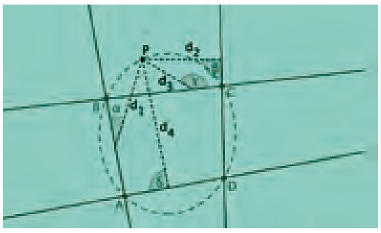
NEWSLETTER

OF THE EUROPEAN MATHEMATICAL SOCIETY



December 2011
Issue 82
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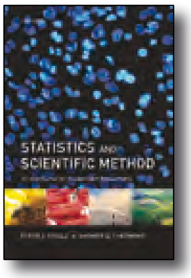


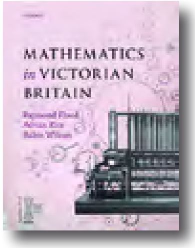
Statistics and Scientific Method

An Introduction for Students and Researchers

Peter J. Diggle and Amanda G. Chetwynd

An antidote to technique-orientated approaches, this text avoids the recipe-book style, giving the reader a clear understanding of how core statistical ideas of experimental design, modelling, and data analysis are integral to the scientific method. No prior knowledge of statistics is required and a range of scientific disciplines are covered.

August 2011 | 192 pages
Paperback | 978-0-19-954319-9 | EMS member price: **~~£19.95~~ £15.96**
Hardback | 978-0-19-954318-2 | EMS member price: **~~£50.00~~ £40.00**



Mathematics in Victorian Britain

Edited by Raymond Flood, Adrian Rice, and Robin Wilson

With a foreword by Adam Hart-Davis, this book constitutes perhaps the first general survey of the mathematics of the Victorian period. It charts the institutional development of mathematics as a profession, as well as exploring the numerous innovations made during this time, many of which are still familiar today.

September 2011 | 480 pages
Hardback | 978-0-19-960139-4 | EMS member price: **£29.99 £23.99**

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Edited by Gernot Akemann, Jinho Baik, and Philippe Di Francesco

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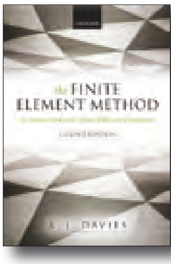
Oxford Handbooks in Mathematics

July 2011 | 952 pages
Hardback | 978-0-19-957400-1 | EMS member price: **~~£110.00~~ £88.00**

NEW EDITION



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Second Edition

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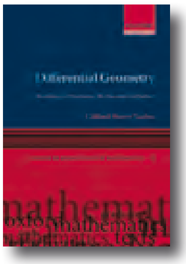
September 2011 | 312 pages
Paperback | 978-0-19-960913-0 | EMS member price: **~~£29.99~~ £23.99**



Differential Geometry

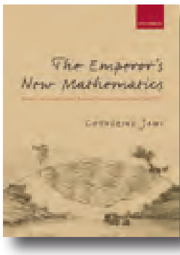
Bundles, Connections, Metrics and Curvature

Clifford Henry Taubes

Bundles, connections, metrics & curvature are the lingua franca of modern differential geometry & theoretical physics. Supplying graduate students in mathematics or theoretical physics with the fundamentals of these objects, & providing numerous examples, the book would suit a one-semester course on the subject of bundles & the associated geometry.

Oxford Graduate Texts in Mathematics No. 23

October 2011 | 312 pages
Paperback | 978-0-19-960587-3 | EMS member price: **~~£27.50~~ £22.00**
Hardback | 978-0-19-960588-0 | EMS member price: **~~£55.00~~ £44.00**



The Emperor's New Mathematics

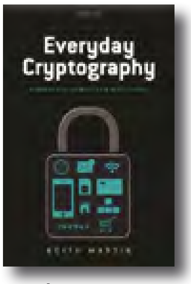
Western Learning and Imperial Authority During the Kangxi Reign (1662-1722)

Catherine Jami

Jami explores how the emperor Kangxi solidified the Qing dynasty in seventeenth-century China through the appropriation of the 'Western learning', and especially the mathematics, of Jesuit missionaries. This book details not only the history of mathematical ideas, but also their political and cultural impact.

December 2011 | 452 pages
Hardback | 978-0-19-960140-0 | **~~£60.00~~ £48.00**

FORTHCOMING



Everyday Cryptography

Fundamental Principles and Applications

Keith M. Martin

A self-contained and widely accessible text, with almost no prior knowledge of mathematics required, this book presents a comprehensive introduction to the role that cryptography plays in providing information security for technologies such as the Internet, mobile phones, payment cards, and wireless local area networks.

March 2012 | 592 pages
Paperback | 978-0-19-969559-1 | EMS member price: **~~£29.99~~ £23.99**

FORTHCOMING

Computability and Randomness

André Nies

The book covers topics such as lowness and highness properties, Kolmogorov complexity, betting strategies and higher computability. Both the basics and recent research results are desribed, providing a very readable introduction to the exciting interface of computability and randomness for graduates and researchers in computability theory, theoretical computer science, and measure theory.

Oxford Logic Guides No. 51

January 2012 | 456 pages
Paperback | 978-0-19-965260-0 | EMS member price: **~~£25.00~~ £20.00**

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EMS Agenda

2012

17–19 February
Executive Committee Meeting, Slovenia
Stephen Huggett: s.huggett@plymouth.ac.uk

23–24 March
Meeting of ERCOM, Budapest, Hungary

31 March–1 April
Meeting of presidents of EMS member mathematical societies, Prague, Czech Republic
Stephen Huggett: s.huggett@plymouth.ac.uk

14 April
Meeting of the EMS Committee for Developing Countries, Limoges, France
Tsou Sheung Tsun: tsou@maths.ox.ac.uk

29–30 June
Executive Committee Meeting, Kraków
Stephen Huggett: s.huggett@plymouth.ac.uk

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

2–7 July
6th European Mathematical Congress, Kraków, Poland
www.euro-math-soc.eu

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

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

Editorial

Vicente Muñoz (Madrid, Spain) and
Martin Raussen (Aalborg, Denmark)

Book reviews on www.euro-math-soc.eu

We invite you to become a regular user of the European Mathematical Society's website www.euro-math-soc.eu. This web portal contains information about the life and history of the society and its committees but much more than that. Among other items, you will find a collection of interesting news viewed from a European mathematical perspective, a calendar of mathematical conferences and workshops and a list with descriptions of jobs for mathematicians that are currently available in academia, administration and industry.

These facilities have the potential to become all the more dynamic if you upload your own pieces of news, your own conference announcements and your own job advertisements. This can easily be done using a web form. In order to avoid spam, all incoming material will be moderated. Normally, relevant information is visible on the website after a very short delay.

We would like this opportunity to advertise a new feature on the EMS website. Do you remember the recent books section that appeared in the EMS Newsletter until the end of 2009? For many years, colleagues from the Czech Republic had been writing short reviews for all books sent to them and we owe them our thanks! Many publishers used this channel to make their products known to a wider audience. Readers of the newsletter were able to obtain brief information on new books, ranging from general audience textbooks to highly specialised research monographs.

This section ceased to appear in the newsletter but the information will not now be lost! The Chairman of the RPA committee Ehrhard Behrends (FU Berlin) has formed a team of students from his university that uploads these reviews in a structured form to a database that the society maintains at the Helsinki headquarters. Many of these "old" reviews are already visible and searchable on *http://www.euro-math-soc.eu/bookreviewssearch.html*.

You can search this facility if you know the author or the title or want to search books within a main MSC classification or by using a search phrase of your own. If you want to know about books related to, say, K-theory, choose the option <All> under MSC main category and type K-theory as your search string; you should receive a list with more than ten books as a result. Not all work is finished yet; some book reviews are still missing and not all of them have the correct MSC classification. But this is work in progress.

It would be awkward to not have book reviews for books available after 2009. This is why a new group of reviewers has been formed, this time in Spain. Publishers like Springer and the AMS are sending their books on mathematical topics to Universidad Complutense de Madrid. Several faculty members and post-graduate students volunteer to read them and write informative and detailed reviews, which are then uploaded to the database.

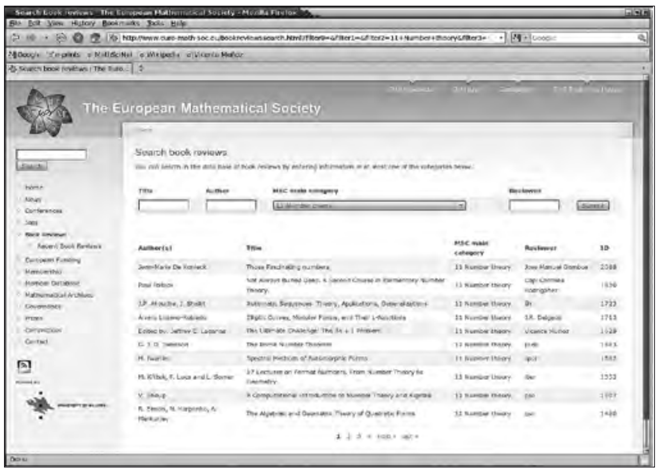
If you are an editor and want to see your books show up on the EMS webpage or if you are an author and want your own book reviewed there, just send one complimentary copy to:

Vicente Muñoz
Facultad de Matemáticas
Universidad Complutense de Madrid
Plaza de Ciencias 3
28040, Madrid, Spain

We take this opportunity also to invite people at other institutions to participate in this reviewing process. The possibility of forming another team of reviewers at another institution will mean an increase in the number of books that can be reviewed. If you are interested in such an idea, please contact Martin Raussen [*raussen@math.aau.dk*] or Vicente Muñoz [*vicente.munoz@mat.ucm.es*].

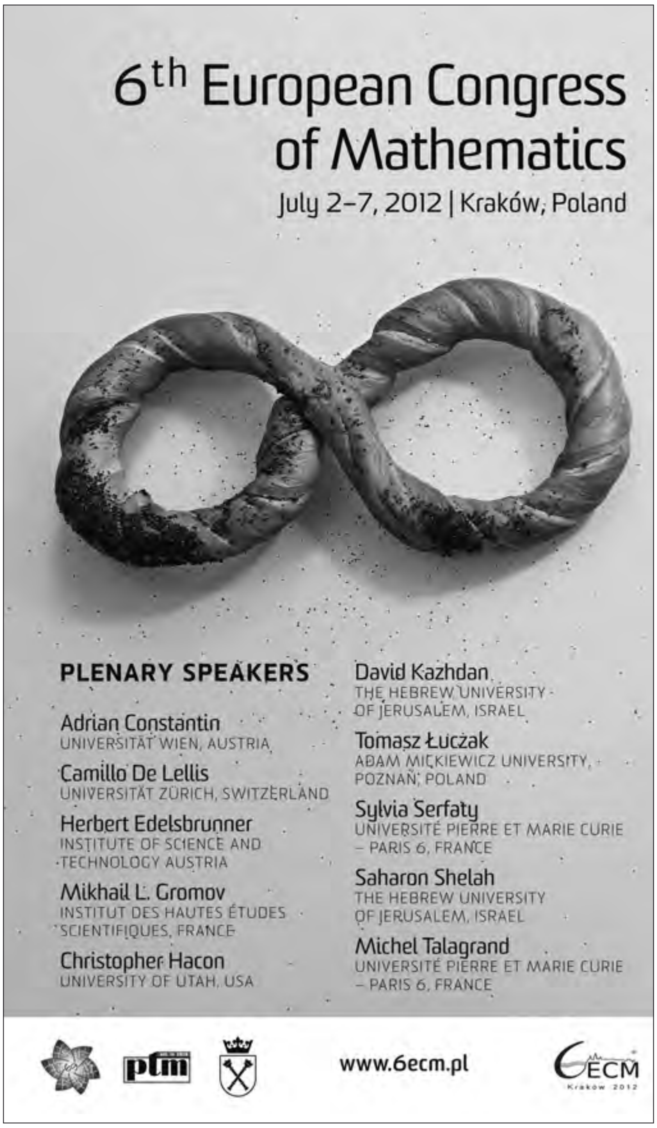
What is the added value of this database compared to the usual reviews in *Mathematical Reviews* or *Zentralblatt*? The reviews on the EMS webpage allow for discussion. Readers can add comments to all new book reviews and comments can be commented upon as well. This may be useful to get an idea of what the reader can expect from a book. Is it too difficult for a graduate course? Does it give interesting new insights? How does it compare to another book in the same area? Are there many disturbing typos? Comments can be positive or negative but they should always be written with respect; this is why some moderation will be necessary. Even the book's author can reply to them. We encourage all EMS members and other users of the website to contribute to this project.

This idea has just started but it is already developing well. There are already more than 800 reviews visible on *http://www.euro-math-soc.eu/bookreviewssearch.html*. Like a snowball, this initiative will hopefully grow exponentially with time.



The Registration for the 6th European Congress of Mathematics (6ECM), Kraków, 2–7 July 2012, is Opened

Krystyna Jaworska (Secretary of the Polish Mathematical Society)



Poster of the 6ECM. © Polish Mathematical Society.

The registration for 6ECM began in November 2011. The European Mathematical Society (EMS), the Polish Mathematical Society (PTM) and the Jagiellonian University (UJ), the organisers of 6ECM, cordially welcome mathematicians from all over Europe and elsewhere to participate in this important meeting.

The preparations for 6ECM are in full swing. The list of 10 plenary speakers (published in the EMS Newsletter 81) and 34 invited speakers (see the list in the frame) is complete. The proposals for mini-symposia and candidates for prizes are under consideration. There are 14 registered satellite conferences, organised in the Czech Republic, Estonia, Finland, Germany, Hungary, Poland and Romania. The Polish organisers have undertaken a broad range of publicity with a view to spreading information about 6ECM. The congress poster has been sent out to several hundred scientific institutions and national societies in Europe and updated information is presented on the website www.6ecm.pl.

The electronic registration for 6ECM is performed through the specially prepared electronic conference services and payments system, which will also serve some satellite conferences. It can be accessed from the 6ECM website www.6ecm.pl or directly at the address pay.ptm.org.pl, proceeding to the website 6ecm.ptm.org.pl. Each prospective participant who registers for 6ECM is asked to create their own personal account by providing personal and contact data as well as their field of mathematical interest. The participant is then able to log in as many times as they need. After logging in they can: view and add account details, pay conference fees, download printable receipts, apply for financial support, submit a research poster and indicate their sightseeing interests. Moreover, the electronic system confirms via email completion of registration and payment (it also sends the receipts) and applications for financial support.

The registration fees approved by the EMS Executive Council are as follows:

6ECM fees in Polish Zloty (PLN)		
Fee	Early registration (until 31 March 2012)	Late registration (after 31 March 2012)
Conference fee	1050 PLN	1250 PLN
EMS/PTM member conference fee	900 PLN	1050 PLN
Student conference fee	600 PLN	650 PLN
Accompanying person fee	600 PLN	600 PLN

The conference fee includes attendance of the 6ECM activities, congress materials, beverages and cookies during the breaks, welcome reception, conference banquet and guided sightseeing in Kraków. The accompanying person fee includes the social programme of the congress and assistance in arranging an individual sightseeing and cultural programme. Reduced fees apply to those individual members of the EMS and members of the PTM who paid their dues for 2011. The student fee applies to those who are enrolled on a graduate (Master's or doctoral) programme in mathematics or a related field.

In order to ensure broad participation in 6ECM and to reduce economic barriers, a certain number of grants from the Foundation for Polish Science and the EMS will be offered to support the participation of young mathematicians and senior mathematicians from eligible countries. The grants may cover the conference fee, accommodation in Kraków and living expenses (per diem).

Applications for financial support have to be submitted by mid-February 2012 by filling out a form on the personal 6ECM account. Applicants are asked to provide references to their publications and conference talks as well as a name of a senior researcher affiliated in Europe who could provide a letter of recommendation. Decisions will be taken in March 2012 by a committee nominated by the EMS, the PTM and the Jagiellonian University.

All registration fees are payable to the Polish Mathematical Society (only after logging into a personal account) by the following payment methods:

- Online payment using a credit/debit card. Accepted card types are: Visa, MasterCard, JCB, Diners Club and Maestro.
- Offline payment by bank transfer in Polish Zloty (PLN) to the PTM account – after logging in to a personal account all necessary data will be available to perform this transfer.

Immediately after receiving the payment, the PTM will issue an electronic receipt which will be sent by email and can also be downloaded from the 6ECM personal account.

Note that the exchange rate was about 4 PLN for € 1 as of October 2011. The graph below shows how the exchange rates have been fluctuating in September and October 2011.

On the registration portal, prospective participants are also asked about their sightseeing interests. It is important for the organisers to know in advance how many people will choose the various excursions which will be offered at extra charge. The excursions include scenic cruises along the Vistula River, visits to the magnificent medieval salt mine in Wieliczka and the silver mine in Tarnowskie Góry and a hike in the Tatra Mountains. There will also be the possibility of visiting the Museum of the Auschwitz-Birkenau German Nazi Concentration and Extermination Camp (1940–1945), located not far from Kraków.

Dear Colleagues and Mathematicians, register now for 6ECM and come to Poland!

See you in Kraków!

The list of invited speakers

Anton Alekseev
Kari Astala
Jean Bertoin
Serge Cantat
Vicent Caselles
Alessandra Celletti
Pierre Colmez
Alessio Corti
Amadeu Delshams
Hélène Esnault
Alexandr A. Gaifullin
Isabelle Gallagher
Olle Häggström
Martin Hairer
Nicholas J. Higham
Arieh Iserles
Alexander S. Kechris
Bernhard Keller
Sławomir Kołodziej
Gady Kozma
Frank Merle
Andrey E. Mironov
David Nualart
Alexander Olevskii
Hans G. Othmer
Leonid Parnovski
Florian Pop
Igor Rodnianski
Zeev Rudnick
Benjamin Schlein
Piotr Śniady
Andrew Stuart
Vladimír Sveřák
Stevo Todorčević

EMS-RSME Joint Mathematical Weekend in Bilbao

G. A. Fernández-Alcober, L. Martínez and J. Sangroniz (University of the Basque Country)

Purpose and venue

The European Mathematical Society (EMS) and the Royal Spanish Mathematical Society (Real Sociedad Matemática Española, RSME) organised a joint Mathematical Weekend in Bilbao, 7–9 October, in the autonomous community of the Basque Country in Spain. This conference was intended as one of the highlights in the wide programme of activities arranged by the RSME to commemorate its centennial in 2011. The meeting in Bilbao makes the fifth in the series of Mathematical Weekends of the EMS, after those in Lisbon in 2003, Prague in 2004, Barcelona in 2005, Nantes in 2006 and Copenhagen in 2008. The Mathematical Weekends have always been organised jointly with the mathematical society of the host country.

Participants of the EMS-RSME Joint Mathematical Weekend

The venue of the event was the Bizkaia Aretoa, the new assembly hall and conference centre of the University of the Basque Country, an elegant, L-shaped structure designed by the Pritzker Prize-winning Portuguese architect Álvaro Siza. The Bizkaia Aretoa is situated in the very centre of Bilbao, in the trendy area of Abandoibarra. A dark industrial zone only 20 years ago, Abandoibarra has been transformed into the most attractive part of the city, full of green and encompassing a number of distinctive buildings, among them the world renowned Guggenheim Museum, already an icon of Bilbao.

Structure of the meeting

The RSME designated an organising committee and a programme committee for the Mathematical Weekend. The organising committee was composed of the three signatories of this note, all of them belonging to the University of the Basque Country. The members of the programme committee were Rui Loja Fernandes (Technical University of Lisbon), Alexander Moretó Quintana (University of Valencia), Silvie Paycha (Blaise Pascal University), Joaquim Ortega Cerdá (University of Barcelona) and Luis Vega González (Chair, University of the Basque Country).

More than 120 people attended the meeting, with a significant contribution of Spanish mathematicians and also an important number of people from all over Europe. Among the participants, we were happy to have both presidents of the organising societies: President of the RSME Antonio Campillo and President of the EMS Marta Sanz-Solé.

Following the format of previous editions, the meeting lasted three days, from Friday afternoon to mid-Sunday. It was organised in four special sessions, corresponding to the following fields: Groups and Representations (S1), Symplectic Geometry (S2), Partial Differential Equations in Mechanics and Physics (S3) and Functional Analysis Methods in Quantum Information (S4). The choice of these topics was motivated, on the one hand, because they have been fields of high activity in recent times and, on the other hand, because they are very close to the interests of the research groups at the host university, the University of the Basque Country.

Opening ceremony

Each of the four sessions had a main speaker, who gave a one-hour plenary lecture in the Mitxelena Auditorium of the Bizkaia Aretoa. The main speakers were Dan Segal (Oxford University) for S1, Miguel Abreu (Technical University of Lisbon) for S2, María Jesús Esteban (University of Paris Dauphine) for S3 and David Pérez (The Complutense University of Madrid) for S4. Every session had six more invited speakers (seven in the case of S4), who gave 45 minute parallel talks. The invited speakers were chosen by a special commission for each of the sessions, formed by the corresponding main speaker together with Gabriel Navarro (University of Valencia) in S1, Marisa Fernández (University of the Basque Country) in S2 and Jesús Bastero (University of Zaragoza) in S4. They did a wonderful job and selected a team of a very high scientific level. It was remarkable that a significant number of them were still under 40, a sign that the fields of the meeting are very much alive and have emerging figures bringing new blood to the subject.

Besides the session talks, there was a plenary talk delivered by Gabriel Navarro, of the University of Valencia, who was appointed EMS Distinguished Speaker. We elaborate on this below.

In order to encourage the participation of young researchers, a poster session was organised, open to all participants. A total of 18 posters were presented. There was also the opportunity to complement each poster with a short 10 minute talk on Saturday morning. This was a successful initiative, since 13 of the people presenting a poster decided to give a short talk.

EMS Distinguished Speaker

An important new feature of this Mathematical Weekend was the so-called EMS Distinguished Speaker. The appointment went to Gabriel Navarro, Professor of Algebra at the University of Valencia, a leading expert in character theory and representation theory of finite groups.

Professor Navarro delighted the audience with his talk 'Main problems in the representation theory of finite groups', in which he gave an account of some of the deepest and most interesting open problems in that field, such as the McKay conjecture and the Alperin Weight Conjecture. He pointed out recent progress, in which he has actively participated and which allows one to reduce the solution of some of these conjectures to finite simple groups. Having in mind a public with very different mathematical backgrounds, Professor Navarro succeeded in making his exposition accessible to the non-experts, starting from simple concepts in group theory and moving forward step by step until he reached the point where the main problems of the theory could be formulated, sometimes in partial form in order to avoid an excess of technicalities.

It is the general feeling that the appointment of an EMS Distinguished Speaker has been an excellent idea, which might be interesting to extend to forthcoming Mathematical Weekends.

Social programme

The scientific programme of the meeting was complemented with a social programme on Saturday evening that allowed the participants to enjoy some of the attractions of the city of Bilbao. After the last talk of Saturday was over, the participants met at the stairs of the Guggenheim Museum for the group picture. Afterwards, they had the opportunity of visiting the museum in small groups with a guide that explained to them about both the building itself, designed by Frank Gehry, and the current exhibition, including works of Richard Serra, who is one of the most significant contemporary sculptors.

At 21:30 we gathered for the social dinner at the Alhóndiga building, formerly a wine warehouse and now a modern cultural and sports centre at the heart of Bilbao.

Distinguished speaker Gabriel Navarro (Universidad de Valencia)

Let us only mention that, at the time that the conference was being held, the Chess Masters Final (the chess competition of the highest level in the world nowadays) was being played in the Alhóndiga. In the Yandiola Restaurant, on the second floor of the building, the participants relaxed after a hard day over a dinner that lived up to the reputation of Basque cuisine.

Conclusions and further information

This event was possible and successful because of the initiative of the European Mathematical Society and the Royal Spanish Mathematical Society and thanks to the effort of all the people involved in it, from the local organisers to the scientific committees, and from the speakers and participants to the doctoral students who helped with so many practical things. Special thanks should go to the institutions that have given financial support to the meeting: the Basque Government, the i-Math Consolider project of the Spanish Ministry of Science and Innovation, the University of the Basque Country and the European Mathematical Society itself. We also thank the Guggenheim Bilbao Museum for offering free entrance to the museum to all participants.

It is our hope that the Mathematical Weekend in Bilbao will soon be followed by the announcement of a next Weekend in 2012, thus recovering the annual periodicity with which the Mathematical Weekends were born. We look forward to it and wish the best of luck to the organising committee of the next Weekend.

All information regarding the EMS-RSME Joint Mathematical Weekend, including the presentations of most of the speakers, can be found at the conference webpage *www.ehu.es/emsweekend*. If you need to contact the organisers, you can do so by sending a message to *emsweekendbilbao@gmail.com*, the email address of the meeting.

Organising Committee:
Gustavo A. Fernández-Alcober
Luis Martínez
Josu Sangroniz

In 2009, the editors and publisher of Zentralblatt decided to discontinue its traditional print service of 25 volumes per year totalling more than 15,000 pages. Instead, *Excerpts from Zentralblatt MATH* was released. *Excerpts* is published monthly; each issue carries about 240 reviews on 150 pages.

Hence, only a carefully selected choice of reviews appears in print. Basically, all book reviews from the database are presented plus reviews of journal articles with more than a narrow interest for the mathematical community. This selection, made by the editors and the scientific staff at Zentralblatt, is meant to appeal to a wide audience of mathematicians; special emphasis is put on choosing items describing interesting mathematics in informative reviews, ranging from work by Fields medallists to survey articles and brief notes with new and simple proofs of well-known results.

Each issue of *Excerpts* starts with a "Looking Back" section that contains a specially commissioned review of a piece of the mathematical literature of enduring interest. Here one may find new reviews of all-time classics like F. Hausdorff's *Grundzüge der Mengenlehre* (reviewed by O. Deiser), of papers whose relevance was overlooked at the time of their publication, like J.W. Cooley and J.W. Tukey's *An algorithm for the machine calculation of complex Fourier series* (reviewed by D. Braess) or of epoch-making books or articles like J. Lindenstrauss and A. Pełczyński's *Absolutely summing operators in $\mathcal{L}_p$-spaces and their applications*" (reviewed by A. Pietsch).

From 2012 on, members of the EMS are entitled to subscribe to *Excerpts* at a personal rate of 59€, which corresponds to a discount of almost 90% of the list price for institutions. As the Deputy Editor-in-Chief of Zentralblatt MATH, I would like to invite you to take advantage of this offer. I hope you will enjoy reading the *Excerpts from Zentralblatt MATH*!

Dirk Werner

25 Year Anniversary of European Women in Mathematics

Lisbeth Fajstrup

The 15th general meeting of European Women in Mathematics (EWM) took place at CRM, Barcelona, 5–9 September 2011.

EWM began as an idea at the ICM in Berkeley 1986, when the Association of Women in Mathematics (AWM) had organised a panel discussion on "Women in mathematics, 8 years later – an international perspective".[1] The panel included four women based in Europe: Bodil Branner (Denmark), Marie-Françoise Roy (France), Gudrun Kalmbach (Germany) and Caroline Series (England). They decided to meet in Paris in December, where more people joined, and the EWM was born. The next meeting took place in Copenhagen in 1987; even though the legal foundation of the organisation was not in place until a meeting in Warsaw in 1993, the basic structures were decided at the meeting in Copenhagen.

Meeting every year turned out to be too much, both to organise and to participate in, so the schedule since 1991 has been to have biannual general meetings.

The meeting in Barcelona was attended by more than 80 participants from 18 different countries, including Mexico and Burkina Faso (the EWM has never restricted its activities to Europeans). Among them were Bodil Branner, Marie-Françoise Roy and Caroline Series, who were at the panel in Berkeley in '86. There was a reception with a celebration of the anniversary where Bodil Branner and Caroline Series gave a short talk on the history of EWM and several people had brought material from the 25 years – photos, proceedings, newsletters – which were displayed during the conference for everyone to enjoy.

The 2011 EMS lecturer Karen Vogtmann gave three talks with a joint title "The topology and geometry of automorphism groups of free groups"; the individual talks were focused on geometry, topology and algebra, respectively, giving different perspectives on the area. Both Vogtmann and the six other main speakers (see text

Some of the participants in the EWM meeting in Barcelona.

[1] The eight years refer to the last such panel at the ICM in Helsinki. There were no women speakers at that meeting and this became the focus of the AWM Helsinki meeting.

box) managed the difficult task of speaking about specialised mathematics to a general audience, leaving everyone with a better understanding of areas that they may not have seen since entering PhD studies. To further this, the "planted idiot", a concept invented at the first EWM meetings, was reintroduced. At each general lecture, someone in the audience, the "idiot", was responsible for asking questions during the talk if something was unclear, if she thought a definition might not be generally known or if she plainly had not understood herself. This tends to help give an atmosphere where asking questions is easier for everyone.

For those of us from countries with very few female mathematicians, being among so many women all excited about mathematics and to see so many women giving mathematical talks is a very uplifting experience. Probably it is not something anyone, including our female students, would claim to miss in their day-to-day work. Even so, supervisors of female PhD-students should consider encouraging them to go to such a general meeting, even if it is not a specialised conference within the field she is working in. The general talks provide the opportunity of getting to know a broader field of the mathematical landscape and there is plenty of opportunity for networking.

Submitted talks were given in parallel sessions. In addition, Gina Rippon, a professor of cognitive neuroimaging at Aston University, gave a general lecture on 'Sex, Maths and the Brain'.

At the general assembly, which is held at the biannual general meeting, new members of the Standing Committee were elected and activities from the previous two years were reported. Marie-Françoise Roy was elected convenor for the next two years. The convenor, together with the standing committee, is responsible for the organisation in the years between meetings, in particular arranging the next general meeting.

The present EWM supports and organises different activities primarily aimed at women but open to male participants as well: workshops, panels, summer schools, etc. It co-organised the ICWM (International Conference of Women Mathematicians) in Hyderabad and will help in organising the second ICWM before the ICM in Korea. Before the EMS meeting in Krakow, there will be a one day EWM-workshop.

These activities are often co-organised with the EMS Women in Mathematics Committee, (WiM). The WiM was set up by the EMS to address issues relating to the involvement, retention and progression of women in mathematics. The WiM will be organising a panel discussion 'Redressing the gender balance in mathematics: strategies and outcomes' to take place during the EMS Congress in Krakow and is collaborating with EWM on the one-day meeting for women mathematicians in Krakow prior to the congress.

In the anniversary year, there has also been a summer school at the Lorenz Institute in Leiden, the Netherlands, for PhD students. Not only was this *for* PhD students; the organising was also done by a group of very young and very efficient people, who had decided at the previous summer school that they would have such an event in the Netherlands and that they would arrange it themselves. This group: Dion Coumans (Nijmegen, the Netherlands), Andrea Hofmann (Oslo, Norway), Janne Kool (Utrecht, the Netherlands) and Erwin Torreao Dassen (Leiden, the Netherlands), used their experience from the previous summer school with great success. There were three main topics: logic, geometry and history of mathematics, with three speakers for each. In each of these topics there were problem sessions which were aimed at both newcomers and students who already had a background in the field. The aim was to get the participants actively involved and to foster scientific discussion, and this worked very well. Moreover, there were "present your work" sessions in smaller groups. The presentations were done in groups of 4-5 PhD students and a senior mathematician and gave the participants the opportunity to meet each other scientifically while giving and getting advice. Two very lively discussions related to gender and mathematics took place, each following a talk. One was on practices in recruitment of full professors and the under-representation of women in mathematics in the Netherlands. The other was on girls not choosing STEM (Science, Technology, Engineering and Mathematics) subjects with a focus on Norwegian girls but with data also from, for example, Denmark, Italy and the UK.

The next summer school is planned for 2013 at ICTP (Trieste) in collaboration with the Women in Mathematics Committee of the African Mathematical Union

Lisbeth Fajstrup [fajstrup@math.aau.dk] is an associate professor at the University of Aalborg, Denmark, and deputy convenor of the EWM. Her research area is in directed topology with an eye to applications in computer science. Lisbeth tends to get herself involved in dissemination projects and has written more than 200 entries on the Danish Numb3rs blog.

Speakers at the EWM general meeting in Barcelona

Karen Vogtmann (Cornell University; 2011 EMS lecturer), *The topology and geometry of automorphism groups of free groups I, II, III*

Pilar Bayer (Universitat de Barcelona) *Shimura curves as moduli spaces for fake elliptic curves*

Annette Huber-Klawitter (Freiburg Universität), *Period numbers*

Laure Saint-Raymond (Université de Paris VI), *The sixth problem of Hilbert – a century later*

Caroline Series (University of Warwick), *Recent developments in hyperbolic geometry*

Susanna Terracini (Università di Milano Bicocca), *Analytical aspects of spatial segregation*

Corinna Ulcigrai (University of Bristol), *Dynamical properties of billiards and surface flows*

Horizon 2020 – the Framework Programme for Research and Innovation

Luc Lemaire (Université Libre de Bruxelles)

The Green Paper and the public consultation

The seventh Framework Programme of the European Commission (FP7) runs from 2007 to 2013.

The Commission has started its reflections on the programme to follow, under the provisional name "Common Strategic Framework for EU Research and Innovation" (CSF). It will bring together in a single programme the funding currently provided through FP7, the innovation actions of the Competitiveness and Innovation Framework Programme (CIP) and the European Institute of Technology (EIT).

The aim of this merger is to increase the efficiency of the programmes and unify and simplify the procedures.

During these reflections, it has been decided (on the basis of an open competition) that the programme will not be called CSF or FP8 but "Horizon 2020 – the Framework Programme for Research and Innovation".

To prepare the new programme, the Commission has produced a Green Paper and has launched a public consultation on its contents. The Green Paper can be found at *http://ec.europa.eu/research/csfri/pcom_2011_0048_csf green_paper_en.pdf*.

The EMS, partially in response to this consultation, has produced a Position Paper, sent to the Commission and the E.C. administration and published in the EMS Newsletter, Issue 80, pages 13–17 (*http://www.ems-ph.org/journals/newsletter/pdf/2011-06-80.pdf*). It triggered an answer which went beyond a simple acknowledgement of receipt.

In June, the Commission issued an analysis of the reactions and organised a presentation meeting in Brussels.

In fact, the response to the consultation was overwhelming: 1300 answers through an online questionnaire and a staggering 775 position papers. For the Brussels meeting, where EMS was represented by Marta Sanz-Solé and myself, registration was closed at 700 people.

The large number of position papers may have been caused by the fact that a number of stakeholders did not find in the online questionnaire the questions allowing them to express their views.

The analysis of the answers to the public consultation was presented in Brussels and can be found at *http://ec.europa.eu/research/horizon2020/pdf/consultation-conference/summary_analysis.pdf*.

The resolution of the European Parliament

No mention was made in this process of a rather extraordinary "resolution on simplifying the implementation of the research Framework Programmes" voted *unanimously* by the European Parliament on 11 November 2010. It is a detailed analysis of the Framework Programmes and a set of recommendations for improvements. The full text can be found at *http://www.europarl.europa.eu/sides/getDoc.do?type=TA&reference=P7-TA-2010-0401&language=EN*.

The main recommendation of the Parliament is for a strong simplification of the procedures; many interesting aspects are considered and well worth reading, including a very detailed analysis (70 points are presented), various criticisms on the present situation and a request for a better show of respect to the scientists and their motivations.

The ERC and the Marie Curie actions

Here are some of the conclusions of the consultation on the Green Paper.

- Two programmes of FP7 are particularly suitable for mathematicians: the European Research Council – ERC (Ideas) and the Marie Curie actions (People).

It is therefore good news to observe that there is general agreement to maintain – or even increase – these two programmes, seen as clear success stories.

- The ERC obtains a very strong support and overall satisfaction with its current functioning. There is also repeated suggestion that the "Starting Grants" and the "Advanced Grants" should be supplemented by "Consolidation Grants", for researchers in between the former two in the development of their careers.

The Commission also quotes the contribution of Business Europe, saying: "Although the ERC is only of indirect benefit to the business sector, substantial investments in frontier research are essential for Europe's future and the ERC has to be continued in Horizon 2020."

BusinessEurope (formerly called *UNICE*) is a very large association of the 40 main European enterprise federations (like the *Bundesverband der Deutschen Industrie*, the *Mouvement des Entreprises de France (MEDEF)* and the *Confederation of British Industry)*.

It is gratifying to see the business sector massively joining the academic world in its support of the curiosity-driven research of the ERC, as a necessary means to improve development.

- Note that already in the present programme, the ERC has launched a new initiative, the ERC Synergy Grants, where a small group of two to four principal investigators can propose projects none of them would be able to accomplish alone. The first Synergy Grant Call was published on 25 October 2011. The deadline is 25 January 2012 (see *http://www.euresearch.ch/index.php?id=544*).

The ERC also introduced new funding for the beneficiaries of its grants, to establish proof of concept for results applicable to commercial developments.

- The Marie Curie actions for mobility and training of researchers is generally considered to be one of the most successful and most appreciated elements of FP7. According to the CERN contribution: "The Marie Curie Actions have been for many years the most popular, competitive and useful EU-funded instruments and their role should be maintained and further enhanced under the next framework programme."

Other conclusions of the consultation

- Many other aspects come under scrutiny and we refer to the documents mentioned above for a complete description. As before, the main bulk of the funding will go to calls about specific priorities, at times related to societal needs. Usually, mathematicians have great difficulties in finding their place in such projects, although their presence in interdisciplinary teams would be of real added value.

Some of the main recommendations are as follows.

- All stakeholders call for simplification of procedures. Indeed, the complexity of EU funding is well-known and is a real obstacle to participation in calls, particularly for small structures. This is also the core of the Parliament's resolution. However, the same problem has been identified during the elaboration of each preceding Framework Programme and obviously couldn't be solved so far – one can but hope…
- There is a call for more integrated actions, leading from research to market. The aim would be to bring research and innovation closer together. The merger of the three programmes mentioned above goes in that direction. In fact, emphasis is put on innovation in many places in the documents and this might have the effect of separating scientists from the decision-making process in many programmes. On the other hand, the resolution of the European Parliament specifically notes that research and innovation need to be clearly distinguished as two different processes.
- There is a recurring call for funding opportunities to be less prescriptive and more open, with sufficient scope for smaller projects and consortia. The Parliament's resolution also mentions this point, noting that "reducing the size to smaller consortia, whenever possible, contributes to simplifying the process". Needless to say, mathematics projects are usually on the small side.
- There is a widespread view that Horizon 2020 will need both curiosity-driven and agenda-driven activities. There is strong support for more bottom-up approaches.
- Excellence needs to remain the key criterion for distributing EU research and innovation funding.

These recommendations, together with the Parliament's resolution, represent different views, which are sometimes conflicting.

What will happen now?

Discussion about the programme will go on over the next few years and past experience shows that lobbying will take place on a grand scale, each stakeholder trying to promote their interests.

Mathematicians should of course take all possible opportunities to show the importance of their field in European research, for instance on the basis of the EMS Position Paper and on some of the elements above.

Evaluation of Faculty at IST – a Case Study

Rui Loja Fernandes (Instituto Superior Técnico, Lisboa)

1. The Context

These days we all hear about the use and misuse of numbers and indicators in the evaluation and ranking of mathematical research. Although I have always paid some attention to what individuals and associations in the mathematics community have been reporting on these issues, this seemed to be a distant reality until recently, when I have come to experience it face-to-face. My colleagues at the department must have felt the same when a new evaluation procedure of the faculty, largely based on "numerology", came into place.

Instituto Superior Técnico (IST) is the top science and engineering school in Portugal and its mathematics department is a large research department, with a faculty of around 100 active researchers covering many areas of mathematics, including both pure and applied mathematics. IST is a public school, which has to obey the legislation and regulations set up by the Ministry of Science and Education. In 2009, new legislation for public universities came into force, which for the first time imposed a regular evaluation of faculty. Evaluations will occur every three years and the results of each individual evaluation are used to determine salary increases and the teaching load for the following three-year period. The new legislation also opened up the possibility of evaluating faculty in the period 2004–2009, during which salary increases had been frozen.

The need for a proper evaluation of faculty was long recognised by all active researchers in Portugal. Before the new regulations, salary increases were automatic for everyone after each three-year period of activity (although salary increases had been frozen by the Government since 2003, when the country first experienced some economic troubles) and the teaching load was the same for every faculty member (but it varied from school to school). The new regulations set up some general rules and guidelines for evaluation of the faculty (e.g. four levels of performance: Poor, Good, Very Good, Excellent) as well as its consequences (e.g. a weekly teaching load between six and nine hours) but left most of the details of the evaluation method to be determined by each university and school.

At IST, the scientific council elaborates most regulations that apply to the faculty. The entire faculty at IST elects the council so its membership reflects the sizes and the number of the departments. At the time that the rules for evaluation were set up, there were only two members from the mathematics department among the 25 members of the Council, a computer scientist and myself. The council, after an intense debate, which lasted for around one year and included public consultation, approved the new rules for evaluation of the faculty. There was consensus among most of the engineering departments, some objections from the physics department and some strong objections from a part of the mathematics department, including myself (but not the other representative of the department on the council).

In the end, the council approved an evaluation system to be described below, which is largely based on numbers/indicators, with a small input of evaluation by peers. Since IST was the first school to implement this procedure, most science and engineering schools in universities throughout the country adopted this evaluation system or slight variations of it. I don't know of any other country where an institution has adopted a similar evaluation procedure but I suspect this may happen in the near future. In this article we argue that such evaluation methods are not effective. We use the very same indicators that these methods seek to measure to invalidate them. It is also a warning about a science fiction movie that can come to a theatre near you.

2. The Evaluation System at IST

In order to understand how much in this evaluation system is based on indicators and how much depends on evaluation by peers, we will have to get into the details of the IST evaluation system.

Faculty at IST are evaluated with respect to four different aspects:

1. Teaching (which includes lecturing, pedagogical publications, advising of students, etc.)
2. Research (which includes research publications, participation and leadership of research projects, etc.)
3. Transfer of knowledge (which includes outreach activities, organisation of conferences, patents, etc.)
4. Administration (which includes participation in school committees and councils, performance in departmental and school jobs, etc.)

The evaluation system gives an individual a certain number of points in each of these four different aspects. Then, depending on the level of the position, they are combined with certain weights. So, for example, a full professor has weights:

- Teaching: 20%–40%.
- Research: 40%–60%.
- Transfer of knowledge: 5%–30%.
- Administration: 10%–20%.

These are ranges, not fixed values. Since faculty members can have different profiles (some are more research oriented, others are more teaching oriented, etc.), the weighted sum is subject to an optimisation to maximise the final score, so that the sum of the weights is 1.

In the end, each faculty member gets a total score which, depending on the range where the score falls, corresponds to one of four levels of performance: Poor (0–20), Good (20–50), Very Good (50–100) and Excellent (100 or larger). The total score remains confidential; only the level of performance is public and relevant for salary increases and other issues (e.g. determining future teaching load).

It remains to explain how each of the four different topics is evaluated. This is where a combination of indices and peer-to-peer evaluation comes in. We will concentrate here on the topic "Research". The other three topics are evaluated in a similar fashion but have certain peculiarities, related to the country's university system, which would require longer explanations.

Evaluation of research is divided into two different criteria:

a) Research Publications.
b) Research Projects.

In each of these criteria the score is a product of two factors:

$$S = Q \times m \qquad (1)$$

where:

- Q is a *qualitative factor*, which by default is 1. For each faculty member it is nominated an evaluator (a peer of the same scientific area of equal or higher rank), who can attribute a different value to the qualitative factor, with the values: 0.5, 0.75, 1.0, 1.25, 1.5.
- M is a *quantitative factor*, computed from a formula.

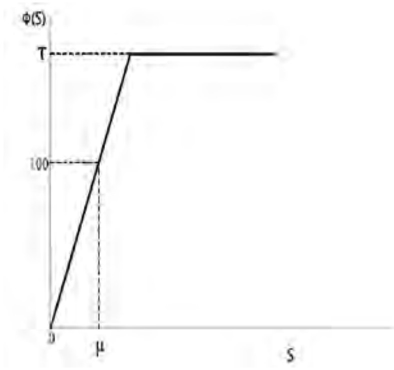
For example, for Research Publications the formula for the quantitative factor is:

$$M = \sum_{i=1}^{N} A_i \times \left(T_i + \frac{C_i}{\rho}\right) \qquad (2)$$

where:
- N is the number of publications that have appeared during the three-year evaluation period.
- A_i is a factor that accounts for the number of authors of the i-publication (3/2 for one author, 5/4 for two authors, $3/n$ for $n \geq 3$ authors).
- T_i is the type of the i-publication: $T_i = 5.5$ for a research monograph, $T_i = 3$, 1.75 or 0.3 for a paper published in a journal of type A, type B or type C (for this purpose journals were classified into three different types), etc.
- C_i is the number of citations in ISI Web of Knowledge (Thomson Reuters) of the i-publication, excluding self-citations.
- ρ is a factor that takes into account the reference number of citations of a given area but which was taken to be equal to 5 for all areas in the first evaluation period.

The formula for the quantitative factor M for the criteria Research Projects has a similar nature and takes into account the number of projects, the value of the projects, the number of members of the project, the type of the project, whether the subject was a project coordinator or not, etc.

Since the values of the score S for different criteria (either in the same topic or in different topics) are not commensurable, a transfer function $\Phi(S)$ is applied. The transfer function is a piecewise linear function that depends on two parameters, the target μ and the ceiling τ, as depicted in the figure below. The specific values of these parameters vary with the criteria. So, for example, for "Research Publications" the values were set as $\mu = 4.5$ and $\tau = 600$. In principle, the values of these parameters could vary with the scientific area but for the evaluation period 2004–2009 they were taken to all be the same.



Transfer function

After the rescaling obtained by applying the transfer function, the various criteria are combined with fixed weights. So the final score in "Research" is a combination of 75% research publications with 25% research projects.

There are two important aspects in the evaluation system used at IST which deserve to be analysed. One is that the model allows for parameters that can be adapted to different fields of science and even to different areas in each field. The other is that it allows for limited peer-to-peer evaluation. These two aspects will be discussed in the next section.

3. Evaluation of different fields of research

IST has, besides the mathematics department, a physics department and seven engineering departments. The members of each department are grouped in scientific areas. For example, the mathematics department has eight areas: algebra and topology; real and functional analysis; differential equations and dynamical systems; geometry; mathematical physics; numerical analysis and applied analysis; probability and statistics; and logic and computation. Comparing researchers in different areas is a very challenging exercise, to say the least. For example, we all know that different areas of mathematics have different publication traditions. Even in a given area, there are researchers with different profiles so any evaluation system that attempts to value the number of published papers is most likely to be a failure. Still, it is possible to give some evidence for how much these differences can vary between different disciplines.

Soon after the new evaluation system at IST came into force I became head of the mathematics department. I had expressed my objections of the evaluation system while I was a member of the scientific council and I was now very worried about the implementation of the new system. As head of the department I proposed different values for some of the parameters (e.g. the value of the target μ for the criterion of research publications) but the central administration decided to keep the same values for all the school for the evaluation period 2004–2009. As a consequence, when the results came out, the mathematics department had 60% of its faculty evaluated at "Excellent", compared with an average of 75% for the whole school. The top departments were the chemistry and biological engineering department (95% with "Excellent") and the physics department (89% with "Excellent"). As a response to this, I created a working group in the department to promote an international "benchmark" of the relevant parameters in the model among different areas of science and engineering.

The first problem that the working group faced was the lack of data at the European level. Detailed statistics about science and engineering in Europe seem to be lost somewhere in between the national science councils and the mess of European directorates that fund various aspects of science. Answers to simple questions like: 'How many PhD students in a given field (say mathematics, chemistry, physics, etc.) are there working in academia in Europe?' or 'How much funding does Europe devote to research in mathematics or in physics?' don't seem easy to answer. The last complete, detailed report on science in Europe that we could find dates back to 2003, from the times of Commissioner Philippe Busquin.[1]

By contrast, in the USA every other year the National Science Foundation publishes the report "Science & Engineering Indicators" and makes them freely available at *http://www.nsf.gov/statistics/*, including all tables and data in a format that can readily be used. Using these reports the working group easily produced evidence for how much indicators can vary for different fields of science. First, data was collected on the decade 1997–2006 for the various fields of science for the academic sector (since the focus was on the evaluation of an academic institution, the study excluded industry and the private sector). The working group considered the following aspects:

- Scientific productivity.
- Impact.
- Funding.

The next paragraphs describe some of the findings of the working group.[2]

[1] "Third European Report on Science & Technology Indicators", European Communities, 2003.

[2] The full report (in Portuguese) is available at *www.math.ist.utl.pt/~rfern/ BenchmarkReport.pdf*.

[3] Source: Science and Engineering Indicators 2010 – Appendix table 5-15 (full-time faculty with S&E doctorates employed in academia by degree field) and Appendix table 5-42 (S&E articles from academic sector by field).

3.1 Productivity by field

In order to compare productivity in different fields of science, one can determine the ratio between the number of articles and the number of researchers in a given field. Associating a researcher or an article to a given field may be problematic but to avoid this problem the degree field was defined as the field of the researcher, while data about articles were taken from the *Science Citation Index (SCI)* and the *Social Sciences Citation Index (SSCI)*, which assign fields to articles.

A first indicator is given by the ratio between the number of articles and the number of researchers in the field:

$$\frac{\text{number of articles in the field}}{\text{number of researchers in the field}} \Big/ \frac{\text{number of articles in mathematics}}{\text{number of researchers in mathematics}}$$

Table 1 presents this ratio normalised to mathematics.

Field	1997	1999	2001	2003	2006	Average
Mathematics	1.0	1.0	1.0	1.0	1.0	1.0
Physical Sciences	5.7	4.7	4.3	4.9	5.0	4.9
Computer sciences	2.2	1.7	1.7	1.7	1.6	1.8
Engineering	1.6	1.5	1.4	1.6	1.7	1.6
Life sciences	6.9	5.4	4.9	5.3	5.0	5.5
Psychology	1.6	1.3	1.1	1.3	1.3	1.3
Social sciences	1.1	0.9	0.8	0.9	0.9	0.9

Table 1. Ratio articles/researchers in the USA by field, gauged to mathematics.[3]

Note that the data in Table 1 does not represent the average number of articles that a researcher in a given field publishes but rather the average number of articles per capita in a given field, gauged to mathematics.

In order to estimate the average number of articles that a researcher in a given field publishes one observes that:

$$\text{average number of articles per researcher} = \frac{\text{number of articles}}{\text{number of researchers}} \times (\text{average number of authors per article})$$

Therefore, in order to determine the average number of articles that a researcher in a given field publishes we need data about the average number of authors per article in a given field. This is presented in Table 2.

Field	1997	1999	2001	2003	2006	Average
Mathematics	1.8	1.8	1.9	1.9	2.0	1.9
Physical Sciences	3.6	3.8	4.0	4.2	4.6	4.0
Computer sciences	2.2	2.4	2.5	2.6	2.8	2.5
Engineering	3.0	3.2	3.3	3.4	3.6	3.3
Life sciences	3.5	3.7	3.9	4.1	4.4	3.9
Psychology	2.4	2.6	2.7	2.8	3.0	2.7
Social sciences	1.6	1.6	1.7	1.8	1.9	1.9

Table 2. USA authors per S&E articles, by field.[4]

[4] Source: Science and Engineering Indicators 2010 – Table 5-16 (Authors per S&E articles, by field). For the fields "Physical Sciences" and "Engineering", the averages of their subfields was taken and the missing years were obtained by linear interpolation.

Finally, Table 3 shows the average number of articles of a researcher in a given field, gauged to mathematics.

Field	1997	1999	2001	2003	2006	Average
Mathematics	1.0	1.0	1.0	1.0	1.0	1.0
Physical Sciences	11.6	9.7	9.1	10.8	11.8	10.6
Computer sciences	2.8	2.2	2.2	2.3	2.3	2.3
Engineering	2.8	2.6	2.5	2.9	3.2	2.8
Life sciences	13.8	11.0	10.3	11.3	11.2	11.5
Psychology	2.3	1.8	1.6	1.9	2.0	1.9
Social sciences	1.0	0.8	0.8	0.9	0.9	0.8

Table 3. Average number of articles of a USA researcher by field, gauged to mathematics.

This data seem to suggest that in the academic institutions in the USA a computer scientist publishes on average twice as many articles as a mathematician, an engineer publishes three times more articles than a mathematician and a physicist, a chemist or a biologist publishes 11 times more articles than a mathematician.

The fact that these numbers vary so much across different fields, together with the fact that boundaries between fields are usually diffuse, leads to the conclusion that even different areas in the same field should also have very different publication profiles. Of course, collecting data like the above for different areas in the same field is almost impossible. Needless to say, we all know in our own areas of research that even top researchers have different publication profiles. This should be enough to prevent using number of papers for evaluation purposes but, unfortunately, as we saw before this is not the case.

3.2 Impact factors by field

Other parameters that enter into formula (2) can be similarly examined. For example, to see how impact factors can vary across fields, we consider the ratio:

$$\frac{\text{number of citations in the field}}{\text{number of articles in the field}} \Big/ \frac{\text{number of citations in mathematics}}{\text{number of articles in mathematics}}$$

The values of this ratio for different fields are presented in Table 4.

Field	1995	1997	1999	2001	2003	Average
Mathematics	1.0	1.0	1.0	1.0	1.0	1.0
Physical sciences	4.4	5.0	4.6	4.1	3.7	4.3
Engineering and Computer Science	1.7	1.8	1.6	1.6	1.4	1.6
Life sciences	6.3	7.8	6.9	6.6	6.0	6.7
Social sciences and Psychology	4.4	5.6	5.1	5.2	4.8	5.0

Table 4. Citations per article by field, gauged to mathematics.[5]

This data suggests that, on average, an article in engineering receives 1.6 times as many citations as one in mathematics, an article in physics or in chemistry receives four times as many citations as one in mathematics and an article in the life sciences receives seven times as many citations as one in mathematics.

It is also interesting to compare other indices related to citations, which exhibit the different nature of the fields and which also have strong consequences when it comes to evaluation. It is common to use citations as a measure of impact and this is usually limited to some period of time. However, depending on the field, articles may take different times to receive citations. For example, in ISI Web of Knowledge one finds the following indices for journals:

- *Immediacy Index*: the number of citations to an article in the journal during the year it is published.
- *Cited Half-Life*: the average time it takes an article in the journal to receive half of its citations.
- *Citing Half-Life*: the median age of the articles cited by the articles published in the journal.

Table 5 shows the aggregate values of these indices for the journals in each field.

Field	Aggregate Immediacy Index	Aggregate Cited Half-Life	Aggregate Citing Half-Life
Mathematics	0.160	>10	>10
Physical sciences			
Astronomy	1.461	6.6	7.1
Chemistry	0.543	6.4	8.1
Physics	0.553	7.1	8.1
Computer sciences	0.298	8.0	7.0
Engineering			
Bioengineering/ biomedical	0.410	5.9	7.3
Chemical	0.306	6.9	8.3
Civil	0.290	7.0	8.4
Electrical	0.195	7.2	7.0
Mechanical	0.184	7.7	9.6
Life sciences			
Agricultural sciences	0.232	8.2	9.2
Biological sciences	0.731	6.7	7.2
Psychology	0.448	>10	9.0
Social sciences			
Economics	0.246	>10	9.0
Political science	0.193	9.2	8.5
Sociology	0.158	>10	9.7

Table 5. Aggregate indices of impact in time for journals in a given field.[6]

This data clearly shows that articles in mathematics take much longer to be cited and have a far longer influence than articles in most other fields of science. Needless to

5 Source: Science and Engineering Indicators 2006 - Appendix table 5-24 (Worldwide citations of U.S. scientific articles, by field).

6 Source: Thomson Reuters, Science Citation Index and Social Sciences Citation Index.

say, an article can stay unnoticed and become influent years after its publication. In each field, the number of researchers in a given area can vary widely. In mathematics there are clear differences between pure and more applied areas. All this invalidates any formula that attempts to measure individual research by using citation data.

3.3 Funding by field

Administrators tend to value the amount of funding a researcher is able to raise. In the IST evaluation system, there is a formula for research projects where the amount of funding of each project is part of the data. Is it equally easy or difficult for researchers in different areas to raise funds? We all suspect that applied areas have more generous funding than basic areas of research. After all, applied areas require expensive equipment and labs, which can justify large differences in funding.

Unfortunately, as has already been mentioned above, there is not enough data available about Europe that allows for a clear picture of funding of different fields of science.[7] On the other hand, expenditures of research and development of different sectors in the USA, by field, is readily available in the NSF Science and Engineering Indicators Report. As for articles and citations, we can estimate the funding per researcher in a given area, relative to mathematics, by computing the ratio:

$$\frac{\text{expenditures of R\&D in the field}}{\text{number of researchers in the field}} \Big/ \frac{\text{expenditures in R\&D in mathematics}}{\text{number of researchers in mathematic}}$$

Table 6 below shows the values of the ratio for different fields.

Field	1997	1999	2001	2003	2006	Average
Mathematics	1.0	1.0	1.0	1.0	1.0	1.0
Physical Sciences	6.9	6.6	5.9	6.4	6.1	6.4
Computer sciences	11.1	10.3	10.1	10.3	7.7	10.0
Engineering	8.4	8.4	8.0	8.9	8.4	8.4
Life sciences	11.6	11.2	11.1	12.4	11.7	11.6
Psychology	0.9	0.9	0.9	1.2	1.0	1.0
Social sciences	1.4	1.3	1.3	1.4	1.1	1.3

Table 6. Expenditures in R&D per researcher, by field, gauged to mathematics.[8]

The data in this table suggests that in U.S. academic institutions, on average, a physicist, an engineer and a biologist receive, respectively, six times, eight times and 11 times more funding than a mathematician.

In Europe, the current trend is to establish research programmes and fund research projects with visible applications to the real world. This makes life hard for researchers working in more fundamental research. Evaluating research output of a person by using a formula measuring raised funds is highly unfair.

3.4 Peer-to-peer evaluation

The IST evaluation system allows for a limited peer-to-peer evaluation (see (1)). The proponents of the evaluation system recognised that even a sophisticated formula by itself couldn't possibly measure the quality of research of a person. The IST evaluation system limits the possible intervention of the evaluator because the authors of the system were afraid that the evaluator could simply overwrite the output of the formulas and invert what the "numbers say".

The data presented above shows not only that the use of formulas to measure quality of research is inadequate but also that the order of magnitude of the error is such that peer-to-peer evaluation is needed and cannot be limited in any way. One may argue that the order of magnitude in the differences in the tables above is due to the fact that we are considering different fields. However, even inside the same field (indeed, even in the same area of research) the differences can be huge. A Fields Medallist can have few publications compared to a largely unknown author.

The problems with evaluation by formulas tend to worsen after the people under evaluation know how the formulas are built: they will act to potentially increase the numbers. The classical example is to try to multiply the number of papers by publishing shorter papers. This increase in the numbers does not necessarily mean an increase in the quality of research. Actually, it is the reason for many circumstances of fraud with articles and journals. Again, the only way to fight this is through peer-to-peer evaluation.

Certainly, peer-to-peer evaluation is a human procedure and so it has many flaws. The way to minimise them is through public scrutiny, making available any relevant data and reports used in an evaluation. Just like democracy, peer-to-peer evaluation is not a perfect system but we don't know of any other system that is more perfect.

4. Concluding Thoughts

Although inside mathematics there is a general consensus about the dangers of using numbers and indicators in the evaluation and ranking of research, this is not so outside mathematics. Since the practices and the cultural environments in other sciences vary widely from mathematics, it may be helpful to have at hand data of the type collected here, to convince our colleagues in other departments why it is dangerous. At IST this seems to have produced some results: the administration of the school received the document produced by the mathematics department's working group and is considering modifying the evaluation systems accordingly.

7 The EMS executive committee is currently leading an effort to produce data of funding of mathematics in Europe.

8 Source: Science and Engineering Indicators 2010 – Appendix table 5-6 (Expenditures for academic R&D, by field).

Annales scientifiques de l'École normale supérieure

The *Annales scientifiques de l'École normale supérieure* were founded in 1864 by Louis Pasteur. Today, they are open to all fields of mathematics. The Editorial Board, with the help of referees, selects articles which are mathematically very substantial. The Journal insists on maintaining a tradition of clarity and rigour in the exposition.

Here is a selection of three papers published in the last few years.

Strichartz estimates for water-waves,
by T. ALAZARD, N. BURQ & C. ZUILY (vol. 44, 2011)

This paper studies the water-wave equation in dimension 2, a system of two non linear partial differential equations describing the evolution of a "vertical slice" of fluid located between a free surface and a fixed, known bottom. Using very fine tools of microlocal analysis, the authors establish regularity properties for the solutions of this equation and obtain almost optimal consequences concerning the associated Cauchy problem.

Multiple zeta values and periods of moduli spaces $\overline{\mathfrak{M}}_{0,n}$
by Francis C. S. BROWN (vol. 42, 2009)

The moduli space $\overline{\mathfrak{M}}_{0,n}$ parametrizes n points on the Riemann sphere; their periods are integrals on subvarieties of certain differential forms. The author establishes a conjecture of Goncharov & Manin which claims that these periods are linear combinations with coefficients in $\mathbf{Q}(2\pi i)$ of multiple zeta values, i.e. series defined by

$$\zeta(n_1,\ldots,n_r) = \sum_{0<k_1<\cdots<k_r} \frac{1}{k_1^{n_1}\ldots k_r^{n_r}},$$

for some positive integers such that $\max(n_1,\ldots,n_r) \geq 2$. The proof of this Theorem relies on the study of unipotent differential systems on compactifications of these moduli spaces, and on their motivic fundamental group.

Herman's Last Geometric Theorem,
by Bassam FAYAD & Raphaël KRIKORIAN (vol. 42, 2009)

The authors give a detailed proof of a theorem about diffeomorphisms of the annulus that was stated without proof by M. Herman in 1998. This implies in particular the stability of an elliptic fixed point of an area-preserving diffeomorphism of the plane, provided that the angle of the rotation given by its linear part is Diophantine.

The *Annales scientifiques de l'École normale supérieure*, previously published by Gauthier-Villars and later, Elsevier, are published and distributed by the **Société Mathématique de France**, since January 2008.

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Newton, the Geometer

Nicole Bloye and Stephen Huggett

1 Introduction

Isaac Newton was a geometer. Although he is much more widely known for the calculus, the inverse square law of gravitation, and the optics, geometry lay at the heart of his scientific thought. Geometry allowed Newton the creative freedom to make many of his astounding discoveries, as well as giving him the mathematical exactness and certainty that other methods simply could not.

In trying to understand what geometry meant to Newton we will also discuss his own geometrical discoveries and the way in which he presented them. These were far ahead of their time. For example, it is well-known that his classification of cubic curves anticipated projective geometry, and thanks to Arnol'd [1] it is also now widely appreciated that his lemma on the areas of oval figures was an extraordinary leap 200 years into Newton's future.

Less well-known is his extraordinary work on the *organic construction*, which allowed him to perform what are now referred to as Cremona transformations to resolve singularities of plane algebraic curves.

Geometry was not a branch of mathematics; it was a way of doing mathematics and Newton defended it fiercely, especially against Cartesian methods. We will ask why Newton was so sceptical of what most mathematicians regarded as a powerful new development. This will lead us to consider Newton's methods of curve construction, his affinity with ancient mathematicians and his wish to uncover the mysterious analysis supposedly underlying their work.

These were all hot topics in early modern geometry. Great controversy surrounded the questions of which problems were to be regarded as geometric and which methods might be allowable in their solution. The publication of Descartes' *Géométrie* [7] was largely responsible for the introduction of algebraic methods and criteria, in spite of Descartes' own wishes. This threw into sharp relief the demarcation disputes which arose, originally, from the ancient focus on allowable rules of construction, and we will discuss Newton's challenge to Cartesian methods.[1]

It is important to note that Descartes' *Géométrie* was to some extent responsible for Newton's own early interest in mathematics, and geometry in particular.[2] It was not until the 1680s that he focused his attention on ancient geometrical methods and became dismissive of Cartesian geometry.

This will not be a review of Guicciardini's excellent book [11] but we will refer to it more than to any other. We find in this book compelling arguments for a complete reappraisal of the core of Newton's work.

We would like to thank June Barrow-Green, Luca Chiantini and Jeremy Gray for their help and encouragement.

2 Analysis and synthesis

As Guicciardini[3] argues, the certainty Newton sought was "guaranteed by geometry" and Newton "believed that only geometry could provide a certain and therefore publishable demonstration". But how, precisely, was geometry to be defined? In order to obtain this certainty, it was necessary to *know* and *understand* precisely what it was that was to be demonstrated. This had been a difficult question for the early modern predecessors of Newton. What did it really mean to have *knowledge* of a geometrical entity? Was it simply enough to postulate it or to be able to deduce its existence from postulates, or should it be physically constructed, even when this is merely a representation of the object?

If it should be physically constructed then by what means? For example, Kepler (1619) took the view[4] that only the strict Euclidean tools should be used. He therefore regarded the heptagon as "unknowable", although he was happy to discuss properties that it would have were it to exist. On the other hand, Viète (1593) believed that the ancient *neusis* construction should be adopted as an additional postulate and showed that one could thereby solve problems involving third and fourth degree equations.[5]

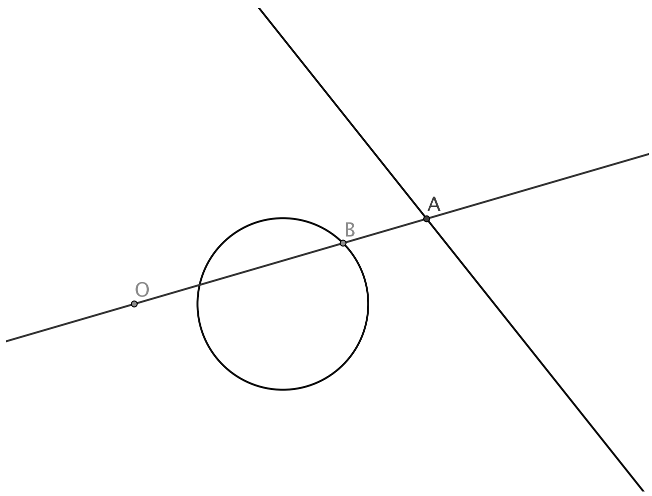

Figure 1. Neusis – given a line with the segment *AB* marked on it, to be able to rotate this line about *O* and slide it through *O* until *A* lies on the fixed line and *B* lies on the fixed circle.

We defer until later a discussion of Newton's preferred construction methods. [Given that *neusis* was used in ancient times, it is striking that Euclid chose arguably the most restrictive set of axioms. We are attracted by the hypothesis in [9] that these were chosen because the anthyphairetic sequences which are eventually periodic are precisely those which come from ratios with ruler and compass constructions. In other words, those ratios for which the Euclidean algorithm gives a finite description have ruler and compass constructions. However, this is a digression here as there is no evidence that Newton was aware of this property of Euclidean constructions.]

The early modern mathematicians followed the ancients in dividing problem solving into two stages. The first stage, *analysis*, is the path to the discovery of a solution. Bos[6] explores in great depth the various types of analysis that may have been performed. The main distinction we shall make here is between algebraic and geometric analyses. We shall see that in the mid 1670s Newton became sceptical of algebraic methods and the idea of an algebraic analysis no less so.

The second stage, *synthesis*, is a demonstration of the construction or solution. This was a crucial requirement before a geometrical problem could be considered solved. Indeed, following the ancient geometers, early modern mathematicians usually removed all traces of the underlying analysis, leaving only the geometrical construction.

Of course, in many cases this geometrical construction was simply the reverse of the analysis and Descartes tried to maintain this link between analysis and synthesis even when the analysis, in his case, was entirely algebraic. Newton argued, however, that this link was broken:

> Through algebra you easily arrive at equations, but always to pass therefrom to the elegant constructions and demonstrations which usually result by means of the method of porisms is not so easy, nor is one's ingenuity and power of invention so greatly exercised and refined in this analysis.[7]

There are two points being made here. One is that the constructions arising from Cartesian analysis were anything but elegant and that one should instead use the method of porisms, about which we will say more in a moment. The other is that the Cartesian procedures are algorithmic and allow no room for the imagination.

In spite of the methods in Descartes' *Géométrie* having become widely accepted, Newton believed that there not only could but should be a geometrical analysis. Early in his studies he mastered the new algebraic methods and only later turned his attention to classical geometry, reading the works of Euclid and Apollonius and Commandino's Latin translation of the *Collectio* (1588) by the fourth century commentator Pappus. According to his friend Henry Pemberton (1694–1771), editor of the third edition of *Principia Mathematica*, Newton had a high regard for the classical geometers:[8]

> Of their taste, and form of demonstration Sir Isaac always professed himself a great admirer: I have heard him even censure himself for not following them yet more closely than he did; and speak with regret for his mistake at the beginning of his mathematical studies, in applying himself to the works of Des Cartes and other algebraic writers, before he had considered the elements of Euclide with that attention, which so excellent a writer deserves.

It was from Pappus' work that Newton learned of what he believed to be the ancient method of analysis: the porisms. Guicciardini explores the possibility that Newton may have been trying to somehow recreate Euclid's work on porisms in order to identify ancient geometrical analysis.[9] Agreement on precisely what the classical geometers meant by a porism is still elusive but as the early modern geometers understood it, the porisms required the construction of a locus satisfying set conditions, such as the ancient problem that came to be known as Pappus' Problem.

3 Pappus' Problem

The contrast between Newton and Descartes is perhaps nowhere more evident than in their approaches to *Pappus' problem*. This was thought to have been introduced by Euclid and studied by Apollonius but it is often attributed to Pappus because the general problem, extending to any number of given lines, appeared in his *Collection* (in the fourth century). The classic case, however, is the *four-line locus*:[10]

> Given four lines and four corresponding angles, find the locus of a point such that the angled distances d_i from the point to each line maintain the constant ratio $d_1d_2 : d_3d_4$.

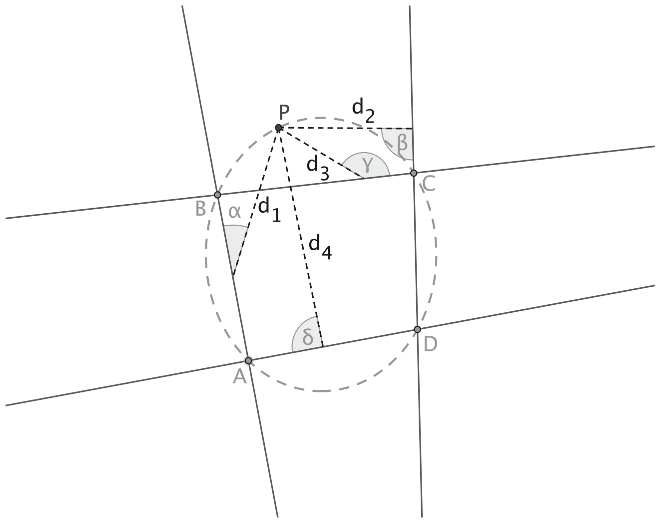

Figure 2. The four-line locus problem

Descartes dedicated much time to the problem, reconstructing early solutions in the case with five lines.[11] In his extensive study of the problem in the *Géométrie*, Descartes introduces a coordinate system along two of the lines and points on the locus are described by coordinates in that system. He was able to reduce the four-line problem to a single quadratic equation in two variables. Bos argues[12] that the study of Pappus' problem convinced Descartes more than anything else of the power of algebraic methods.

Indeed, Descartes claimed that every algebraic curve[13] is the solution of a Pappus problem of n lines, which Newton shows to be false. Newton considered the case $n = 12$. He noted[14] that 6th degree curves have 27 parameters, whilst the corresponding Pappus problem would involve 11 or 12 lines. But the 12 line problem requires that

$$d_1d_2d_3d_4d_5d_6 = kd_7d_8d_9d_{10}d_{11}d_{12},$$

which has 22 parameters in determining the position of 11 lines with respect to the 12th, and the factor k, making 23 parameters. So, there must exist algebraic curves that are not solutions of Pappus problems.

He then develops a completely synthetic solution, in his manuscript *Solutio problematis veterum de loco solido*,[15] a version of the first section of which was later included in the first edition of the *Principia*[16] (1687), Book 1 Section V, as Lemmas 17–22.

Guicciardini [11] describes how Newton's solution is in two steps. Firstly, from Propositions 16–23 of Book 3 of the *Conics* of Apollonius,[17] he shows that (in the words of Lemma 17):

> If four straight lines PQ, PR, PS, PT are drawn at given angles from any point P of a given conic to the four infinitely produced sides AB, CD, AC, DB of some quadrilateral $ABCD$ inscribed in the conic, one line being drawn to each side, the rectangle $PQ \cdot PR$ of the lines drawn to two opposite sides will be in a given ratio to the rectangle $PS \cdot PT$ of the lines drawn to the other two opposite sides.[18]

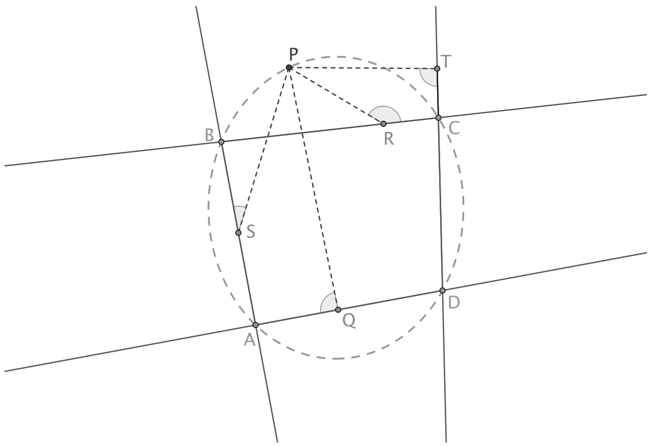

Figure 3. Lemma 17

The converse is Lemma 18: if the ratio is constant then P will be on a conic. Then Lemma 19 shows how to *construct* the point P on the curve.

Newton's second step is to show how the locus which solves the problem – a conic through five given points – can be constructed. Commenting that this was essentially given by Pappus, Newton then introduces the startling *organic construction*. We will discuss this in much more detail later but the essence is this. Newton chose two fixed points B and C called poles and around each pole he allowed to rotate a pair of rulers, each pair at a fixed angle (the two angles not having to be equal). In each pair he designated one ruler the directing "leg" and the other the describing "leg".

There is a third special point: when the directing legs are chosen to coincide then the point of intersection of the describing legs is denoted A.

In general, of course, the directing legs do not coincide and as their point M of intersection moves, it determines the movement of the point D of intersection of the describing legs. Newton showed that if M is constrained to move along a straight line then D describes a conic through A, B, and C, and conversely that any such conic arises in this way.

This beautiful result appears in the *Principia* as Lemma 21 of Book 1 Section V:

> If two movable and infinite straight lines BM and CM, drawn through given points B and C as poles, describe by their meeting-point M a third straight line MN given in position, and if two other infinite straight lines BD and CD are drawn, making given angles MBD and MCD with the first two lines at those given points B and C; then I say that the point D, where these two lines BD and CD meet, will describe a conic passing through points B and C. And conversely, if the point D, where the straight lines BD and CD meet, describes a conic passing through the given points B, C, A, and the angle DBM is always equal to the given angle ABC, and the angle DCM is always equal to the given angle ACB; then point M will lie in a straight line given in position.

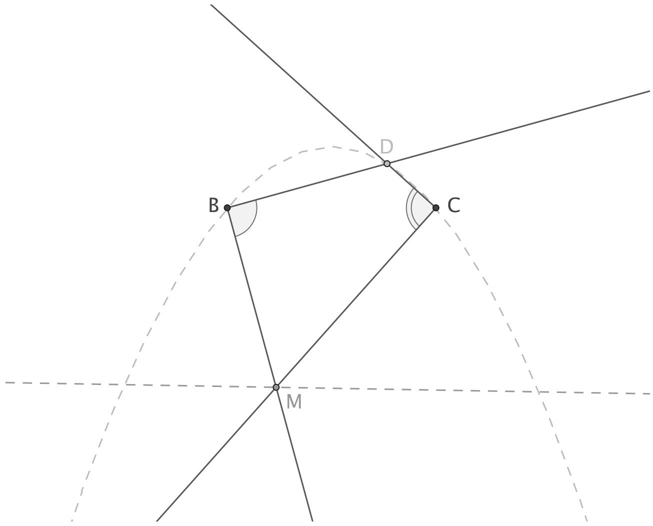

Figure 4. The organic construction

Newton's proofs of both the result and its converse are elegant and clear.[19] They follow from the anharmonic property of conics (his Lemma 20) and the fact that two conics do not intersect in more than four points (his Lemma 20, Corollary 3). Guicciardini [11] argues that this sequence of ideas came from an extension of the "main porism" of Pappus to the case of conics and Newton had indeed been determined to restore this ancient method.

Newton's description of conics was in a fairly strong sense what we would now refer to as the projective description. In Proposition 22 he shows how to construct the conic through five given points. In fact he gives two constructions. Whiteside and others interpret the first as evidence that Newton had at least an intuitive if not explicit grasp of Steiner's Theorem.[20] The second uses the organic construction but this should not be taken as indicating any reserve about this construction on Newton's part, as he also published it in the *Enumeratio* (1704) and the *Arithmetica Universalis* (1707).

However, in the *Principia* Newton's solution of the classical Pappus problem appears as a corollary to Lemma 19, after which he cannot resist the following comments:

> And thus there is exhibited in this corollary not an [analytical] computation but a geometrical synthesis, such as the ancients required, of the classical problem of four lines, which was begun by Euclid and carried on by Apollonius.

4 Rules for construction

> Among geometers it is in a way considered to be a considerable sin when somebody finds a plane problem by conics or line-like curves and when, to put it briefly, the solution of the problem is of an inappropriate kind.[21]

The influence of this remark by Pappus was very great in the early modern period. Bos[22] gives three examples, from Descartes, Fermat and Jacob Bernoulli, in which this passage on sin was explicitly quoted. Mathematicians wishing to extend geometrical knowledge struggled to formulate precise definitions of the subject itself and of the simplicity of the various types ("plane", "solid" and "linear") of geometrical constructions.

It was accepted that straight lines and circles formed a basis for classical geometry and that the way to construct them in practice was by straight edge and compasses. In addition, it was also well-known that the ancients had studied other curves, such as conic sections, conchoids, the Archimedian spiral and Hippias' quadratrix, and other means of construction, such as *neusis*. However, these wider ideas were somehow less well defined than the strict Euclidean ones and hence the focus on demarcation.

Indeed, some of these constructions were dismissed as being "mechanical" but for Descartes this did not make sense: circles and straight lines were also mechanical, in fact, and yet they were perfectly acceptable. He introduced his own "new compasses"[23] for solving the trisection problem and wrote that the precision with which a curve could be understood should be the criterion in geometry, not the precision with which it could be traced by hand or by instruments.[24]

From our point of view, Descartes' extension of the geometrical boundaries to include all algebraic curves was a dramatic and important one. Bos [4] argues that although Descartes' attempts to define the constructions which would generate all algebraic curves were neither explicit nor conclusive, they were nevertheless the deepest part of the *Géométrie*. We describe them very briefly and then consider Newton's fierce criticisms of them.

Descartes started by claiming that:

> nothing else need be supposed than that two or several lines can be moved one by the other and that their intersections mark other lines

and in the interpretation by Bos these curves satisfied the four criteria:

1. The moving objects were themselves straight or curved lines.
2. The tracing point was defined as the intersection of two such moving lines.
3. The motions of the lines were continuous.
4. They were strictly coordinated by one initial motion.

For example, Descartes objected to the quadratrix on the grounds that it required both circular and linear motions,[25] which could not be strictly coordinated by one motion because this would amount to a rectification of the circumference of a circle, which he believed "would never be known to man".[26]

This is also why Descartes rejected methods of construction in which a string is sometimes straight and sometimes curved, such as the device generating a spiral described by Huygens.[27] In contrast, he accepted pointwise constructions but was careful to distinguish those in which generic points on the curve could be constructed from those in which only a special subset of points on the curve could be reached. He argued that curves with these generic pointwise constructions could also be obtained by a continuous motion so that their intersections with other similar curves could be regarded as constructible.

Having shown how to reduce the analysis of a geometrical problem to algebra and having decided that algebraic curves were precisely those acceptable in geometry, Descartes still had to demonstrate how to perform the *synthesis*.

Descartes was faced with the task of providing the standard constructions that were to be used once the algebra had been performed. He divided problems into classes according to the degree of their equation. In each case a standard form of the equation was given and this was to be accompanied by a standard construction. For the plane problems Descartes simply referred to the standard ruler and compass constructions, while for problems involving third and fourth degree equations he gave his own constructions using the parabola and circle. He then claimed that analogous constructions in the higher degree cases "are not difficult to find", thus dismissing the subject.

Pappus' remark depends upon having a clear criterion for the simplicity of a construction. Here Descartes adopted an unequivocally algebraic view: simplicity was defined by the degree of the equation. Guicciardini argues that Newton was in a weak position when he criticised this criterion because Newton's arguments were based on aesthetic judgements, while Descartes' criterion was at least precise, whether right or wrong.

It is ironic that Newton's organic construction satisfied Descartes' criteria for allowable constructions, given that Newton so explicitly distanced himself from Descartes on construction methods. Newton was scornful of pointwise constructions because one has to complete the curve by "a chance of the hand" and he also rejected, in an argument reminiscent of Kepler's,[28] the "solid" constructions involving intersections of planes and cones. The underlying difference, though, was that (in modern terminology) to Descartes only algebraic curves were geometrical, the others being "mechanical", while to Newton *all* curves were mechanical:

> But these descriptions, insofar as they are achieved by manufactured instruments, are mechanical; insofar, however, as they are understood to be accomplished by the geometrical lines which the rulers in the instruments represent, they are exactly those which we embrace … as geometrical.[29]

Of course, before one reaches the stage of construction, one has to perform an analysis of the problem and here the distinction between Newton and Descartes is even clearer. For Newton, the link between analysis and construction was extremely important:

> Whence it comes that a resolution which proceeds by means of appropriate porisms is more suited to composing demonstrations than is common algebra.[30]

But it was not merely a question of adopting a method which would lead to clear and elegant constructions. Newton also felt that mechanical (that is, geometrical) constructions had another crucial feature:

> [I]n definitions [of curves] it is allowable to posit the reason for a mechanical genesis, in that the species of magnitude is best understood from the reason for its genesis.[31]

We note that Newton is not alone in regarding geometry as yielding deeper insights. A striking modern example comes from [5]. In the "Prologue" to his book Chandrasekhar says:

> The manner of my study of the *Principia* was to read the enunciations of the different propositions, construct proofs for them independently *ab initio*, and then carefully follow Newton's own demonstrations.

In his review [20] of this book, Penrose describes Chandrasekhar's discovery that

> In almost all cases, he found to his astonishment that Newton's "archaic" methods were not only shorter and more elegant [than those using the standard procedures of modern analysis] but more revealing of the deeper issues.

5 The Organic construction

Exercitationum mathematicarum libre quinque (1656–1657), by the Dutch mathematician and commentator Frans van Schooten, includes some 'marked ruler' constructions and a reconstruction of some of Apollonius' work *On Plane Loci*. According to Whiteside [27], it was through a study of the fourth book, *Organica conicarum sectionum*, together with *Elementa curvarum linearum* by Schooten's student Jan de Witt,[32] that Newton learnt of the organic construction.

We have seen Newton's brilliant use of the organic construction of a conic in his solution of the Pappus problem and indeed Whiteside notes that the organic construction can, in fact, be derived almost as a corollary of Newton's work on that problem. But Newton knew that these rotating rulers could do much more: he thought of them as giving a transformation of the plane.

It was therefore natural for him to think of the construction in Lemma 21 as a transformation taking the straight line (on which the directing legs intersect) to the conic (on which the describing legs intersect). This is clear from his manuscript[33] of about 1667:

> And accordingly as the situation or nature of the line PQ varies from one place to another, so will a correspondingly varying line DE be described. Precisely, if PQ is a straight line, DE will be a conic passing through A and B; if PQ is a conic through A and B, then DE will be either a straight line or a conic (also passing through A and B). If PQ is a conic passing through A but not B and the legs of one rule lie in a straight line [...], DE will be a curve of the third degree [...][34]

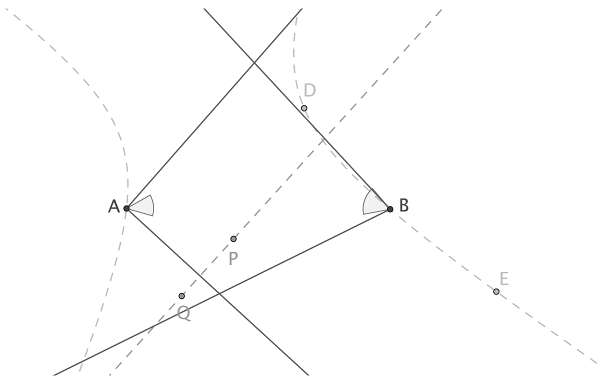

Figure 5. Another view of the organic construction

In fact Newton went much further than this, as is evident for example in his lovely construction[35] of the 7-point cubic in the *Enumeratio* (1704). In this extract, note that "curves of second kind" are cubics and that the letters do not correspond to those in our figure.

> All curves of second kind having a double point are determined from seven of their points given, one of which is that double point, and can be described through these same points in this way. In the curve to be described let there be given any seven points A, B, C, D, E, F, G, of which A is the double point. Join the point A and any two other of the points, say B and C, and rotate both the angle $\widehat{CAB}$ of the triangle ABC round its vertex A and either one, $\widehat{ABC}$, of the remaining angles round its vertex, B. And when the meeting point C of the legs AC, BC is successively applied to the four remaining points D, E, F, G, let the meet of the remaining legs AB and BA fall at the four points P, Q, R, S. Through those four points and the fifth one A describe a conic, and then so rotate the before-mentioned angles $\widehat{CAB}, \widehat{CBA}$ that the meet of the legs AB, BA traverses that conic, and the meet of the remaining legs AC, BC will by the second Theorem describe the curve proposed.

Even in his earlier manuscript (1667), Newton studied various types of singular point and indeed he went so far as to devise a little pictorial representation of them. He also gave a long list of examples, up to and including quintics and sextics. Finally, we note that just after the construction of the 7-point cubic he considers the case in which the double point A is at infinity, as he often did elsewhere, thus in effect working in the projective plane.

As noted by Shkolenok [25], the transformations effected by the organic construction are in fact birational maps from the projective plane to itself, now known as Cremona transformations.[36] (We give a short technical account of this in the Appendix.)

Of course one wonders how Newton could possibly have discovered such extraordinary results, so far ahead of their time, and it seems clear at least (as Guicciardini argues) that Newton actually made a set of organic rulers. For example, in the 1667 manuscript referred to above Newton uses terms such as *manufactured*, *steel nail* and *threaded to take a nut*. Guicciardini also draws our attention to Newton's choice of language in his letter (20 August 1672) to Collins explaining his constructing instrument:

> And so I dispose them that they may turne freely about their poles A & B without varying the angles they are thus set at.[37]

Finally, Guicciardini also notes that the drawing accompanying this letter is quite realistic. We return to this point in the next section.

6 Cubics, and projective geometry

In the early 17th century very little was known about cubic curves. Newton revealed the potential complexities of these curves, which, to quote Guicciardini[38] "reinforced his conviction that Descartes' criteria of simplicity were foreign to geometry". Newton's first manuscript on the subject, *Enumeratio Curvarum Trium Dimensionum*, thought to have been written around 1667, contained an equation for the general cubic

$$ay^3 + bxy^2 + cx^2y + dx^3 + ey^2 + fxy + gx^2 + hy + kx + l = 0$$

which he was able to reduce to four cases by clever choices of axes.

$$Axy^2 + By = Cx^3 + Dx^2 + Ex + F,$$
$$xy = Ax^3 + Bx^2 + Cx + D,$$
$$y^2 = Ax^3 + Bx^2 + Cx + D,$$
$$y = Ax^3 + Bx^2 + Cx + D.$$

He then divided the curves into 72 species by examining the roots of the right-hand side. It is often remarked that there are in fact 78 species, Newton failing to identify six of them. However, as Guicciardini points out, Newton had in fact identified the remaining six but had chosen to omit them from his paper for some unknown reason.[39] Newton returned to his classification of cubic curves in the late 1670s with a second paper[40] *Enumeratio Linearum Tertii Ordinis* appearing as an appendix to his *Opticks* (1704).

The 1704 *Enumeratio* contained Newton's astonishing discovery that every cubic can be generated by centrally projecting one of the five divergent parabolas (encompassed by the equation $y^2 = Ax^3 + Bx^2 + Cx + D$), starting with the evocative phrase:[41]

> If onto an infinite plane lit by a point-source of light there should be projected the shadows of figures . . .

This remained unproven until 1731 and was first demonstrated by François Nicole (1683–1758) and Alexis Clairaut (1713–1765).

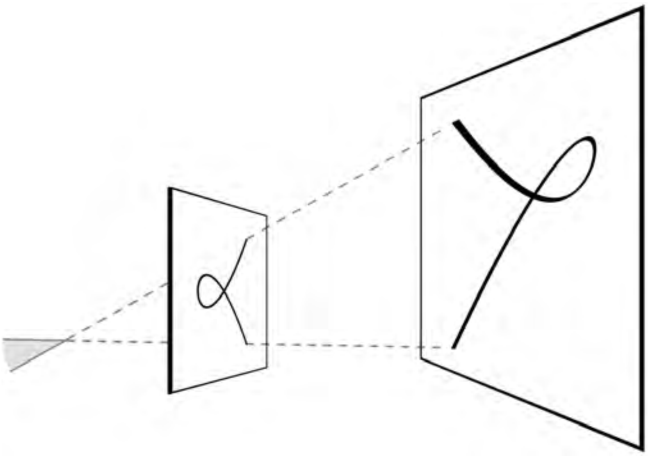

Figure 6. Projection of cubics

Here again, it seems extremely plausible that Newton's intuition was supported by his use of an actual projection from a point source of light but Guicciardini notes that there have been differing views on this question. Rouse Ball[42] argued that the result was obtained using the projective transformations given in the *Principia*, Book 1 Section V, Lemma 22. Thus, the discovery that all the cubics can be generated by projecting the five divergent parabolas was essentially algebraic.

Talbot[43] preferred the view that Newton might have followed a geometrical procedure. He argued that Newton generated all the cubic curves by projection of the five divergent parabolas, using a method in which he began by noting that the position of the horizon line determined the nature of the asymptotes of the projected line.

There is no real evidence for either hypothesis in Newton's work. Guicciardini and Whiteside both seem to favour Talbot's geometrical explanation. We agree: Newton may well have used Lemma 22 to test specific cases but the general result must surely have been perceived by him as a geometrical insight.

7 Physics

Some of the most extraordinary examples of Newton's geometrical power arose in the exposition of his physical discoveries. In this section we note, rather briefly, three such cases, starting with a question in the foundations of the subject. Newton clearly and explicitly understood the Galilean relativity principle[44] and, as was pointed out by Penrose [22], Newton even considered[45] adopting it as one of his fundamental principles. But in what framework was this principle to operate? We agree with DiSalle, who argues[46] that Newton's absolute space and time shares with special and general relativity that space-time is an objective *geometrical* structure which expresses itself in the phenomena of motion.

Our second example comes from Section 6 of Book 1 of *Principia*, which is called *To find motions in given orbits*. Lemma 28 is on algebraically integrable ovals:

> No oval figure exists whose area, cut off by straight lines at will, can in general be found by means of equations finite in the number of their terms and dimensions.

Newton's proof simply takes a straight line rotating indefinitely about a pole inside the oval and a point moving along the line in such a way that its distance from the pole is directly proportional to the area swept out by the line. This point describes a spiral, which intersects any fixed straight line infinitely many times.

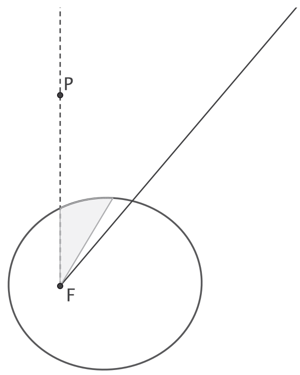

Figure 7. Lemma 28

Then, after noting almost as an aside what is essentially Bézout's Theorem (1779) on the intersections of algebraic curves, the proof is completed by the observation that if the spiral were given by a polynomial then it would intersect any fixed straight line finitely many times.

At the end of his proof Newton applies the result to ellipses (which were of course the original motivation) and defines "geometrically rational" curves, noting casually that spirals, quadratrices and cycloids are geometrically irrational. Thus, he leapt to the modern demarcation of algebraic curves, while demonstrating that a restriction to these curves (following Descartes) would not be enough for a description of orbital motion.

This is how Arnol'd puts it:[47]

> Comparing today the texts of Newton with the comments of his successors, it is striking how Newton's original presentation is more modern, more understandable and richer in ideas than the translation due to commentators of his geometrical ideas into the formal language of the calculus of Leibnitz.

Unfortunately, Newton did not make explicit what he meant by an oval, which has led to considerable controversy.[48] Although in later editions of the *Principia* Newton inserted a note excluding ovals "touched by conjugate figures extending out to infinity", he never made clear his assumptions on the smoothness of the oval. Nor did the statement of the Lemma distinguish between local and global integrability. There is therefore a family of possible interpretations of Newton's work, which has been elegantly dissected in [24], where it is concluded that:

> ... Newton's argument for the algebraic nonintegrability of ovals in Lemma 28 embodies the spirit of Poincaré: a concern for existence or nonexistence over calculation, for global properties over local, for topological and geometric insights over formulaic manipulation ...

Our final example comes from Section 12 of Book 1, which has the title *The attractive forces of spherical bodies*. Here Newton shows that the inverse square law of gravitation is not an approximation when the attracting body is a sphere instead of a point, and one of the key results is Proposition 71:

> a corpuscle placed outside the spherical surface is attracted to the centre of the sphere by a force inversely proportional to the square of its distance from that same centre.

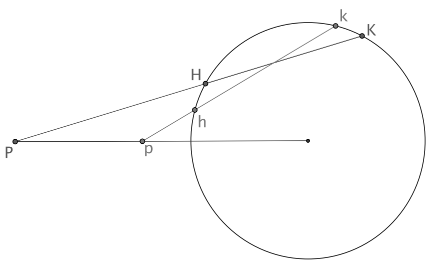

Figure 8. Gravitational attraction of a spherical shell

Newton's proof is utterly geometrical and utterly beautiful.[49] Here is a sketch of the argument. The spherical surface attracts "corpuscles" at P and p and we wish to find the ratio of the two attractive forces. Draw lines PHK and phk such that $HK = hk$ and draw infinitesimally close lines PIL and pil with $IL = il$. (These are not shown in our figure.) Rotate the segments HI and hi about the line Pp to obtain two ring-shaped slices of the sphere. Compare the attractions of these slices at P and p respectively, merely using the many similar triangles in the construction, and obtain the result.

Littlewood [15] felt that the proof's key geometrical construction (of the lines PHK and phk cutting off equal chords HK and hk) "must have left its readers in helpless wonder" but conjectured that Newton had first proved the result using calculus, only later to give his geometrical proof. We agree with [5] that this is highly implausible. As Chandrasekhar says: "his physical and geometrical insights were so penetrating that the proofs emerged whole in his mind."[50] We would argue, further, that the integration Newton is supposed to have performed would in no way have suggested the key geometrical construction. In other words, there is absolutely no link between the supposed analysis and the synthesis.

8 Concluding remarks

In focusing on Newton's geometry we do not mean to imply that he was not also a brilliant algebraist, of which there is ample evidence in the *Principia*, and as we noted in our introduction he is of course widely known for his calculus.

However, it is unfortunate, to say the least, that Newton claimed that he had first found the results in the *Principia* by using the calculus, a claim for which there is no evidence at all.[51]

On the contrary, many scholars have given clear and convincing arguments that Newton's claim is simply false. Guicciardini [11] rehearses these, as do Cohen [6] and Needham [17], for example. The claim was made during the row with Leibnitz over priority and simply does not make sense.

Of course the calculus was another profound achievement of Newton's but just because the calculus came to dominate mathematics it should not be assumed that Newton must always have used it in this way. Why ever *should* he?

Newton was one of the most gifted geometers mathematics has ever seen and this allowed him to see further, much further, than others and to express this extraordinary insight with precision and certainty.

Appendix: Cremona transformations

In [18] Book 1 Section 5 Lemma 21 it is shown that the organic transformation maps a line to a conic through the poles B and C, and conversely that any conic through the three points B, C and A will be mapped to a line.

The crucial part of this is that the conic goes through the point A (as well as the two poles B and C). This point A is special: it is the third of the three points which are needed for the Cremona transformations.[52]

Note also that it is clear from this Lemma that the organic transformation is generically one-one and self-inverse. It can be shown by a short analytical argument that organic transformations are rational maps.[53] But a rational map is birational if and only if it is generically one-to-one.[54] So the organic transformation is a birational map from $\mathbf{P}^2$ to itself, and hence a *Cremona transformation*.

Without loss of generality we can take the points A, B and C to have homogeneous coordinates $(1, 0, 0)$, $(0, 1, 0)$ and $(0, 0, 1)$. Conics in $\mathbf{P}^2$ through these three points have the form

$$axy + byz + czx = 0.$$

Consider the *standard quadratic transformation* $\phi : \mathbf{P}^2 \to \mathbf{P}^2$

$$\phi(x, y, z) = (yz, zx, xy),$$

which is a special case of a Cremona transformation. Let L be a line in the codomain. Then L is

$$axy + byz + czx = 0,$$

which is a conic through $(1, 0, 0)$, $(0, 1, 0)$ and $(0, 0, 1)$ in the domain. So the space of lines in the codomain is the same as this linear system of conics in the domain and $\phi^{-1}(L)$ is one of these conics.

In fact, the organic transformation is this standard quadratic transformation. To see this we use Hartshorne's argument,[55] as follows.

Let $\mathcal{S}$ be the subsheaf of $\mathcal{O}(2)$ consisting of those elements which vanish at the three base points and let

$$s_0, s_1, s_2 \in \Gamma(\mathbf{P}^2, \mathcal{S})$$

be global sections which generate $\mathcal{S}$. In other words s_0, s_1 and s_2 are three conics which generate the linear system of conics through the three base points. Also, let

$$x_0, x_1, x_2 \in \Gamma(\mathbf{P}^2, \mathcal{O}(1))$$

be global sections which generate $\mathcal{O}(1)$. Then x_0, x_1 and x_2 are simply lines generating the linear system of lines in $\mathbf{P}^2$.

Note that we are thinking of the conics as being in the *domain* $\mathbf{P}^2$ and the lines as being in the *codomain* $\mathbf{P}^2$, as in the diagram below:

$$\begin{array}{ccc} \mathcal{S} & & \mathcal{O}(1) \\ \downarrow & & \downarrow \\ \mathbf{P}^2 & \overset{\phi}{\to} & \mathbf{P}^2 \end{array}$$

Then there is a *unique* rational map

$$\phi : \mathbf{P}^2 \to \mathbf{P}^2$$

such that

$$\mathcal{S} = \phi^*(\mathcal{O}(1)),$$

with $s_i = \phi^*(x_i)$. In other words there is a *unique* rational map from $\mathbf{P}^2$ to itself with the property that for any line L in the codomain, $\phi^{-1}(L)$ is a conic in the domain through the three base points. So the organic transformation is the same as the standard quadratic transformation.

Notes

1. According to David Gregory, Newton referred to people using Cartesian methods as the "bunglers of mathematics"! See page 42 of [13].
2. Newton studied van Schooten's second Latin edition of the *Géométrie*.
3. See page 13 of [11].
4. See Section 11.3 of [4].
5. See pages 167–168 of [4].
6. See Chapter 5 of [4].
7. This dates from the 1690s. See page 102 of [11] and page 261 of Volume 7 of [19].
8. See page 378 of [26].
9. See page 82 of [11].
10. The three-line problem occurs when two of these four given lines are coincident. In the general case of many lines, the angled distances must maintain the constant ratio $d_1 \dots d_k : d_{k+1} \dots d_{2k}$ for $2k$ lines or $d_1 \dots d_{k+1} : \alpha d_{k+2} \dots d_{2k+1}$ for $2k + 1$ lines.
11. The general solution to this is the Cartesian parabola. See Sections 19.2 and 19.3 in [4].
12. See Chapters 19 and 23 of [4].
13. He thought of these as the geometrical curves.
14. This dates from the late 1670s. See page 343 in Volume 4 of [19].
15. See page 282 in Volume 4 of [19].
16. See [18]. Newton needed these results in this part of the *Principia* in order to find orbits of comets but in the 1690s he considered removing them from the second edition and publishing them separately. Sections IV and V are also discussed in [16].
17. Approximate dates for Apollonius are (260–190).
18. Whiteside [27] observes that this is equivalent to Desargues' *Conic Involution Theorem* and also notes that the condition amounts to the constancy of a cross-ratio.
19. The point A is crucial to the construction and it may be helpful to the reader to note that in his thesis [27] Whiteside did not appear to grasp its importance and drew the conclusion that the proof of the converse was flawed. He corrected this misunderstanding on page 298 of Volume 4 of [19].
20. Steiner's Theorem (1833) states that if p and p' are pencils of lines through vertices A and B respectively and if there is a correspondence between the lines of p and p' having the property that the cross-ratio of any four lines in p is equal to the cross-ratio of the corresponding four lines in p' then the locus of the point of intersection of corresponding lines is a conic through A and B.
21. Here, "finds" means "solves" and the strong language – sin – comes from the Latin translation of Pappus' *Collection* published by Commandino in 1588. See page 49 of [4].
22. See note 31 on page 50 of [4].
23. In Descartes' *Cogitationes Privatae* (1619–1620) he sketched three such instruments, one for angle trisection and two others for solving particular cubic equations. The first was an assembly of four hinged rulers OA, OB, OC, OD, extending from a single point O. These rulers were connected by a further four rulers of fixed length, also hinged, such that the three inner angles, AOB, BOC, COD, would always be equal. These instruments certainly fulfilled Descartes' criteria for curve tracing (see below). See also Section 16.4 of [4].
24. See page 338 of [4].
25. Bos shrewdly observes that "it is not necessary to pre-install a special ratio of velocities to draw a quadratrix. The ratio ... arises only because the square in which the quadratrix is to be drawn is supposed as given". See note 15 on pages 42–43 of [4].
26. See page 342 of [4].
27. This is from a manuscript of 1650 and Bos suggests that Huygens may have learned about this device from Descartes. See page 347 of [4]
28. See page 188 of [4].
29. See page 104 of [11].
30. See page 102 of [11].
31. See page 72 of [11].
32. This appeared in the second edition of Schooten's translation of Descartes' *Géométrie* (1659–1661).
33. See pages 106 and 135 of Volume 2 of [19].
34. In this context it is interesting to note that the general problem of constructing algebraic curves by linkages was solved in [14].
35. See page 639 of Volume 7 of [19].
36. These were published by Luigi Cremona in *Introduzione ad una teoria geometrica delle curve piane* Tipi Gamberini e Parmeggiani, Bologna, 1862.
37. See page 94 of [11]
38. See page 112 of [11].
39. See note 8 on page 111 of [11].
40. See Volume 2 of [28].
41. See page 635 in Volume 7 of [19].
42. See Sections 6.4.2 and 6.4.3 of [11].
43. C R M Talbot (1803–1890) published a translation of Newton's 1704 *Enumeratio* in 1860, with notes and examples.
44. See page 28 of [8].
45. This is in *De motu corporum in mediis regulariter cedentibus*.

See pages 188–194 in Volume 6 of [19].
46. See page 16 of [8].
47. See page 94 of [1].
48. Whiteside's own counter-example (which he gave in note 121 on pages 302–303 in Volume 6 of [19]) was elegantly ruled out in [23].
49. It certainly meets Whitehead's criterion of style! See page 19 of A N Whitehead, *The Aims of Education and Other Essays*, New York: Macmillan, 1929
50. Compare Penrose's discussion of this feature of inspirational thought and his remarks on Mozart's similar ability to seize an entire composition in his mind, on page 423 of [21].
51. See page 123 of [6].
52. Newton only refers to the third base point A in the converse. In fact it is easy to see that if CA, BC and AB intersect the line in Q, R and S, respectively, then the organic transformation maps Q to B, R to A and S to C.
53. We would prefer a synthetic argument for this but have not yet found one.
54. See page 493 of [10], for example.
55. See page 150 of [12].

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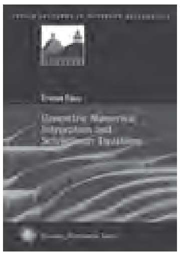

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Decorated Teichmüller Theory (The QGM Master Class Series)

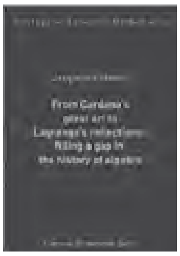
ISBN 978-3-03719-075-3. 2012. Approx. 380 pages. Hardcover. 17 x 24 cm. 58.00 Euro

There is an essentially "tinker-toy" model of a trivial bundle over the classical Teichmüller space of a punctured surface, called the decorated Teichmüller space, where the fiber over a point is the space of all tuples of horocycles, one about each puncture. This model leads to an extension of the classical mapping class groups called the Ptolemy groupoids and to certain matrix models solving related enumerative problems, each of which has proved useful both in math and in theoretical physics. This volume gives the story and wider context of these decorated Teichmüller spaces as developed by the author over the last two decades in a series of papers, some of them in collaboration. Sometimes correcting errors or typos, sometimes simplifying proofs and sometimes articulating more general formulations than the original research papers, this volume is self-contained and requires little formal background. Based on a master's course at Aarhus University, it gives the first treatment of these works in monographic form.

Peter M. Neumann (University of Oxford, UK)
The mathematical writings of Évariste Galois (Heritage of European Mathematics)

ISBN 978-3-03719-104-0. 2011. 421 pages. Hardcover. 17 x 24 cm. 78.00 Euro

Although Évariste Galois was only 20 years old when he died, his ideas, when they were published 14 years later, changed the course of algebra. He invented what is now called Galois Theory, the modern form of what was classically the Theory of Equations. For that purpose, and in particular to formulate a precise condition for solubility of equations by radicals, he also invented groups and began investigating their theory. His main writings were published in French in 1846. Very few items have been available in English up to now.
The present work contains English translations of almost all the Galois material. They are presented alongside a new transcription of the original French, and are enhanced by three levels of commentary. This work will be a resource for research in the history of mathematics, especially algebra, as well as a sourcebook for those many mathematicians who enliven their student lectures with reliable historical background.



Jacqueline Stedall (University of Oxford, UK)
From Cardano's great art to Lagrange's reflections: filling a gap in the history of algebra (Heritage of European Mathematics)

ISBN 978-3-03719-092-0. 2011. 236 pages. Hardcover. 17 x 24 cm. 68.00 Euro

This book is an exploration of a claim made by Lagrange in the autumn of 1771 as he embarked upon his lengthy *Réflexions sur la résolution algébrique des équations*: that there had been few advances in the algebraic solution of equations since the time of Cardano in the mid sixteenth century. That opinion has been shared by many later historians. The present study attempts to redress that view and to examine the intertwined developments in the theory of equations from Cardano to Lagrange. A similar historical exploration led Lagrange himself to insights that were to transform the entire nature and scope of algebra.
The book is written in three parts. Part I offers an overview of the period from Cardano to Newton (1545–1707) and is arranged chronologically. Part II covers the period from Newton to Lagrange (1707–1770) and treats the material according to key themes. Part III is a brief account of the aftermath of the discoveries made in the 1770s. The book attempts throughout to capture the reality of mathematical discovery by inviting the reader to follow in the footsteps of the authors themselves.

Galois and his groups

Peter M. Neumann (Oxford, UK)

When Évariste Galois died aged 20 in 1832, shot in a mysterious early-morning duel, he had already created mathematics which, in the context of its time, was of such extraordinary novelty that experienced academicians failed to understand it. After his main manuscripts were published by Liouville in 1846, however, his name was soon immortalised by its use in the terms 'Galois Theory' and 'Galois groups'.

This article, which has been written to celebrate the 200th anniversary of his birth, focuses on a study of his relationship with his groups: how Galois defined them; how he used them; what he knew about them; and his inventiveness. It is conceived as a contribution to the history of mathematics but with a mathematical readership primarily in mind.

1 Introduction

Évariste Galois (1811–1832), who died on 31 May 1832 after being shot in a mysterious early-morning duel the previous day, was described by one of his biographers as a 'Révolutionnaire et Géomètre' (Dalmas [13]). As a republican and a revolutionary he was passionate but not – so far as I read the evidence – a great success. He was, however, a *géomètre révolutionnaire*, a revolutionary mathematician. His great contributions to mathematics were the invention of Galois Theory and a theory of groups. He created groups as a tool for his study of the theory of equations. Having done so, he went further and began to study them as objects of interest in their own right, that is to say, he embarked on a theory of groups. Galois was not alone in this. Cauchy invented his version of groups, and instituted a theory of them, about 16 years later, in 1845. Although they had some points in common, the discoveries of Galois and of Cauchy occurred in quite different contexts and were almost certainly independent. It is the former that are to be the focus of this article, which is devoted to a detailed study of groups in Galois' writings, together with an assessment of the originality of his ideas about them.

2 Context: Évariste Galois and his mathematical writings

When Galois' 'Mémoire sur les conditions de résolubilité des équations par radicaux' (Memoir on the conditions for solubility of equations by radicals – a charmingly ambiguous title in English), familiarly known as the *Premier Mémoire*, was published by Joseph Liouville in 1846, it changed the direction of algebra, transforming the Theory of Equations from its classical form into what is now known as Galois Theory, a major branch of 'modern' or 'abstract' algebra that is taught as an advanced option in many university courses in pure mathematics. Famously, he spent the eve of the fatal duel organising and correcting some of his papers and writing a long letter, now known as the *Lettre testamentaire*, to his friend Auguste Chevalier. In it he summarised his work, announcing discoveries that go considerably beyond what he had got around to writing up. He also, in effect, appointed Chevalier as his literary executor and it was Chevalier who published (at Galois' express request) the testamentary letter [11] in September 1832, took charge of the manuscripts that Galois had left behind, copied many of them and in 1843 gave them to Joseph Liouville who, three years later, published an edition [24] of the 'Œuvres mathématiques d'Évariste Galois'. Some comments on the long silent period from 1832 to 1846, ended by the sudden explosion of interest in Galois' work that was sparked by its publication in 1846, may be found in § VII.2 of [30].

As a reminder, and for context and reference, here is a brief chronology of Galois' short and somewhat tormented life:

25 October 1811: Évariste Galois born in Bourg-la-Reine, a town (now a suburb) about 10 km south of the centre of Paris, the second of three children born to Nicolas-Gabriel Galois and his wife Adelaïde-Marie (née Demante).

6 October 1823: Entered the Collège Louis-le-Grand. His six-year stay there started well but ended badly.

August 1828: Failed to gain entrance to the École Polytechnique.

April 1829: Aged 17, had his first article (on continued fractions) published in Gergonne's *Annales de Mathématiques*.

25 May and 1 June 1829: Submitted, through Cauchy, a pair of articles containing algebraic research to the Académie des Sciences in Paris. Poinsot and Cauchy were nominated as referees. The manuscripts are now lost; René Taton published evidence in [37] that Galois probably withdrew them in January 1830.

2 July 1829: Suicide of Évariste's father Nicolas-Gabriel Galois.

July or August 1829: Second and final failure to gain entrance to the École Polytechnique.

November 1829: Entered the École Préparatoire, as the École Normale (later, since 1845, the École Normale Supérieure) was briefly called at that time.

February 1830: Re-submitted his work on equations to the Académie des Sciences in competition for the Grand Prix de Mathématiques. His manuscript was lost by the academy. The prize was awarded jointly to Abel (posthumously) and Jacobi for their work on elliptic functions.

April–June 1830: Had three items published in Férussac's *Bulletin*. One 'Sur la théorie des nombres' was (and is) of great originality and importance. Wrote the unfinished draft 'Des équations primitives qui sont solubles par radicaux', now known as the *Second Mémoire*.

December 1830: Another item published in Gergonne's *Annales*.

4 January 1831: Official confirmation of his provisional expulsion from the École Préparatoire in December 1830.

17 January 1831: Submitted his 'Mémoire sur les conditions de résolubilité des équations par radicaux', now often known as the *Premier Mémoire*, to the Académie des Sciences. It was given to Lacroix and Poisson to be examined.

10 May 1831: Arrested for offensive political behaviour; acquitted on 15 June 1831.

4 July 1831: Poisson, on behalf of Lacroix and himself, reported back negatively on the 'Mémoire sur les conditions de résolubilité des équations par radicaux'.

14 July 1831: Arrested on the Pont-neuf during a Bastille Day republican demonstration. Held in the Sainte-Pélagie prison.

23 October 1831: Convicted of carrying firearms and wearing a banned uniform; sentenced to six months further imprisonment.

16 March 1832: Released from Sainte-Pélagie prison during an outbreak of cholera in Paris and sent to live in the 'maison de santé du Sieur Faultrier', a sort of safe house.

Late May 1832: Mysteriously engaged to duel. There is little evidence and much contradictory conjecture as to by whom and about what.

29 May 1832: Wrote his *Lettre testamentaire* addressed to his friend Auguste Chevalier and revised some of his manuscripts.

30–31 May 1832: Shot in an early-morning duel; died a day later in Paris.

Readers interested in more detail about Galois' life are referred to one of the many published biographies, of which [12, 13, 14, 32, 38] are just a few. Accounts, though in overall agreement, do not coincide in all details. That is not surprising. Galois died too young to leave a rich supply of evidence to posterity and much of what survives contains contradictions.

Here is a brief summary of Galois' mathematical work. Again, the interested reader is referred elsewhere to the editions by Bourgne & Azra [4] and myself [30] for more detail. The following are the major items:

(1) The article 'Sur la théorie des nombres', published in the June 1830 issue of Férussac's *Bulletin des sciences mathématiques, physiques et chimiques*. This introduced what used to be (and perhaps still are) called 'Galois Fields'; it contains a precursor of the theory of finite fields in relatively concrete (as opposed to 'abstract' or 'axiomatic') form, including most of the salient facts.

(2) The 'Mémoire sur les conditions de résolubilité des équations par radicaux', known as the *Premier Mémoire*. This article was submitted to the Académie des Sciences in Paris in January 1831. It was rejected (on the basis of a fair and rational if unfortunately non-prescient report) on 4 July 1831 and the manuscript was returned to Galois. It introduced what is now known as 'Galois Theory', the modern version of the Theory of Equations that goes far beyond equations and Galois' own presentation of his new ideas into the theory of fields, field extensions and their automorphism groups.

(3) The manuscript entitled 'Des équations primitives qui sont solubles par radicaux', known as the *Second Mémoire*. This is an unfinished first draft, probably written in June 1830, of an article that, in effect, develops the theory of groups, a theory that had been introduced in the *Premier Mémoire* as a tool for studying solubility of equations by radicals.

(4) The letter to Auguste Chevalier dated 'Paris, le 29 Mai 1832', known as the *Lettre testamentaire*. As has already been mentioned, it was first published (at Galois' express request) in September 1832 in the *Revue Encyclopédique*. It has been republished many times since.

Besides item (1) there were four other mathematical articles published when Galois was 17 or 18 years old; they are respectable but not revolutionary. And besides items (2)–(4) there were a number of minor manuscripts and scraps containing jottings and odd calculations in the material collected from Galois' room after his death. The manuscripts are now held in the library of the Institut de France in Paris, catalogued as Ms 2108. In June 2011 digital images were placed on the web – see [17].

3 Context: groups now and then

To most mathematicians born and bred in the 20th century the word group conjures up something abstract defined by axioms. But the word abstract has very little meaning on its own. Is there any difference between a group and an abstract group? Is not a group nowadays simply a model of the first-order theory of groups? What is the force of the adjective? Context matters: for example, numbers may be thought of as abstractions, yet additive or multiplicative groups of numbers are thought of as 'concrete' realisations of groups as defined by axioms. To take an example mentioned above, Galois Fields, as created in Galois' 1830 paper 'Sur la théorie des nombres', though to most eyes very abstract entities, are 'concrete' realisations of the notion of a finite field as described by the axioms that came along just over 60 years later. Time also matters: language evolves and the word 'abstract', in common with many other familiar terms, has changed its meaning with time. For example, in the first half of the 20th century an 'abstract group' was often a group described by generators and relations.

I have heard the terms 'modern algebra', 'abstract algebra', 'axiomatic algebra' used more or less synonymously. That seems to distort in two directions. First, focusing just on groups, and setting aside rings, fields, vector spaces, etc., it distorts what they are in common mathematical usage. Although the group axioms are excellently clear and precise for describing groups in general by the properties of their multiplication tables, it is very rare that it is the details of the actual multiplication of elements of a group that matter in mathematics. What we care about in algebra, in number theory, in geometry, in quantum mechanics, indeed in almost any of the areas where finite groups play a role, is the subgroups of our group and their cosets, its conjugacy classes, its actions on its conjugacy classes or on coset spaces of subgroups, and its complex or modular linear representations. For infinite groups it is the actions on graphs or metric spaces or topological structures that give us our feeling for them. The elements themselves and the laws describing the detail of how they are to be multiplied rarely give us much insight.

The axiomatic description of groups, which makes the theory of groups so general, which pins down a common understanding of what a group should be in basic terms, and which led to wonderful progress in pedagogy – so that groups could be introduced to schoolchildren in the latter half of the 20th century whereas before they had been confined to high level university courses – came long after a sophisticated theory of groups was already established. Mathematics is like that. Discoveries in calculus or analysis between perhaps 1650 and 1850 far outran the critical tools used from about 1750 to 1900 by mathematicians, logicians and philosophers to set the subject onto a sound logical basis, with clear understanding of how real (and complex) numbers can be usefully described, what we can usefully agree a function to be, what are continuity, differentiability, integrability and the like. Similarly, huge progress was made in algebraic geometry long before Zariski, van der Waerden, Weil, Grothendieck, Serre (and perhaps others) made great efforts to create firm foundations for the subject.

Group theory is no different. My point is this. If in what follows you find Galois' concept and treatment of groups rather special, if not outright weird, then please remember *amice lector* that he was a pioneer. Groups did not exist before his time. He created them for himself. Between his groups and ours lies a century and a half of development by hundreds of mathematicians, many of whom were thinkers and teachers of the first rank.

4 The emergence of groups in the writings of Galois

Let us begin with groups as they appeared in the *Premier Mémoire* in the form that it was submitted to the Paris academy in January 1831. This is the manuscript that, as reported on 4 July that year, stumped the referees Lacroix and Poisson – though the evidence suggests that Poisson took a lead role and quite possibly Lacroix put little effort into trying to read the paper. Although for what follows the reader is invited to have Dossier 1 of the manuscripts [17], or one of the editions [4, 30], open at the relevant page, I shall try to make this account self-contained. I shall, however, suppress historical instinct and quote Galois in translation. The original French may be seen in the sources cited above.

Groups first appear in the statement of Galois' Proposition I, which is as follows:

Theorem. *Let an equation be given of which the m roots are* a, b, c, ... *There will always be a group of permutations of the letters* a, b, c, ... *which will enjoy the following property:*

(1) *that every function of the roots invariant under the substitutions of this group will be rationally known;*
(2) *conversely, that every function of the roots that is rationally determinable will be invariant under the substitutions.*

Here it is – a great and rare moment in mathematics: great because it is essentially the point at which groups are introduced (we shall return to that weasel word 'essentially' below); rare because it is not often that clear defining moments for mathematical concepts can be identified. Mostly, mathematical definitions and theorems emerge from a long period of evolution and refinement.

Notice that the statement refers to a group of permutations and that this group has substitutions. The word *permutation* is ambiguous in French, as it is in English. In English school syllabuses the word 'permutation' in the phrase 'permutations and combinations' refers to an arrangement of symbols. In undergraduate mathematics it acquires a second meaning as a bijection of a set to itself. Thus it is used to mean a (static) arrangement and also to mean an act of (dynamic) rearrangement. Indeed, in an article [7] published in 1815 (though written three years earlier) and in a long series of articles [8] written and published in 1845 (see Neumann [26]), A.-L. Cauchy used the nouns *permutation* and *arrangement* as synonyms even though he used the verb-form *permuter* 'to permute' in their titles. The word *substitution*, on the other hand, is quite unambiguous. It always means the act of rearranging, that is to say in modern terms a bijective mapping, a permutation. The ambiguity in the word 'permutation' is going to give some trouble and although the meaning in any given instance will usually be clear from the context, there are points where Galois confused the two meanings and great care is required in interpreting what he wrote.

In the writings of Galois, a group of permutations is a collection, in the first instance a list, of arrangements (of the roots a, b, c, ... of an equation) to which is associated a collection of substitutions. The substitutions are those that change the first arrangement in the list to itself or to any one of the others. In the January 1831 version of the *Premier Mémoire* this rather primitive information about groups emerges during the proof (which will not be repeated here) of Proposition I. Moreover, the small and natural next step of calling the collection of substitutions corresponding to the group of permutations (arrangements) a 'group of substitutions' was not explicitly taken there, nor indeed, rather surprisingly, anywhere else in the paper, though one senses that it lies just below the surface, ready to pop up when relevant. There were two points to help the reader. First, following his proof of this theorem Galois appended some explanation as follows:

Scholium. *It is clear that in the group of permutations which is discussed here, the disposition of the letters is not at all relevant, but only the* substitutions *of letters by which one passes from one permutation to another.*

Thus a first permutation may be given arbitrarily, and then the other permutations may always be deduced by the same substitutions of letters. The new group formed in this way will evidently enjoy the same properties as the first, because in the preceding theorem, nothing matters other than substitutions of letters that one may make in the functions.

Secondly, the reader who was not already stymied and could proceed beyond Proposition I would find his understanding growing naturally as he worked through the systematic use of groups of permutations and their associated substitutions in the further development of the theory. The next steps are theorems that describe what happens when one root of an auxiliary equation is adjoined to the domain of known quantities or what happens when all roots of an auxiliary equation are adjoined to the domain of known quantities, theorems which have been transformed over time into what is now known as the Fundamental Theorem of Galois Theory. There is some faint evidence that perhaps Poisson did not get this far – see [30, Note 14 to Ch. IV] – and certainly not as far as what comes later, namely the formulation in terms of its group of a necessary and sufficient condition for solubility of an equation by radicals, followed by the special case of irreducible equations of prime degree.

Later readers were in a happier position than the academy referees. On the eve of the fatal duel in May 1832, Galois added his famous explicit definition, essentially an amplification of the scholium quoted above, as one of the many emendations he made to the manuscript. This is included in all published editions of the *Premier Mémoire* from 1846 (Liouville) onwards. In the manuscript it appears in the margin against Proposition I, accompanied by an instruction to move it back to the introductory page of definitions. Here it is in translation:

> Substitutions are the passage from one permutation to another.
>
> The permutation from which one starts in order to indicate substitutions is completely arbitrary, as far as functions are concerned, for there is no reason at all why a letter should occupy one place rather than another in a function of several letters.
>
> Nevertheless, since it is impossible to grasp the idea of a substitution without that of a permutation, we will make frequent use of permutations in our language, and we shall not consider substitutions other than as the passage from one permutation to another.
>
> When we wish to group some substitutions we make them all begin from one and the same permutation.
>
> As the concern is always with questions where the original disposition of the letters has no influence, in the groups that we will consider one must have the same substitutions whichever permutation it is from which one starts. Therefore, if in such a group one has substitutions S and T, one is sure to have the substitution ST.

Here, now, we have groups of substitutions. Moreover we have explicit recognition of the closure property, which, in the pristine state of the *Premier Mémoire*, had remained implicit. Given that associativity is automatic in the context of composition of substitutions, and given that identity and inverse follow from closure since the sets involved are finite, what we have here are substitution groups, that is to say, what are nowadays called permutation groups.

There is of course much about groups in other writings by Galois but it was the *Premier Mémoire* that made most impact as a result of the 1846 publication of his main works in [24]. It was the *Premier Mémoire* therefore that was the article through which Galois made his contribution to the introduction of groups (and that word for them) into mathematics. Others made contributions too. Notably, A.-L. Cauchy introduced them in [8, 9], a year earlier (1845) as far as publication goes, as his *systèmes de substitutions conjuguées* (which I translate as 'systems of conjoined substitutions' for reasons that are explained in [29]). And once groups were established, that is to say by about 1870, mathematicians looked back and recognised that there were concepts of number theory and of geometry that could usefully be described as being groups; likewise, as expounded in [35], early mathematical crystallographers had lists that could be reinterpreted as groups once that concept was established. Every time this happened the theory of groups grew (by more than mere accretion) in breadth and depth and richness. The reader is referred to the famous account by Wussing [39] (whose title puzzles me, however, because I do not understand what would change if the adjective 'abstract' were deleted) or to my different and more limited account [27] of one aspect of the development of the theory of groups in the 19th century.

5 Dating Galois' invention of groups

Probably the *Premier Mémoire* was written early in 1831: its foreword is signed and dated 16 January 1831; according to the academy minutes the paper was received there the next day. In spite of this strong evidence there remains a little doubt because there is a discrepancy between this and the dating of the 'Discours Préliminaire' [17, Dossier 9] (see [30, p. 209] for a paragraph drawing attention to this problem). We can, however, be sure that the article was written no more than a few months earlier at the very most.

To create the *Premier Mémoire*, presumably Galois reconstructed from memory the article that he had submitted to the academy 11 months earlier to compete for the 1830 *Grand Prix de Mathématiques* and which had been lost. Of course it would be rash to conjecture that the presentation of February 1830 was the same as that of January 1831 but his memory would have been supported both by his deep understanding of the mathematics he had created and also by some fragmentary manuscripts such as those now found in Dossiers 6, 7 and 16 (see [17], [4, pp. 88–109], and [30, Ch. VI, §§ 1, 2, 11]). Thus it seems a safe assumption that the mathematical content of the lost manuscript of February 1830 would have been much the same as that of the extant *Premier Mémoire*.

We know nothing about the first version of the material submitted to the academy on 25 May and 1 June 1829 except that it came in two parts. The first is described rather vaguely in the academy minutes as algebraic research (though it is quite possible that Galois had himself given it the title 'Des Recherches algébriques'). By way of contrast, the second is specified in the minutes as being entitled 'Recherches sur les équations du degré premier' [Research on equations of prime degree]. The *Premier Mémoire* breaks naturally into two such parts: the first occupies folios 1–5 and finishes nicely with the example of how the Galois group decomposes in the case of the general equation of degree 4; the second begins on a new page with the header 'Application aux Équations irréductibles de degré premier' [Application to irreducible equations of prime degree]. It seems not unnatural to conjecture that, even if the presentation differed in some respects, at least the content of the 1829 version of the work was similar to that which has come down to us from the January 1831 academy submission. With high probability therefore we can date the creation by Galois of his groups to May 1829.

Although it is very rare that one can pin down with such precision the date of the creation of a mathematical concept, coincidentally, we can do the same with Cauchy's version of groups, his 'systèmes de substitutions conjuguées' mentioned above. As is shown in [26], he created them in September 1845. Was Cauchy influenced by having had the Galois manuscripts of 1829 in his hands for seven months? The evidence suggests not. Although their purposes were loosely related through the approach to the theory of equations initiated in the great 1770/71 paper by Lagrange [23], Cauchy's invention was made in order to tackle the combinatorial question of how many different functions can be obtained from a given function of n variables by permutation of those variables, whereas Galois created them 16 years earlier for direct application to equations. Cauchy used very different language from that of Galois (1829–32). Moreover, Cauchy's attitude in his writings of 1845 seems very different from that of Galois – but of course assessment of attitude is too reader-subjective to have any proper evidential value.

I wrote above that the statement of the theorem that is Galois' Proposition I is essentially the point at which groups are introduced to mathematics and I promised to return to the qualifier 'essentially'. It seems to me that, broadly speaking there are four principal steps to invention in mathematics:

> first comes the idea;
>
> next the formulation or capture of that idea in writing;
>
> third, its publication,
>
> and finally, its acceptance by the mathematical community, followed by gradual refinement and development.

As the preceding discussion was intended to show, Galois almost certainly had originated his idea of a group by May 1829 and he had written it down and submitted it to the academy in Paris straightaway. Presumably he rewrote it in February 1830 for resubmission to the academy; a few months later he wrote more about groups in his *Second Mémoire* and in various other fragments of manuscript that survive; late in 1830 or in the first two weeks of January 1831 he drafted his main work for the third time (the extant *Premier Mémoire*); finally, on 29 May 1832, the eve of the fatal duel, he wrote again about groups in his *Lettre testamentaire*.

What about publication of Galois' idea? There is a printed reference to it already in 1830 in Galois [16, p. 435]. It follows an explicit description of the 1-dimensional affine semilinear transformations of the Galois Field $\mathrm{GF}(p^\nu)$ as transformations of the form $k \mapsto (ak+b)^{p^r}$ (or, equally, of one of the forms $k \mapsto a'k^{p^r} + b'$ or $a''(k+b'')^{p^r}$). These are the members of the substitution group now known as $\mathrm{A\Gamma L}(1, p^\nu)$. The reference, with the second instance of the word 'permutations' corrected to 'substitutions', is this:

> Thus for each number of the form p^ν, one may form a group of permutations such that every function of the roots invariant under its substitutions will have to admit a rational value when the equation of degree p^ν is primitive and soluble by radicals.

There are many references to groups in the *Lettre testamentaire* [11]. But neither the 1830 paragraph quoted above nor the 1832 publication of the letter had any influence. Divorced from context they could not possibly have been understood at the time. The 1846 publication of the *Premier Mémoire* in [24] must count for the third of the steps listed above.

As for the fourth step of acceptance and development, well, it seems clear that Liouville had captured Galois' idea even before publication (though he did not contribute anything of his own to its development – see Lützen [25]); in Italy, Enrico Betti [2, 3] was developing the idea by 1851; in 1854, Cayley famously tried to pin down a general concept of group in [10] (referring in a footnote to Galois for the word 'group') and if the outcome was not mathematically a great success (see [27]), Cayley's later influence in getting mathematicians interested in groups (if not in Galois Theory, with which he never seems to have come to terms) was great; between 1856 and 1858 Dedekind lectured on groups and Galois Theory in Göttingen (see Scharlau [34]); and in the 1860s, Camille Jordan vigorously developed Galois' ideas in work which culminated in the great *Traité des Substitutions et des Équations Algébriques* [22] of 1870. For a fuller picture readers are referred to the sources cited in [30, Ch. I, § 5], but to summarise briefly: Galois' idea took wing soon after its 1846 publication.

6 What Galois did with groups in the Premier Mémoire

It is one thing to invent something new, quite another to show its utility and its richness. In the writings of Galois his groups had an immediate use in the Theory of Equations. That is, of course, what he invented them for; they were what we now call Galois groups. He proved several facts about them and seems to have known others (for which explicit proof is not given) instinctively.

First, coset decompositions in all but name. Proposition II of the *Premier Mémoire* may be expressed as follows. Let $f(x) = 0$ be a polynomial equation with distinct roots a, b, c, ... and let G be its group of arrangements (with, say, $a\,b\,c\,\ldots$ as the first of them). Let H be the corresponding group when a root of an auxiliary equation has been adjoined to the domain of rationally known quantities (in modern parlance the base field). Then G will be partitioned as $H + HS + HS' + \cdots$ for suitable substitutions S, S', ... (for $+$ read $\cup$ of course). Then Proposition III is the information that if not just one but all the roots of an auxiliary equation are adjoined then the groups $HS^{(i)}$ will all have the same substitutions. Think of it like this. Let $X \leqslant \mathrm{Sym}(n)$ be the group of substitutions of G and Y the group of substitutions of H, a subgroup of X in modern sense. Thus if A is the starting arrangement $a\,b\,c\,\ldots$ then $G = \{AU \mid U \in X\}$ and $H = \{AU \mid U \in Y\}$. The group of substitutions of HS will be $S^{-1}YS$, of HS' will be $S'^{-1}YS'$, and so on. Thus in the case of Proposition III the subgroup Y of X has the property that $U^{-1}YU = Y$ for any U in X, so it is normal in X in our modern sense. All this is summarised neatly and clearly, and slightly extended, in the *Lettre testamentaire*:

> According to Propositions II and III of the first memoir one sees a great difference between adjoining to an equation one of the roots of an auxiliary equation or adjoining them all.
>
> In both cases the group of the equation is partitioned by the adjunction into groups such that one passes from one to another by one and the same substitution; but the condition that these groups should have the same substitutions does not necessarily

> hold except in the second case. That is called a proper decomposition.
>
> In other words, when a group G contains another H, the group G can be partitioned into groups each of which is obtained by operating on the permutations of H with one and the same substitution, so that $G = H + HS + HS' + \cdots$. And also it can be decomposed into groups all of which have the same substitutions, so that $G = H + TH + T'H + \cdots$. These two kinds of decomposition do not ordinarily coincide. When they coincide the decomposition is said to be proper.
>
> It is easy to see that when the group of an equation is not susceptible of any proper decomposition one may transform the equation at will, and the groups of the transformed equations will always have the same number of permutations.
>
> When, on the contrary, the group of an equation is susceptible of a proper decomposition, so that it is partitioned into M groups of N permutations, one will be able to solve the given equation by means of two equations: the one will have a group of M permutations, the other one of N permutations.

Notice that here Galois introduced a technical term, *décomposition propre* (proper decomposition) and that it refers to decomposition into cosets of a normal subgroup or, equivalently, partition into cosets which are both left and right cosets.

Proposition V of the *Premier Mémoire* gives the criterion for solubility of an equation in terms of a structural property of its group. Again, it is neatly and effectively summarised in the passage in the *Lettre testamentaire* that continues after the one cited above:

> Therefore once one has effected on the group of an equation all possible proper decompositions on this group, one will arrive at groups which one will be able to transform, but in which the number of permutations will always be the same.
>
> If each of these groups has a prime number of permutations the equation will be soluble by radicals; if not, not.

This criterion, translated into modern terminology, is *an equation will be soluble by radicals if and only if its Galois group has a composition series all of whose factors are of prime order*. Although this says everything that needed to be said, what is missing here by comparison with modern treatments is the notion of quotient group and any form of Jordan–Hölder Theorem. An adequate though weak version of the latter was supplied by Camille Jordan and appears in his *Traité* [22, §§ 54–59]; it was refined to the modern form in 1899 by Otto Hölder [20], who invented quotient groups for the purpose. For amplification of these points see Nicholson [31] and my review of a reprint of Jordan's *Traité* in *Mathematical Reviews* (1994). It should be clear, however, that there is no real need for any of these later developments for the purpose that Galois had in view. His use of his groups here was self-contained and decisive.

There are two more group theoretic nuggets that can be mined from later parts of the *Premier Mémoire*: first a relatively small one, the working out of decompositions, essentially of composition series, for the symmetric group Sym(4); secondly, there is the important theorem to the following effect (see [17, Folio 6 verso], [4, p. 67], [30, p. 129]):

> *If an irreducible equation of prime degree is soluble by radicals, the group of this equation must contain only substitutions of the form*
>
> $$x_k \mapsto x_{ak+b}\,,$$
>
> *a and b being constants.*

Here a, k, b are to be read as integers modulo the prime number p which is the degree of the equation, a is not 0 modulo p, and the roots of the equation have been suitably labelled $x_0, x_1, \ldots, x_{p-1}$. In the language of modern group theory, what Galois showed was that if G is a soluble transitive subgroup of the symmetric group of prime degree p then G is conjugate in the symmetric group to a subgroup of AGL$(1, p)$; he showed also that any such subgroup is soluble. He then reformulated his discoveries as the theorem:

> *In order that an irreducible equation of prime degree should be soluble by radicals, it is necessary and sufficient that any two of its roots being known, the others may be deduced from them rationally.*

This is Proposition VIII of the *Premier Mémoire*. Galois was sufficiently proud of it that its statement was announced in the foreword to the memoir. In anachronistic terms it is the theorem that a transitive permutation group of prime degree is soluble if and only if it is a Frobenius group, that is, the stabiliser of any two points is trivial.

7 What Galois did with groups elsewhere in his writings

The insights exhibited in the *Premier Mémoire* were presumably to be found in the lost paper of February 1830 and probably also in the two articles submitted in May and June 1829. Groups and their theory became the main focus of the *Second Mémoire*, which is dated to June 1830 by Robert Bourgne [4, p. 494] (though comparison with Galois [15] suggests that it might possibly have been written a month or two earlier than that). Although it is ostensibly about primitive equations that are soluble by radicals, in fact equations play a minor role and the paper quickly turns into a study of group theory. It contains a number of false starts, obscurities and slips, and it tails away inconsequentially. It is very much a first draft and an incomplete one at that. Nevertheless it is an exciting, if difficult, document. Roughly it may be seen as contributing three significant points: the classification of equations and groups as non-primitive or primitive; the theorem that a primitive soluble group (or equation) has prime-power degree; and a detailed but incomplete study of the groups AGL$(2, p)$, GL$(2, p)$, PGL$(2, p)$, PSL$(2, p)$. Let's look at these in turn.

The definition that Galois gave (on four separate occasions) for what he meant by his word *primitif* is ambiguous. In modern usage a permutation group (that is, a substitution group) is said to be *primitive* if it is transitive and there are no non-trivial proper invariant equivalence relations on the permuted set; it is said to be *quasi-primitive* if every non-trivial normal subgroup is transitive. A group is primitive if it is transitive and a one-point stabiliser (an isotropy subgroup) is maximal amongst proper subgroups. Every primitive group is quasi-primitive but there are quasi-primitive groups that are not primitive – think for example of a non-cyclic simple group acting on itself by right (or left) multiplication: such an action is quasi-primitive but it is not primitive. Line by line reading of the first few pages of the *Second Mémoire* and lengthy discussion of the arguments written there led me in [28] to the conclusion that what Galois probably had in mind was what we now call quasi-primitivity. As it happens, however, a soluble permutation group is primitive if and only if it is quasi-primitive. Therefore Galois' conclusions are unaffected by the ambiguity in his definition. Unfortunately, his arguments are not. Nevertheless, the idea of primitivity was picked up by Camille Jordan who, in papers in the 1860s and in his *Traité* [22], showed its great importance in group theory and developed it extensively.

The fundamental fact about primitive soluble groups is this. Let G be such a group acting on the set Ω. Then $|\Omega| = p^\nu$ for some prime number p and some positive integer ν. Moreover, Ω may be identified with a ν-dimensional vector space V over the prime field $\mathbb{Z}/p\mathbb{Z}$ in such a way that all the permutations (substitutions) of G take the form $v \mapsto Av + b$, where $A : V \to V$ is linear and invertible and $b \in V$. In other words, G is similar to a subgroup of the affine group $\mathrm{AGL}(\nu, p)$ acting in the natural way on V. Although his language is different it is clear that Galois knew this. The first part of the *Second Mémoire* is devoted to a proof that the degree is a prime-power; the theorem itself was announced in his published abstract [15]. Although Galois did not complete a proof that his group is similar to an affine group, one can see him struggling towards this insight in the middle part of the memoir. As in the case of the *Premier Mémoire*, by the time he came to write his *Lettre testamentaire* Galois was able to summarise the facts briefly and very clearly: the fact that the group is similar to an affine group is clearly stated there (see [17, Folio 8 verso], [4, p. 175], [30, p. 86]). Moreover, he proceeded to give the order of $\mathrm{AGL}(\nu, p)$ as $p^\nu(p^\nu - 1)(p^\nu - p) \cdots (p^\nu - p^{\nu-1})$. When Camille Jordan first proved this in Chapter V of his doctoral thesis of 1859/60, submitted a few months later in slightly extended form in competition for the 1860 Paris Academy Grand Prix de Mathématiques, and published in 1861 as [21], it took him eight pages to do so – and even then he did not have the notation or technique to write the proof down in its full generality.

As listed above, the third item of group theory in the *Second Mémoire* is a study of 2-dimensional linear groups over the prime field. One can see Galois embarking on a search for the subgroups of $\mathrm{GL}(2, p)$, especially the soluble ones, but some of the arguments are obscure, some are not quite right and the calculations peter out with a promise, never fulfilled, to continue. His interest in these groups came partly from $\nu = 2$ being the first 'non-trivial' case of his general theory and partly from an interest in the 'modular equation', the equation of degree $p + 1$ to which the equation of degree p^2 that gives the p-division points of elliptic functions can be reduced. In this connection he announced in his 1830 abstract [15] that the modular equation of degree 6 (related to quintisection of elliptic functions) can be reduced to one of degree 5, an assertion which is equivalent to the group-theoretic fact that $\mathrm{PSL}(2, 5)$ has a subgroup of index 5; he also announced incorrectly that the analogous assertion for a prime p is false if $p > 5$. Although, as was indicated above, the *Second Mémoire* peters out somewhat ineffectually, Galois must nevertheless have continued thinking and calculating along these lines because again the *Lettre testamentaire* contains some sophisticated information about the groups $\mathrm{PSL}(2, p)$. He announced there that for $p \geqslant 5$ these groups are simple (*groupes indécomposables*). He also corrected the statement in [15], announcing (and partly proving) that for $p \geqslant 5$ the groups $\mathrm{PSL}(2, p)$ have subgroups of index p if and only if $p = 5$, 7 or 11. Two points are surprising here. First that he should have gone so deep into group theory so quickly. This is sophisticated mathematics. Although some titbits from the *Second Mémoire* and the corresponding part of the *Lettre testamentaire* were dealt with piecemeal over the decade or so after the 1846 publication of [24], it was not really until Gierster's dissertation [19] appeared in 1881 that there was a complete and systematic account of what Galois' astonishing intuition had led him to. Secondly, there is something of a mystery in that these results require some non-trivial calculations but there is very little of any relevance in the many scraps and jottings that have come down to us. I have not yet undertaken a systematic search but even so, I find it surprising; I would have expected the necessary calculations to be visible even on a cursory reading of the extant material.

There are three further morsels of group-theoretic information about the writings of Galois that complete the picture. First, in [16] he claimed, in effect, that except in the cases $p^\nu = 9$ or 25, a primitive equation of degree p^ν is soluble by radicals if and only if its Galois group is similar to a subgroup of the 1-dimensional semilinear group $\mathrm{A\Gamma L}(1, p^\nu)$. This is quite wrong but it seems to me to be the sort of error that can only be made by a very clever and intuitive genius. The second morsel is this. In the *Lettre testamentaire* Galois claimed that the smallest number of permutations which can have an indecomposable group, when this number is not prime, is 5.4.3. In other words, the least possible composite order for a simple group is 60. How can Galois possibly have known this? I have written a few paragraphs on this point in [30, Ch. VI] and do not propose to repeat the argument here. Third, it is worth noting what is missing from Galois' writings. Nowhere did he treat alternating groups and prove that they are simple. Again, I have written a few paragraphs on this matter in [30, Ch. VI] and there is no cause to repeat them here.

8 Originality in the ideas of Galois

There are two substantial points to be made about the originality of Galois' ideas in relation to group theory (setting aside their application to equations and the invention of Galois Theory). First, Galois discovered or invented groups for himself. With the possible exception of Ruffini in 1799 (to be treated below), no mathematician had published anything like them before. In his fundamental paper [23] Lagrange had focused on a study of how functions of the roots of an equation behave under permutations (substitutions) of their arguments but the idea of considering collections of substitutions that are closed under composition is not there. Indeed, his proof [23, § 97] of 'Lagrange's Theorem', which at that time (1770/71) and for some 60 years thereafter was the assertion that the number of values of a function of n variables (that is, the number of different functions obtainable by permuting the variables) divides $n!$, is defective mainly because Lagrange had failed to notice the crucial fact that the collection of substitutions that leave a function invariant is closed. Cauchy, in [7] (1815), had developed a calculus of substitutions but nowhere did he consider closed collections of them. That came 30 years later in [8, 9]. In the many papers where Abel used substitutions he never found a need to consider closed collections of them – but this is not really relevant since it seems pretty certain that Galois did not know Abel's work until early 1830 and that he had had his ideas about groups already in 1829.

The exception mentioned above is Paolo Ruffini. Sadly, I am unable to read his work in the original and must rely on secondary sources such as [1, 5, 6] and the references they quote. There does not seem to be agreement about what Ruffini achieved or did not achieve. He seems to have had an insight about groups, at least in the case of subgroups of Sym(5). Unfortunately, his exposition was confused and confusing and in spite of his efforts he was unable to persuade the mathematical establishment to invest the effort required to understand his writings. Although Cauchy later wrote approvingly of Ruffini's proof of insolubility of the general quintic, I have found no evidence in his mathematical writings that he had properly understood even the strategy of Ruffini's argument, still less the details. The 1815 paper [7], which explicitly extends a result by Ruffini, would have been improved and made more efficient if Cauchy had at that time recognised the importance of closed sets of substitutions. When he did recognise that importance in this context and came to invent his *systèmes de substitutions conjuguées* in 1845 he made no acknowledgement of Ruffini and there is no trace of Ruffini's ideas in his papers. Thus if we measure Ruffini's work using the criteria proposed above, we see that although ideas were there, were captured in writing and were published, acceptance by the mathematical community followed by gradual refinement and development was signally missing. Referring specifically to Galois, there seems to have been no influence on him at all. There is just one mention of Ruffini in all of Galois' writings (see [17, Dossier 8, Folio 57 recto], [4, p. 33], [30, p. 204]) and that is in a context which gives me the impression that Galois was doing little more than mouthing conventional words.

The second major point about the originality of Galois' ideas in relation to group theory is this. His groups are sets of permutations (arrangements) and sets of substitutions. He gave these sets names such as G, he gave to subgroups (often called *groupes partiels*, 'partial groups', or *diviseurs*, 'divisors', especially in the *Second Mémoire*) names such as H and he manipulated these sets, comparing them and multiplying them by substitutions, for example, using such names. I have not understood how far Ruffini went with manipulating collections of substitutions but reading the secondary sources cited above I get a strong impression that he did not go nearly as far as this.

Gauss, in his *Disquisitiones arithmeticae* (1801), treated collections of objects in (at least) two different contexts. In §§ 223, 224 he divided binary quadratic forms into classes. Then in § 226 he divided the classes into orders and in §§ 228–231 the orders into genera. In § 249 he turned to composition of classes. Here the classes have single-letter names and + is used for their composition. That what Gauss had here are early instances of abelian groups became clear some 70 years later. He concentrated much less, however, on these collections of objects than on their significance for organising an understanding of binary quadratic forms, and I do not read the *Disquisitiones* as showing him studying them more deeply to elucidate their properties. Later, in § 343 of [18], Gauss had certain collections of roots of unity and gave both to the collection and to the sum of its members the name *periodus*, 'period'. So far as I can see, however, he always manipulated the period as a sum of roots of unity, never as a set.

It has been pointed out by Stedall in [36, p. 354] that Cauchy was highly innovative in his 1815 paper [7] where he introduced algebraic notation for arrangements and substitutions, and created a 'calculus of substitutions' to manipulate these objects and handle, for example, products and powers of substitutions. With the exception of Gauss in a different context and a different language, until then letters had been used to denote quantities (variable or fixed), functions, points, etc., nothing other than the 'classical' entities of numerical and spatial mathematics. I estimate that, in giving single letter names to his groups in order to be able not only to refer to them but also to manipulate them, Galois showed the same level of inventiveness, an originality that was highly sophisticated in the context of the mathematics of his time. His genius really was out of the ordinary – extraordinary in the proper sense of that word.

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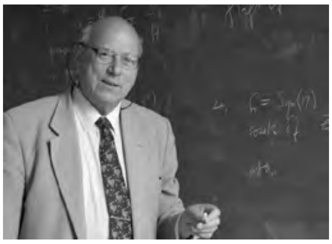

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Interview with Professor Constantin Corduneanu

Vasile Berinde (Baia Mare, Romania)

Professor Corduneanu as a plenary speaker at the "Alexandru Myller" Mathematical Seminar Centennial Conference, 21–26 June 2010, Iași, Romania

Short Biographical Note

Constantin Corduneanu is an Emeritus Professor at the University of Texas at Arlington, U.S.A. He was born on 26 July 1928 in Iași, Romania. He graduated at "Al. I. Cuza" University of Iași (UAIC), Faculty of Mathematics, in 1951 and obtained his PhD in mathematics (1956) at the same university. Besides his usual duties as a professor, he had many other activities, such as participating in various national or international conferences (more than 100), paying short visits and talking about his research work in over 60 universities or institutes on all the continents except Australia and in over 20 countries (including Russia, Ukraine, Germany, England, France, Italy, China, Japan, Hungary, Poland, Portugal and Chile). During the last 59 years he has published about 200 research papers, including six books in a total of 13 editions (Romanian Academy, Academic Press, Springer, Cambridge University Press, Taylor and Francis, John Wiley & Sons, Allyn & Bacon). His association with UAIC lasted until 1977, a period in which he held positions of assistant, lecturer, associate professor, professor, Dean of Mathematics and Vice-Rector for Research and Graduate Studies, as well as some research positions with the Mathematical Institute of the Romanian Academy in Iași. He has also served, on different occasions, the Iași Polytechnic Institute and for three years the newly created institution which is known today as the University of Suceava (where he has also served as Rector over the period 1966–1967). He is the founding Editor of *Libertas Mathematica* and a corresponding Member of the Romanian Academy.

Tell us about your mathematical education and the very beginnings of your scientific career.

My mathematical education has known several periods, each with a certain specificity. During my secondary education (1940–1947) in Iași and Predeal, I had the privilege to be taught by two distinguished teachers, both of them being trained and occupying positions at the universities of Iași and Bucharest. The first one (Constantin Menciu) was for a good number of years an assistant (mechanics) with his Alma Mater in Iași. He occupied temporary positions as associate professor and was a really gifted pedagogue. When a colleague of mine visited his grave, the guardian of the cemetery told him that his was the most often visited grave (by his former students). The second teacher (Nicolae Donciu) also had academic experience at higher level, serving as assistant to one of the best known Romanian mathematicians Dimitrie Pompeiu. They encouraged and supported me to participate in the activities at *Gazeta Matematica*, including the participation at the competitions organised yearly by this publication and its supporters. I got the fifth prize in 1946 and the first prize in 1947. These teachers and my growing interest and knowledge in mathematics convinced me that my career should be dedicated to this discipline. And, in the Fall of 1947, I became a student at the University of Iași, taking mathematics as the subject of my studies. From 1947 until 1977, I was associated as student, teaching assistant, assistant, lecturer, associate professor, professor, dean and vice-rector with UAIC. I had very well educated professors, with PhD degrees or postdoctoral periods in Romania, Italy, France and Germany. The courses I had to take covered a wide area of mathematics, at the level achieved by this science before the Second World War. They included abstract algebra, real analysis, differential geometry (classic and Riemann spaces), mechanics, complex variables and many special topics (Fourier series, relativity, minimal surfaces, number theory, probability theory). A final year course on topological groups (following Pontrjagin's book – the English edition) prompted me to write my thesis, required for obtaining the Diploma of Licenntiate in Mathematics (something between a Bachelor's and a Master's degree), on "The group of automorphisms of a topological group". My first results (1950) to be published were part of my thesis. I defined a topology on the group of automorphisms in the case of what is called a bounded topological group (i.e., a topological group on which all Markov's seminorms are bounded). Therefore, I started my research activity exactly 60 years ago. But in 1953, feeling isolated with these preoccupations, I changed my field of research, moving to differential and related equations. After 63 years of campus life, I am still involved in research work and enjoying this kind of life. I believe that the academic communities constitute the best parts of this unsettled world. Of course, finishing my college studies, including the PhD degree obtained while teaching and doing research (Publish or Perish!), I did not consider my education as terminated. I had many opportunities to progress as a scientist by attending seminars, conferences, symposia, etc. In particular, I attended the seminar organised by A. Halanay at the Institute of Mathematics of the Romanian Academy (Bucharest) and, starting in 1957, I organised, helped by other colleagues from Iași, our seminar on "Qualitative Theory of Differential Equations". In 1961, I participated at the Congress of the International Union of Mechanical Sciences, organised by Academician Iurii Mitroploskii in Kiev, at which I met for the first time several well-known mathematicians from various countries: Solomon Lefschets came from the RIAS Institute he created in Baltimore, V. V. Nemytskii came from Moscow, Jack Hale came from RIAS and L. Cesari came from Purdue University. It was for me a memorable event, the chance of meeting for the first time in my life leading scientists who belong to my "Invisible College".

Your field of research, differential equations, is strongly represented at UAIC Iași. Could you tell us some of the history behind the Iași mathematical school, its present status and, maybe, speculate on its future?

The subject of differential equations is certainly rather vast, with ramifications, and is largely cultivated by the mathematical community. It starts, on solid ground, with Isaac Newton, who emphasised their importance in mechanics/astronomy, explaining with their aid why and how the planets move around the Sun. Nowadays, differential equations occur in many fields of knowledge, playing a leading role in explaining evolutionary phenomena from nature and society. Of course, I have in mind also their sisters and numerous application fields, and it seems to me that the old adage "Mundum regunt numeri" should be replaced by "Mundum regunt aequationes". Naturally, other classes of equations, like integral, integro-differential, functional, with differences, have their part to play in solving problems from science, engineering, biology, economics, etc. Concerning the beginnings of differential equations at UAIC, where this field of research took off only after World War II, I can mention several names who have been related to this process: A. Myller investigated problems in mechanics by means of integro-differential equations; C. Popovici, who studied astronomy in France, has been concerned with functional or functional differential equations, his results being quoted in a book of J. Peres; several professors of mechanics like V. Valcovici, Al. Sanielevici and I. Placinteanu have also embraced problems leading to both differential and partial differential equations; and G. Bratu (1881–1941), who moved to the University of Cluj in the 1920s, is known from what is called the "Bratu equation". In the period 1930–1945, D. Mangeron from the Iasi school of Mathematics, with his doctorate under M. Picone, has been active in partial differential equations and applications. Also, A. Haimovici has been involved in applications of DE or IE to problems in biology (in cooperation with medical researchers). The period after 1950, when the number of faculty grew considerably, both at the AUIC and the Technical University of Iași, as well as the number of researchers with the newly created Mathematical Institute "O. Mayer" of the Iași Branch of the Romanian Academy, is characterized by the formation of several groups/schools of research. Besides the traditional group dedicated to geometry, currently under the leadership of Radu Miron, continuing the work of A. Myller, O. Mayer, M. Haimovici, Ilie Popa and others in differential geometry, there is a second group under the leadership of Viorel Barbu, consisting of a good number of specialists in functional equations, control theory and related areas. This group includes I. Vrabie, Aurel Rascanu, Gh. Morosanu (now at the Central European University in Budapest), Catalin Popa, S. Anita, C. Zalinescu, O. Carja and others. Other groups, or Seminars as we used to call these associations, are in algebra, mechanics, mathematical analysis and operations research. With the opening of relations with Western Europe in the last 20 years, most people, faculty and researchers, had many opportunities to visit schools and research centres throughout Europe, making considerable progress in their work. Actually, a large number of them occupied positions in the West: two in Paris, one at Oxford and at least six of them in leading universities in the USA and Canada.

What is needed for a school or a tradition in mathematics to be established and to last?

I believe I was fortunate enough to attend and then belong to a mathematical school that has just celebrated its centennial, that is, AUIC, where a young professor, in 1910, was appointed as the Chair of Geometry. Of course, I have in mind the founder of the mathematical school in Iasi, the late Professor Al. Myller, who in 1906 defended his PhD thesis at the Georgia Augusta University of Goettingen. He spent three years there (1903–1906) and had as his teachers David Hilbert (also his thesis supervisor), Felix Klein, Hermann Minkowski and Karl Schwarzschild. Myller spent 37 years as a professor in Iasi and all his career has been guided by emulating what he saw while in Goettingen, keeping of course in mind the local conditions. Firstly, he founded the mathematical library, which did not exist as a unit when he came to Iasi. He took advantage of the fact that one of his professors at the University of Bucharest, where he obtained his Bachelor's degree, was now the Secretary of Education. Myller obtained from him consistent financial help and started to build up the library which nowadays counts almost one hundred thousand volumes (books and journals). This library, which carries his name, has been the place of training for five or six generations of mathematicians, spread in more than 10 countries. Secondly, the introduction of advanced courses, which currently we call graduate courses (at that time named free courses). Myller's colleagues at the university voluntarily taught advanced courses for young people interested in improving their knowledge of mathematical subjects. Myller was the "primus inter pares" to teach such courses without compensation, inspiring young attendants to get involved in mathematics and helping them to advance in the academic hierarchy. Nowadays, we regularly teach graduate courses in many universities in North America and the European Union. A century ago, this was not a frequent occurrence. Thirdly, Myller had to fight the conception that disciplines without laboratories do not need young assistants, or other types of auxiliary persons, in order to carry out academic activities (primarily teaching and research). After 10 years of perseverance, he obtained his first assistant O. Mayer, who later became his colleague at the university and at the Romanian Academy. I take the opportunity to mention that these three requirements for a group of scientists, to become a *school* in the broad academic sense, are still valid. Of course, we have to consider the progresses made in the last century with the change of information, like new electronic means including the Internet, as well as the almost recognised fact that any academic unit must contain young people capable of continuing the activities started by their educators. Last but not least, one has to consider the facilities of travel created by modern technology, which allow scientists to communicate even more efficiently than using the Internet. I could not conclude my answer better than saying that a spirit of congeniality should reign in any research group, if they want to be a school. We, the scientists, are competing daily against our peers. But this competition should not be transformed into a continuous contradictory opposition, which will finally lead to the dissolution of the group. Maybe, a really great school will generate, in such circumstances, several schools.

How was it possible for you to keep up your mathematical interest and research work when strongly involved in administration activities (Rector in Suceava, Vice-Rector of UAIC, etc.)?

During the last 60 years, I have been involved in research work and publishing but I've held administrative positions for only 14 years. For a few years I chaired what we called in Europe a "Chair" with general or applied mathematical profile. I have been a Dean for four years but the faculty was mainly mathematics and just a few in computer science. The Vice-Rector position was in charge of research and PhD programmes. That's why I did not feel a heavy pressure fulfilling my duties. And the period dedicated to administrative duties represents less than a quarter of my active life. I think a leader in an academic institution must be aware of the diverse aspects of the activities his colleagues are engaged in. In the U.S., there is a different perception of his role, the president being the person who represents the institution in front of people and Government (or Board of Trustees in case of private schools), while the Provost has to deal with people inside the school. I don't think there is a simple answer to this question. The result will depend very much on the abilities of the people involved.

How do you see the classical dichotomy between "pure" and "applied" mathematics, with special emphasis on your field of interest?

Indeed, in most branches of science, there exists what you are calling a "dichotomy" between pure and applied. Mathematics does not offer a counterexample and I believe that numerical analysis, control theory and mathematics of finance, to list only a few, are considered applied mathematics, while mathematical logic, number theory and the classical fields belonging to mathematics (geometry, analysis, algebra – "Les structures fondamentales de l'Analyse" according to Bourbaki) will be considered as pure mathematics. We know, and there are many examples, that there can't be a strict separation between "Applied" and "Pure". For instance, mathematical logic has applications in computer science, a field that some people were tempted to call "Engineering Mathematics". The part of mechanics known as kinematics would be inconceivable without geometry. One has to notice the fact that applications of various theories (hence pure mathematics) in other fields of science have generated new concepts and even theories, complementing the traditional ones. One can think to the algorithms and mathematical logic, or systems science. What is nowadays called population dynamics has generated a new chapter in differential equations. One talks about numerical linear algebra, which appeared quite recently. I would close my answer to this question with an episode which I found a long time ago, reading about the discussions in the Moscow Mathematical Society. One of the participants, presenting his opinion on this matter, expressed the idea that Soviet mathematicians are often involved in abstract research, while they avoid the applications of science in practice. The late, well-known mathematician I.G. Petrovskii intervened in the discussion, saying (somewhat paraphrasing): "If we'll be mainly concerned with applications, in short time we won't have anything to be applied." It was an act of courage at that time! As far as I am concerned, I would say that I dealt with "pure" mathematics when I investigated the "qualitative inequalities" in a paper in the *Journal of Differential Equations* and then I dealt with "applied" mathematics when I applied them to the Stability of Motion. I believe this situation is present in the case of many authors, except those that are "puristic". I am convinced that both "pure" and applied" mathematics will continue to enrich themselves, and expand successfully, remaining in conjunction.

You used earlier the term "Invisible College". Can you elaborate a bit more on its meaning?

Yes. To the best of my knowledge, this term was used for the first time in the 1960s by Professor de Solla Price (Columbia University of New York). He was concerned with the organisation of scientific research under the new conditions created by the fast development of education and research after the Second World War. He authored a study which appeared by Yale University Press under the title "Little Science, Big Science". I read this study in the early 1970s, finding incidentally a copy of the book in Romanian translation. According to de Solla Price, by Invisible College we should understand the group of researchers, regardless of their place of work and country, who are conducting research in the same field of specialisation. To provide an example, taken from the theme of our discussion, I would say that there was an Invisible College at the time I began being involved in ordinary differential equations and related topics, in the mid 1950s. Of course, a structure like this must be supported by a certain number of "pillars" and I was fortunate enough to get acquainted with several of them and read their books, which helped me to build up my career. Who were, in my perception, the "pillars" of the Invisible College I joined? Firstly, I found in the mathematical library of my Alma Mater the two volumes of G. Sansone's *Equazioni Differenziali nel Campo Reale*, which appeared in Bologna in 1948. I learnt from that book a lot more than you could get in a textbook dedicated to the subject. Besides this work of Sansone (who I had the chance to meet in Florence in 1965 and thereafter carried on many discussions with him), I found the book from Princeton University Press entitled *Qualitative Theory of Differential Equations*, authored by Stepanov and Niemytskii from Moscow University. This book was of great help in advancing my knowledge in the field of the modern theory of ordinary differential equations. I met several times Niemytskii in Moscow and Kiev while attending mathematical meetings. Discussing with him about the Fixed Point Method in proving existence of solutions to ordinary differential equations, he mentioned the fact that what we are calling the Contraction Mapping Principle in complete metric spaces was formulated by him in 1927 in Uspekhi Mat. Nauk, starting from Banach's paper which dealt with linear normed spaces. Furthermore, in the late 1950s and early 1960s, I had the chance to meet Tadeusz Wazewski from Krakow and received in Russian translation the books by Coddington-Levinson and S. Lefschetz. I had used their books and papers in my training as a member of the college and I had several occasions to meet these distinguished mathematicians. I consider them as "pillars" of my Invisible College.

Are you predominantly a researcher or a teacher?

After my retirement in 1996 from the University of Texas at Arlington, I can say that I am a researcher only. I do not have teaching, a preoccupation that kept me busy for 47 years. The only occasions I am still doing some "teaching" are when I am presenting my research results to meetings or, very seldom, to real students or other persons interested in the kind of topics I am concerned with. Before retirement, I can say that I was dividing my time, almost equally, between teaching and research. If the profile of the institution hiring you is teaching and research then you have to perform both kinds of activities. Formulated in a more dramatic fashion, you have to subject yourself to "Publish or Perish". During the first four years in academic life, I had to learn how to do research work in order to get my doctoral degree and keep my position at the university. Of course, I continued the research work, participating in seminars and attending various events that helped me to advance in this direction. This interest for progress in your field must remain alive for the rest of your career. Besides the seminars and conferences attended in Romania, I spent two months in Florence (Italy) with the Group of Functional Analysis and Applications (Professors G. Sansone and R. Conti were the leaders). Then, while visiting the University of Rhode Island, I had the exquisite opportunity to attend a course (Spring Semester 1968) given by S. Lefschetz at Brown University in Providence, coming weekly from Princeton. The course was on "History of Algebraic Geometry" but Lefschetz was talking about how his life was shaped by mathematics, what were the most relevant findings in this field, and much more. It was a delight to listen to Lefschetz and have the opportunity to discuss with him, asking him questions which he answered with wide accolades and pertinent references (sometimes with humour). Finally, I would like to mention that during my career, and continuing after retirement, I have enjoyed writing six mathematical books, mostly on courses at undergraduate or graduate level, which have had a good reception among peers and students. Writing such books, you combine both teaching and research skills and this fact tells me that I've been doing both activities simultaneously. I would conclude with the remark that I know very few people who have done only research work in their career but a large number who have performed only teaching duties (using textbooks written by other qualified persons).

What are you working on right now?

For the last 7–8 months, I have been involved with Almost Periodicity, both from the point of view of the general theory of various classes of Almost Periodic Functions, as well as applying the results to some classes of functional equations: ordinary differential equations, integral or integro-differentials, partial differential equations of hyperbolic type or other kind of equations encountered in physics or diverse applied fields. The study is concerned with a new classification of almost periodic functions in Besicowitch sense, more precisely with those for which the Parseval equality holds – something I came across almost 30 years ago but then postponed because I was concerned with other topics in functional equations (stability in the first place, asymptotic behaviour, control problems, etc.). Some norms, known as Minkowski's norms, are leading to some types of almost periodicity which have interesting applications. These functions form a scale of spaces with similar properties, starting with the space which I have called Poincare's space of a.p. functions and ending with the space of Besicovitch. It appears that most cases of almost periodicity of solutions to various classes of equations can be naturally established for this type of almost periodic functions, using series (Fourier, generalised). Another project, starting soon in cooperation with my former PhD students Mehran Mahdavi (Tehran, now in Maryland) and Yizeng Li (Shanghai, now in Texas) is concerned with the writing of a monograph, entitled "Special Topics in Functional Differential Equations". This will contain results, in most cases obtained by us (individually or jointly), regarding special classes of such equations and some of their applications in physics, engineering and other fields.

What is your opinion about the extent to which the current brain drain affects mathematics in East European countries, and in particular in Romania?
I think that the phenomenon, at the level we see nowadays, is new. But in the past century there have been many examples of American academics who started their careers in Eastern Europe. Examples like John von Neumann, N. Minorsky and A. Ostrovski were quite abundant. They just migrated as a part of larger groups of migrants from Eastern Europe to America or Western Europe. Presently, the proportion of those who leave their countries and resettle in more affluent or technologically advanced countries is significantly greater. A former student of mine, while a Vice-Rector of UAIC, informed me that half of the graduates in computer science already had jobs assured in countries like the U.S., Canada, Germany, France, even before they obtained their Diplomas. In mathematics it is not that dramatic but scores of young graduates, and sometimes specialised individuals, are doing the same. In my department, currently, there are five of us from Romania, Bulgaria and Russia. I would say that mathematics is more internationalised, or globalised if you want, than many other fields of research and education. I think we are leading the process towards the population's homogenisation in the world. Our students here, in Texas, come from many countries, from all continents.

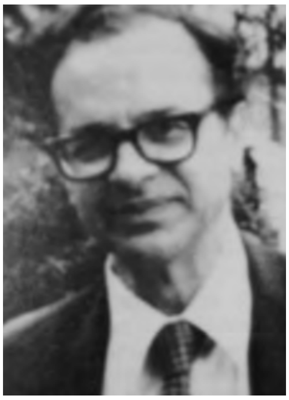
Professor C. Corduneanu (right) and the interviewer during the 7th International Conference on Applied Mathematics (ICAM7), Baia Mare, 1–4 September 2010, after the first part of the interview had been conducted.

I believe that this phenomenon of scientific transplant will continue for a long time, knowing different degrees of intensity in both directions, depending on various changes in the world. And these changes are very difficult to predict now.

Feza Gürsey Institute of Fundamental Sciences

Kürşat Aker, Arif Mardin and Ali Nesin

On 15 July of this year, the *unique* research institute of Turkey on fundamental sciences (theoretical physics and pure mathematics, to be precise) has effectively ceased to exist: the Feza Gürsey Institute (FGI) of Fundamental Sciences had its mission modified by the Scientific and Research Council of Turkey (TÜBİTAK) so as to let it become a sub-unit of the Centre of Research for Advanced Technologies of Informatics and Security (BİLGEM). The decision of TÜBİTAK to relocate the Feza Gürsey Institute to some 80 kilometres out from central Istanbul to an environment which is known for its industrial activity and contract-based research in electronics, optics and several other applied sciences has been very swift and unilateral, in other words without any consultation with the institute's director Kayhan Ülker or any other member of its executive council.



Feza Gürsey

The institute was founded at Gebze in 1983 and its initial name was Research Institute of Fundamental Sciences, part of the Marmara Research Centre. It is thanks to the efforts of Professor Erdal Inönü, an eminent physicist and a very close friend and colleague of Feza Gürsey, and Professor Tosun Terzioğlu, a distinguished pure mathematician who was the President of TÜBİTAK at the time, that the institute was relocated to Istanbul in 1997 and changed its name to the Feza Gürsey Institute of Fundamental Sciences. The aim of this move was to provide the members of the institute closer contact with the researchers of more than 10 universities in and around Istanbul. The city has the highest concentration of establishments of higher education in Turkey. What remained behind in Gebze was still called the Marmara Research Centre (MAM) and its principal activities were applied sciences, partly financed by contract-based research from the industry as well as the armed forces of Turkey.

The danger looming over the institute has been almost visible since 2008, when TÜBİTAK decided, unilaterally again, not to renew an important number of part-time researchers' contracts, obliging them to find positions elsewhere as lecturers. More precisely, the institute had only four full-time research personnel and an even smaller number of post-docs, in addition to a half-dozen part-time researchers from 2008 until its effective closure in July this year. What a contrast this forms with the fact that when the institute opened its doors in 1997, it had 29 full-time and part-time researchers in all. Two of the four full-time research staff resigned in protest against the decision (one of them being the director of the institute, who refused to be part of such an irresponsible act).

Since the relocation of the institute back to Gebze was perceived as its effective closure, an immediate and widespread reaction and protest movement in Turkey and abroad sprang up in the following days. One of the earliest reactions came from Marta Sanz-Solé, the President of the European Mathematical Society. In the letter she addressed on 17 July to Professor Nüket Yetiş, the President of TÜBİTAK at that time, and Mr Nihat Ergün, head of the newly created Ministry of Science, Industry and Technology, she said:

> *I am writing as President of the European Mathematical Society, to express our deep concern about the plans to terminate the present structure of the Feza Gursey Institute. We feel this would be a serious mistake, with very negative consequences for the further development of mathematics and theoretical physics in Turkey.*
>
> *In about fifteen years, the Feza Gursey Institute has become a renowned and active centre for multidisciplinary research in mathematics and physics. It has played a crucial role in the training and exposure of Turkish researchers and in the consolidation of scientific international collaborations. Hence, its termination will result into a great loss and will diminish Turkey's scientific presence and influence in the scientific world.*
>
> *We strongly hope that the Ministry of Science, Technology and Industry will reverse its plans about FGI.*
>
> *Yours sincerely,*
> *Professor Marta Sanz-Solé*

The Presidents of the American Mathematical Society and the Société Mathématique de France, respectively, Eric Friedlander and Bernard Helffer, also sent letters to both Professor Yetiş and Mr Ergün to express their serious concern for the severe coup inflicted upon the future of research activities on basic sciences in Turkey.

Among many letters sent in protest, one came from Bernard Teissier, an eminent mathematician from France, who was a visitor to the institute in 2010 as a lecturer during a conference. As a researcher with considerable experience in the management of such research establishments in France, he had the following strong words to say in his letter:

I visited the Feza Gürsey Institute last year on the occasion of a CIMPA meeting on commutative algebra and algebraic geometry. I formed at that time a quite positive impression of the development of the Turkish mathematical community and of the role of the FGI in this development, in particular with respect to the formation of young scientists. As former president of the board of the Institut Henri Poincaré in Paris, which is a center for Mathematics and theoretical Physics, and was on several occasions threatened with relocation, I have some experience in such matters.

I do not believe that the modifications planned for the FGI would allow it to continue to play such a positive role. Considering the nature of the Tubitak Bilgem research center I am tempted to think that these modifications constitute a serious mismanagement of scientific resources.

Bureaucrats may believe that integrating the FGI would make that center scientifically more efficient, but that is not the way science works and in all probability the FGI would simply wither and die. It would be a dire loss for fundamental research in Mathematics and Physics in Turkey, and I need not remind you of the numerous studies that have shown how important these are for applied research.

The media, written and visual, both in Turkey and abroad, were also active in reporting the event. The totality of all these protests as well as a petition signed by more than 1500 people against the closure of the institute can be seen at *http://savefezagursey.wordpress.com/*.

This webpage also includes the entire body of activities of the institute during the 14 years of its existence. It clearly shows that with a very modest budget, a highly productive scientific environment can be created by a small but dedicated group of researchers.

Feza Gürsey

One of the reasons there was such strong condemnation from the scientific community in Turkey and abroad against the effective closure was that the institute bore the name Feza Gürsey, who epitomised the life devoted to, despite all odds and difficulties, research in fundamental sciences. Feza Gürsey was the greatest theoretical physicist of Turkey. To give a short portrait of the person, we can do no better than the following text on the webpage of the FGI, which was written by Murat Günaydın, one of Feza Gürsey's research students who later became an eminent theoretical physicist, and Edward Witten, a Fields Medallist from the Institute for Advanced Sciences at Princeton:

7 April 1921 – 13 April 1992

Feza Gürsey was one of the most respected members of the physics community and his untimely death on April 13, 1992 was a great loss to theoretical physics. He will always be remembered for his many seminal and deep contributions to theoretical physics as well as for his kindness, civility and scholarship. For those of us who knew him he epitomized a style of physics and an epoch in the history of physics.

Feza's scientific work is marked with remarkable originality and elegance as well as intellectual courage. He never hesitated to pick problems that were not fashionable. He worked at them in depth, planting seeds that in some cases developed into whole branches of our discipline. Outstanding examples would include his conception of the pion in terms of spontaneously broken chiral symmetry, and his contributions to the introduction of exceptional gauge groups for grand unification. To the end of his life he was tackling the most difficult problems, planting new seeds in unknown soil.

In the early part of his career, Gürsey studied the conformal group and conformally invariant quantum field theories, concepts whose role in physics are now central. This developed into his long and multifaceted interest in the unitary representations of non-compact groups and their applications to space-time. In the late fifties he did his work on Pauli-Gürsey transformations and later introduced the non-linear chiral Lagrangian, one of his most seminal contributions to theoretical physics. Chiral symmetry and non-linear realizations of symmetry groups have since become an integral part of theoretical physics. In the 1960s, Feza became well known for his work on the SU(6) symmetry that combines the unitary spin SU(3) of the eight-fold way with non-relativistic spin degrees of freedom of quarks. Subsequent attempts to understand the origin of SU(6) symmetry led to the introduction of the color degrees of freedom of quarks. Feza's introduction in the mid-1970s of the grand unified theory based on the exceptional group E6 – which has continued to fascinate theoretical physicists ever since – was one facet of his long interest in the possible role of quaternions and octonions in physics. This interest also led to Feza's work on quaternion analyticity, which continued practically to the end of his life.

Feza was an exceptionally inspiring teacher. He trained many PhD students who now hold academic positions in numerous countries of the world.

Throughout his life he retained a youthful spirit and was always enthusiastic about learning new things. He had a special rapport with the young people and enjoyed their company.

Reminiscing only about Feza Gürsey the physicist would not do full justice to him. He was a very cultured man who distilled the essential and sublime elements of Western and Turkish cultures and synthesized them into a singularly unique whole in his personality and wisdom. One could have deep and penetrating discussions with him on the music of Franz Schubert and Dede Efendi, on the poetry of Yunus Emre and Goethe, on the novels of Thomas Mann and Marcel Proust, on the paintings of Van Gogh and Giotto, in short, on essentially any subject of depth and beauty.

Murat Günaydın
Edward Witten

(Courtesy of the Editors of Strings and Symmetries, Proceedings, Istanbul, Turkey, 1994, Aktas et al.)

Events since 15 July

Several important events have taken place since the effective closure of the FGI. Some are quite encouraging but others are very worrying, in particular about the independence of scientific institutions in their own constitution and mode of functioning. We summarise some of the developments below:

i) The Senate of Bosphorus University (together with TÜBİTAK, this distinguished university in Istanbul is the patron of FGI, providing, in particular, the premises the institute occupied until it was relocated to Gebze), at a meeting in August, decided to revive the institute at its usual location in Istanbul.
ii) Nüket Yetiş has been relieved of her duties as the head of TÜBİTAK. Her husband, Professor Önder Yetiş, the director of the Marmara Research Centre at Gebze, has also been replaced.
iii) A new director to TÜBİTAK has been named by the government. Before taking up this position, Professor Yücel Altunbaşak was the rector of the TOBB (standing for Union of Bars and Chambers of Commerce of Turkey) University of Economics and Technology in Ankara.

At a time when Turkey is yearning for the long-promised steps towards a more democratic society, such meddling of politicians with the internal affairs of the most prestigious institutions of higher education is truly worrying. Precisely on this point, two articles which appeared in *Nature* (Vol. 477, page 33, published online 31 August; *ibid*. page 131, published online 7 September) describe how alarming the situation is.

As regards the future of the FGI, despite the encouraging decision of the Bosphorus University's Senate, TÜBİTAK has so far excelled by its mutism. Given

Feza Gürsey Institute

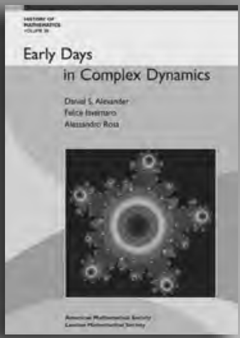
this state of affairs, we can do no more than maintain our pessimism concerning the foresightedness and the sensitivity of the governing body of TÜBİTAK on questions related to the support of research activity in fundamental sciences. It has been more than three months now since the activities of the institute came to a halt. A remarkable number of summer schools, workshops and seminars have been cancelled (for more detailed information, see the institute's webpage at *www.gursey.gov.tr*). An enormous amount of time spent preparing them has been wasted. The question has nothing to do with whether TÜBİTAK possesses sufficient funds to support the institute's activities. Indeed, as can be verified from the documents available on the institute's webpage, the annual funding of the institute is only a very small part of TÜBİTAK's unspent financial resources (1/175, to be more accurate). It is rather a question of apprehension of the vitality of the existence of such institutes in order to progress in basic sciences, a direct consequence of which should be in the country's advances in technology and applied sciences.

Conclusion

Following the terrible damage inflicted upon the institute by the outgoing administration of TÜBİTAK, an important amount of time and energy have been spent in order to reduce this damage to a minimum. All is not won, yet, but we are not at the same point as on 15 July. Thanks to the widespread protest movement of the international scientific community, the sad affair has been brought to the attention of many. We feel that we are no longer alone in this battle to maintain research activities on basic sciences carried out without hindrance. We have hopes that, despite all odds, the institute will open its doors in the very near future.

Kürşat Aker (former permanent member of the FGI, 2007–July 2011)
Arif Mardin (former affiliate member of the FGI; 2010–July 2011)
Ali Nesin (professor of mathematics, Istanbul Bilgi University; former member of the FGI)

American Mathematical Society



EARLY DAYS IN COMPLEX DYNAMICS

A history of complex dynamics in one variable during 1906-1942

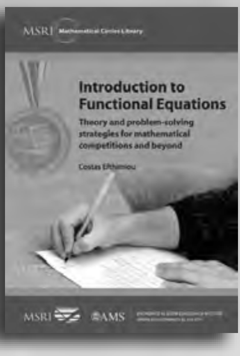
Daniel S. Alexander, *Drake University*, Felice Iavernaro, *Università di Bari* & Alessandro Rosa

Jan 2012 448pp
978-0-8218-4464-9 Hardback €84.00

History of Mathematics, Vol. 38

The theory of complex dynamics, whose roots lie in 19th-century studies of the iteration of complex function conducted by Kœnigs, Schöder, and others, flourished remarkably during the first half of the 20th century, when many of the central ideas and techniques of the subject developed. This book by Alexander, Iavernaro, and Rosa paints a robust picture of the field of complex dynamics between 1906 and 1942 through detailed discussions of the work of Fatou, Julia, Siegel, and several others.
A recurrent theme of the authors' treatment is the centre problem in complex dynamics. They present its complete history during this period and, in so doing, bring out analogies between complex dynamics and the study of differential equations, in particular, the problem of stability in Hamiltonian systems.

A co-publication of the AMS and the London Mathematical Society



INTRODUCTION TO FUNCTIONAL EQUATIONS

Theory and problem-solving strategies for mathematical competitions and beyond

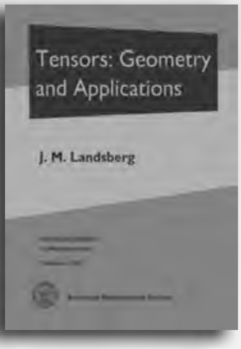
Costas Efthimiou, *University of Central Florida*

Nov 2011 346pp
978-0-8218-5314-6 Paperback €44.00

MSRI Mathematical Circles Library, Vol. 6

The majority of the books on functional equations remain unreachable to the curious and intelligent precollege student. This title attempts to eliminate this disparity. It opens with a review chapter on functions, before presenting a working definition of functional equations and explaining the difficulties in trying to systematize the theory. Each chapter is complemented with solved examples, the majority of which are taken from mathematical competitions and professional journals.

A co-publication of the AMS and Mathematical Sciences Research Institute



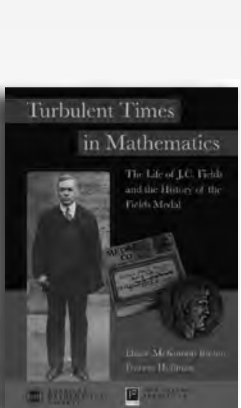
TENSORS: GEOMETRY AND APPLICATIONS

J. M. Landsberg, *Texas A&M University*

Dec 2011 438pp
978-0-8218-6907-9 Hardback €63.00

Graduate Studies in Mathematics, Vol. 128

Tensors are ubiquitous in the sciences. The geometry of tensors is both a powerful tool for extracting information from data sets, and a beautiful subject in its own right. This book has three intended uses: a classroom textbook, a reference work for researchers in the sciences, and an account of classical and modern results in (aspects of) the theory that will be of interest to researchers in geometry. For classroom use, there is a modern introduction to multilinear algebra and to the geometry and representation theory needed to study tensors, including a large number of exercises. For researchers in the sciences, there is information on tensors in table format for easy reference and a summary of the state of the art in elementary language.



TURBULENT TIMES IN MATHEMATICS

The Life of J.C. Fields and the History of the Fields Medal

Elaine McKinnon Riehm & Frances Hoffman

Nov 2011 255pp
978-0-8218-6914-7 Paperback €38.00

"Drawing on a wide array of archival sources, Riehm and Hoffman provide a vivid account of Fields' life and his part in the founding of the highest award in mathematics. Filled with intriguing detail – from a childhood on the shores of Lake Ontario, through the mathematics seminars of late 19th century Berlin, to the post-WW1 years of the fragmented international mathematical community – it is a richly textured story engagingly and sympathetically told. Read this book and you will understand why Fields never wanted the medal to bear his name and yet why, quite rightly, it does".
– June Barrow-Green, Open University, Milton Keynes, United Kingdom

A co-publication of the AMS and Fields Institute

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Deutsche Mathematiker-Vereinigung (DMV) The German Mathematical Society

Thomas Vogt

The *Deutsche Mathematiker-Vereinigung* (DMV) – the German Mathematical Society – speaks for mathematics and all who do mathematics. It was founded in 1890 to stimulate the dialogue between mathematicians working in different branches of mathematics. Today, the society advances mathematical research, education and applications of mathematics and it conveys the dialogue on mathematics in Germany and beyond. The DMV supports mathematics and promotes mathematics related initiatives and activities. The association has 5,000 personal members at universities and research institutes, in business and in schools. The DMV also represents Germany in the European Mathematical Society (EMS) and the International Mathematical Union (IMU). The IMU and the DMV together award the Gauss-Prize for application of mathematics every four years at the International Congress of Mathematicians (ICM).

The German Mathematical Society came into existence as a spin-off of the GDNÄ (Society of German Natural Scientists and Physicians) in 1890. Its first president was the outstanding mathematician and founder of set theory Georg Cantor. Among the presidents of DMV have been other famous names like Felix Klein (1897), David Hilbert (1900), Hermann Weyl (1932) and Friedrich Hirzebruch (1962, 1990). Between 1961 and 1990 the society existed as DMV in West Germany and as Mathematische Gesellschaft in East Germany. Both societies reunited in 1990. DMV's head office is located in Berlin.

The German Mathematical Society came into existing as an off-shoot from the GDNÄ (the Society of German Natural Scientists and Physicians) in 1890. (Source: J. Ortgies Jr., Bremen.)

Since the very first days of the DMV, its members have come together every year in a large conference. Today, this involves the meeting of the sections, workshops on scientific topics (mini-symposia), public lectures, a teachers' day, a students' conference and a cultural program. Commemorating its first president Georg Cantor, the DMV has awarded the Georg-Cantor Medal since 1990 for outstanding academic accomplishments every second year. Among the medallists have been Yuri Manin (2002), Friedrich Hirzebruch (2004), Hans Föllmer (2006), Hans Grauert (2008) and Matthias Kreck (2010).

In 2008, the DMV took a big step forward by intensely expanding its outreach activities. That year was the German Year of Mathematics, a science year to promote mathematical sciences in Germany. The DMV established infrastructure to support public relations during and after that science year by bringing two offices into being: the DMV Media Office and the DMV Network Office.

The DMV Media Office supports the media in search of experts and in finding interesting mathematics related topics, texts, pictures and interviews. It publishes press releases and comments on current affairs like academic and education reforms. The DMV Media Office is also active in fundraising. Every two years, a Media Prize is awarded for outstanding contributions (articles, books, etc.) in communicating mathematics to the general public.

The DMV Media Office is also responsible for running the website *www.dmv.mathematik.de*. It contains not only documentation of the events of the DMV and an internal site for members, with special offers like book sales and other specific information, but also a news blog for everybody, a database on famous mathematicians and background information on selected items for the media. Additionally, the DMV runs the website *www.mathematik.de* with general information about mathematics, including aid for pupils and students on different mathematical fields. Basic information on algebra, analysis, geometry and other fields is given at various levels. There are also news, interviews, book reviews and other information.

A further important activity of the DMV Media Office is MathMonthMay (M^3), addressing the general public and schools in particular. The intent is to establish a mathematics awareness month in Germany: one month in the year which is explicitly dedicated to mathematics activities. MathMonthMay bundles activities of universities, schools and companies. There is even a small budget to support the realisation of their ideas. The call for propos-

A maths awareness month for Germany: DMV's MathMonth-May project bundles activities for maths of universities, school and companies – here at the city of Magdeburg. (Source: University of Magdeburg.)

als is organised by the DMV Media Office at the beginning of each year. A small jury decides how to split the money, depending on the target group (schoolchildren, teachers, parents), the expected impact, originality, feasibility, etc. In this way, about 10 different projects take place every May.

To encourage people to commit themselves to mathematics, the DMV has continued an activation campaign which started in The Year of Mathematics in 2008. Anyone who dedicates part of their life to mathematics as a professional or amateur may register as a "Mathemacher" (Mathmaker) via the DMV website. Mathmakers are ambassadors for mathematics, attempting to make mathematics more popular in their environment. Every month, the DMV Media Office awards the title "Mathmaker of the Month" to honour these people publicly.

The main target group of the Year of Mathematics was pupils and young talents. The central aim was to reduce teenagers' fear of mathematics and to show that mathematics is difficult but also fun. In continuing that work, the Network Office aims to improve communication and the exchange of information between teachers in schools and professors at universities. The idea is to bridge the gap between schools and universities. Teachers should get the opportunity to learn more about "today's mathematics", about current research topics and about what knowledge in mathematics is needed to study natural sciences today. On the other hand, professors should get better contact to schools to help understand the needs of pupils and teachers. Problems and solutions may be discussed bilaterally or on the online forum of the DMV, which is organised by the Network Office. The office is partly funded by Deutsche Telekom Stiftung.

The DMV Network Office also organises the DMV award for best high school graduates in mathematics, the so-called "Mathematics Abitur Prize". Every high school in Germany is invited to nominate one or more excellent students for this prize each year. The prize consists of a certificate, a (popular) book prize sponsored by Springer publishing house and free DMV membership for one year, including four issues of the *Mitteilungen*, DMV's mathematics journal for its members. The number of Abitur Prizes awarded rose from 1320 in 2008 to 2600 in 2011.

A third activity organised by the DMV Media and Network Office together is a digital advent calendar. Instead of chocolate behind 24 little doors of a conventional advent calendar, the digital mathematics advent calendar offers from December 1 a different small mathematical problem each day that is presented on the web. The mathematics problem is embedded in a little story with an advent or Christmas context and is illustrated in a humorous way. Participants can register online and open one door of the calendar each day.

DMV awards prizes for best high-school graduates in mathematics: Mona (right) is awarded by Stephanie Schiemann of DMV's Network Office. (Photographer: Robert Woestenfeld.)

Winning Class of DMV's digital advent calendar: Class 6a of Geschwister Scholl Schule (Tübingen) during the award ceremony at Berlin, Jan. 2010. (Photographer: Kay Herschelmann).

The calendar is free of charge for participants and offered at three different levels. The most difficult level, which has been running since 2004, addresses advanced high school students and is even challenging for adults; it is organised by the DFG Research Center MATHEON in Berlin. The two lower-level calendars, for younger pupils, are provided by the DMV Media and Network Offices. The calendars have become more popular each year: 70,000 people registered to play in one of the three calendars in December 2010. About 100 winners in various age and prize categories are selected among those who have solved all or nearly all of the problems correctly. The winners, the best school class and the most committed school get award certificates and attractive prizes at a public ceremony in Berlin.

Traditionally, the DMV also publishes several periodicals. Every member gets a printed copy of the *Mitteilungen* of the DMV quarterly; a digital version is published online with a certain delay. The traditional Annual Report and the Documenta Mathematica are academic journals published by the DMV, each with one issue per year.

www.mathematik.de
www.dmv.mathematik.de

Thomas Vogt [th.vogt@fu-berlin.de] studied geology, German literature and science journalism in Berlin. He worked for several years as a science journalist in press offices of large research centres in Germany with emphasis on physics and computer science. He joined the German Mathematical Society (DMV) as science journalist for mathematical topics in 2008, Germany's Year of Mathematics. He addresses the media and the general public via DMV's Math Media Office, located at Freie Universität Berlin.

Capacity & Networking Project (CANP) Mathematical Sciences in the Developing World

Bill Barton (New Zealand, President of ICMI)

CANP is a major development focus of the ICMI and the IMU in conjunction with UNESCO and ICIAM. The project is a response to *Current Challenges in Basic Mathematics Education* (UNESCO, 2011), a White Paper prepared by Michèle Artigue for UNESCO in 2010. In this there is a call not just for mathematics education for all but for a mathematics education *of quality* for all.

CANP aims to enhance the mathematical capacity of developing regions and to promote and sustain effective networks of mathematicians, mathematics teacher educators and mathematics teachers in these regions. The project consists of an ongoing series of programmes, one in a different developing region each year. The first programme was held in Mali in September 2011 (see below). The second will be in Costa Rica in 2012 and the third will be in Cambodia in 2013.

Each programme has, at its centre, a two-week workshop of 40 to 50 people, about half from the host country and half from regional neighbours. It is aimed mainly at mathematics teacher educators but also includes mathematicians, researchers, policymakers and teachers. Each workshop has associated activities such as public lectures, satellite workshops for students and exhibitions. The coordination of the workshop is undertaken by a group of nine: four mathematics educators (two international and two from the region); four mathematicians (two international and two from the region); and a liaison person for the ICMI/IMU.

The workshop is focused on providing teacher educators in the region with enhanced mathematical and pedagogical expertise, based on the idea that continued updating and development both in mathematical knowledge and contemporary pedagogical research and techniques will be the basis for continued collaborative activity.

An evaluation and one-year follow-up is part of the programme. The experience of earlier programmes will be used for the design of later ones.

Each programme, and in particular the workshop, will build on current activities in the region and will not seek to reproduce or compete with existing development programmes.

The annual cost of CANP is of the order of €200,000 although the major part of this cost (the contribution of the people involved from the wider international community of IMU/ICMI/ICIAM) is essentially voluntary or borne by institutions in the sense that no salary components are paid, only expenses.

The Mali Programme

Held at the Faculty of Science and Technology of the University of Bamako, 18–30 September, the first instance of CANP was entitled EDiMaths and exceeded many expectations in terms of participation and networking outcomes. Other participating countries were: Burkina-Fasso, Ivory Coast, Mali, Niger, Benin and Senegal (that is, it focused on the sub-region of French-speaking West Africa). Mali was selected as host because of an existing link with French mathematics educators and an existing UNESCO office in Bamako.

News of the programme was spread throughout the region and requests for participation were received from other French-speaking countries (Cameroon, Congo Brazzaville, Democratic Republic of the Congo and Madagascar). Unfortunately the design and funding of the programme made it impossible to extend it in this way but this was an early indication of the need and timeliness of CANP.

The programme had five components: fundamental mathematics for teaching, contemporary mathematics, research situations, technology and transverse topics.

Fundamental mathematics

This topic combined mathematical and didactic aspects of the central content of teaching mathematics: progressive extension of the number field up to real numbers, algebra and functions, 2- and 3-dimensional geometry and the interactions between numbers, measurement and geometry. Each topic was presented by two speakers, one of whom was from the region. A particular stress was laid on connections existing between these various topics. Work alternated between presentations, group work and phases of discussion and synthesis.

Contemporary mathematics

The choice of the topic "Word Combinatorics" was justified by whether it was recent mathematics for which access did not require sophisticated technical tools and whether resources to continue in this field were present in the region (a CIMPA school on this topic will be held next year in Burkina Faso). One of the main objectives was to show the various processes involved in the study of the field and how one can facilitate proofs. This section was managed by Pierre Arnoux and Idrissa Kabore.

Research situations for the classroom

The situations exploited in this part concerned discrete mathematics. They were developed and tested by a collaborative team of mathematicians and didacticians from the University Joseph Fourier and the Research Federation "Maths To Be Modelled". They particularly aimed at questions of definition, reasoning and proof, and the development of associated competences. The topic was directed by Denise Grenier, a member of "Maths To Be Modelled" and the Scientific Co-Director of the EdiMaths School.

Technology and teaching of mathematics

Work on this topic comprised two parts, the first related to the use of the Geogebra software for the teaching of algebra and functions, the second related to probability and the use of Maple software. The majority of the participants had not used either of these programs before so the meetings combined mathematical work, didactic reflection and initiation guided by this software. The topic was led by Morou Amidou (Niger) and Moustapha Sokhna (Senegal) for the Geogebra meetings and by Morou Amidou and Pierre Arnoux for the probability section.

Transverse topics corresponding to regional priorities

Four topics were selected for this part: local numbering systems and their influence on the teaching of number and operations in the region, teaching with large groups of pupils, the evolution of curriculum reforms involving the competency approach, and taking multilingualism into account in the teaching of mathematics. The discussions on each topic were prepared and controlled by Kalifa Traore, Patricia Nebout, Mustapha Sokhna, Sidi Bekaye Sokona, Mamadou S. Sangaré and Mamadou Kanouté.

In all sections, and in keeping with the philosophy of "practising what we preach", the sessions were a mix of groups and formal presentations, with a considerable amount of interaction amongst participants.

The development of communities of practice was focused on reports prepared by each country into their teacher education practices. Subsequent CANP programmes will build this collection of national reports. Promotion activities had two components. The first, Gender Issues, was presented by Nouzha el Yacoubi and Daouda Sangaré. The second took the form of a tale written by Valerio Vassallo and related by him and Sidi Bekaye Sokona. This tale accompanied the exhibition "Balls and Bubbles" whose nine panels had been brought to Bamako. In addition, 15 DVDs of the film "Dimensions" was provided by Etienne Ghys and extracts were projected on the last half-day. EdiMaths was covered by Malian television and the Mali Minister of Education was present at the opening ceremony.

EdiMaths follow-up includes a regional network website, publication of the country reports and the formation of a regional community that plans to hold a second EdiMaths meeting in 2012 in Dakar.

EDiMaths was made possible by the support of UNESCO, the IMU, the ICMI, the International Center of Mathematics Pure and Applied (CIMPA), the SCAC of the Embassy of France in Mali, the University Joseph Fourier in Grenoble and the substantial support of the Ministre de l'Education, de l'Alphabétisation et des Langues Nationales. In addition, the FAST of the University of Bamako gracefully placed at the disposal of EDiMaths an amphitheatre for the opening ceremony and a big room and a computer room, as well as ensured wifi access to the internet for the participants. The Director of the Department of Mathematics provided further office space.

We are seeking sponsors for ongoing funding for future programmes in the Capacity & Networking Project. We hope that others will join the ICMI/IMU community in this major international initiative in the mathematical sciences in the developing world.

Do Theorems Admit Exceptions? Solid Findings in Mathematics Education on Empirical Proof Schemes

Education Committee of the EMS

One of the goals of teaching mathematics is to communicate the purpose and nature of mathematical proof. Jahnke (2008) pointed out that, in everyday thinking, the domain of objects to which a general statement refers is not completely and definitely determined. Thus the very notion of a "universally valid statement" is not as obvious as it might seem. The phenomenon of a statement with an indefinite domain of reference can also be found in the history of mathematics when authors speak of "theorems that admit exceptions".

This discrepancy between everyday thinking and mathematical thinking lies at the origin of problems that many mathematics teachers encounter in their classrooms when dealing with a universal claim and its proof. The solid finding (the term "solid finding" was explained in the previous issue of this newsletter) to be discussed in this article emerged from results of many empirical studies on students' conceptions of proof. In a simplified formulation, the finding is that *many students provide examples when asked to prove a universal statement*. Here we elaborate on this phenomenon.

Universality refers to the fact that a mathematical claim is considered true only if it is true in all admissible cases without exception. This is contrary to what students meet in everyday life, where the "exception that confirms the rule" is pertinent. It is therefore not necessarily surprising that many students simply provide examples when asked to prove a universal mathematical claim, such as showing that the sum of any five consecutive integers is divisible by 5. Indeed, considerable evidence exists that many students rely on validation by means of one or several examples to support general statements, that this phenomenon is persistent in the sense that many students continue to do so even after explicit instruction about the nature of mathematical proof, and that the phenomenon is international and independent of the country in which the students learn mathematics (Harel and Sowder, 2007). A student who seeks to prove a universal claim by showing that it holds in some cases is said to have an *empirical proof scheme*. The same student is also likely to expect that a statement, even if it has been 'proved', may still admit counterexamples. The majority of students who begin studying mathematics in high school have empirical proof schemes and many students continue to act according to empirical proof schemes for many years, often into their college mathematics years. For example, Sowder and Harel studied the understanding, production and appreciation of proof by students who had finished an undergraduate degree in mathematics. Their findings indicate the appearance of empirical proof schemes among such graduates and also how difficult it is to change these schemes through instruction. For example, one student insisted on the use of numerical examples as a way of proving the uniqueness of the inverse of a matrix.

Some mathematics teachers also hold empirical proof schemes. For example, after explicit instruction about the nature of proof and verification in mathematics, Martin and Harel (1989) presented four statements, each with a general proof and with a 'proof' by example to a group of about 100 pre-service elementary teachers. An example of one of the statements was: "If c is divisible by b with remainder 0 and b is divisible by a with remainder 0, then c is divisible by a with remainder 0." Fewer than 10% of the students consistently rated all four 'proofs by example' as invalid. Depending on the statement, between 50% and 80% of the pre-service teachers accepted 'proofs by example' as valid proofs – just about the same number as accepted deductive arguments.

While the issue of empirical proof schemes has been mentioned by Polya and others, Bell (1976) may have been the first to report an empirical study about students' proof schemes. Bell identified what he called students' "empirical justifications" and gave illustrations. Balacheff (1987) later pointed out at least two subcategories of empirical proofs: naïve empiricism and crucial experiment. Naïve empiricism means checking specific cases, often a few cases or the 'first few' cases; it may include systematic checking. Crucial experiment, on the other hand, uses one supposedly 'general' case, say a large number; the idea behind the crucial experiment is that such a large number represents 'any number' and, hence, if 'it' works for this number then 'it' will work for any number.

Fischbein (1982) investigated the notion of universality. He showed that only about a third of a rather large sample of Israeli high school students reasoned according to universality. He showed that even students who claimed that a specific given statement is true, that its proof is correct and that the proof established that the statement is true in general, thought that a counterexample to the statement was possible and required more examples to increase their confidence. The issue of universality has been re-examined many times, usually with similar results. For example, when presented with an empirical argument, only 46% of a sample of German senior high school students recognised that this argument was insufficient for proving the statement. High school students in U.S. geometry classes were found to employ empirical proof schemes and did not seem to appreciate the differences between empirical and deductive arguments. Also in the U.S., university bound students at the end of a college preparatory high school class emphasising reasoning and proof provided an example when asked to prove a simple statement from number theory.

It may be less surprising that in junior high school, about 70% of students used examples when asked to prove something (Knuth, Slaughter, Choppin and Sutherland, 2002), especially in view of the fact that a majority of teachers investigated also showed a strong use of empirical proof schemes, identifying examples as being more convincing than other proof schemes.

Empirical proof schemes may be a consequence of students' experiences outside of mathematics classes. Mathematical thought concerning proof is different from thought in all other domains of knowledge, including the sciences as well as everyday experience; *the concept of formal proof is completely outside mainstream thinking*. Teachers of mathematics at all levels (mathematicians, mathematics educators, schoolteachers, etc.) thus require students to acquire a new, non-natural basis of belief when they ask them to prove (Fischbein, 1982). We all need to be acutely aware of this situation.

The studies mentioned above firmly establish the robustness of the phenomenon, i.e. the existence and the widespread nature of empirical proof schemes, although the following studies show that the situation is, as always in mathematics education, complex. One of the results of the London proof studies (see, for example, Healy and Hoyles, 2000) was that even for relatively simple and familiar questions the most popular approach was empirical verification, adopted by on average 34% of the students, with a much higher percentage for harder questions. This result should be considered significant since the study included a sample of 2,459 14–15 year old, high-attaining (roughly the top 25%) students from 94 classes across England (1305 girls and 1154 boys). Nevertheless, the authors concluded that even though the students appeared unable to construct completely valid proofs, many correctly incorporated some deductive reasoning into their proofs and most valued general and explanatory arguments. Additionally, these studies found that significantly more students were able to recognise a correct proof than to write one and, crucially, they made different selections depending on two criteria for choice: whether it was their own approach or to achieve the best mark. In the number/algebra questions, for best mark, formal presentation (using letters) was by far the most popular choice with empirical argument chosen infrequently. The opposite was the case for students' own approaches, with empirical or prose-style answers much more popular than formal responses. A similar though less clear-cut pattern was reported for geometry, with 'pragmatic' arguments more popular for their own approach but not for achieving the best mark.

Another result, according to which many students do not grasp the universality notion, is the opacity of the notion of "logical consequence", which is a basic ingredient in proving activities. For example, many students of different ages, when asked to check the validity of the following two "syllogistic" arguments:

a) From the sentences "no right-angled triangle is equilateral" and "some isosceles triangles are equilateral", it follows that "some right-angled triangles are not isosceles";
b) From the sentences "no dog is ruminant" and "some quadrupeds are ruminant", it follows that "some dogs are not quadrupeds";

answer that a) is correct while b) is not and justify their answer by observing that while the three sentences in a) are all true, the last one in b) is false (Lolli, 2005). However the two arguments are logically equivalent.

In summary, the research studies mentioned above (and it would be possible to cite many more with similar results) underline the phenomenon that students' major approach to proving is based on empirical proof schemes. This raises a more general issue with respect to research in mathematics education (and more generally in the social sciences); are some, or even many, examples sufficient to make a finding solid? Or do we err in using an "empirical proof scheme" to establish a solid finding in mathematics education? We begin answering this question by noting that 'argument' in the social sciences, including mathematics education, is not equivalent to 'proof' in mathematics. Mathematics and mathematics education have much in common but the latter makes statements on human beings, in particular on students, teachers and teacher educators. This means that mathematics education is a complex interdisciplinary field where, in addition to mathematical issues, pedagogical, psychological, social and cultural issues also play crucial roles.

Anyway, as mathematicians and mathematics educators we might ask whether our solid finding, namely that students' major approach to proving is based on empirical proof schemes, has a general explanation? One hypothesis is the following. Students' specific problems with regard to proving are part of a more general challenge: to make a distinction between reasoning in mathematics and reasoning in everyday life. As mathematicians and mathematics educators, we have learned to flexibly switch between these two "worlds". However, students, in particular young children, have little experience with mathematics as a wonderful world with its own objects and rules. They need time and support to understand this new world. This is true in particular with respect to the nature of proving which has quite different meanings in mathematics and everyday life. From this point of view, it is very well understandable that students, when entering a new field, start using the methods they have successfully used so far. Don't we also frequently use such a strategy? Shouldn't students' so-called 'misconceptions' and 'errors' be regarded under this new light? Can such 'errors' still be regarded as individual deficiencies? Are they not, at least in part, due to an unavoidable and hard to overcome obstacle on the path of every learner of mathematics, an epistemological obstacle, an inevitable challenge that any learner has to face, namely the gap between everyday life and mathematics?

In mathematics education research we know many other manifestations of this obstacle, for example the Rosnick-Clement-phenomenon (Rosnick and Clement, 1980): when asked to algebraically express that in a certain college, there are six times as many students as there are professors, using the variables S and P, the vast majority of students write $6S = P$ rather than $6P = S$. Regarding S and P as variables representing the numbers of students and professors, respectively, the sentence $6P = S$ represents that one should multiply the number of professors P by six in order to get the number of students S. However, students – influenced by everyday life – regard S and P as objects rather than as variables, and from that point of view writing $6S = P$ is correct since it represents that 6 students correspond to one professor. Similarly, we write 1 euro = 100 cents (not a mathematical equation!) but we would need to write the mathematical equation $100E = C$ in order to indicate that we need to multiply the number of euros by 100 in order to get the number of cents. In everyday life we rarely write $100E = C$. In mathematics classrooms, however, the students need to learn that in this particular case everyday life and mathematics have opposite ways of expressing a similar situation. This and similar situations make mathematics education challenging!

It is our task as teachers, teacher educators and mathematicians to find ways of supporting students to overcome the challenge of recognising the differences between mathematics and everyday life. The special case of proving makes students' challenges regarding the relationship between everyday life and mathematics very visible. But it also probably shows that "errors" of individual students might have their roots in a much more general challenge. Hence we need to propose forms of proof (Dreyfus, Nardi, and Leikin, in press) that might support students in making the transition from empirical arguments to valid proofs and to investigate how such progress might be achieved. This transition includes experiencing a need for general proof, for a proof that covers all cases included in a universal statement. It also includes grasping that and why examples do not constitute proof in mathematics. The transition process also includes acquiring an ability to produce proofs that are not example-based. Research points to the transition process from empirical to conceptual proof in terms of learning how to "switch" toward the use of more formal mathematics (Leng, 2010). Students have to feel a need for general proof and make the transition to general patterns of mathematical reasoning, possibly grounded in but not relying exclusively on evidence from examples.

Concerning the need for proof, some researchers have suggested approaches that focus on how teachers can foster students' intellectual need (Harel, 1998), whereas others have focused more on task design that generates a psychological need for proof (Dreyfus and Hadas, 1996). For example, students are likely to accept the statement that the three angle bisectors of a triangle meet in a single point as natural and hence in no need of proof or explanation. However, students may be prepared by first investigating the angle bisectors of a quadrilateral and realising that only in special cases do they intersect in a single point. Students may be further prepared by investigating possible mutual positions of three lines in a plane, seeing that they may but need not intersect in a single point. Students asked to investigate the angle bisectors of a triangle after such preparation are less likely to expect them to intersect in a single point and are often surprised that they do intersect in a single point for any triangle whatsoever. This surprise easily leads to the question of why this happens and hence to a need for proof.

Concerning the transition to general proof, some researchers have recommended exploiting generic examples for facilitating the transition (e.g. Malek and Movshowitz-Hadar, 2011). A generic example exemplifies the general proof argument using a specific case. For example, a generic example for proving that the sum of any five consecutive integers is divisible by five might run as follows: "Let's, for example, take 14+15+16+17+18. The middle number is 16; the number before it, 15, is smaller than 16 by 1; the number after it, 17, is larger than 16 by 1; together these two, 15 and 17, equal 2 times 16. Similarly, the first and the last number, 14 and 18, together equal 2 times 16; hence altogether, we have 5 times 16, which is clearly divisible by 5. A similar procedure can be carried out for any five consecutive integers."

Others have presented evidence that letting students come up with and formulate conjectures themselves may support proof production by creating a cognitive unity between conjecture and proof (Bartolini Bussi, Boero, Ferri, Garuti and Mariotti, 2007). Still others contend that carefully designing a transition from argument to proof holds some potential. This transition is particularly delicate when more sophisticated types of proofs are concerned, such as proofs by contradiction and proofs by mathematical induction. Generally, students' mistakes in such cases are found largely to be manifestations of deficient proof schemes. It seems that pushing students' intellectual need for proof and supporting the development of specific proof schemes in the classroom (e.g. the so-called transformational one, see Harel and Sowder, 2007) can help students in approaching more advanced forms of proof.

Finally, the method of scientific debate in the classroom has been proposed, implemented and investigated. During scientific debates, students formulate conjectures, which they consider scientifically grounded; the lecturer does not express an opinion on their correctness but manages a debate with the objective of collectively building a proof. Such debates have been organised for many years in France and their consequences have been analysed (Legrand, 2001). Compared to traditional lectures, such arguments have been found to change the attitudes of students towards mathematics, leading them to experience the need for proof.

In summary, while the findings about students' empirical proof schemes are solid, the evidence about the transition from empirical to general proof schemes is based on limited evidence collected in suitable environments. This leaves many questions open for further research.

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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Negligible Numbers

Olaf Teschke

The question "Who is the top author in mathematics?" may appear to be a less sensible one but, some weeks ago, Microsoft[1] was bold enough to answer it: Claude Shannon with more than 11,000 citations, followed by Warren Weaver and Barry Simon. The Top Ten were completed by Ingrid Daubechies, Elias M. Stein, Sir Michael Atiyah, William Feller, Scott Kirkpatrick, Mario P. Vecchi and C. D. Gelatt – making up a list one would expect from such an attempt: objective, transparent and meaning nothing.[2]

Actually, it is perhaps less than transparent, after looking into the details. Having such a ranking, one might ask about the origin of the most blatant failures for inclusion and omission. In general, mistakes of the first type are more obvious and can usually be traced back to some systematic misconceptions of the criteria (or even, as in the case of several recent events pertaining to ISI rankings, active enhancement of the data). In the list above, Kirkpatrick, Vecchi and Gelatt reached their position due to their single Science publication on simulated annealing. The main contribution to the citation count comes from outside mathematics so the completely different citation behaviour in another discipline is sufficient to push a single borderline article.

Top mathematicians, according to a certain citation count

On the other hand, knowing the vast number of citations in physics, one might wonder why, for example, Witten didn't make it to the top. The simple answer is that he is not considered by Microsoft as a mathematician so his more than 31,000 citations didn't help. A standard remark is that people from outside the American System are typically mistreated by such measures; there are no comparable citation achievements for Kolmogorov or Gelfand. A funny footnote is that both Bernhard Riemann the German-writing guy (36) and Bernhard Riemann the English-writing guy (26) belong to the very bottom of the list. (The often discussed details for journal rankings will not be covered here – it is sufficient to say that the Annals didn't make it into the top 20 of the Microsoft mathematics ranking).

The example illustrates, in a nutshell, some of the problems inherent to bibliometric computations:

Systems, classification and data quality may strongly influence the outcome. There are many possible error sources and the dependence on the input is not stable; a single misassigned publication may completely change the results (which also contradicts one of the main assumptions of bibliometrics: that it is sufficient to evaluate a small fraction of "core data" to obtain comprehensive results). Nice interfaces and features may be tempting for the user but are no good replacement for content; indeed, the generation of pseudo-knowledge may often be more dangerous than no information at all.

With a continuing demand for citation-related measures, however, it was at least worth an attempt at investigating what might be the outcome on a corpus like the ZBMATH database, which is both more homogeneous and far more complete in its area than the example above (Microsoft considers about one million articles as mathematics, which include a lot of descriptive statistics and computer science, compared to greater than three million in ZBMATH). With the addition of a considerable amount of references over the last two years, one might at least hope to have a critical mass; and there might be the hope that some intrinsic knowledge of the data originating from mathematics may help to avoid common pitfalls.

The starting point was the collection of about 7,000,000 (raw) references in ZBMATH, about 5,000,000 in display-ready format and about 4,000,000 with reliably identified ZBMATH IDs (a necessary basis for statistics). One immediately realises that this means only a small fraction of the three million articles have such reference lists – indeed, the number is about 200,000 (or less than 10%). The main difficulty is, indeed, getting reliable data – the scale of the figures is indeed similar to those in MathSciNet (approximately

[1] http://academic.research.microsoft.com/?SearchDomain=15.

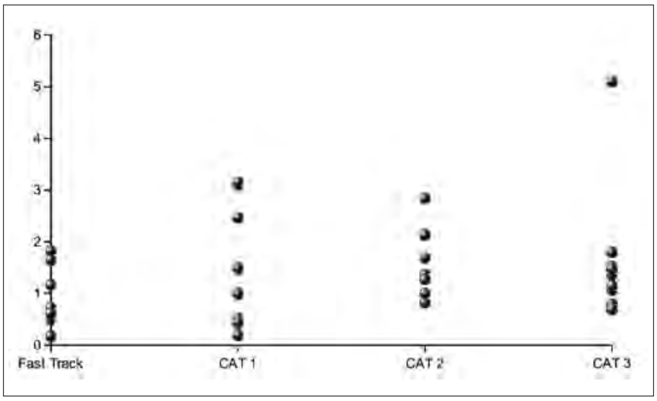
[2] Now, a few weeks later, the site has switched to another bibliometric ranking criterion as a standard: the H-index. This result is quite a different top list, where Shannon goes to mathematics oblivion, while Simon, Atiyah, Lions, Yau and Fan are at the top)..

5,500,000 identified references for about 300,000 articles of a total of 2.7 million) or ISI (less than 100 journals both in the lists of pure and applied mathematics compared to greater than 2000 currently existing) . The exclusion of most journals (like Chaos, Solitons & Fractals and International Journal of Nonlinear Sciences and Numerical Simulation, whose citation enhancement has been the topic of recent discussions) from the reference list helps to avoid some distortions but implicitly acknowledges that citation statistics are not a suitable, objective measure (indeed, an exclusion decision will always be a subjective one, however well-founded).

The possible influence of the uncertainties of author identification has already been a subject of several articles in this column.[3] By now, the progress is sufficiently substantial to expect only minor errors from this source compared to the influence of the lack of reference data for most articles.

Taking these ambiguities into account, the different samples still indicated several tendencies. First, in the short-term, articles and authors from mathematical physics completely dominated the top lists. Articles from the very border of mathematics (like that of Albert and Barabási on Statistical Mechanics of Complex Networks) could easily collect enough citations from mathematical physics to make it to the top of every short-term list. The situation becomes slightly different when increasing the timescale – to give an impression, here is a list of the 20 top-referenced authors for the overall database: Louis Nirenberg, Barry Simon, Pál Erdős, Theodore E. Simos, Elias M. Stein, Stanley Osher, Shing-Tung Yau, Sir Michael Atiyah, Hans Grauert, Saharon Shelah, Haïm Brézis, Edward Witten, Peter D. Lax, Olvi L. Mangasarian, Jürgen Moser, Michio Jimbo, Isadore M. Singer, Elliott H. Lieb, Chi-Wang Shu and Pierre-Louis Lions. Though this is certainly no longer fully physics-dominated, several heavy biases become visible: at best, one may describe the list as mixed, with citations in some cases collected over a rather short period thanks to intense citation behaviour in the field, while others have received citations over decades. The complete absence of several fields of mathematics is especially striking (this continues when going down to the top 50). Obviously, even within pure mathematics, different fields cite differently so one cannot expect to find anything from a comparison without completely dissolving the unity of mathematics (including the splitting of authors who work in different fields).

On the journal level, it may not come as a surprise that (somewhat depending on the timescale) mathematical physics performs quite well: their impact factors (for ZBMATH data) put, for example, *Archive for Rational Mechanics and Analysis* and *Communications in Mathematical Physics* just behind *Acta Mathematica*, *Annals*, *Inventiones and Communications on Pure and Applied Mathematics* and in front of many others. A good illustration is a correlation display like the one of D. Arnold and K. Fowler for journals in applied mathematics.[4] While they used the four Australian categories for mathematics journals, we performed a similar test for a sample of journals with respect to the internal ZBMATH categories (which serve primarily to decide workflow schedules but are naturally influenced by their mathematical content).



Correlation between impact factor and journal categories.

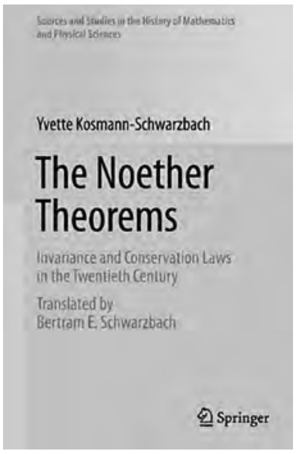
The results are striking – there is even less correlation than the Arnold/Fowler example. Some patterns can be identified but only for negative correlation: Fast Track journals with very low impact factors are often high-quality Russian while low category journals with high impact factors belong to the class which has recently been under suspicion of enhancing citations. As mentioned, the correlation with the field appears to be much higher than with the category.

Finally, there was some hope that one could resolve the effects at least partially by evaluating review citations instead of references. They are much less numerous and are the result of an additional intellectual analysis. Even more importantly, they are expected to be much more homogeneous throughout the database. Unfortunately, these expectations are only partially fulfilled. Several negative effects mentioned above can be excluded but it turns out that reviewers in different fields still cite differently within their reviews. As an example, the top list would now look like Pál Erdős, H.M. Srivastava, Israel M. Gelfand, Sergio Albeverio, Noga Alon, Haïm Brézis, Vladimir G. Mazya, Jean Bourgain and Béla Bollobás – and again one would miss some very well-known names.

From a certain viewpoint, the most satisfying results were produced when asking for a huge time difference between the publication and the citation: when requiring mathematical viability of several decades (the Jahrbuch data contribute heavily to such a statistic), one ends up with probably agreeable collections including Riemann, Poincaré, Hilbert, Hardy, Ramanujan, Banach, Weyl, Kolmogorov, Gödel and von Neumann (all of them outdone by their younger colleagues when using other counts). Fortunately, we do not need citation statistics to generate this; unfortunately, it may be hard to convince politicians that such long-term evaluation measures may be the best suited for mathematics.

[3] See, for example, EMS Newsletter 79 (March 2011).
[4] Nefarious numbers, EMS Newsletter 80 (June 2011).

Book Reviews



Yvette Kosmann-Schwarzbach

The Noether Theorems
Invariance and Conservation Laws in the Twentieth Century

Translated by Bertram E. Schwarzbach

Sources and Studies in the History of Mathematics and Physical Sciences

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Reviewer: Erhard Scholz (Wuppertal)

The *Noether Theorems* have risen to fame in physics and mathematics over the last third of the twentieth century, more than 50 years after they were first published. In 1918, *Emmy Noether* (1882–1935) formulated two important theorems on "invariant variational problems" (invariant under the action of finite or infinite dimensional Lie groups) in the sequel referred to as Noether I and Noether II. Moreover, she analysed an assertion of Hilbert with regard to the energy problem in general relativity from a general group theoretic point of view (Noether 1918). This work was a service to the community of Göttingen mathematicians, in particular to F. Klein and D. Hilbert in their quest for a better mathematical understanding of the principles of general relativity (GRT) (Rowe 1999). During the first 30 years after their publication Noether's theorems received little explicit response, although their content was known by practitioners of GRT (in most cases through Klein's publications of 1918). The theorems started to be more broadly received only after 1950 (Byers 1996, Byers 1999) but for a long time the reception of the two theorems went down separate paths and occurred in different contexts. In the foundations of classical mechanics, quantum mechanics and elementary particle physics Noether I attracted increasing interest in the period 1950 to 1980, while Noether II was known and nourished mainly among general relativists. Only after 1970, with the rise of gauge theories and modern differential geometrical methods in variational calculus (jet bundles and generalized symmetries), did the whole package of the Noether theorems and "genuine generalizations" become finally accepted in mathematics and physics (Chapter 7).

Yvette Kosmann-Schwarzbach, herself an actor in the development of generalized symmetries in variational calculus, and highly interested in the history of recent mathematics, has published an English version of a book-length study and documentation of the Noether theorems and their reception during the 20th century.[1] The book starts with an English translation of Noether's original paper "Invariante Variationsprobleme".[2] Part II of the book contains a mathematical commentary to Noether's theorems (Chapter II 2) and a description of the historical setting in which Noether's study was made, in particular the discussion of the energy problem of general relativity around 1918 (II 1). The rest of Part II consists of a scholarly documentation of the perception of the Noether theorems by contemporaries and historians of science (II 3), their broken transmission between 1920 and 1950 (II 4), the phase of rising reception of the two theorems in different contexts mentioned above (II 5, II 6) and the final "victory" for Noether's work, i.e. the appreciation of the theorems in their original generality and further generalizations (II 7). The main text ends with a short historical reflection on the strange history of reception. The appendix of the book contains several historical sources from the correspondence between E. Noether, F. Klein, A. Einstein and W. Pauli starting in 1918, and the titles of talks by Noether, or relating to her work, in the Göttingen Mathematische Gesellschaft between 1915 and 1918.

In her 1918 paper Emmy Noether analysed in great generality and extremely concisely the mathematical consequences of the existence of continuous (infinitesimal) symmetries for the Lagrangian of a variational problem $\delta \int \Lambda \, dx = 0$. The Lagrangian could depend on independent variables $x = (x_1, \ldots, x_n)$ and (dependent) field variables $u = (u_1, \ldots, u_\mu)$ and their partial derivatives up to a specified order, $\Lambda = \Lambda\,(x, u, \partial u, \partial^2 u, \ldots)$. At first she considered the infinitesimal operations of a Lie group G_ρ of dimension ρ as symmetries of the Lagrangian (Noether I). In her second theorem she investigated an infinite dimensional group action (denominated $G_{\infty\rho}$ by Noether) expressed in terms of point dependent operations of G_ρ, where the point dependence was given by functions in the independent variables x and their derivatives up to a specified order σ (Noether II). The last case was extremely general. It allowed one to analyse situations as different as infinitesimal diffeomorphisms of the underlying manifold (coordinate variables x) for the field constellations in general relativity and, in later terminology, the action of a gauge group in a fibre bundle with structure group G_ρ (in the case $\sigma = 1$).

For higher derivatives the symmetries could be expressed geometrically only much later, after the invention of jet bundles. No wonder, from an historical point of view, that the reception of the second theorem went through specializations and approached the degree of generality originally envisioned by Noether only slowly and stepwise. Even in the reception of the first theorem, the derivatives of the field variables u were for a long time restricted to first order. This restricted transmis-

[1] The documentation was originally published in French (Kosmann-Schwarzbach 2004). The English version has been considerably refined and extended.

[2] Another one was published by AM. A. Tavel in *Transport Theory and Statistical Physics* 1 (1971), 186–207.

sion of Noether I was due to the influence of a paper by E. Hill written in 1951 (II 4.7). Kosmann-Schwarzbach argues that Hill's paper shaped the understanding of Noether I among physicists for a long time (II 5). When physicists started to pass over to higher order derivatives in the 1970s, they often thought of their work as "generalizations" of Noether I, without realising that the full generality was already present in E. Noether's original publication (II 5.5).

In the case of Noether I the consequence drawn from such symmetries was a set of relations among the Euler-Lagrange derivatives of L, equal to a divergence. For solutions of the dynamical equations ("on shell" in physics terminology) the divergences vanish and a conserved "Noether current" arises. If one of the independent variables is time ($x_1 = t$) and adequate boundary conditions can be assumed, the time component of the Noether current can be integrated over space-like folia and leads to a conserved integrated quantity, the corresponding Noether charge. That such a type of symmetry (later called "global" in the physics literature) lies behind the conserved quantities of mechanics, energy, momentum and angular momentum had already been realised in different form for classical mechanics by C. G. J. Jacobi (1842/43) and G. Hamel (1904), and in the special relativistic case by Herglotz (1911) (II 1.1). But it was Noether who gave an all-embracing general analysis of such conservation laws of a globally operating group.

The point dependent symmetries of Noether II were considered of utmost importance for understanding the role of energy in general relativity by Hilbert and Klein. In his paper of 1915 on the foundations of physics Hilbert claimed that the coordinate independence of the general relativistic Lagrangian (actively interpreted, the invariance of the Hilbert action under infinitesimal diffeomorphisms) resulted in an interdependence of the electromagnetic and gravitational equations, rather than in a proper conservation law.[3] In his correspondence with Klein he even considered this as a "characteristic feature" of GRT (Hilbert 2009, 17). Noether analysed Hilbert's claim under the most general assumptions sketched above. She was able to derive a set of vanishing differential expressions of the Euler-Lagrange terms, later often called *Noether equations* (Noether II). For both assertions (Noether I, II) she could show the inverse direction also.

In the last section of her paper she discussed the question of what happens to the Noether currents of the first theorem if the finite dimensional group operates as a subgroup of an infinite dimensional one of type.[4] Colloquially spoken, what happens if one can "marry" the symmetries of Noether I and II?

Noether showed that in this case the Noether currents have a special form and are equal, up to a divergence, to differential expressions of the Euler-Lagrange terms, which can be isolated from the expressions appearing in Noether II. In allusion to Hilbert's terminology she called the divergences of Noether currents in this case "improper divergence relations" but did not touch upon the question of possible physical interpretations of such "improperness". Kosmann-Schwarzbach reminds us that this Hilbert-Noetherian terminology "has not been retained in the literature" (II 2.3). In a way, the terminology was even turned round when the additive part of Noether's "improper" expressions, which was itself a divergence, became called a "strong conservation law" in the 1950s by J. Goldberg and A. Trautman. The reason for such a terminology was that this divergence vanishes without assuming the dynamical equations being satisfied ("off shell"). But the identification of "strong conservation laws" did not help much for a satisfying solution of the energy problem of general relativity. The terminology was even quite unlucky insofar as such "conservation laws" do not contain any dynamical information. In *this sense* Hilbert's and Noether's qualification of the differential conservation laws as "improper" still seems justified, even if no longer retained. In the modern gauge theories of the 1970/80s, Noether's differential identities of her Theorem II turned out to be of structural importance in themselves, beyond any relation to ("proper") conservation laws. They are now seen as the classical analogue of an important feature of quantized gauge theories, the so-called BRST identities (Becchi, Rouet, Stora, Tyutin) which lie at the base of renormalizability of gauge field theories (II 6.2). In this sense, the "improper conservation laws" have finally outplayed in importance the Noether charges for modern quantized gauge theories.

In the context of Weyl's perspective on gauge theory, the terminology would seem less well adapted, however. The historical role of gauge theories in the reception of the Noether theorems is an intriguing and historically quite twisted story. Kosmann-Schwarzbach reports on H. Weyl's only very peripheral reference to Noether in the third edition of *Raum, Zeit, Materie* (Chapter II 3.1) and discusses the role of Noether II in modern gauge theories (II 6.2). Weyl was intrigued by finding an explanation for conservation of charge in his first (scale) gauge theory of gravity and electromagnetism in 1918 from the local scale invariance. He used a Noether-like variational symmetry argument developed on his own in early 1918 before Noether's article was published. But even later he never discussed the relation between his derivation of charge conservation from gauge principles and Noether's theorems; he rather continued to give a derivation of his own, adapted, later in 1929, to the specific context of $U(1)$ gauge theories of electromagnetism. The reason may have been trivial in the sense that Weyl may not have read Noether's 1918 paper carefully enough to realise the general import of it, even after he quoted it in 1919. Another, more epistemic reason is also conceivable. Noether's qualification of the "improperness" of conservation laws derived from the symmetries

[3] More precisely, Hilbert originally claimed (1915) that the electromagnetic equations could be derived from the gravitational ones. He weakened this (wrong) statement with reference to Noether in his 1924 re-edition of his 1915 paper (Rowe 1999, 228).

[4] This would be the case in the later gauge theories by global fibrewise operation of the structure group.

of a finite dimensional globally operating subgroup of the gauge group might have seemed to weaken Weyl's gauge argument for conservation of charge. For his purpose he would have been forced to show that Noether's "improperness" did not curtail his argument relating to the Noether current of gauge theories, although it was to be taken seriously for energy conservation in GRT. His own line of argument avoided the slippery terrain of "proper" or "improper" conservation laws, discussed among Hilbert, Klein and Noether in 1918.

So it was left to authors of the next generations to establish a link between conservation of charge ("proper" in ordinary language, not in Noether's) and Noether's first and second theorems. In fact, the link seems to have been laid open only two generations later. With regard to an explicit link between electromagnetism and Noether II, Kosmann-Schwarzbach quotes authors only after the breakthrough of (quantized) gauge theories in the early 1970s (Logan 1977 and O'Raifeartaigh 1997).[5]

The book under review does a splendid job in collecting and carefully presenting a huge range of material on the strange story of the belated reception of Emmy Noether's symmetry investigations in variational problems and their further development. One may be struck by the scarcity of quotations and acknowledgments of Noether's work before 1950. In this respect the presentation of the material sometimes comes across as if the author suspects the suppression of acknowledgement was due to the fact that E. Noether was a female and Jewish mathematician. In this she concurs with the evaluation in Rowe, 1999, 227f. But in the final passage of the book she reflects the intricacies and difficulties of the validation of a mathematical subject, which depends so much on research lines and tendencies of the community. Here she comes to the conclusion "that the lack of reception of Noether's theorems had more to do with the nature of interests of the mathematical physicists of the time than with the quality of her results or her person" (p. 147). If that is true, the reception of the Noether theorems is no different than the rest of mathematics. Be that as it may, in any case this book presents a highly interesting case study of an important mathematical development of the last century.

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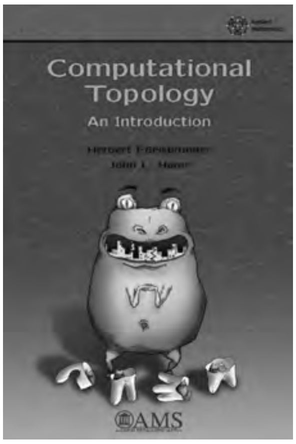
5 A beautiful discussion of this link is given by Brading (2002).



Herbert Edelsbrunner
John L. Harer

Computational Topology

American Mathematical Society, 2010

ISBN 978-0-8218-4925-5

Reviewer: Martin Raussen

Topology has been developing for a little over a century, initially as a common framework underpinning developments in a variety of mathematical areas, in particular geometry, combinatorics, homological algebra and functional analysis. Moreover, set-theoretic topology has to a certain extent entered theoretical computer science and order topologies are used to reason in domain theory.

Only recently, and in connection with the wide availability of high speed computing, has algebraic topology gone computational. New tools of a topological nature have been developed for the analysis and interpretation of huge data sets assembled in all sorts of investigations, which have otherwise been in the realm of statistical methods. To apply these tools, a combination of insights from the design of algorithms (better known from the area of computational geometry) with core material from algebraic topology is needed.

This book, authored by two major players of the game,[1] arose from lecture notes developed during courses (at Duke and at Berlin) for a mixed audience that presented the authors with the challenge of how to teach topology

1 After many years in the United States, Herbert Edelsbrunner has returned to his native Austria as a professor at the newly established Institute of Science and Technology Austria. He will be a plenary speaker at the 6th European Congress of Mathematics in Krakow in July 2012.

to students with a limited background in mathematics and how to convey algorithms to students with a limited background in computer science? In fact, no prior knowledge of definitions, methods and machinery of a topological nature is assumed and topological notions are explained with a great deal of motivation. Proofs of results from algebraic topology are only occasionally given.

For a topologist, it is quite uncommon to care about data structures or implementations of algorithms or to reason about their complexity but this is unavoidable if the methods conceived are to be exposed to real world data. This book is a quite exceptional blend of presentations of theoretical background with implementation details. Many algorithms in the book are described in pseudo-code (often quite self-explanatory); some familiarity with notions for the analysis of the complexity of algorithms is tacitly assumed.

The final goal of the book is the description and assessment of a key tool in this development, so-called persistent homology. It deals with the qualitative assessment of data over a scale of observations via invariants from algebraic topology and often allows one to guess and to discover underlying features and/or to distinguish between such features and noise. Roughly speaking, data is translated into a series of spaces of a geometric or combinatorial nature, filtered according to a scale. Then, one calculates homology groups of each of the spaces and observes at which threshold (on the scale) a homology class is "born" and at which threshold it "dies". The collection of these data is represented as a barcode; long bars are usually due to features, short bars due to noise. For a very lucid survey article, have a look at R. Ghrist, "Barcodes: The persistent topology of data", *Bull. Amer. Math. Soc.* **45** (2008), 61–75.

The book consists of three parts: Geometric Topology, Algebraic Topology and Persistent Topology (all of them with the prefix "computational"). Each of these parts consists of three chapters; each chapter has four sections corresponding to one lecture. Chapters end with a list of eight exercises of varying difficulty (and credits!). There is ample bibliographic information and a list of references comprising 161 entries. The book contains many figures explaining and illuminating the text.

Part A (Computational Geometric Topology) discusses topological and geometric concepts and it develops data structures and algorithms related to those. It covers a wide range of topics starting from graphs and planar curves via surfaces to simplicial complexes. All chapters contain material that cannot be seen in most available textbooks, e.g. the section on knots and links states the Calugareanu-White formula '*Link* = *Writhe* + *Twist*' for a ribbon around a knot. The section on surfaces focuses on triangulations of abstract and of immersed surfaces and of simplifications of those. The final section on simplicial complexes is essential for further development, introducing and investigating Cech-, Vietoris-Rips, Delaunay and alpha complexes (filtering the Delaunay complex and most useful for computations) for point clouds in Euclidean space.

Part B (Computational Algebraic Topology) introduces homology with Z/2-coefficients and important properties together with explicit algorithms computing the homology for a chain complex via the Smith normal form but not the "pre-linear algebra" reduction algorithms on the chain complex level due to Mrozek and collaborators. A chapter on duality – mainly for triangulated manifolds – starts with an old-fashioned, beautifully geometric view on cohomology, dual block decompositions and intersection theory and arrives at Alexander duality without the use of cup and cap products. The last chapter deals with Morse functions, Morse inequalities and the Morse-Smale-Witten complex leading to Floer homology. For applications, it is important how to interpret and implement the methodology of Morse functions, critical points, and stable and unstable manifolds for *piecewise linear* functions on simplicial complexes.

Part C (Computational Persistent Topology) is at the heart of the book. The essential idea seems to have come up around the turn of the century, independently in the works of Frosini and Landi, Robins, and Edelsbrunner and Letscher and Zomorodian. An important example and application area arises with the study of the homology of *sublevel* spaces associated to a Morse function and the birth and death of associated homology classes. These are pictured in the plane as *persistence diagrams*, recently generalised to extended persistence diagrams whose interpretation relies on Poincaré and Lefschetz duality. It is then interesting to study the stability of the persistence data under perturbations of the shape and/or the Morse function associated to it. This can be formally done using notions of distance between persistence diagrams (the bottleneck and the Wasserstein distance that can be calculated using optimal matchings in bipartite graphs using Dijkstra's algorithm). The final chapter deals with "real" applications: gene expression data in terms of 1-dimensional real functions on a circle; periodicity measured via persistence; and elevation functions and extended persistence for protein docking (binding between proteins), image segmentation (clean-up after watershed algorithm for a PL Morse function) and root architectures (featuring persistent *local* homology).

In summary, this book is a very welcome, untraditional, thorough and well-organised introduction to a young and quickly developing discipline on the crossroads between mathematics, computer science and engineering. This book's scope is certainly wider than that of its predecessor Afra Zomorodian's *Topology for Computing*, Cambridge University Press, 2005.

Although underpinned by intuitive reasoning, the topological sections will be tough reading for the uninitiated computer scientist and the algorithmic sections will not be easily digested by a topologist. But how could that be otherwise?

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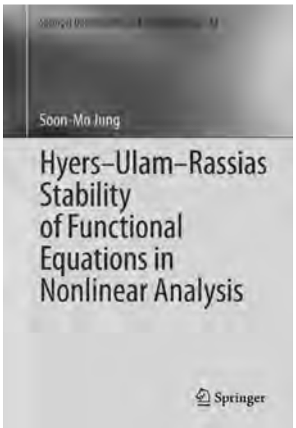

Soon-Mo Jung

Hyers-Ulam-Rassias Stability of Functional Equations in Nonlinear Analysis

Springer, 2011, xiii+362 pp.
ISBN 978-1-4419-9636-7

Reviewed by Themistocles M. Rassias
(Athens, Greece)

It has already been more than 70 years since Stanislaw M. Ulam presented in the Autumn of 1940 a wide-ranging talk before a mathematical colloquium at the University of Wisconsin in which he discussed a number of important unsolved problems, including a question concerning the stability of homomorphisms. The study of stability problems for various types of functional equations stem from his legendary discussion in 1940. Donald H. Hyers attempted to provide a partial solution to Ulam's problem for approximate homomorphisms between Banach spaces; his result is still recognised as the first significant breakthrough and step towards the solution of Ulam's problem. In 1978, Themistocles M. Rassias extended Hyers's stability theorem and led the concern of mathematicians towards the study of a large variety of stability problems of functional equations as well as differential equations.

It is only recently that books dealing with a comprehensive account of the quickly developing field of functional equations in nonlinear mathematical analysis have been published. It was more than half a century until D. H. Hyers, G. Isac and I published the book *Stability of Functional Equations in Several Variables* (Birkhäuser, 1998), which provided a self-contained and unified account of this domain of research. I am more than happy to write my opinion on Soon-Mo Jung's book, as a new addition in this rapidly growing field of mathematics, which will help interested mathematicians and graduate students to understand further this beautiful domain of research.

Jung systematically compiled this book, which not only complements those previously published books on the subject of Hyers-Ulam-Rassias stability, including S. Czerwik's *Functional Equations and Inequalities in Several Variables* (World Scientific, 2002), but also discusses in a unified fashion several classical results. In each chapter, S.-M. Jung provides a discussion of the Hyers-Ulam-Rassias stability as well as related problems with various approaches. For example, it is interesting to note the way the author studies the interrelation of the Hyers-Ulam stability problem with a question of Th. M. Rassias and J. Tabor in Chapter 3. The book contains 14 chapters.

Chapter 1 provides an extensive summary of the main approaches and results treated in the book. Chapter 2 deals with the Hyers-Ulam-Rassias stability problems as well as related problems connected with the additive Cauchy equation. In Chapter 3, the Hyers-Ulam-Rassias stability of certain types of generalised additive functional equations is proved along with a discussion of the Hyers-Ulam problem. In Chapter 4, Hosszú's functional equation is studied in the sense of C. Borelli along with the proof of the Hyers-Ulam stability of Hosszú's equation of Pexider type.

Chapter 5 is devoted to stability problems of the homogeneous functional equation using the proof of the Hyers-Ulam-Rassias stability of the homogeneous functional equation between Banach algebras, as well as between vector spaces. In Chapter 6, the author introduces and discusses a few functional equations, including all the linear functions as their solutions while concerning the superstability property of the system of functional equations $f(x+y) = f(x)+f(y)$ and $f(cx) = cf(x)$ and the stability problem for the functional equation $f(x+cy) = f(x)+cf(y)$. Chapter 7 is devoted to an application of Jensen's functional equation (that is, the most important functional equation among several variations of the additive Cauchy functional equation) to the proof of Hyers-Ulam-Rassias stability problems. In particular, the author provides an elegant proof of the stability of Jensen's functional equation by applying the fixed point method. Chapter 8 is devoted to an exposition on the stability problems for quadratic functional equations as well as to the proof of the Hyers-Ulam-Rassias stability of these equations.

In Chapter 9, the author discusses the stability problems for the exponential functional equations while proving the superstability of the exponential Cauchy equation and dealing with the stability of the exponential equation in the sense of R. Ger. In Chapter 10, the author has provided a nice survey of several results on the stability problems for the multiplicative functional equations. In this chapter, the author connects the superstability problems with the Reynolds operator. In addition, the author introduces a proof that a new multiplicative functional equation $f(x+y) = f(x)\ f(y)\ f(1/x+1/y)$ is stable in the sense of Ger. Chapter 11 introduces another new functional equation $f(x) = yf(x)$ with the logarithmic property, along with discussion on the functional equation of Heuvers $f(x+y) = f(x)+f(y) +f(1/x+1/y)$. In Chapter 12, the author deals with the addition and subtraction rules for the trigonometric functions which can be represented in terms of functional equations.

Chapter 13 is devoted to the Hyers-Ulam-Rassias stability of isometries. Furthermore, the author has discussed the Hyers-Ulam-Rassias stability of the Wigner functional equation on restricted domains. Chapter 14 presents the proofs of the Hyers-Ulam stability of a functional equation, the gamma functional equation and a generalized beta functional equation, and the Fibonacci functional equation along with the superstability of the associativity equation.

The book concludes with a very useful bibliography of 364 references and an index. It will definitely guide mathematics students to a decisive first step into this abstract, yet intriguing, field of mathematics.

As a fellow scholar, I would like to congratulate Dr Soon-Mo Jung for his endeavour in presenting such a fine and well written mathematical text. The author has succeeded in presenting to both mathematicians and graduate students an invaluable source of essential mathematics. The book will certainly become a standard reference for stability of functional equations in nonlinear analysis.

Themistocles M. Rassias [trassias@math.ntua.gr] is a professor of mathematics at the National Technical University, Athens, Greece. He received his PhD under the supervision of Professor Steven Smale at the University of California at Berkeley. He is a well-known author of many articles and books, mainly in the areas of mathematical analysis, global analysis, geometry and topology. He is a member of the editorial boards of several international mathematical journals including the EMS Newsletter.

Letter to the Editor

A 'Swell' Intervention in the Math Wars

The March 2011 issue of the EMS *Newsletter* contained an unusual article, 'Teaching general problem solving does not lead to mathematical skills or knowledge', by John Sweller, Richard Clark and Paul Kirschner. It was striking because it seemed not to fit the Newsletter's remit.

According to the EMS website, the Newsletter is the 'journal of record' of the EMS and features 'announcements … and many other informational items'. With respect, this article was not a record, or an announcement; it was mis-informational and rather clearly an intervention in the so-called Math Wars. The authors revealed their motivation when they wrote that:

> *"Recent 'reform' curricula both ignore the absence of supporting data and completely misunderstand the role of problem solving in cognition."*

This was an extremely large and bold claim which they did not support by any references. They continued:

> *"If, the argument goes, we are not really teaching people mathematics but are teaching them some form of general problem solving the mathematics content can be reduced in importance. According to this argument, we can teach students how to solve problems in general and that will make them good mathematicians able to discover novel solutions irrespective of the content."*

Whose argument is this supposed to be? Once again this is a very large claim for which they give no references. I am aware of no one who has ever made this extreme and frankly absurd argument.

The authors then turn to the work of De Groot (1946, 1965) but present a travesty of his conclusions:

> *"The superiority of chess masters comes not from having acquired clever, sophisticated, general problem-solving strategies but rather from having stored innumerable configurations and* the best moves associated with each *in long-term memory." [My accentuation]*

This is straightforwardly false and suggests that Sweller et al. do not understand how the game is played, although they use it as a central feature of their argument. (De Groot himself was an experienced chess player who played for Holland in the 1937 and 1939 chess Olympiads.) The configurations chess players remember (excluding opening sequences and maybe the late endgame) and can recall are *parts* of complete positions, and what they associate with them are not specific 'best moves' but *concepts* and *plans*. The 'best move' will generally depend on the whole board position. Since games (with almost no exceptions, with the qualification already noted) do not repeat other games, the question of the 'best move' being retrieved from long-term memory does not arise.

At most (again with the same qualifications), a position may be recalled as similar to the present position but since the positions are not identical, there is no 'best move' to recall. Thus Alekhine, in one of De Groot's protocols responds: "At first sight there is a dark memory of a tournament game Botwinnik-Vidmar (Nottingham). There's a certain resemblance: the same Queen position on Q3." Alekhine then recognises that the opening had been a Queen's Gambit Accepted, and then goes on to consider the actual position in front of him. [De Groot 1978: 409]

Note also that Sweller et al. contrast 'chess masters' and 'weekend players'. This is misleading. De Groot's subjects ranged from six top grandmasters via masters and experts to five 'skilled players'. In other words there were no weak players among his subjects and his conclusions cannot be used to draw, by analogy, conclusions about weak school mathematics students. Thus, I have no doubt, from my own experience of teaching chess in evening classes to very keen but weak amateurs, that one difference is that weak players have little capacity to think ahead, display limited grasp of even simple tactics and strategies and cannot remember the moves of a game they have just played. Similarly, weak school mathematics pupils are poor at 'mental algebra', have limited grasp of 'tactics and strategy' and have weak memories for mathematical situations.

Why do Sweller et al. make their claim? I do not know but I will notice that the idea that both mathematics and chess consist of memorising and then recalling large numbers of positions with the 'best move' for each does support their focus on *worked examples*, while an emphasis – far more valid – on chess playing involving concepts and interpretation and *novel* calculations of *possible* lines of play using *imagination* does not support their worked examples as a method but does link strikingly to reformers' emphases on mathematics with understanding.

Having given their false account of how chess is played, they then make this extraordinary claim:

> *"How do people solve problems that they have not previously encountered? Most employ a version of means-ends analysis in which differences between a current problem-state and goal-state are identified and problem-solving operators are found to reduce those differences.* There is no evidence that this strategy is teachable or learnable because we use it automatically.*"*

The accentuation is mine. Once again they give no references. 'We' – who is 'we'? – 'use it automatically'. I'm sorry but I have taught many pupils who do not use even the most fundamental 'problem-solving operators' 'automatically' and who do benefit, therefore, from being introduced to problem solving tactics and strategies in a mathematical context.

The tendentious nature of the article is supported by the fact, noted in the EMS, that two near-identical articles were published almost simultaneously in the *Notices* of the American Mathematical Society in their DOCEAMUS column [Nov 2010], under the title "Teaching General Problem-Solving Skills Is Not a Substitute for, or a Viable Addition to, Teaching Mathematics", and in the *American Educator*, with the title 'Mathematical Ability Relies on Knowledge, Too' [Winter 2010–2011]. Needless to say, such simultaneous publication is contrary to all the usual academic conventions.

The *Notices* are read by '30,000-plus mathematicians worldwide', while the *American Educator* claims a current total circulation of more than 900,000 and the EMS Newsletter claims potentially in excess of 2500 readers, so the total readership for these articles, many of whom may be interested in mathematics education but will not be experts on its psychology, could be very large.

The ideological slant of the article is also suggested by the response when the AMS submitted the article to the judgment of Alan Schoenfeld, without doubt one of the most distinguished students of mathematical problem solving in schools. His judgment, which he has placed on the internet, read as follows:

> *Subject: Re: Sweller et al. article for Notices*
> *Dear Steven [Krantz],*
> *This piece is easy to review. Anyone who purports to talk about mathematical problem solving without mentioning Polya [sic: Polya is mentioned twice in the article as finally published] (or for that matter, Krantz or Schoenfeld – your book "techniques of problem solving" is on my bookshelf) is completely clueless. Sweller and colleagues set up a straw man, the notion of "general problem solving" as a counter-point to mathematical knowledge.*
> *The point is that there are techniques of mathematical problem solving, and there's plenty of evidence that students can learn them, so the opposition Sweller and colleagues use to frame their paper is nonsensical.*
> *And any hints that his false dichotomy can "solve" the math wars are nonsense – he's fighting a battle ("discovery" versus "direct instruction") that is of neither mathematical nor pedagogical interest. If anything, this kind of argument enflames the math wars rather than resolving them.*
> *I could write a standard, sterile review ("fails to be scholarly", etc.) but I trust that isn't needed – or if it is, that you'll write back.*
> *Cordially, Alan*

Steven Krantz published the article anyway.

Let me return to the content of the article. Sweller et al. claim that:

> *"There is no body of research based on randomised, controlled experiments indicating that such teaching [DW: based on general problem-solving strategies] leads to better problem solving",*

implying that the reformers' claims are unscientific, but they themselves are unscientific when they fail to explain what their goals for mathematics instruction are. (I am using the American terminology: in the UK we talk of maths education, not instruction.) What might they mean by 'better problem solving'?

This is a crucial omission. The goals of traditional teaching tend to be the accurate solution of standard problem-types: the goals of the reformers, judging for example by the NCTM website, tend to be to increase understanding. (I am greatly simplifying.) It is no surprise therefore that they also fail to mention that learning via worked examples much resembles traditional rote learning.

Thus, turning to W. P. Workman's *Tutorial Arithmetic* of 1905, a popular book and many times reprinted, Chapter XIV on Least Common Multiple starts with a worked example, with quite a long explanation. The next chapter is on Vulgar Fractions and 'addition' starts with an introduction followed by a worked example with a brief explanation. And so on.

It does *not logically follow* that worked examples, with explanations, cannot be a good method of instruction, according to traditional goals. It could be that with better understanding, worked examples can be more effective today than they were 50 or 100 years ago. (Perhaps modern teachers place much more emphasis on explanation, or explain better.) It is, however, disingenuous of Sweller et al. to contrast worked examples and learning through problem solving, as if they were genuine alternatives with the same goals, rather than accepting they have different goals, and then explaining why worked examples today are more effective than history suggests they used to be in the days of 'rote learning', long since discredited in the eyes of so many.

(I personally applaud an emphasis on understanding but I do believe that as this emphasis increased during the twentieth century, reformers underestimated the difficulty of teaching-with-understanding, which is far harder than they have commonly supposed.)

At this point, let me give a personal example. Suppose that I am helping H. to pass his GCSE exam. (I am.) He has completely forgotten how to solve frequency density problems. (He has.) What do I do? With a few days to go to the exam I go through several FDPs with him, explaining the ideas behind them and the steps to be taken. I am thus using *worked examples with reasons*, matching perfectly the Workman example already mentioned, except that I insert much more explanation. This is by far the closest I ever get to direct US-style instruction.

Now suppose that I am introducing quadratic equations to a young pupil with no immediate exam. My goal now is to *educate* him or her in mathematics to appreciate a network of concepts and connections and tactics and strategies that, *inter alia*, will enable them to solve quadratic equations but will also enable them to do much more than that. Such an approach is more akin to *discussing* an interesting chess position with a learner, focusing on particular tactical and strategical points.

Spot the difference between *instruction* and *education*.

That concludes my necessarily very brief and incomplete response to this article. If the EMS Newsletter wishes to intervene again in the current debates then may I suggest that an editorial note be added to make clear that this is done in the context of highly-charged controversies about mathematics teaching and learning in schools; and that you invite protagonists from both sides to contribute and so attempt to bring them together, rather than allowing one party to present one-sided views likely to inflame passions and drive the sides even further apart.

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David Wells
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Personal column

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

Awards

The London Mathematical Society (LMS) and the Institute of Mathematics and its Applications (IMA) announce that **John Barrow** (University of Cambridge, UK) will receive the **Christopher Zeeman Medal** for the Promotion of Mathematics to the Public.

The first **Stephen Smale Prize** was awarded at the FoCM'11 meeting in Budapest on 14 July 2011 to **Snorre H. Christiansen** (University of Oslo).

Antonio Córdoba Barba has been awarded the **Spanish National Award for Research 2011 "Julio Rey Pastor"** in the area of Mathematics and Information Technologies.

Hendrik de Bie (Ghent University, Belgium) has been awarded the first **Clifford Prize** by the 2011 International Conference on Clifford Algebras and their Applications in Mathematical Physics (ICCA).

The **Shaw Prize** in the Mathematical Sciences 2011 is awarded in equal shares to **Demetrios Christodoulou** (ETH Zurich) and to **Richard S. Hamilton** (Columbia University, NY).

The **Rollo Davidson Prize** for 2011 has been awarded jointly to **Christophe Garban** (École Normale Supérieure de Lyon, France) and **Gábor Pete** (University of Toronto, Canada).

Christopher Hacon (University of Utah, US) has been awarded the **Antonio Feltrinelli Prize** in Mathematics, Mechanics and Applications by the Accademia Nazionale dei Lincei, Italy.

Johan Håstad (Stockholm's Royal Institute of Technology) has received the 2011 **Gödel Prize**, sponsored jointly by SIGACT and the European Association for Theoretical Computer Science (EATCS).

Harald Andrés Helfgott (CNRS/École Normale Supérieure, Paris) and **Tom Sanders** (University of Oxford, UK) have been jointly awarded the 2011 **Adams Prize**.

Raul Ibañez (Universidad País Vasco, Spain) has received the **Prize for Dissemination of Science** 2011, awarded by COSCE (Confederation of National Associations of Spain).

Christian Kirches (University of Heidelberg, Germany) has been awarded the **Klaus Tschira Prize** in Mathematics for 2011.

Angela McLean (University of Oxford) has been awarded the 2011 **Gabor Medal** of the Royal Society of London.

The **Heinz Hopf Prize** 2011 at ETH Zurich has been awarded to **Michael Rapoport** (University of Bonn, Germany).

The von-Kaven Prize in Mathematics 2011 is awarded by DFG to **Christian Sevenheck** (University of Mannheim, Germany).

Herbert Spohn (Technical University of Munich, Germany) has been awarded the 2011 **Dannie Heineman Prize** for Mathematical Physics and the 2011 **AMS Leonard Eisenbud Prize** for Mathematics and Physics.

Jean-Pierre Winterberger (Université de Strassbourg, France) has been awarded the 2011 **AMS Frank Nelson Cole Prize** in Number Theory.

The Clay Mathematics Institute has awarded its 2011 **Research Awards** to **Yves Benoist** (CNRS, Université de Paris Sud 11, France), **Jean-François Quint** (Université de Paris 13, France) and Jonathan Pila (University of Oxford, UK).

The **Ferran Sunyer i Balaguer Prize 2011** has been awarded to **Jayce Getz** (McGill University, Canada) and **Mark Goresky** (Princeton University, USA).

One of the **Alexander von Humboldt Professorships** for 2011 has been awarded to **Friedrich Eisenbrand** (École Politechnique Fédérale de Lausanne).

The London Mathematical Society has awarded several prizes for 2011: the **Polya Prize** to **E. Brian Davis** (King's College London, UK); the **Senior Whitehead Prize** to **Jonathan Pila** (University of Oxford, UK); the **Naylor Prize and Lectorship in Applied Mathematics** to **J. Bryce McLeod** (University of Oxford, UK); and several **Whitehead Prizes** to **Jonathan Bennett** (University of Birmingham, UK), **Alexander Gorodnic** (University of Bristol, UK), **Barbara Niethammer** (University of Oxford, UK) and **Alexander Pushnitski** (King's College London, UK).

Rościsław Rabczuk (University of Wrocław, Poland) was awarded the **Dickstein Main Prize** of the Polish Mathematical Society.

Adam Paszkiewicz (University of Łódź, Poland) was awarded the **Banach Main Prize** of the Polish Mathematical Society.

Deaths

We regret to announce the deaths of:

Thierry Aubin (21 March 2011, France)
Frank Bonsall (22 February 2011, UK)
Hans-Jurgen Borchers (10 September 2011, Germany)
Jesús de la Cal Aguado (25 August 2011, Spain)
Albrecht Dold (26 September 2011, Germany)
Christof Eck (14 September 2011, Germany)
William Norrie Everitt (17 July 2011, UK)
Hans Grauert (4 September 2011, Germany)
Harro Heuser (21 February 2011, Germany)
Michel Hervé (3 August 2011, France)
Jaroslav Jezek (13 February 2011, Czech Republic)
Heinrich Kleisli (5 April 2011, Switzerland)
Pierre Lelong (7 October 2011, France)
Heinrich-Wolfgang Leopoldt (28 February 2011, Germany)
Mikael Passare (15 September 2011, Sweden)
Gerhard Preuss (2 September 2011, Germany)
Juan B. Sancho Guimerá (15 October 2011, Spain)
Sarah Shepherd (13 September 2011, UK)
Werner Uhlmann (11 February 2011, Germany)
Francis Williamson (8 January 2011, France)
Mario Wschebor (16 September 2011, Uruguay)