such as tasks that have a change in conceptual understanding as an aim or tasks that focus only on fluency and accuracy.

Tasks generate activity which affords opportunities to encounter mathematical concepts, ideas and strategies and also to use and develop mathematical thinking and modes of enquiry. Teaching includes the selection, modification, design, sequencing, installation, observation and evaluation of tasks. This work is often undertaken by using a textbook and/or other resources designed by outsiders. Textbooks are not the only medium in which sequences of tasks, designed to afford progressive understanding or shifts to other levels of perception, can be presented and we expect that study conference participants will also look at the design of online task banks.

Tasks also arise spontaneously in educational contexts, with teachers and/or learners raising questions or providing prompts for action by drawing on a repertoire of past experience. We are interested in how these are underpinned with implicit design principles.

It is important to address also the question of sequences of tasks and the ways in which they link aspects of conceptual knowledge. In some sequences, the earlier tasks might be technical components to be used and combined later; in others, the earlier tasks might provide images or experiences which enable later tasks to be undertaken with situational understanding.

The communities involved in task design are naturally overlapping and diverse. Design can involve designers, professional mathematicians, teacher educators, teachers, researchers, learners, authors, publishers and manufacturers, or combinations of these and individuals acting in several of these roles. In the study we wish to illuminate the diverse communities and methods that lead to the development and use of tasks.

Themes of working groups
The work for the study will take place mainly within five working groups. We expect there to be several aspects (such as the use of digital technology, teacher education and curriculum design) which appear in several themes and the conference will be designed to allow these to emerge and be discussed.

Theme A: Tools and representations
Theme B: Accounting for student perspectives in task design
Theme C: Design and use of text-based resources
Theme D: Principles and frameworks for task design within and across design communities
Theme E: Features of task design informing teachers' decisions about goals and pedagogies

More information will be available soon through the ICMI website: http://www.mathunion.org/icmi/conferences/icmi-studies/ongoing-studies/.

General Assembly of the ICMI Meeting

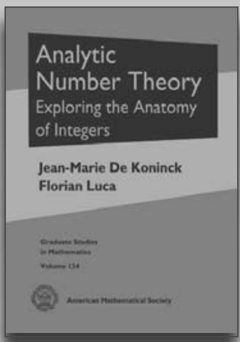
The General Assembly of the ICMI will meet in Seoul in July 2012 at the International Congresses on Mathematical Education (ICME12), which is held every four years. This assembly is responsible in particular for the election of the Executive Committee of the ICMI, which includes the presiding officers of the ICMI. According to the procedures of elections a slate will be presented by the ICMI Nominating Committee for the following positions:

President
Secretary General
Two vice-presidents
Five members at large.

The ICMI EC elected will serve four-year terms, beginning 1 January 2013.
Ex-officio members will be the past President of ICMI (Bill Barton as from 1 January 2013), and the President and Secretary of the International Mathematical Union.

More information about the slate and the procedures will be available soon through the ICMI website:
http://www.mathunion.org/icmi.

American Mathematical Society



ANALYTIC NUMBER THEORY

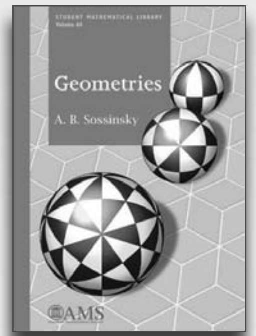
Exploring the Anatomy of Integers

Jean-Marie De Koninck, *Université Laval* &
Florian Luca, *Universidad Nacional Autonoma de México*

The authors assemble a fascinating collection of topics from analytic number theory that provides an introduction to the subject with a very clear and unique focus on the anatomy of integers, that is, on the study of the multiplicative structure of the integers. Some of the most important topics presented are the global and local behaviour of arithmetic functions, an extensive study of smooth numbers, the Hardy-Ramanujan and Landau theorems, characters and the Dirichlet theorem, the *abc* conjecture along with some of its applications, and sieve methods.

Graduate Studies in Mathematics, Vol. 134

Jun 2012 420pp 978-0-8218-7577-3 Hardback €68.00



GEOMETRIES

A. B. Sossinsky, *Independent University of Moscow*

An innovative modern exposition of geometry, or rather, of geometries. This is the first textbook in which Felix Klein's Erlangen Program (the action of transformation groups) is systematically used as the basis for defining various geometries. The course of study presented is dedicated to the proposition that all geometries are created equal - although some, of course, remain more equal than others.

Student Mathematical Library, Vol. 64

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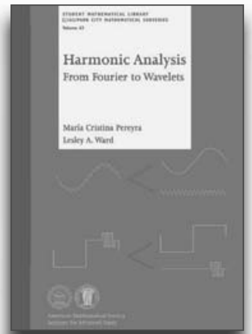

HARMONIC ANALYSIS

María Cristina Pereyra, *The University of New Mexico* &
Lesley A. Ward, *University of South Australia*

In the last 200 years, harmonic analysis has been one of the most influential bodies of mathematical ideas, having been exceptionally significant both in its theoretical implications and in its enormous range of applicability throughout mathematics, science, and engineering. In this book, the authors convey the remarkable beauty and applicability of the ideas that have grown from Fourier theory.

Student Mathematical Library, Vol. 63

Jun 2012 411pp 978-0-8218-7566-7 Paperback €52.00

LINEAR AND QUASI-LINEAR EVOLUTION EQUATIONS IN HILBERT SPACES

Pascal Cherrier, *Université Pierre et Marie Curie* & Albert Milani, *University of Wisconsin*

Considers evolution equations of hyperbolic and parabolic type. These equations are studied from a common point of view, using elementary methods, such as that of energy estimates, which prove to be quite versatile. The authors emphasise the Cauchy problem and present a unified theory for the treatment of these equations.

Graduate Studies in Mathematics, Vol. 135

Aug 2012 378pp 978-0-8218-7576-6 Hardback €68.00

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What are the Reciprocal Expectations between Teacher and Students? Solid Findings in Mathematics Education on Didactical Contract

Education Committee of the EMS

Episodes from school

Four examples of episodes from school are used in order to address a relevant solid finding in mathematics education. Nevertheless, the idea and importance of reciprocal expectations between teacher and students may be illustrated by means of many other episodes.

Episode 1

This episode is known in the community of didacticians under the name "the captain's age". At the end of the 1970s, the researchers from IREM (Institut de Recherche sur l'Enseignement des Mathématiques) Grenoble proposed, without really manifesting reasons, the following assignment to primary school students: "There are 26 sheep and 10 goats on the boat. How old is the captain?" Seventy-six of 97 students calculated the captain's age by combining the given numbers by some operation like addition or subtraction.

Various versions of the captain's age problem were used in many countries (see e.g. Verschaffel, Greer & de Corte, 2000). Similar behaviour of students was observed in most cases. It is governed by their belief that the data in the problem assignment are to be used in the calculations and these calculations give the required answer. Most students do not try to make sense of the assignment and trust the teacher that the problem is correctly assigned.

Episode 2

This story took place in a class of 9–10 year old students. The teacher taught the following algorithm facilitating calculation of the difference between two numbers:

$$\begin{array}{ccccc} 328 & \xrightarrow{+3} & 331 & \xrightarrow{+50} & 381 \\ \underline{-\,47} & \xrightarrow[+3]{} & \underline{-\,50} & \xrightarrow[+50]{} & \underline{-\,100} \\ & & 281 & & 281 \end{array}$$

Several weeks later, the students were assigned the following task.

How would you carry out the following calculations?

a) 875 – 379 = ______________________________

b) 964 – 853 = ______________________________

c) 999 – 111 = ______________________________

Most students (16 out of 19) applied the algorithm they were taught in all the three exercises including the third one:

$$999 - 111 = 1008 - 120 = 1088 - 200 = 888$$

The students' reaction is governed more by what they suppose the teacher expects from them than by the nature of the question. Most of them prefer to show their ability to use the taught algorithm than to calculate the difference between 999 and 111 directly.

Episode 3

This story takes place in a class of 13–14 year old students. The following equation from homework is written on the whiteboard:

$$5\frac{2}{3}x - \frac{3}{2} = 4\frac{1}{6}x + \frac{1}{2}$$

What happened is that several students replaced the mixed numbers $5\frac{2}{3}, 4\frac{1}{6}$ by the expressions $5 \times \frac{2}{3}, 4 \times \frac{1}{6}$ and then carried on using the correct solving procedures. This mistake was not anticipated by the teacher and the differences between the two cases became the topic of the ensuing whole class discussion.

The main characteristic of this teacher's work is that she keeps referring back to reasoning about rules that were taught and validated a long time ago. Her students trust that this reference is to the former knowledge that is useful when solving the assigned problem(s) and rely on it. However, when assigning the homework, the teacher did not make any link to her students' former knowledge of mixed numbers. As a consequence several students worked with them incorrectly.

Episode 4

This story takes place in a class of 15–16 year old students. In the test, the students are asked to solve the following problem. Find $x \in \mathbb{R}$ such that: a) $\sin x = \pi/3$; b) $\cos x = \pi/2$.

Only 25 % of the students give the correct answer to a) and 29 % to b).

The students act according to their belief that the teacher always presents tasks that have a solution. For example, explicitly citing from a discussion with one student: "It is strange that an exercise could have no solution."

In some mathematics classes these or similar situations happen quite often, while in other classes they do not happen or are very rare. If the source of these types of situation was the mathematical content, they should occur in all classes. Research in mathematics education has confirmed that the source is more or less in implicit rules that regulate the relations between the teacher and their students and are class-specific. This set of implicit rules is called the *didactical contract*.

Genesis of the notion of didactical contract and its relevance at present

The concept of didactical contract (DC) was proposed by Brousseau in France at the end of the 1970s with the objective of explaining specific failures that can be found only in mathematics (Brousseau called them "elective failures"[1]). As we can see from the above examples, students often answer to comply with what they think is expected from them by the teacher rather than to cope with the assigned situation. The DC is a theoretical construct invented in order to deal with this phenomenon. It is certainly one of the most fundamental theoretical constructs in the didactics of mathematics both on a French and an international scale.[2]

The most cited definition of DC is Brousseau's (1980, p. 127): the DC corresponds to "the set of the teacher behaviours (specific to the taught knowledge) expected by the student and the set of the student behaviours expected by the teacher".

The DC is the set of reciprocal obligations and "sanctions" which

- each partner in the didactical situation imposes or believes to have imposed with respect to the knowledge in question, explicitly or implicitly, on the other;
- or are imposed, or believed by each partner to have been imposed on them with respect to the knowledge in question.

The DC is the result of an often implicit "negotiation" of the mode of establishing the relationships for a student or group of students, a certain educational environment and an educational system. It can be considered that the obligations of the teacher with respect to the society which has delegated to them their didactical legitimacy are also a determining part of the "didactical contract".

The DC is not a real contract because it is not explicit, nor freely consented to; moreover, neither the conditions in which it is broken nor the penalty for doing so can be given in advance because their didactical nature, the important part of it, depends on knowledge as yet unknown to the students.

Furthermore the DC is often untenable. It presents the teachers with a genuinely paradoxical injunction: everything that they do in order to produce in the learners the behaviour they want tends toward diminishing the students' uncertainty and hence toward depriving them of the conditions necessary for the comprehension and the learning of the notion aimed at. If the teacher says or indicates what they want the student to do, they can only obtain it as the execution of an order and not by means of the exercise of the students' knowledge and judgment (this is one of several didactical paradoxes brought about by the DC). But the student is also confronted with a paradoxical injunction: the student is aware that the teacher knows the correct solving procedure and answer; hence, according to the DC, the teacher will teach them the solutions and the answers; they do not establish them for themselves and thus do not engage the necessary (mathematical) knowledge and cannot appropriate it. Wanting to learn thus involves the student refusing the DC in order to take charge of the problem in an autonomous way. Learning thus results not from the smooth functioning of the DC but from breaking it and making adjustments. When there is a rupture (failure of the student or the teacher), the partners behave as if they had had a contract with each other.

The DC is not an illness of the didactical relation; it shows that learning mathematics consists not only of memorising algorithms and knowing definitions. It is the object of the theory of didactical situations (Brousseau, 1978, 1997) to study situations that allow the learner to learn to do mathematics and not only to memorise it. For example, if students practise a number of exercises for addition of two numbers and the teacher inserts a subtraction exercise, students who only memorise mathematics will continue adding the numbers.

The DC is structurally analogous to the well known social contract of J.-J. Rousseau: the social contract allows us to understand theoretically the conditions of existence of relationships between an individual and a group, without postulating that this social pact occurred, in a certain way, among social agents; everything happens as if this apparent accordance had been contracted some time ago. It equally concerns subjects of all didactical situations (students and teachers).

Another paradox implied by the DC is the one identified in the theory of situations under the name 'the paradox of devolution': the teacher has to talk to students who de facto cannot understand because they must learn and, as Brousseau says, "if the teacher says what it is that she wants, she can no longer obtain it" (Brousseau, 1997, p. 41). An example of such a situation is published in Novotná (2009). One of the questions discussed is the following: are students able to recognise, clarify and explain similarities and differences between problems and are they able to recognise various problems related to one mathematical model in different conditions? All the activities are organised in such a way that they involve spontaneous emergence of the notion of mathematical model; the notion is not taught explicitly by the teacher.

At every moment of the lesson the teacher might be assuming that as a consequence of teaching, their students have or have not learned something. Analogically, the student may think that they really understand what the teacher is trying to teach. But when one or the other tries to verify this, a system of expectations evolves, with whose help all the involved parties make decisions on the

1 Students with a specific failure are those "who have deficiencies in acquisition, learning difficulties or lack of liking, shown in the domain of mathematics but who do sufficiently well in other disciplines" (Brousseau, 1978).

2 Guy Brousseau was the first to be awarded the Felix Klein medal from ICMI, not least because he proposed the notion of DC to the community of mathematics education researchers.

extent of concord between what was observed and what was expected (called *hypothetical contract* by Brousseau). Even in the case that the observation matches the expectation, nothing can guarantee that this concord testifies that the learning itself is really in accord with what the teacher expected.

The following example illustrates the aforementioned phenomenon. When working with multiplication tables, the teacher assigns the task '*Fill in numbers into the boxes*':

(♠♠) (♠♠) (♠♠)

$\square \times \square = \square$

The answer expected by the teacher is $\boxed{3} \times \boxed{2} = \boxed{6}$.

It is not possible to decide unequivocally merely on the basis of this answer (unless it is supplemented by additional comments) whether the student really grasped multiplication of natural numbers. The student might have used a simple process based on the following algorithm: I know that the teacher requests that I should fill in the number of ellipses in the first box and the number of spades in one ellipse in the second box and I know that the total number of spades is to be written in the third box. Therefore, what students do is count the number of spades rather than multiplying 3×2.

Authorship

Even though certain authors have taken the lead in each article of this series, all publications in the series are published by the Education Committee of the European Mathematical Society. The committee members are Ferdinando Arzarello, Tommy Dreyfus, Ghislaine Gueudet, Celia Hoyles, Konrad Krainer, Mogens Niss, Jarmila Novotná, Juha Oikonnen, Núria Planas, Despina Potari, Alexei Sossinsky, Peter Sullivan, Günter Törner and Lieven Verschaffel.

Additional information

A slightly expanded version of this article with a more complete list of references may be found on the web at http://www.euro-math-soc.eu/comm-education2.html.

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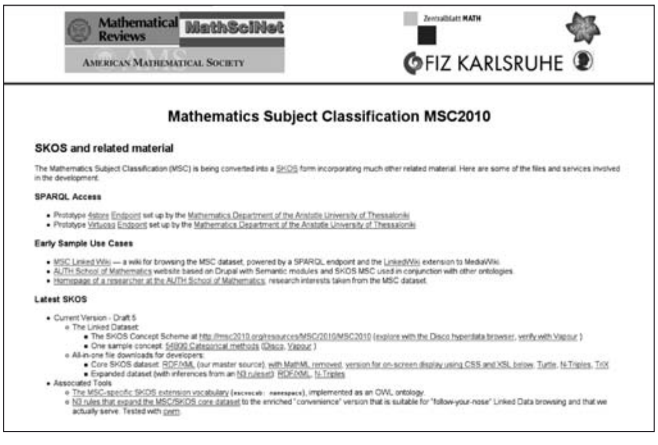
MSC 2010 in SKOS – the Transition of the MSC to the Semantic Web

Patrick Ion (AMS/Mathematical Reviews) and Wolfram Sperber (FIZ Karlsruhe/Zentralblatt MATH)



Abstract: The most comprehensive subject classification scheme in mathematics is the MSC (Mathematics Subject Classification), based originally on an AMS classification scheme, a standard more than 20 years ago. Zentralblatt MATH and Mathematical Reviews jointly developed the first MSC 1990. The current version is MSC 2010 following another 10-year update. With the trend from printed to digital publications, it turns out that the original structure of the MSC is not capable of expressing what we want to convey in the semantics of electronic publications. SKOS (Simple Knowledge Organization Scheme) is an open standard vocabulary intended for the modelling of thesauri and classification schemes for the Web. The SKOS representation of the MSC 2010 will be a start to making it more usable for electronic publishing and integrated into the Semantic Web. This article outlines why and how this has been done.

1. Mathematics classification systems

Classification schemes are intellectually created conceptualisations for organising particular subjects. For searching mathematical publications by subject, ordering them in catalogues and bookshelves or describing the context of a publication in a systematic way, indexing of these publications according to subject is a basic requirement. In the past, several schemes have been developed for this purpose. Those currently used, in addition to the Mathematics Subject Classification MSC 2010, are the mathematics section of the DDC (Dewey Decimal Classification), the mathematics part of the UDC/UDK (Universal Decimal Classification) and some other schemes such as the relevant Subject Headings from the Library of Congress. In contrast to other sciences, mathematics does not have an accepted thesaurus or other kind of controlled vocabulary. There were subject indexes for the different encyclopaedias in mathematics but these could not be considered as a satisfactory replacement for a controlled vocabulary.

MSC 2010 is the current update, after three years of work, of a scheme used since 1990. The original MSC was developed at the end of the 1980s from a scheme used by the AMS. Starting with MSC 1990, Mathematical Reviews (MR) and Zentralblatt MATH (Zbl) agreed to use the MSC as a common standard and jointly to maintain it and to make updates that take new developments in mathematics into account. MSC 2010 has reached a high level of refinement. It has a tree-like hierarchical structure and covers all areas of mathematics and its applications. A typical MSC classification code is 05C10: 05 is one of 63 top-level 2-digit classes, 05C is one of 528 3-digit subclasses at the second level and 05C10 is among the 5606 5-digit classification codes [1,2]. The codes of the MSC mainly represent mathematical subjects; thus 05C10 has the description "Planar graphs; geometric and topological aspects of graph theory [See also 57M15, 57M25]". This class covers mathematical objects, but some also refer to theories, methods, properties, applications, etc. Working with the over 6,000 codes of the MSC is difficult for most people. Mathematics is a dynamic subject and the MSC ought to evolve with it.

2. Why a new version of the MSC?

With MSC 2010, a unique electronic master version of the MSC was defined for the first time. In a sense, the master version was a full specification of the printed version. Mathematical expressions require special markup; it seemed natural to choose TeX for this since it was used in the production streams of both Zbl and MR. It is a small step on to using customised TeX markup for encoding the structure of the MSC. The existing electronic TeX master version has been made to provide some additional features and advantages beyond the simple print version, e.g. other encodings, update comparison tables and MSCwiki, a KeyWords in Context (KWIC) index, etc.

Use of the MSC by the MR and Zbl is a good reason for keeping it as stable as possible. However, there are difficulties inherent in having a proprietary classification scheme. The semantics of the MSC classes and their relational structure were not explicitly declared and described, other than by their appearance in the printed MSC, because inside MR and Zbl there was a culture that passed on their meanings as needed. This makes automatic semantic processing involving MSC codes difficult. Linking the MSC with other classification schemes, embedding it in library automation, and further semantic annotation of the MSC, e.g. by addition of characteristic key phrases to the MSC classes, are also not easy. These problems are mitigated by making a SKOS version of the existing MSC.

3. What does a 'SKOS version of the MSC' mean?

SKOS [3] stands for Simple Knowledge Organization System. It is a public standard from the World Wide Web Consortium, which has brought us HTTP, HTML, MathML, SVG and many other standards that make the Web work. SKOS is a vocabulary focused on modelling of thesauri and classification schemes for the Web. Adopting such a vocabulary explicitly enables the standardisation of elements and relations that are typical for classification schemes, e.g. classes and hierarchical relations.

SKOS fits in the framework of the so-called Semantic Web, the general approach today for the semantic annotation and automatic processing of information. SKOS is based on the Resource Description Framework (RDF), a graph model for the semantic annotation of information, and the Ontology Web Language (OWL). Our SKOS documents are written in an XML framework (eXtensible Markup Language), as is MathML like many other types of modern documents. Many initiatives, e.g. from libraries, have started to transform their specialist classification schemes into SKOS. This brings some advantages: separate classification schemes can be maintained simultaneously, publications can be classified automatically, better search functionalities can be provided and more.

3.1 Some basics of SKOS:

As stated above, SKOS is a standardised vocabulary for thesauri and classification schemes. We list some examples of what it allows one to define (with the corresponding SKOS elements to be used listed in parentheses):

- a scheme (using the elements skos:ConceptScheme, skos:inScheme, skos:hasTopConcept, skos:topConceptOf),
- classes, their notations and their labels (skos:Concept, skos:notation, skos:prefLabel, skos:altLabel, skos:hiddenLabel),
- hierarchical relations (skos:hasTopConcept, skos:narrower, skos:broader, skos:narrowerTransitive, skos:broaderTransitive),
- similarity relations (skos:related, skos:semanticRelation),
- additional groupings of SKOS concepts (skos:Collection, skos:OrderedCollection, skos:member, skos:memberlist),
- mapping properties of a SKOS scheme with further schemes (skos:closeMatch, skos:exactMatch, skos:mappingRelation, skos:narrowMatch, skos:relatedMatch),
- further properties (skos:changeNote, skos:definition, skos:editorialNote, skos:example, skos:historyNote, skos:note).

3.2 MSC reimplementation

SKOS is used straightforwardly for the definition of:

- the concept scheme MSC and the MSC classes,
- the notations and the labels of the MSC classes,
- multilingual labels (marked using the language attribute of XML),
- hierarchical relations,

and also for definition of:

- groupings of MSC classes,
- mappings of the MSC to older versions of the MSC,
- mappings of the MSC to other classification schemes.

The SKOS reimplementation of the MSC requires some additional work to handle mathematical expressions: symbols encoded in TeX have to be replaced by the corresponding Unicode characters. More complex mathematical expressions have to be expressed in MathML. Furthermore the MSC has some special similarity relations: "See also...", "See mainly ...", "For ... see ..." as in the example 05C10 above. Such relations are not covered by SKOS and have to be defined in the MSC's own vocabulary (namespace). Arrangements for such extensions are part of the SKOS and OWL specifications.

The first SKOS version of the MSC was produced by the authors, at MR and Zbl, in collaboration with colleagues in Bremen and Thessaloniki, see [1]. It is available at http://msc2010.org/resources/MSC/2010/MSC2010.

Note that to process a SKOS resource requires particular software. The reader is advised to consult http://msc2010.org/mscwork/ for other resources and descriptions of the new SKOS version and its derivatives.

4. Further development

The current MSC-SKOS is only a start in overcoming the problems with the MSC in its traditional setting. It is an adequate technical frame for the representation of the MSC with much potential for future development. Future improvements may provide possibilities for a more precise definition of subject classes by using keyterms, developing a smart faceted structure for the MSC or linking with other schemes and tools. Furthermore the availability of the machine readable and usable form of the MSC may lead to surprising responses from the machine agents on the Semantic Web. In addition, we should probably be looking to describe reasonable structures in the universe of mathematical subjects by employing clustering methods like Latent Semantic Indexing or Latent Dirichlet Allocation.

Finally, the availability of MathML and its use for encoding formulas offers new opportunities for a knowledge management specific to mathematics that also includes classification by formulae, for example. The SKOS implementation of the MSC is a first important and necessary step for this. In the past, Zbl and MR have assiduously pursued the development of the MSC in consultation with the mathematical community, by asking for proposals of new subjects and comments on the revised form of a subject area. For the further development of the MSC in the SKOS framework more involvement of the mathematical community will be needed, possibly by creating an expert group giving further advice and governing this development. Hence, please send your comments and suggestions regarding the MSC and its use to msc@msc2010.org or record them on a form at the website http://msc2010.org.

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[2] http://msc2010.org/mscwiki/.

[3] http://www.w3.org/TR/skos-reference/.

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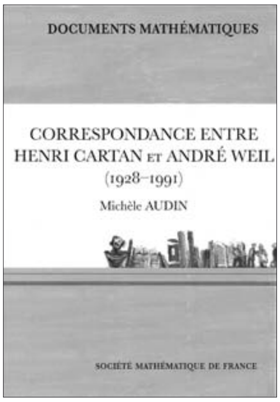
Book Reviews



Michèle Audin

Correspondance entre Henri Cartan et André Weil (1928–1991)

Société Mathématique de France, 2010
750 pages, hardcover
ISBN: 978-2-8529-314-0

Reviewer: Javier Fresán

This book assembles more than 200 letters exchanged by Henri Cartan and André Weil from November 1928 to May 1991. Most of them were discovered a few years ago within the archives of Cartan, who does not seem to have thrown away a single paper in his life.[1] It would be hard to imagine a better editor for this correspondence than Michèle Audin, an expert, among other things, on the history of French and German mathematicians during the World Wars and the interbellum. The exquisite research she has carried out becomes evident from the first page. In particular, her extensive notes at the end of the volume are not reduced to a mere identification of the various characters and situations to which the letters refer; on the contrary, they "tell another story", in the same way that the commentaries added by Weil to his collected works form an independent book. One can find there, just to mention a few examples: a long letter in which a very young Weil displays all his mathematical knowledge; a chronology of the Cartan seminar through Serre's memories; and a thorough reconstruction of the anticommunism hysteria surrounding the ICM 1950, which part of the French delegation was planning to boycott if Hadamard and Schwartz did not get their visas in time.[2] Several documents from the recently declassified files of Bourbaki have also been included.

Let it be said from the beginning that this correspondence is quite different in style from the one maintained, partly at the same time, by Grothendieck and Serre,[3] of which it could be reminiscent at first sight. While the main topic is of course mathematics, it is not the only one: as Cartan and Weil were close friends and founding fathers of Bourbaki, many letters address practical problems regarding the organisation of the group and questions of a more personal nature (such as family holidays, health issues and music). A particularly sad leitmotif is Weil's recurring desire to find an academic position in France,[4] for instance when Lebesgue retired from his chair at the Collège de France. Despite the great deal of time and energy Cartan devoted to supporting his friend, all his attempts were frustrated by political resentment and the direct application of Weil's own law, "first-rate people attract other first-rate people but second-rate people tend to hire third-raters and third-rate people fifth-raters" (p. 621). Back to mathematics, it is thrilling to discover how the slowly emerging notion of "cohomologie à domaines de coefficients variables" (p. 142) had already led, in the late 1940s, to a perfectly modern definition of spectral sequences (pp. 246–247). Young and not-so-young readers will probably smile at how breakthroughs such as the Steenrood operations (p. 217) and the Kodaira embedding theorem "toute variété de Hodge est variété projective" (p. 346) were disseminated before the arXiv!

Bourbaki

It could be seen as disappointing not to find any scoop here on the birth of Bourbaki. But this is not surprising: at that time, Cartan and Weil were colleagues at the University of Strasbourg so why should they exchange letters when they could speak in person? The first time Bourbaki is mentioned, on 29 May 1939, is just to say, "Bourbaki devient très populaire par tout: à Cambridge il est à présent le mathématicien dont on parle de plus. Il m'est revenu que Chevalley a fait une grosse propagande à Princeton" (p. 33). This shows that it was not yet the secret society it was going to become in the following years; in contrast, Weil was angry to learn on 4 May 1955 that Saunders Mac Lane had delivered a public speech at New York University in which he described himself as a "fellow-traveller" of Bourbaki (p. 365). Thanks to the letters, other elements of the legend can be put into historical context. For instance, one confirms that retirement at 50 was not a rule until the moment that Weil reached this age and wrote to Cartan, "le meilleur service que les membres fondateurs puissent actuellement rendre à Bourbaki est de disparaître progressivement mais dans un temps fini" (p. 382). If something is to be taken from the correspondence, it is that our protagonists always had Bourbaki in mind. Three early letters show Weil's insistence on replacing the term "ensemble vasculaire" by "ordonné filtrant" (pp. 39, 45, 47). Far from being an exception, that was the general trend. Even the smallest typographical details were discussed at length; nevertheless, Weil was not unaware of the risks of this way of working, as the following extract from Bourbaki's bulletin *La Tribu* shows: "nous ne pouvons continuer à perdre tous notre temps sur des broutilles. Lorsque le contenu

[1] Moreover, he was ready to complain to the postal service whenever necessary (p. 663).

[2] In Cartan's own words: "Je crois que la seule chose que nous, mathématiciens, pouvons faire, c'est de tenter de faire déplacer le Congrès; et si on y arrive, ce sera déjà beaucoup. Mais il faut que nous fassions tout ce qui est en notre pouvoir dans ce sens, sinon nous serons aussi coupables, sur le plan de la collaboration internationale, que les Allemands qui ont admis la dictature hitlérienne." (p. 265).

[3] *Grothendieck-Serre correspondence*, edited by Pierre Colmez and Jean-Pierre Serre, AMS, 2004.

[4] As Weil says on 26 August 1946, "Bien entendu, les USA me dégoûtent, et je n'y retournerai que contraint et forcé" (p. 130).

d'un chapitre devient stable, plus n'est besoin d'un congrès plénier pour en discuter les détails" (p. 597). Taking into account the method, the scarcity of paper and the slowness of postal service, it can only be regarded as a miracle that Bourbaki survived during the war. A letter not to be missed, dated 19 July 1946, is the one in which Cartan suggests, following Chevalley, that modules could be expelled from Bourbaki's *Algebra*: "Si l'on se borne aux espaces vectoriels, l'exposition est beaucoup plus esthétique, on évite incontestablement des lourdeurs, et on facile la tâche de la majorité de lecteurs qui, évidemment, ne s'intéresseront qu'aux espaces vectoriels. Il va sans dire que ce sacrifice ne peut être consenti que si l'intérêt des modules, dans la suite de l'Algèbre, doit être suffisamment limité pour qu'on puisse, sinon s'en passer tout à fait, du moins les reléguer à l'endroit précis où on en aura besoin" (p. 114); it follows a choleric five-page answer by Weil which definitively closed the issue.

The Weil conjectures

Another set of letters concerns the proof of the Riemann hypothesis for curves over finite fields during the Spring of 1940. In those days, Weil was imprisoned in Rouen after what he would later call "a disagreement with the French authorities on the subject of my military *obligations*".[5] He did not waste this opportunity to work "sans souci extérieur", as Cartan put it (p. 63): besides proof-reading his first book and reconstructing a report on integration for Bourbaki, which had been confiscated by the Finnish police, Weil continued thinking about zeta functions. On 26 March, he writes to Cartan, "je crois toucher à des résultats très importants sur la fonction ζ des corps de fonctions algébriques" (p. 70). He then insists on the urgency of getting the answer to a question he has already asked his friend: 'What is the number of n-torsion points of the Jacobian of a curve of genus g over a finite field?' This was needed for the "important" lemma on which his whole argument to prove the Riemann hypothesis relied. On 8 April, the same day that he wrote an illuminating letter to his sister,[6] Weil announced to Cartan that he had submitted a note to the *Académie des sciences*: "Chose plus sérieuse, j'ai expédié la note sans attendre d'avoir démontré le lemme fondamental; mais j'y vois assez clair à présent sur ces questions pour en prendre le risque. Jamais je n'ai rien écrit, et je n'ai presque jamais rien vu, qui atteigne un aussi haut degré de concentration que cette note. Hasse n'a plus qu'à se pendre, car j'y résous (sous réserve de mon lemme) *tous* les principaux problèmes de la théorie" (p. 79). As Weil imagined, German mathematicians did not take long to react, initiating a true "war of reviews";[7] however, the correspondence gives no clue about his feelings regarding the accusation of "unfair play". In 1942, Weil already knew how to prove the lemma[8] but the complete argument would only be published "eight years and more than five hundred pages later"; some letters (starting at p. 97) treat this unusual delay, which is partly due to Weil's refusal to split one of his memoirs into several articles. Somewhat more surprisingly, no mention is to be found in the remaining correspondence either to Weil's paper *Number of solutions of equations in finite fields* or to the long-range programme culminating in the proof of the conjectures stated there.[9] To remedy this, Audin has included a fascinating letter from Weil to Delsarte, dated 13 September 1948, in which he sketches the proof of his conjectures for Fermat hypersurfaces and relates the Ramanujan conjecture to this circle of ideas (pp. 590–592).

Algebraic topology

A less expected chapter of the correspondence deals with ideas on topology and complex analysis around the invention of sheaf theory. Let us recall that Cartan was the first person to unravel the obscure papers by "l'illustre Leray" and to embark, through his seminar, the new, brilliant generation upon the search for applications. On his side, Weil was perfectly up to date with the progress on topology, as this was the field he had chosen to collaborate with the recently created *Mathematical Reviews*. Of course, the correspondence contains the already published letter[10] in which Weil explains how to prove De Rham's theorem on duality between singular chains and differential forms; but this is now completed with a second letter in the same vein. Cartan's manuscript margin notes show that he had studied both texts in detail: in particular, he asks how to define, in the topological setting, "l'anneau de cohomologie (i.e. l'opération de produit)" (p. 142), which should correspond to the wedge product of differential forms. This was at the origin of his theory of "carapaces", an alternative to Leray's "couvertures", which appears on stage for the first time on 5 February 1947. Naturally reserved, Cartan was really enthusiastic about the power of this new notion: "En y réfléchissant, tu apercevras peu à peu toi même la portée de cette nouvelle théorie, qui englobe, en les simplifiant considérablement, tous les aspects connus, en apparence si divergents, de la topologie algébrique." (p. 160). Even if Weil remained sceptical for a long time, this did not stop him from encouraging Cartan to pursue his research. Another interesting exchange (from p. 311 on) was intended to help his friend prepare his ICM talk *Problèmes globaux dans la théorie des fonctions analytiques de plusieurs variables complexes*; several letters concern

[5] *Œuvres scientifiques* vol. I, p. 547. The notes to the correspondence add many details to Weil's own account of his draft evasion: let us just mention Audin's beautiful defence of his position (p. 482–483) and the three-page letter he wrote to the Director of the New School Herbert Solow (p. 509–512).

[6] A. Weil, "A 1940 Letter of André Weil on Analogy in Mathematics", translated by Martin H. Krieger, *Notices of the AMS* 52 (2005), 334–341.

[7] See M. Audin, "La guerre des recensions (autour d'une note d'André Weil en 1940)", arxiv:1109.5230.

[8] This is clear from the letter he wrote to Artin on 10 July 1942; see *Œuvres scientifiques* vol. I, pp. 280–298.

[9] In fact, Weil only refers to Grothendieck twice: the first time to ask Cartan to give him an offprint (p. 380) and the second one in these terms: "je termine la 2e édition des Foundations (je suppose que Grothendieck ne manquerait pas de dire à ce sujet: énergie admirable, digne d'une meilleure cause)" (p. 393).

[10] *Œuvres scientifiques* vol. II, pp. 45–47.

the second Cousin problem and the difference between topological and analytically trivial fiber bundles.

Needless to say, this precious document deserves much more careful analysis. Just to mention an aspect not treated in the preceding sections, the beautiful letter dated 15 June 1984 leaves no doubt as to how highly Weil thought of his friend's father Élie Cartan, one of the secondary characters of the correspondence. My only aim here has been to draw attention to some of the passages I liked the most. Find your own!

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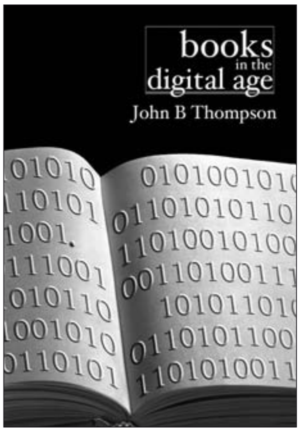

John B. Thompson

Books in the Digital Age

The Transformation of Academic and Higher Education Publishing in Britain and the United States

Polity, Cambridge 2005
Paperback, 480 pages
ISBN 978-0-74563-478-4

Reviewer: Manfred Karbe

The author of this book is a professor of sociology at Cambridge University and co-founder of Polity Press, a leading British publisher in the social sciences and humanities. On p. 189 he writes:

> *"The academic world has come to depend on the field of academic publishing (together with that of scholarly journals) as a principal means for the dissemination of scholarly work and as a key mechanism of professional certification, and, yet, ironically, most academics are woefully ignorant of what is happening in this field upon which so much of their own success now depends.*
> *'I think that academics are very, very, very sadly misinformed,' commented one university press director. 'I'd say that after ten years of proselytizing about this, I've made zero inroads.' This director had her own theory of why academics were so ill-informed about the real conditions of academic publishing: because so much of their own self-esteem is wrapped up in their scholarly work, they tend to share only the success stories with their colleagues. … Whether or not her theory is correct, it is undoubtedly the case that most academics understand very little about the real conditions of academic publishing and how they have changed in recent decades."*

This extract is one of many insights and conclusions reached through more than 230 interviews carried out over a period of three years with staff employed at all levels by 16 unidentified academic and higher education publishers in the UK and North America.

From these sources a first draft was written and comments solicited from twelve senior members in the publishing profession. The result is the first systematic in-depth study in many years of all aspects of scholarly book publishing. While the author concentrates on book publishing in the social sciences and humanities, his analysis applies, by and large, to STM (scientific, technical, and medical) publishing as well.

The book consists of four parts. It starts with an introduction of 80 pages about the business of publishing in general, which is followed by two other parts of about 100 pages each on academic publishing and higher education publishing, that is, publication of textbooks that are used as teaching material in courses at colleges and universities, from first-year undergraduate to postgraduate level. The final part, about 140 pages, is on "the digital revolution".

Textbook publishing, as a result of conglomeratization through mergers and acquisitions, is a wholly corporate enterprise, with Pearson, Thomson and McGraw-Hill the dominant players accounting for 73% of the U.S. college market in 2002 (p. 204). Academic publishing presents a more diversified picture involving participation of a large number of university, non-corporatized and non-profit publishing companies, mostly with output of research in the form of books which range from high-level monographs and proceedings to books written for a broader readership. Here a dramatic change has taken place since the mid-1980s, which is usually referred to as the so-called "crisis of the monograph" also widely known as "death of the book": During the 1970s academic publishers could comfortably expect to sell 2,500 hardback copies of a scholarly monograph; today many of them must accommodate to total sales as low as 400–500 copies worldwide (pp. 93–94). Three reasons are identified for this: firstly, the squeezing of higher education budgets in general, and library budgets in particular, from the early 1970s onward (p. 98); secondly, higher expenditures for periodicals caused by both a steep rise in cost of journal subscriptions and growth of volume (especially in the STM fields but also in the humanities); thirdly, growing investment in IT services (p. 99). Special attention is given to the role of consolidation in journal publishing, which is elaborated on the example of Elsevier (p. 100–101). In recent years much heated debate has been generated over the impact of a small number of publishers having emerged as the key players, controlling a large proportion of journal titles, putting "them in a position of considerable strength when it comes to determining price increases and negotiating with libraries and library consortia" (p. 100). We are now witnessing in the mathematics community the first serious dispute on this issue.

Part IV on "the digital revolution" gives a comprehensive account of the state of e-book publishing at the time of writing, that is, up to 2005. Of course, this is a rapidly changing area so that it is unavoidable that some of the information presented quickly becomes obsolete or outdated. This, in particular, affects Chapter 13, where electronic publishing models for scholarly books are reviewed. For instance, "Google Print" is mentioned in a footnote on p. 370. In the 1990s, we all looked skeptically at the many promises that the age of e-books was almost upon us. It would take another decade for e-books to grow into something more than hype. In October of 2011 Apple claimed that since its launch of iBooks, 180 million books have been downloaded. Amazon is said to have sold 314 million e-books for the Kindle in 2011 alone. Still, the lesson that Thompson tells us – and which is confirmed in his latest book on trade publishing (see reference at the end, p. 349) – is that "the principal market for scholarly book content in electronic form is likely to be institutional rather than individual" (p. 368). It is the best way "to treat individual books as part of a scholarly corpus or database which has scale, selectivity and focus" (p. 369).

Chapter 15, "The hidden revolution", is most revealing: the digital age constitutes "not so much a revolution in the product as a revolution in the process: while the final product may look the same as the old-fashioned book, the process by which it is produced has been, and is being, radically transformed" (p. 403). Publishers require electronic files from authors, outsource composition and printing, often with little or no editorial involvement at all. Technical advancement may have drawbacks:

"'You were giving up a lot that you got from a traditional typesetter – proofreading, three hyphenations in a row, widows, that kind of stuff,' recalled one production manager. 'Everybody was dabbling in desktop but I think in many ways floundering. They weren't making good books.' … When a publisher paid the old-fashioned compositor $10 or $12 a page, it knew exactly what it was getting, but what exactly was it getting for $3 or $4 a page from the new typesetter?" (p. 406–407). Many publishers and mathematicians still believe in LaTeX as the miracle weapon, which is true if used with care and expertise. But there are also a few critical voices which address the problems related to the demise of classical typesetting craftsmanship; the latest is Michael G. Cowling's essay in the April 2012 issue of the *Notices of the AMS*, p. 559. But even as early as fifteen years ago, Edward Tenner, former executive editor for physical science and history at Princeton University Press, noted: "Even experienced electronic manuscript specialists cannot evaluate a TEX manuscript reliably just by looking at the author's laser-printed version. Messy or nonstandard coding may fail to reproduce the same beautiful output when fed into professional typesetting equipment. Consequently, there are real hidden productivity costs associated with an 'inexpensive' TEX manuscript; it may require open-heart surgery rather than a haircut."[1]

The read of "Books in the Digital Age" makes clear that it is time to seriously reconsider the current academic publishing system. This is primarily a matter for the university administrators and scholars, and I cannot think of a better study than Thompson's book to make the academic publishing industry comprehensible to them.

If you got more interested in "this new age of publishing" I recommend to buy a copy of the author's most recent revelations, *Merchants of Culture* (Polity Press 2010, the second ed. is now available, since April 2012, as a cheap Plume paperback), on "trade publishing", by which is meant "the sector of the publishing industry that is concerned with publishing books, both fiction and non-fiction, that are intended for general readers and sold primarily through bookstores and other retail outlets." But be careful, you need to be tough.

Readers who look for a brief and passionate analysis and who are not afraid of polemic not in line with current academic conformity culture should invest €9,99 and buy a copy of *Enemies of Promise: Publishing, Perishing, and the Eclipse of Scholarship*, Prickly Paradigm Press, Chicago, 2005. This pocket-sized paperback has only 87 pages and is written by Lindsay Waters, Executive Editor for the Humanities at Harvard University Press.

Manfred Karbe
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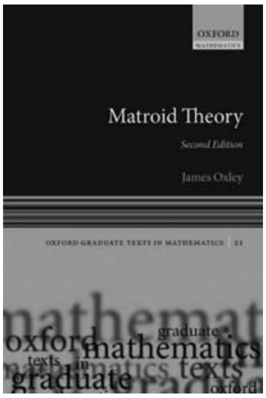
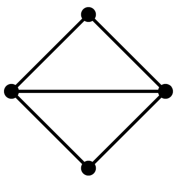


James Oxley
Matroid Theory
Second Edition, Oxford Graduate Texts in Mathematics 21
Oxford University Press 2011
Paperback
£40.0 / 704 pages
ISBN: 978-0-19-960339-8

Reviewer: Emanuele Delucchi

Matroid theory is a sprawling field of combinatorics with unique structural features and far-reaching applications. To convey a first impression of the topic let us consider the similarities between the following objects.



$$\begin{bmatrix} 1 & -1 & 0 & 1 & -1 \\ 1 & 1 & 1 & 1 & 1 \\ 1 & -1 & 0 & -1 & 1 \end{bmatrix}$$

Figure 1. The graph G**, the matrix** A

Both objects consist of 5 'elements' (edges or column-vectors). A closer look shows that there are as many spanning trees in G as there are 'bases' (maximal independent subsets among the columns) of A. An even closer look reveals that a bijection between the set of spanning trees of G and the set of bases of A is induced by any bijection between the edges of G and the columns of A that pairs the 'middle' edge of G to the third column of A and the 'upper' edges with the first two columns. The edges of the planar dual G^* of G are naturally paired with edges of G (Figure 2): under this pairing, a spanning tree of G^* corresponds to the complement of a spanning tree of G.

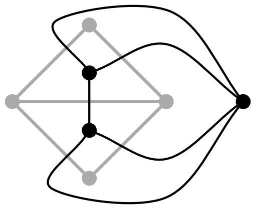


Figure 2. The dual graph G^*

Now consider the set P of planes that have the columns of A as normal vectors (Figure 3). Families of planes containing a common line correspond to cycles of G^*. Moreover, the reader is encouraged to check that P subdivides $\mathbb{R}^3$ into $\chi_G(-1)$ regions, where χ_G is the chromatic polynomial of G (for a solution see the end of the review).

These are not coincidences but shadows of the idea of a matroid on the wall of Plato's cave.

If you are interested in A, you'll say that a (finite) matroid is given by a family of equicardinal subsets of a finite set, satisfying an exchange property inspired by Steinitz' Basis Exchange Theorem. Equivalently, a finite matroid is given by a family of incomparable subsets of a finite set satisfying a characteristic property of cycles in graphs (i.e., that the union of two nondisjoint cycles contains a third cycle); this may be your choice if you are interested in G^* or in the lines of P.

This landscape of different, equivalent definitions is a distinguishing feature of matroid theory, and one that makes the theory powerful. In fact, matroid theory has come to have a wealth of applications in many areas of discrete mathematics and optimization theory – and even beyond, in fields as diverse as the study of Grassmannians or tropical mathematics (see EMS Newsletter No. 83). The usefulness of matroids rests on a solid and lively theory which, of itself, has constituted a fruitful research topic ever since its origins in the 1930s.

The fact that matroid theory has not lost any (and in fact has gained) momentum as a research topic in recent years, coupled with the wide range of applications, makes the task of writing a textbook on the subject particularly challenging. James Oxley, himself a prominent matroid theorist, did not shy from this task and, in 1992, published the first edition of *Matroid Theory*. The book turned out to be a valuable introductory textbook as well as a practical reference work for mathematicians from other fields where matroids are applied. Among the many nice features of the book are the consistently concise yet complete statements and the precise system of internal references, both of which make the book easy to navigate even along paths that do not follow the order of the chapters (the necessity of totally ordering the content of a book being particularly unsuited to matroid theory where – as has been said – the many different, equivalent approaches deserve to be treated as equals).

This review is about the second edition of Oxley's book, which is a major improvement on the first edition.

The whole text has undergone a thorough and detailed revision which has improved many aspects, from the wording of some sentences to the choice of examples and exercises. In particular, the exercises have been thoroughly updated according to the development of the theory: as in the first edition, they are not only numerous but also guide the reader through some important results that are only quickly touched upon in the expository part.

The overall structure of the first seven chapters has been mostly retained. After a preliminary introduction of some background and motivation from linear algebra and graph theory, the first chapter presents some of the most well known axiomatizations of matroids and explains a widely used geo-

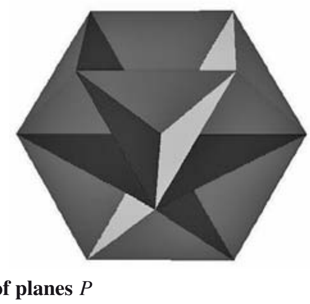


Figure 3. The set of planes P

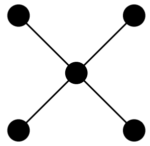


Figure 4. The geometric representation of our example matroid

metric representation of matroids with small rank (according to which our example matroid would be depicted as in Figure 4).

Chapters 2–4 deal with basic structural properties (duality, minors and connectivity), before two important classes of matroids are introduced in Chapters 5 and 6 (about graphical matroids and representable matroids). In Chapter 6, a new section on Dowling matroids, with a view on the more general theory of bias matroids (to which a substantial number of the exercises are devoted), allows the author to state the complete classification of universal models for matroid varieties obtained by Kahn and Kung in 1989. Chapter 7 presents some basic operations between matroids (connections, 2-sums, extensions and quotients) and features a new section about the free product of matroids introduced by Crapo and Schmitt in 2005 in order to study the number of nonisomorphic matroids on a given number of elements.

Starting with Chapter 8, the structure of the book has undergone a major reorganisation showing a shift of focus from the analysis of particular classes of matroids to the description of general theoretical structures. Thus, chapters about ternary matroids and regular matroids have given way to Chapter 10 about excluded-minor theorems, characterizing some matroid classes through 'forbidden submatroids', and to Chapter 13 about Seymour's theorem, a deep structural result with applications in computational matrix theory, which is treated far more extensively than in the first edition and is proved through a previously unpublished argument (Oxley credits much of it to private conversations with Jim Geleen). Chapters 8, 9, 11, 12 about higher connectivity, binary matroids, submodularity and matroid unions and about the 'Splitter theorem' have been kept, albeit in a substantially revised and expanded form. Without going into too much detail let us mention the dramatic expansion of Chapter 8 (on higher connectivity) to include a matroid version of Menger's theorem and Tutte's 'whirls and wheels' theorem characterizing the 'minimal' 3-connected matroids.

Both editions feature a 'window on research'. The first edition's last chapter about unsolved problems has been complemented here by Chapter 14 on research in representability and structure. This reflects Oxley's own research interests and the author himself states in the preface that this has been his criterion in deciding which topics – other than those that, in Oxley's words, "virtually select themselves" – had to be included in the book. To have mathematical topics explained with the words of someone who feels strongly (and positively) about them is helpful and motivating. On the other hand, some of the main gateways between matroid theory and other fields of mathematics get surprisingly little or no mention, a feat that may disorient some potential readers who come to the book from other areas of mathematics seeking to understand how matroid theory relates to their research. Oxley addresses this problem in the beginning of Chapter 15 with a survey of some alternative textbooks and introductory texts. But even there, one does not find any reference of, for example, polynomial invariants of matroids – a topic that is absent from the book except for a passing citation in Section 15.3. As another example, I'll mention my unsuccessful search through the book for any reference to matroid polytopes (even the otherwise very practical and thorough index did not help), an increasingly relevant topic for which, to my knowledge, a comprehensive dedicated introductory text has yet to be published. Of course, it would be a Herculean (if not Sisyphean) task to try to write an all-encompassing textbook on matroids; and so my comment is less a negative point to Oxley's book than it is a call on the community to fill this gap.

All things considered, the improvements in the second edition will ensure that, as matroid theory continues to develop and to broaden the scope of its applications, Oxley's book will remain a valuable companion, both as a reference and as an introductory work, for specialists and neophytes alike.

Solution to the quiz: The chromatic polynomial of G is $\chi_G(t) = t^4 - 5t^3 + 8t^2 - 4t$ and indeed the number of regions is $18 = \chi_G(-1)$.

Emanuele Delucchi [delucchi@math.uni-bremen.de] obtained his PhD in mathematics from ETH Zurich in 2006. He was a postdoctoral researcher at the University of Pisa and at the MSRI in Berkeley. From 2008 to 2010 he has been a visiting assistant professor at SUNY Binghamton and, since then, a senior lecturer at the University of Bremen where he obtained his Habilitation in 2011. His research is in combinatorics and topology, with a special focus on the theory of hyperplane arrangements.

Personal Column

Please send information on mathematical awards and deaths to Mădălina Păcurar [madalina.pacurar@econ.ubbcluj.ro]

Awards

The 2012 **Wolf Prize in Mathematics** has been awarded to **Michael Aschbacher** (California Institute of Technology, USA) and **Luis Caffarelli** (University of Texas, USA).

The **Fermat Prize** 2011 has been awarded jointly to **Manjul Bhargava** (Princeton University, USA) and **Igor Rodnianski** (Massachusetts Institute of Technology, USA).

Lilya Budaghyan (University of Bergen, Norway) has won the 2011 **Emil Artin Junior Prize in Mathematics.**

The **Crafoord Prize in Mathematics** 2012 has been awarded to **Jean Bourgain** (Institute for Advanced Study, Princeton, USA) and **Terence Tao** (University of California, Los Angeles, USA).

The **Prize Ferran Sunyer i Balaguer** 2011 has gone to **Angel Cano** (Universidad Nacional Autónoma de México), **Juan Pablo Navarrete** (Universidad Autónoma de Yucatán) and **José Seade** (Universidad Nacional Autónoma de México) for the book *Complex Kleinian Groups*.

Diego Córdoba (ICMAT, CSIC, Spain) has been awarded the **Prize Miguel Catalán** 2011 by Comunidad de Madrid.

Irit Dinur (Weizmann Institute of Science, Israel) is the recipient of the **Erdős Prize** 2012.

Simon Donaldson (Imperial College, UK) has received a **Knight Bachelor** for his contribution to mathematics.

Ib Madsen (University of Copenhagen, Denmark), **David Preiss** (University of Warwick, UK) and **Kannan Soundararajan** (Stanford University, US) are awarded the 2011 **Ostrowski Prize**.

Philibert Nang (École Normale Supérieure, Laboratoire de Recherche en Mathématiques, Libreville, Gabon) has been awarded the 2011 **Ramanujan Prize** for Young Mathematicians from Developing Countries.

Marc Noy (Universitat Politècnica Catalunya, Spain) has received the **Humboldt Research Award** 2012, awarded by the Alexander von Humboldt Foundation.

Xavier Ros Oton (Universitat Politècnica de Catalunya, Spain) has received the **Prize Évariste Galois** 2011, awarded by the Societat Catalana de Matemàtiques (SCM).

Matt Parker (Queen Mary, University of London) has won the 2011 **Joshua Phillips Award** for Innovation in Science Engagement.

The British Society for the History of Mathematics has awarded the **Neumann Prize** 2011 to the monograph *The Math Book: From Pythagoras to the 57th Dimension* by **Cliff Pickover**.

The **Hans Freudenthal Medal** for 2011 has gone to **Luis Radford** (Université Laurentienne, Canada).

The **Felix Klein Medal** for 2011 has gone to **Alan H. Schoenfeld** (University of California at Berkeley, USA).

The **Abel Prize** for 2012 has been awarded to **Endre Szemerédi** (Alfréd Rényi Institute of Mathematics, Budapest, Hungary, and Department of Computer Science, Rutgers, USA).

David Pardo Zubiaur (Universidad del País Vasco, Spain) has been awarded the 2011 **SEMA Prize** to Young Researchers.

Robin Wilson (Open University, UK) has been elected President of the British Society for the History of Mathematics.

Raymond Flood has been appointed Gresham Professor of Geometry, Gresham College, London, from September 2012, and **Tony Mann** has been appointed Visiting Professor at Gresham College. They will be giving free lectures to the general public, as has been the tradition for the past 400 years.

Ingrid Daubechies (Duke University, US) has been awarded the 2012 **Frederic Esser Nemmers Prize** in Mathematics “for her numerous and lasting contributions to applied and computational analysis and for the remarkable impact herwork has had across engineering and the sciences.”

Deaths

We regret to announce the deaths of:

Julius Albrecht (16 February 2012, Germany)
José Real Anguas (27 January 2012, Spain)
Johannes André (15 August 2011, Germany)
Philip Batchelor (30 August 2011, UK)
Helmut Brass (30 October 2011, Germany)
Marco Brunella (24 January 2012, France)
Nicolaas Govert de Bruijn (17 February 2012, Netherlands)
Hans-Georg Carstens (28 January 2012, Germany)
Bogdan Choczewski (18 September 2011, Poland)
Ludwig Danzer (3 December 2011, Germany)
John Derrick (8 December 2011, UK)
Torsten Ekedahl (23 November 2011, Sweden)
Nácere Hayek Calil (17 April 2012, Spain)
John Howie (26 December 2011, UK)
Eleanor James (15 June 2011, UK)
Dominic Jordan (23 April 2012, UK)
Marvin Knopp (24 December 2011, UK)
Michał Krynicki (12 October 2011, Poland)
Heinz Kunle (5 January 2012, Germany)
Daniel Leborgne (16 February 2012, France)
Alexander Yu Loskutov (5 November 2011, Russia)
Gyula Maurer (8 January 2012, Hungary)
Helmut Mäurer (4 February 2012, Germany)
Viorel Radu (22 January 2011, Romania)
Hans-Jörg Reiffen (29 February 2012, Germany)
Helmut Rüssmann (11 April 2011, Germany)
Nimish Shah (16 November 2011, UK)
Jean-Marie Souriau (15 March 2012, France)
Tonny Springer (7 December 2012, UK)
Erik Thomas (13 September 2011, Netherlands)
Horst Tietz (28 January 2012, Germany)
Andrey Todorov (30 March 2012, Bulgaria)
Boris Borisovich Venkov (10 November 2011, Russia)
Vladimir Zakalyukin (30 November 2011, Russia)

6th European Congress of Mathematics

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Adrian Constantin
UNIVERSITÄT WIEN, AUSTRIA

Camillo De Lellis
UNIVERSITÄT ZÜRICH, SWITZERLAND

Herbert Edelsbrunner
INSTITUTE OF SCIENCE AND TECHNOLOGY AUSTRIA

Mikhail L. Gromov
INSTITUT DES HAUTES ÉTUDES SCIENTIFIQUES, FRANCE

Christopher Hacon
UNIVERSITY OF UTAH, USA

David Kazhdan
THE HEBREW UNIVERSITY OF JERUSALEM, ISRAEL

Tomasz Łuczak
ADAM MICKIEWICZ UNIVERSITY, POZNAŃ, POLAND

Sylvia Serfaty
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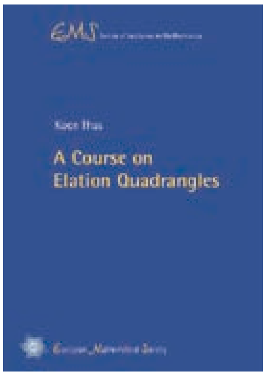
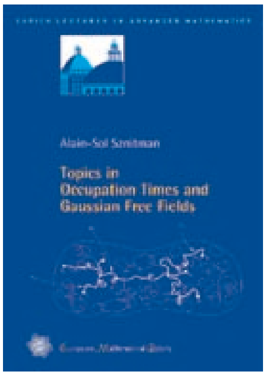


Koen Thas (Ghent University, Belgium)
A Course on Elation Quadrangles (EMS Series of Lectures in Mathematics)

ISBN 978-3-03719-110-1. 2012. 129 pages. Softcover. 17 x 24 cm. 28.00 Euro

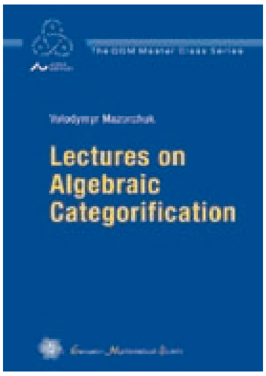
The notion of elation generalized quadrangle is a natural generalization to the theory of generalized quadrangles of the important notion of translation planes in the theory of projective planes. Almost any known class of finite generalized quadrangles can be constructed from a suitable class of elation quadrangles.
In this book the author considers several aspects of the theory of elation generalized quadrangles. Special attention is given to local Moufang conditions on the foundational level, exploring for instance a question of Knarr from the 1990s concerning the very notion of elation quadrangles. All the known results on Kantor's prime power conjecture for finite elation quadrangles are gathered, some of them published here for the first time. The structural theory of elation quadrangles and their groups is heavily emphasized. Other related topics, such as *p*-modular cohomology, Heisenberg groups and existence problems for certain translation nets, are briefly touched.
The text starts from scratch and is essentially self-contained. Many alternative proofs are given for known theorems. Containing dozens of exercises at various levels, from very easy to rather difficult, this course will stimulate undergraduate and graduate students to enter the fascinating and rich world of elation quadrangles. The more accomplished mathematician will especially find the final chapters challenging.



Alain-Sol Sznitman (ETH Zürich, Switzerland)
Topics in Occupation Times and Gaussian Free Fields (Zurich Lectures in Advanced Mathematics)

978-3-03719-109-5. 2012. 122 pages. Softcover. 17 x 24 cm. 28.00 Euro

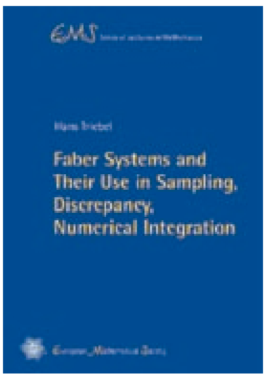
This book grew out of a graduate course at ETH Zurich during the Spring term 2011. It explores various links between such notions as occupation times of Markov chains, Gaussian free fields, Poisson point processes of Markovian loops, and random interlacements, which have been the object of intensive research over the last few years. These notions are developed in the convenient set-up of finite weighted graphs endowed with killing measures.
The book first discusses elements of continuous-time Markov chains, Dirichlet forms, potential theory, together with some consequences for Gaussian free fields. Next, isomorphism theorems and generalized Ray-Knight theorems, which relate occupation times of Markov chains to Gaussian free fields, are presented. Markovian loops are constructed and some of their key properties derived. The field of occupation times of Poisson point processes of Markovian loops is investigated. Of special interest are its connection to the Gaussian free field, and a formula of Symanzik. Finally, links between random interlacements and Markovian loops are discussed, and some further connections with Gaussian free fields are mentioned.



Volodymyr Mazorchuk (Uppsala University, Sweden)
Lectures on Algebraic Categorification (The QGM Master Class Series)

ISBN 978-3-03719-108-8. 2012. 110 pages. Softcover. 17 x 24 cm. 28.00 Euro

The term "categorification" was introduced by Louis Crane in 1995 and refers to the process of replacing set-theoretic notions by the corresponding category-theoretic analogues. This text mostly concentrates on algebraical aspects of the theory, presented in the historical perspective, but also contains several topological applications, in particular, an algebraic (or, more precisely, representation-theoretical) approach to categorification. It consists of fifteen sections corresponding to fifteen one-hour lectures given during a Master Class at Aarhus University, Denmark in October 2010. There are some exercises collected at the end of the text and a rather extensive list of references. Video recordings of all (but one) lectures are available from the Master Class website.
The book provides an introductory overview of the subject rather than a fully detailed monograph. Emphasis is on definitions, examples and formulations of the results. Most proofs are either briefly outlined or omitted. However, complete proofs can be found by tracking references. It is assumed that the reader is familiar with the basics of category theory, representation theory, topology and Lie algebra.



Hans Triebel (University of Jena, Germany)
Faber Systems and Their Use in Sampling, Discrepancy, Numerical Integration (EMS Series of Lectures in Mathematics)

ISBN 978-3-03719-107-1. 2012. 115 pages. Softcover. 17 x 24 cm. 28.00 Euro

This book deals first with Haar bases, Faber bases and Faber frames for weighted function spaces on the real line and the plane. It extends results in the author's book *Bases in Function Spaces, Sampling, Discrepancy, Numerical Integration* (EMS, 2010) from unweighted spaces (preferably in cubes) to weighted spaces.
The obtained assertions are used to study sampling and numerical integration in weighted spaces on the real line and weighted spaces with dominating mixed smoothness in the plane. A short chapter deals with the discrepancy for spaces on intervals.
The book is addressed to graduate students and mathematicians having a working knowledge of basic elements of function spaces and approximation theory.

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