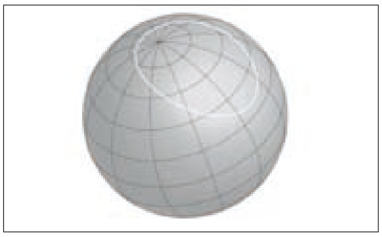
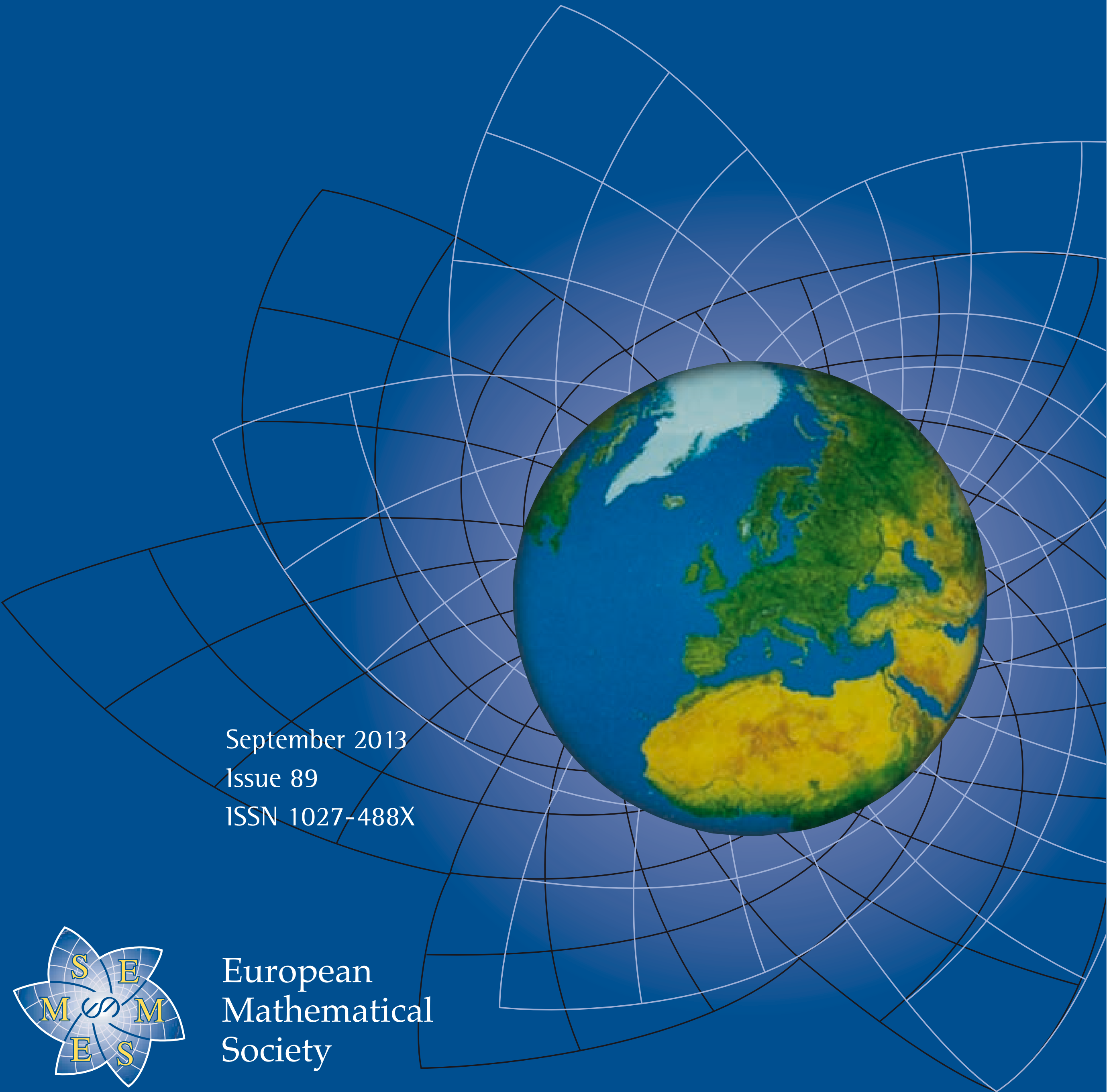
NEWSLETTER

OF THE EUROPEAN MATHEMATICAL SOCIETY





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EMS Agenda

2013

2–6 September
16th Conference of Women in Mathematics, Bonn, Germany
Lectures by the EMS Lecturer for 2013 Tamar Ziegler (Technion, Israel)
http://europeanwomeninmaths.org/activities/conference/16th-general-meeting-ewm

11–12 September
Raising Public Awareness Committee Meeting, Newcastle, United Kingdom
Ehrhard Behrends: behrends@mi.fu-berlin.de

27–28 September
Ethics Committee, Bucharest, Romania
Arne Jensen: matarne@math.aau.dk

1–4 October
Meeting of the Education Committee, Moscow Center for Continuous Mathematical Education, Moscow, Russia
Guenter Toerner: guenter.toerner@uni-due.de

3–19 October
IMPAN-EMS Bedlewo School: “EMS School on Computational Aspects of Gene Regulation”, Będlewo, Poland
http://bioputer.mimuw.edu.pl/school.php

12–13 October
EMS-PTM Joint Meeting “A. Mostowski Centenary”, Warsaw, Poland

30 October
Applied Mathematics Committee
Maria J. Esteban: esteban@ceremade.dauphine.fr

11–12 November
Meeting of the Publications Committee, Lake Bled, Slovenia
Bernard Teissier: teissier@math.jussieu.fr

23–24 November
Raising Public Awareness Committee meeting, Newcastle, UK
Ehrhard Behrends: behrends@mi.fu-berlin.de

2014

25–26 April
Annual Meeting of the Committee for Developing Countries, Berlin, Germany
http://euro-math-soc.eu/EMS-CDC/
Andreas Griewank: griewank@math.hu-berlin.de

28–29 June
EMS Council Meeting, San Sebastian, Spain

2016

18–22 July
7th European Congress of Mathematics, Berlin, Germany

Editorial: Mathematical Documentation: Towards a New Ecosystem

Bernard Teissier (Institut de Mathématiques de Jussieu – Paris Rive Gauche, France)

This text presents the personal views of its author and not necessarily those of the EMS Publications Committee or of the EMS.

The advent of electronic publication has been changing our documentation practices for a quarter of a century but, compared to what has been happening in the last few years, the change in the previous years has been a sort of analytic continuation from the previous era. During that period, journals have created electronic versions and some e-only journals have been created at the initiative of learned societies or groups of mathematicians, sometimes with scientific success, for example in probability theory. But this and the advent of freely accessible preprint archives have not deeply affected the definition of what a mathematical journal is. During that period also, we have heard visionaries explain how wonderful it would be if everything were freely accessible on the web, in Open Access (OA). It was also the time when electronic access allowed publishers to "bundle" their journals more and more, selling them in large interdisciplinary batches. This was the major change in the business model and has succeeded because of many librarians' desire to maximise the number of publications to which their users have access. The financial and scientific drawbacks of bundling for the end users were not immediately apparent but now they are.

Apart from the beautiful dream of having everything freely accessible, one of the motivations of the OA movement has been to fight the financially predatory behaviour of some publishers, based in large part on the bundling technique. Indeed, we mathematicians have criticised the publishers' bundling as much because of its cost as because of the deleterious effect it has on the average quality of publications. It essentially annihilates the influence of readers' judgment and, in particular, the moderating effect which that judgment has had on the creation of new journals.

In the last few years we have entered a new phase. The day of OA has finally dawned; it is supported by everyone and policymakers have been convinced that publicly funded research should be freely accessible for all as quickly as possible. But now that OA is no longer a dream, we must cope with the problems of reality: how to build an economically sustainable publication and retrieval system providing OA but also all the necessities of science, such as the creation of an organised corpus of validated and cross-referenced results in their final form, as our libraries have been providing for centuries, and the preservation of this corpus for the distant future. All these activities are essential for our work and that of our successors and have a cost, which however must not be grossly exaggerated to satisfy shareholders' greed. We must also build a system which is scientifically sustainable. This may seem provocative, since OA is supposed to boost research, but in reality it has its dangers. For example, the multiplication of freely accessible, potentially useful documents competing for our attention makes visibility more and more important. With the growing greed of institutions for visibility they tend to rely more and more on evaluation tools which measure visibility and not true scientific value (admittedly a lot harder to measure but so much more important!) and when applied to promotions, hiring and grants, this leads to an ecosystem of science which many, and not only mathematicians, consider to be disastrous.

If we do not react strongly[1] to this trend, in less than ten years we will be evaluated by ratings agencies which will induce universities to invest more in subject A and less in subject B because A has a greater impact factor and therefore will increase the visibility of the university (such agencies already exist: see Academic Analytics, http://www.academicanalytics.com/). This behaviour is of course not new but it will become much more systematic and, well, "scientific". The more access is free the more visibility becomes a merchandise – a new object of greed. Also, if we do not help our libraries to adapt their role of preservation and help in the access to documentation, we may find ourselves in the hands of private academic data-mining companies which will, for a fee of course, do for us and our students what librarians do now (only it will be on the basis of bibliometry). Major publishers are already investing in such companies. In the process, our libraries will disappear (see Odlyszko's article, http://de.arxiv.org/pdf/1302.1105.pdf). The extent to which they are ignored in discussions on OA is truly amazing.

I believe that we need to consider overhauling the system in its totality: publishing ideas and knowledge, organising their accessibility, preserving them forever and evaluating the quality of research.

It is not enough to experiment with new business models, for example e-journals, which can offer OA because they are managed by dedicated volunteers and supported by a generous institution or association. These experiments are useful but if they remain just that, they compete not only with commercial publishers, which is often a part of their purpose, but also with the academic publishers, which are so precious to us and which are severely handicapped in this time of change because they do not often have the means, financial or otherwise, to

[1] See http://am.ascb.org/dora/ and page 12 of this Newsletter.

make the necessary investments, while the big publishers do. These experiments also contribute to the proliferation of journals and increase the need for time spent refereeing by researchers. There are already voices advising the replacement of referees, which are so difficult to find, by statistics of downloads. Do we really want that?

We do need new business models, and experiments, but they must be compatible with a general vision. For example, few experiments in mathematics involve some measure of scientific control by the readers, as the old subscription system did. It seems that for them the OA economy is entirely a supply-side economy, à la Reagan. From this point of view, OA is the Universal Bundle of publishing and so it has the defects of bundles but worse! It is claimed that, like in the pre-OA era, the scientific quality is guaranteed by the refereeing system but in an OA world, where it is so easy to publish, if the editorial committees and the refereeing system are of course still necessary, they are no longer sufficient. We need other regulatory systems to prevent the proliferation of journals and papers which are published for the sake of publishing, a practice encouraged by bibliometry.

There are many new propositions for the funding of OA publication. The least imaginative, which is a brutal and thoughtless adaptation of the classical "readers pay the costs" system to OA, is the "Authors Pay the Costs (APC)" system, cleverly named "Article Processing Charge" by the publishers. In spite of protestations to the contrary, it will create a documentation bubble and, in addition, puts the researcher under the scientific control of some funding authority and in need of spending even more time with funding requests. It is also, in the end, quite expensive with the charges now requested. Unfortunately, it is supported by an energetic lobby of publishers and some scientists outside mathematics, who are used to it for historical reasons (colour pictures in the life sciences) and who see nothing wrong with it. Most mathematicians reject it as an outrage to freedom and a threat to the quality of publications but some others disagree and believe that it is a viable model provided the financing system is tightly controlled and not driven by greed. I believe that in this regard the honest ones will unfortunately serve to justify predatory behaviours. More acceptable alternatives where institutions such as libraries pay for services (refereeing, editing, attributing compatible metadata, preparing for long term preservation, etc.) rendered by the publishers are being experimented. Being more original, they are less well understood by politicians but are developing well, in particular in the humanities (see Freemium, http://en.wikipedia.org/wiki/Freemium) and should be studied by us.

In the new ecosystem, journals and documentation portals should have the support of a reasonable number of libraries or other public institutions, based on a positive judgment of users on their quality. This support should be conditional on the acceptance by the journal of a charter of good practice covering all the aspects of publishing.

This would help to pare down mediocre journals and in particular keep out "predatory publishers" (see *http://en.wikipedia.org/wiki/Predatory_open_access_publishing*) in which the APC guarantees no serious refereeing or long-term preservation but just posting on some website. If the Gold APC system prevails, and there is no enforcement of a charter of good practice, in a short time it will be extremely difficult to distinguish predatory, mildly predatory, not-too-serious and serious journals. By serious of course I do not mean those with a high impact factor but those who do their job seriously, from refereeing to metadata and contribution to a stable corpus and ensuring very long-term preservation of what they publish.

The support could take the form of "crowd funding" or subscriptions by consortia of libraries or institutions, again based on scientific judgment by competent people and not usage statistics. In this spirit, the French National network of Mathematics Libraries (RNBM) has negotiated a national subscription to all the journals of the EMS Publishing House and is negotiating a similar one with the French Mathematical Society. These subscriptions are funded by the CNRS but they could just as well be funded by a consortium of universities. Alternatively, universities could devote an incompressible part of their budget to the support of academic journals offering OA, chosen by researchers. This should be encouraged by governments as "good practice" for universities.

Other good practices concern the balance between in-depth scientific judgment (which has its dangers) and bibliometry (which is dangerous by nature).

In the new ecosystem, hiring and promotion committees as well as grants committees will make explicit in writing how much of their decision is based on an in-depth scientific judgment of the work and how much on the reputation or the impact factor of the journals in which it is published.

Libraries will make explicit in writing how much of their decision to buy, subscribe or unsubscribe is based on an in-depth scientific judgment by competent scientists and how much on the reputation or the usage statistics or impact factors of the journals.

Concerning the problem of referees, I propose that some institution (for example, the EMS) should organise rather large groups of journals (to preserve anonymity) which would every year publish a list of referees found particularly meritorious by the editors. Those distinguished in this way could use it in their CV as a valuable recognition.

In conclusion, the notion, implicit in the discourse of many advocates of the APC, that we need to replace our existing system by OA and APC journals and "there is no alternative" is part of the intoxication propagated by the APC lobby. The definition of the future system of mathematical documentation is, I think, still, in part and for a little while, in the hands of mathematicians, but not through an explosion of new journals which would guarantee a chaotic transition. It is, rather, through a daily discipline: in the decision to publish; in the decision to post articles, pre- and post-refereeing on a free access archive with a long life expectation (remember that a lot of our research is already in OA thanks to these archives), in the time taken to referee, in the evaluation of research by reading papers and not by the reputation of journals, in supporting the journals with good practices, in particular for corpus-building and copyright freedom, in fighting energetically the author-pays system and the misuse of bibliometry, in helping the libraries to make the transition, and in working to convince academic authorities that it is in their interests, and also a part of their remit, to support such a system instead of following the lures of visibility merchants and giving them money cut out from the documentation budgets (it *has* happened). Of course one of our main collective tools to do this is the learned societies which represent us but our individual actions are crucial.

I am grateful to Frédéric Hélein and Thierry Bouche for numerous exchanges on this subject, and to Tomaz Pisanski and Rui Loja Fernande (members of the EMS Publication Committee) for their comments.

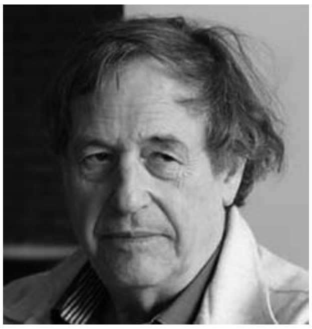

Photo: Fischer, Gerd.
© Mathematisches Forschungsinstitut Oberwolfach

Bernard Teissier is the Chair of the Publications Committee of the EMS. He is an emeritus CNRS research director and member of the Institut Mathématique de Jussieu-Paris Rive Gauche.

New Editor Appointed

Volker Remmert has been trained as a mathematician and as an historian (Diploma in mathematics, 1993; PhD in history, 1997). He is a professor of history of science and technology at Wuppertal University (Germany). His main research interests are in the history of mathematics in Germany in the 19th and 20th centuries and in the history of early modern science (16th to 18th centuries). He has written on the history of mathematical publishing in Germany in the 19th and 20th centuries (together with Ute Schneider: *Eine Disziplin und ihre Verleger – Disziplinenkultur und Publikationswesen der Mathematik in Deutschland, 1871–1949*, Bielefeld 2010), on the history of mathematics in the Nazi period (The German Mathematical Association during the Third Reich: Professional Policy within the Web of National Socialist Ideology, in: D. Hoffmann / M. Walker (eds.): *The German Physical Society in the Third Reich: Physicists between Autonomy and Accommodation*, Cambridge et al. 2012, pp. 246–279), on Jesuit science ("Our Mathematicians Have Learned and Verified This": Jesuits, Biblical Exegesis, and the Mathematical Sciences in the Late 16th and Early 17th Centuries, in: J. van der Meer et al. (eds.): *Nature and Scripture in the Abrahamic Religions: Up to 1700*, 2 vol, Leiden/Boston 2008, vol. II, pp. 665–690), on the connections between landscape design and the mathematical sciences in the early modern period ("Il faut être un peu Geomètre": Die mathematischen Wissenschaften in der Gartenkunst der Frühen Neuzeit, in: *DMV-Mitteilungen* 21(2013), 23–31) and on the role of visual strategies in the 17th century scientific revolution (*Picturing the Scientific Revolution: Title Engravings in Early Modern Scientific Publications*, Philadelphia 2011), which has been made into a nice coffee table book by publishers not belonging to the global STM players giving us so many headaches today.

EMS Executive Committee Meeting in Belfast on the 19th, 20th, and 21st of April 2013

Stephen Huggett (University of Plymouth, UK)

Martin Mathieu, President of the Irish Mathematical Society, welcomed the Executive Committee to Belfast, and Marta Sanz-Solé thanked the Irish Mathematical Society for hosting this meeting of the Executive Committee.

Reports

The President reported that the new EMS scientific secretary Mika Koskenoja had been appointed, as agreed at our Helsinki meeting. An early task of his was to prepare approaches to private foundations requesting support for summer schools, similar to the ones organized as Marie Curie actions previously. For example, we are in correspondence with the Chair of the Scientific Advisory Board of the Clay Mathematics Institute, who might be interested in a partnership with the EMS.

The Treasurer explained the details behind the financial statement for 2012. He expressed the hope that our resources would allow us to support summer schools and other initiatives in a less conservative way in the near future. The Unione Matematica Italiana will be applying to the 2014 Council to move up to class 4. Some other societies might be invited to consider a change of class, upwards.

Dmitry Feichtner-Kozlov will work on compiling a selection of departments to approach for institutional membership. The list of new institutional members was welcomed.

Scientific Meetings

Volker Mehrmann reported on preparations for the 7th European Congress of Mathematics. Organizational matters are proceeding well, and some sponsors have been obtained. A company has been commissioned to do the logo and web design, and these are well under way. The organizers will also create an app. The budget will be presented to the next Council meeting.

The Executive Committee agreed the membership of the Otto Neugebauer Prize Committee, and then the President reported on consultations with Kaiserslautern about the Felix Klein Prize Committee, after which the Executive Committee agreed its membership.

The choice of Chairs of the Scientific and Prize Committees was the subject of a long discussion. Some general principles were agreed, in particular that normally the Chairs should not come from the organizing country, that the Chairs should be very strong mathematicians with a very broad view and a very good network, and that it would be preferable for the Chair of the Scientific Committee not to be in the same field as the corresponding person in previous (recent) ECMs.

Several names were then discussed in detail and at length for the Chair of the Scientific Committee, and subsequently for the Prize Committee. Two short lists were drawn up, and it was agreed that the Executive Committee would have email votes on these lists, in several rounds. It was also agreed that the Chairs of the two committees should come from different fields, which would be assured by voting first for the Scientific Committee and then for the Prize Committee.

Committees and plenary speakers have been chosen for the (Italian-Spanish) Congress in Bilbao in 2014. Discussions about mini-symposia are under way. We expect an application for an EMS distinguished speaker, and perhaps also the participation of an EMS Lecturer. The Executive Committee appointed Laurence Halpern as liaison person.

Society Meetings

The Executive Committee noted the memorandum of understanding agreed for the Council 2014, to be held in Donostia/San Sebastián on the 28th and 29th of June.

Following an invitation from Betül Tanbay, the next Presidents' meeting will be in Istanbul on April 12, 2014, in the Senate room of the Bogaziçi University.

Committee for Developing Countries

The Executive Committee discussed three strong applications for Emerging Regional Centres of Excellence, and agreed to wait for recommendations from the CDC, including more information about educational activities in these centres.

Publications Committee

The report from the Chair was noted. The Executive Committee approved the remit, as follows:

The committee takes up relevant questions related to scientific publications and in particular, mathematical publications. It acts as advisory group of the Executive Committee on publication matters and on publication strategies.

Jouko Väänänen was appointed liaison person for this Committee.

Discussion on the EMS publishing house

The President described the European Mathematical Foundation, its Board of Trustees, and its decision making process. At the next meeting of the Board, open access would be discussed. Should there be two series of journals, with different business models (as for the AMS and LMS journals)? Points made in the ensuing discussion included the following.

- All publications should be open after a certain moving wall.
- Long-term accessibility is an important concern.
- There is a risk that some people with grants will no longer publish in "old-fashioned" journals.
- How do we make sure that the money that now comes from the libraries will still go to (academic) publishers?
- There are several variants of business models.
- An alliance of mathematical societies (and perhaps learned societies in other fields of science) is needed.
- One could charge exactly the costs (the diamond model). This was widely supported by the Executive Committee.

The Executive Committee then turned to the separate question of terms of office for editors. It was agreed that the Board of Trustees should be encouraged to move in that direction at its next meeting.

Finally, the new monograph award has been financed from the profits of the EMS publishing house. Should we consider other ways of using these profits, for example keeping a certain percentage of the revenue for EMS activities such as summer schools?

Many improvements (such as author identification) in ZentralblattMath were noted with applause. The Executive Committee agreed that massive publicity was needed, including the fact that EMS members have free access to ZentralblattMath.

Funding Organisations and Political Bodies

Reporting on Horizon 2020, the President said that no final decisions had been taken about the budget. For research, 70 to 80 billion euros may be expected. The President has tried to have mathematics included among the COST target areas.

The President reported that participation in ISE meetings is a good way to be in touch with other European learned societies. The San Francisco Declaration on Research Assessment http://am.ascb.org/dora/files/SFDeclarationFINAL.pdf was noted with applause.

Closing

At the invitation of the three French mathematical societies, the next Executive Committee meeting will be in Paris, at the Institut Henri Poincaré, on the 8th, 9th, and 10th of November 2013.

The Executive Committee expressed its deep gratitude to the Irish Mathematical Society, and to Martin Mathieu in particular, for their warm hospitality.

EMS Committee for European Solidarity

Carles Casacuberta (University of Barcelona, Spain), Armen Sergeev (Russian Academy of Sciences, Moscow, Russia) and Igor Krichever (Columbia University, New York, USA)

Among the tasks of the European Mathematical Society, great importance has been given since its foundation to cooperation and solidarity between European countries. Indeed, this was one of the major inspiring forces of the First European Congress of Mathematics in 1992 and has continued to be a main aim of the society since then.

The Committee for Support of East-European Mathematicians has played a fundamental role in the EMS for many years, mostly by awarding travel grants to young researchers and offering support to organisers of conferences or advanced courses in target countries. It has been chaired by Jean-Marc Deshouillers until 1997, Heiner Zieschang until 2001, Andrzej Pelczar until 2005, Jan Kratochvíl until 2010 and Carles Casacuberta at present. The vice-chair is Igor Krichever and the Executive Committee liaison member is Armen Sergeev. The current list of members is:

Lucian Beznea (Romanian Academy, Bucharest)
Matej Brešar (University of Ljubljana)
Carles Casacuberta (University of Barcelona)
Andrey Dorogovtsev (National Academy of Sciences, Kiev)
Vladimir Dragović (Serbian Academy of Sciences and Arts, Belgrade)
Jiří Fiala (Charles University, Prague)
Yulij Ilyashenko (Cornell University, Ithaca)
Stefan Jackowski (University of Warsaw)
Igor Krichever (Columbia University, New York)
Frank Neumann (University of Leicester)

After a year-long period of open discussion about the committee's scope and mission, the EMS Executive Committee approved in March 2013 a new remit and changed the name to "Committee for European Solidarity". There were no essential changes, however, in the main purpose of the committee, namely fostering the development of mathematics in economically less-favoured European regions, especially from countries in Eastern

Europe. Besides awarding travel grants to young talented researchers, the committee expects to undertake its own initiatives, such as easing access to digital libraries or other research resources, promoting the organisation of training activities and supporting bilateral or multilateral conferences in suitable geographical areas.

Applications for financial support are to be forwarded to the committee through an online form which is accessible through the committee's webpage on the EMS website (www.euro-math-soc.eu/comm-eur-solid.html). Funding for conferences or courses will normally be devoted to awarding grants to early-career participants selected by the organisers. Decisions will be taken by the committee at two deadlines each year, normally in April and October, or faster in urgent cases.

The committee aims to become more active in the coming years by fostering the organisation of events and offering advice and assistance about the launching of development projects. Proposals by EMS members are very welcome. Indeed, a part of the annual budget of the committee is devoted to new initiatives. If sufficiently purposeful and relevant, such actions may gather EMS funding for two or more years.

Special efforts will be made to promote scientific activities in Eastern European regions that might enhance collaboration between institutions of several countries. The committee also seeks to interact effectively with the Developing Countries Committee of the EMS, which shares essentially the same mission at a worldwide level.

From left to right: Igor Krichever (vice-chair), Armen Sergeev (liaison member) and Carles Casacuberta (chair) at Steklov Mathematical Institute, June 2013.

About Springer Book Archives

EMS Committee for Developing Countries

Springer Verlag has launched an initiative called Springer Book Archives:

http://www.springer.com/authors/oba?SGWID=0-1726313-0-0-0,

where each author of a book published by Springer is requested to give their consent for the book to be included in this database. The forms to be filled in include the following option:

'You will receive royalties or can choose to waive them in support of charitable organisations such as INASP or Research4Life, which help provide the developing world with access to scientific research.'

We invite all authors who wish to do so to send a message to Springer using the link:

http://www.springer.com/authors/oba?SGWID=0-1726313-12-849404-0,

requesting that Springer support the European Mathematical Society and its Committee for Developing Countries (EMS-CDC)*. At present, the only regular income for the EMS-CDC is via reviewers' donations of their honoraria through Zentralblatt. Support through the Springer scheme would greatly aid the committee's ongoing work facilitating access to mathematical education and knowledge in the developing world. For more information on the activities of the EMS-CDC, please visit our website:

http://euro-math-soc.eu/EMS-CDC/.

[1] "Some History and Reminiscences of the Committee for Developing Countries" by Tsou Sheung Tsun has been published in the Newsletter of the European Mathematical Society, Issue 86, December 2012, p. 7.

Third Call for Applications/ Expression of Interest

Emerging Regional Centres of Excellence (EMS-ERCE) European Mathematical Society

Michel Waldschmidt (Université P. et M. Curie – Paris 6, France)

The idea of the EMS-ERCE project is that the EMS selects, endorses and helps a number of emerging regional centres of excellence to offer training to MSc level to students from less developed countries in their region. The designation EMS-ERCE was first awarded to the Abdus Salam School of Mathematical Sciences (ASSMS) in Lahore, Pakistan. In the EMS Newsletter Issue 81 (September 2011), there were two articles about this centre. In 2013, two more centres were elected: the Centro de Investigación en Matemáticas A.C. (CIMAT) in Guanajuato (Mexico) and the Vietnam Institute for Advanced Study in Mathematics (VIASM) in Hanoi (Vietnam).

With the success of this scheme we are now seeking more applications from developing countries. We would like to cover all regions. With the global proliferation of emerging economies worldwide, there are varying degrees of development among developing countries, just as in the developed world. In order to benefit from this situation our strategy of cooperation and help has to be adapted to the different levels of development.

Very good centres exist in emerging economies, where students from the least developed regions can be trained to Master's level or higher. After a Master's degree, such students could be given the option of coming to Europe to do a PhD. This is much more cost-effective than sending these students directly to Europe. It is in this spirit that the Committee for Developing Countries of the European Mathematical Society (EMS-CDC)[1] proposed the scheme of Emerging Regional Centres of Excellence (EMS-ERCE). Based on experience gained with ASSMS, CIMAT and VIASM, we know that such a scheme can work well, with backing from the EMS and provided there are institutions in the emerging economies that are actively getting involved.

This idea is meeting with a positive response from a number of mathematicians from Europe, South America, South Africa and Asia. The advantages of such a procedure are threefold:

1. It is cheaper in general to send a student to a nearby country or region.
2. The students will be less disoriented.
3. The educating institution will gain experience and prestige.

As we know, there are already a number of prestigious institutions of international renown in emerging regions. They are, of course, welcome to apply if the scheme interests them. In that case, they would add lustre to the scheme.

The criteria for eligibility are:

1. The centre is of good scientific standing in the region and in neighbouring regions.
2. It has a good track record in both research and teaching.
3. The centre has an international outlook.
4. The centre has good long-term prospects with sustainable institutional support and financial resources.
5. The centre is willing to admit and educate graduate students from less developed regions. It should have the infrastructure to do so; in particular, the language of instruction should preferably be English, French or Spanish.
6. The degree aimed at is MSc (and PhD in exceptional cases).
7. The centre is willing to welcome distinguished foreign visiting mathematicians for collaboration in research and for teaching graduate courses.
8. The centre should assist smaller centres nearby – the label should have a positive effect not only for the selected institution but more widely for the development of mathematics in the region.

If selected, the centre will be labelled EMS-ERCE, initially for four years but renewable subject to mutual agreement.

The advantages for the centre are:

1. The label can add prestige and visibility to the centre, which will most probably attract more and better students.
2. Often this will, in turn, secure funding from local and regional sources.
3. The members of the CDC will be there to give support and advice whenever needed. Since this will be considered a core activity of the CDC's mission, the centre will get priority of the CDC's time and resources.

4. The CDC will be on hand to help those students who wish to and are capable of continuing their studies after their MSc.
5. The CDC will try to send experienced lecturers to give short or medium courses, e.g. by involving the Voluntary Lecturers Scheme run by the IMU.
6. The CDC will seek European hosts for researchers from these centres for visits and/or collaborations.
7. The CDC will make available small grants for members of the centres to attend conferences when appropriate.

Thanks to this EMS-ERCE scheme, selected institutions will provide assistance to institutions in less developed regions nearby and gain, in return, experience and contacts to further develop themselves. At the same time, with much less expenditure, a larger number of students can receive their first graduate education, in a setting not too removed from their own. This is a practical and efficient way for mathematicians to help other mathematicians.

The members of the ERCE subcommittee of the EMS-CDC are:

Giulia Di Nunno (Oslo)
Anna Fino (Torino)
Michel Jambu (Nice)
Michel Thera (Limoges)
Ramadas Ramakrishnan Trivandrum (ICTP)
Tsou Sheung Tsun (Oxford)
Begona Vitoriano (Madrid)
Paul Vaderlind (Stockholm)
Michel Waldschmidt (Paris)

Michel Waldschmidt is Chair of the Committee for Developing Countries of the European Mathematical Society.

Application or Expression of Interest European Mathematical Society Emerging Regional Centres of Excellence

Each interested institute is asked to send us a brief description of its activities and its suitability, together with a covering letter and supporting material, addressed to:

Giulia Di Nunno: g.d.nunno@cma.uio.no *or*
Tsou Sheung Tsun: tsou@maths.ox.ac.uk *or*
Michel Waldschmidt: miw@math.jussieu.fr

Institutes are welcome to discuss their centre's profile informally with any member of the ERCE subcommittee (listed above) before submitting their application.

The preliminary deadline for application or expression of interest is **28 February 2014**.

http://www.euro-math-soc.eu/comm-develop.html

Turin, Berlin, Paris… Joseph Louis Lagrange, a Truly European Mathematician!

Sylvie Benzoni-Gavage (Université Claude Bernard Lyon 1, Villeurbanne, France)

One of the greatest scientists of the 18th century passed away 200 years ago. Giuseppe Lodovico Lagrangia (1736–1813) was born in Turin and became famous as Joseph Louis Lagrange. He became interested in mathematics at a very young age after reading a memoir by Edmond Halley. At the age of 16, he had already been appointed as a teacher at the Royal School of Artillery in Turin. There, he founded a scientific association which in turn gave rise to the Academy of Turin. He would submit his early work to the great Leonard Euler, who was impressed enough to push him into the Academy of Berlin. When Euler left Berlin, Lagrange was urged to move and replace him as the head of the mathematics section.

His years in Berlin were mostly devoted to celestial mechanics. He was awarded several prizes from the Science Academy of Paris for his work on Lunar libration, on Jupiter's satellites, on the three-body problem, on the secular equation of the Moon and on the perturbation of trajectories of comets.

Preceded by his fame, he moved to Paris in 1787, some 20 years after having met there the very influential encyclopedist Jean Le Rond d'Alembert. Lagrange thus became a professor of analysis at the Ecole Polytechnique, founded in 1794. He also contributed to the definition of the metric system as Chair of the Commission of Weights and Measures.

Modern science owes him a lot, as we can see from the numerous tools widely used nowadays that bear his name. We may think of the "Lagrange interpolation polynomials" in numerical analysis, "Lagrange multipliers" in optimisation theory, "Lagrange points" in space engineering, "Lagrangian coordinates" in mathematical and computational fluid mechanics, etc.

It is fair to say that Lagrange set the foundations for whole theories, and in particular the "calculus of variations" initiated by Euler. This is a branch of mathematics which has seen tremendous developments and has many applications in analytical mechanics and physics, especially through the "principle of least action". Based on this principle, the so-called Euler–Lagrange equation is one of the most ubiquitous mathematical models. It applies to the search of paths that minimise distance ('geodesics') or time - hence the famous brachistochrone curve, for example. We could cite many more, higher dimensional applications, regarding, for instance, liquid crystals, superfluidity and supraconductivity.

In addition, Lagrange made some less well-known but nevertheless important contributions. One of them has to do with water waves theory. Starting from the fluid equations that were established by Euler in his 1757 memoir at the Science Academy of Berlin, Lagrange managed to derive a general equation for surface wave propagation in shallow water. This equation is known simply as the 'wave equation', and happens to be the same as the one established by d'Alembert concerning vibrating strings. Only the physical parameters change, a fact that Lagrange did of course point out.

"Therefore, we can also deal with the wave equation by means of the methods we have already employed in the Theory of sound propagation," he said in his memoir on the theory of fluid motion (1781).

Several events are being organised this year in his honour. A conference will take place 18–19 October 2013 at CIRM, Luminy (France), which will celebrate both the man and his works. Mathematicians and historians from various European countries will give an overview of the themes mentioned above. This event is connected with the '2013, Mathematics of Planet Earth' initiative.

Website: http://math.univ-lyon1.fr/~benzoni/Lagrange/Lagrange-en.html.

For more information, contact:
CIRM – Centre International de
Rencontres Mathématiques
Marseille – Luminy
http://www.cirm.univ-mrs.fr, twitter.com/@_CIRM.

San Francisco Declaration on Research Assessment – Putting science into the assessment of research

The European Mathematical Society has endorsed the San Francisco Declaration on Research Assessment.

"The San Francisco Declaration on Research Assessment (DORA), initiated by the American Society for Cell Biology (ASCB) together with a group of editors and publishers of scholarly journals, recognizes the need to improve the ways in which the outputs of scientific research are evaluated. The group met in December 2012 during the ASCB Annual Meeting in San Francisco and subsequently circulated a draft declaration among various stakeholders. DORA as it now stands has benefited from input by many of the original signers listed below. It is a worldwide initiative covering all scholarly disciplines. We encourage individuals and organizations who are concerned about the appropriate assessment of scientific research to sign DORA."

These concerns have always been crucial to the EMS, as shown by the similarity between DORA and the position of the EMS, in particular as expressed in its Code of Practice on good practice and ethical behaviour in the publication, dissemination and assessment of mathematical research.

For the whole text of the declaration, see http://am.ascb.org/dora/.

"Comics&Science" in Italy – Quite Obviously

Lucca Comics&Games is Italy's main convention focusing on exactly what you would expect it to focus on (if names mean something). As you probably know, Lucca is a wonderful Tuscan town but fans and professionals alike simply don't make any distinction between the town and its main public event. Both the town and the event aim to address "quality" entertainment as a key-moment when it comes to making everyone's lives better and more (intelligently) enjoyable.

Given this premise, it was only a matter of time before a section called "Lucca Comics&Science" was born (quite an obvious tribute paid to how scientifically-minded people, say "nerds", have always had a weak spot for comics, movies and popular culture).

This year's highlights will be the French mathematician Cédric Villani and the Italian cartoonist Leo Ortolani. Villani's flamboyant attitude, dress code and personal passion for *Bandes Dessinées* (French comics) and *Manga* (Japanese comics) alone speak loudly of why he's going to be Comics&Science's guest of honour. The wildly successful "Rat-Man" comic character is why an Italian fan's dream is a quick sketch by, or even just a handshake with, Ortolani. He also happens to be an accomplished geologist – something he hints at in his comics, often featuring references to geology and to other scientific fields – and is thus a fitting choice for a "scientific" comics project he's going to be finally unveiled in Lucca.

Cédric Villani drawn by Leo Ortolani

Lucca Comics&Science
Lucca (Italy), 31 October – 3 November 2013

Organisers:
Andrea Plazzi (Symmaceo Communications, andrea.plazzi@symmaceo.com)
Roberto Natalini (IAC-CNR, Maddmaths! roberto.natalini@cnr.it)

Websites:
http://maddmaths.simai.eu/,
http://www.luccacomicsandgames.com/it/lcg/home/

International Congress of Mathematicians 2014 in Seoul

Hyungju Park (Pohang University of Science and Technology, Korea, and Chairman of the Organising Committee for ICM 2014)

Seoul, Korea
13–21 August 2014
http://icm2014.org

Greetings from the Organisers of ICM 2014

The next International Congress of Mathematicians will take place at Coex in Seoul, Korea, from Wednesday 13 August through to Thursday 21 August 2014. The pre-registration process for ICM 2014 is underway. If you have not yet pre-registered, please do so by following the simple instructions at http://icm2014.org. The ICM e-News is being circulated to people who have pre-registered for the congress. We strongly recommend that you visit the homepage regularly for updated information and ICM related activities. We look forward to welcoming you to the congress in Seoul.

In the centre of Seoul

Korea, with a five-millennia-long history, is an attractive place to visit and Seoul, the capital of Korea for over 600 years, is a vibrant, modern city with a population of 11 million, where traditional culture and cutting-edge trends co-exist in perfect harmony. With a low crime rate and a state-of-the-art subway system, it is one of the safest cities in the world. The congress venue, Coex, is at the heart of downtown Seoul, located right next to a subway station. There are over 7,500 hotel rooms within 5 km of Coex.

Family-friendly ICM

Seoul is full of great family-friendly activities. For the accompanying family members of the ICM participants, an on-site childcare facility will be available. And we recommend some of Seoul's best destinations for the family – from a day at a theme park to outdoor events for youngsters and countless playgrounds in the heart of downtown. Three Disney-style theme parks (Lotte World, Seoul Land and Everland) are around 10 minutes to 1 hour bus/subway ride from central Seoul. Especially during Summer season, all the theme parks are open until late at night performing fantastic laser shows and fireworks shows. Bring your family to Seoul ICM and experience wonderful festivals and magnificent shows at the theme parks in Korea! Family-friendly ICM will make your journey all the more special.

NANUM 2014

To make the congress a true worldwide gathering, the Organising Committee places special emphasis on supporting mathematicians from developing countries. Members of the Korean Mathematical Society fully acknowledge the gracious support received from the international mathematical community in the 1970s and 1980s and hope more countries can share in the benefits. This has motivated the theme of "Solidarity in Mathematics" and 1,000 mathematicians from developing countries will be invited to Korea during ICM 2014. Many of these mathematicians would not have been able to visit an ICM otherwise and stand to take the ICM excitements and new knowledge back to their home countries. The Seoul ICM Travel Fellowship Fund was set up for this purpose and the fund is expected to receive over 2,000,000 USD by 2014, mainly from global corporations and individual donors. By collaborating with IMU/CDC, we are developing selection guidelines for this travel assistance programme, called "NANUM 2014". NANUM is a Korean word meaning "gracious and unconditional sharing". A selection policy integrating age, gender and geographical balance is being carefully crafted.

NANUM 2014 in detail

Criteria:
- Priority will be given to applicants from countries with a GDP of $7,500 (nominal) or less.
- Some underdeveloped regions inside ineligible countries will be included.

Composition of the 1,000:
- 45% senior mathematicians, 45% junior mathematicians, 10% advanced graduate students.
- 'Math School' for 100 (tentative).
- At least 100 female mathematicians.

The applications will be reviewed by five review committees covering the following five regions:
- Africa.
- East and Southeast Asia including China and North Korea.
- South and West Asia including the Indian subcontinent.
- Eastern Europe including North Asia.
- Central and South America.

Timeline of the application and selection procedure:
- 28 Feb 2013: selection of 48 international NANUM ambassadors.

- 10 Jun 2013 – 31 Aug 2013: applications received.
- 31 Dec 2013: review of applications completed.
- Jan 2014: notification of acceptance.

MENAO

A MENAO event (MENAO stands for "Mathematics in Emerging Nations: Achievements and Opportunities"), which features about 100 participants and discussants and is open to an additional 350 observers, will take place on the day immediately preceding the opening of the congress.

The goal of the MENAO event is:

- To listen to the voices of mathematicians and aspiring advanced students of mathematics from the developing world.
- To share success stories of development via partnerships between the local mathematical communities, their governments and international agencies and foundations.
- To review the current status of those efforts and future needs.

A detailed programme will be developed and distributed by the IMU.

Invited plenary, sectional and special lectures

The privilege of sending the invitations belongs to the Organising Committee whereas it is the privilege of the Programme Committee to select the invited plenary and sectional speakers for the congress. Plenary lectures are invited one-hour lectures to be held without other parallel activities. The lectures should be broad surveys of recent major developments, aimed at the entire mathematical community. Sectional lectures are invited 45-minute lectures. Several sectional lectures are scheduled in parallel. The ICM Emmy Noether lecture honours women who have made fundamental and sustained contributions to mathematics and the ICM Emmy Noether lecturer has been chosen by a committee appointed by the IMU Executive Committee. All the invitations have been sent out by the Organising Committee, and hopefully they have them in hand by now.

Hyungju Park [icm@icm2014.org] is Professor of Mathematics at Pohang University of Science and Technology, Korea, and Chairman of the Organising Committee for ICM 2014.

José Luis Rubio de Francia Prize 2012 for Young Mathematicians

María Pe Pereira, visiting professor at the Université de Lille, has been awarded the José Luis Rubio de Francia Prize for 2012. According to the jury statement supporting their decision: "María Pe Pereira (Burgos, 1981) has made some outstanding mathematical contributions to singularity theory, especially in connection to the celebrated Nash problem on arcs for surface singularities. The Nash problem, posed by John Nash in 1968, has been one of the central problems in singularity theory over the last 40 years. In her PhD thesis, in 2011, Maria Pe constructed a unified proof for all quotient singularities of a positive solution to Nash's problem on arcs. Her thesis contained new techniques and insights that could be used in a more general context. Subsequently, Pe Pereira and her PhD advisor Javier Fernández de Bobadilla solved [the Nash problem] in its full generality."

The José Luis Rubio de Francia Prize is awarded by the Real Sociedad Matemática Española (RSME) under the patronage of the Universidad Autónoma de Madrid and the Universidad de Zaragoza, and its aim is to recognise and encourage young mathematicians. Recipients should not be older than 32. It is endowed with 3,000 euros and carries the invitation to give one of the plenary talks at an RSME Congress. The jury for this year's José Luis Rubio de Francia award was chaired by Jesús Bastero and consisted of Professors Noga Alon, Pablo Mira, Gilles Pisier, Marta Sanz-Solé, Agata Smoktunowicz and Cédric Villani.

More information can be found at
http://www.rsme.es/content/view/1282/1/.

Interview with Abel Laureate Pierre Deligne

Martin Raussen (Aalborg, Denmark) and Christian Skau (Trondheim, Norway)

The Abel Prize

Dear Professor Deligne, first of all we would like to congratulate you as the eleventh recipient of the Abel Prize. It is not only a great honour to be selected as recipient of this prestigious prize, the Abel Prize also carries a cash-amount of 6 million NOK, that is around 1 million US$. We are curious to hear what you are planning to do with this money…

I feel that this money is not really mine, but it belongs to mathematics. I have a responsibility to use it wisely and not in a wasteful way. The details are not clear yet, but I plan to give part of the money to the two institutions that have been most important to me: the Institut des Hautes Études Scientifiques (IHÉS) in Paris and to the Institute for Advanced Study (IAS) in Princeton.

I would also like to give some money to support mathematics in Russia. First to the Department of Mathematics of the Higher School of Economics (HSE). In my opinion, it is one of the best places in Moscow. It is much smaller than the Faculty of Mechanics and Mathematics at the University, but has better people. The student body is small; only fifty new students are accepted each year. But they are among the best students. The HSE has been created by economists. They have done their best under difficult circumstances. The department of mathematics has been created five years ago, with the help of the Independent University of Moscow. It is giving prestige to the whole HSE. There I think some money could be well used.

Another Russian institution I would like to donate some money to is the Dynasty Foundation, created by the Russian philanthropist Dmitry Zimin. For them, money is most likely not that important. It is rather a way for me to express my admiration for their work. It is one of the very few foundations in Russia that gives money to science; moreover, they do it in a very good way. They give money to mathematicians, to physicists and to biologists; especially to young people, and this is crucial in Russia! They also publish books to popularize science. I want to express my admiration for them in a tangible way.

The Abel Prize is certainly not the first important prize in mathematics that you have won. Let us just mention the Fields Medal that you received 35 years ago, the Swedish Crafoord Prize, the Italian Balzan Prize and the Israeli Wolf Prize. How important is it for you, as a mathematician, to win such prestigious prizes? And how important is it for the mathematical community that such prizes exist?

For me personally, it is nice to be told that mathematicians I respect find the work I have done interesting. The Fields Medal possibly helped me to be invited to the Institute for Advanced Study. To win prizes gives opportunities, but they have not changed my life.

Pierre Deligne during his acceptance speech at the Abel award ceremony. (Photo: Heiko Junge/NTB scanpix)

I think prizes can be very useful when they can serve as a pretext for speaking about mathematics to the general public. I find it particularly nice that the Abel Prize is connected with other activities such as competitions directed towards children and the Holmboe Prize for high school teachers. In my experience, good high school teachers are very important for the development of mathematics. I think all these activities are marvellous.

Youth

You were born in 1944, at the end of the Second World War in Brussels. We are curious to hear about your first mathematical experiences: In what respect were they fostered by your own family or by school? Can you remember some of your first mathematical experiences?

I was lucky that my brother was seven years older than me. When I looked at the thermometer and realized that there were positive and negative numbers, he would try to explain to me that minus one times minus one is plus one. That was a big surprise. Later when he was in high school he told me about the second degree equation. When he was at the university he gave me some notes about the third degree equation, and there was a strange formula for solving it. I found it very interesting.

When I was a Boy Scout, I had a stroke of extraordinary good luck. I had a friend there whose father, Monsieur Nijs, was a high school teacher. He helped me in a number of ways; in particular, he gave me my first real mathematical book, namely *Set Theory* by Bourbaki, which is not an obvious choice to give to a young boy. I was 14 years old at the time. I spent at least a year digesting that book. I guess I had some other lectures on the side, too.

Having the chance to learn mathematics at one's own rhythm has the benefit that one revives surprises of past centuries. I had already read elsewhere how rational numbers, then real numbers, could be defined starting from the integers. But I remember wondering how integers could be defined from set theory, looking a little ahead in Bourbaki, and admiring how one could first define what it means for two sets to have the "same number of elements", and derive from this the notion of integers. I was also given a book on complex variables by a friend of the family. To see that the story of complex variables was so different from the story of real variables was a big surprise: once differentiable, it is analytic (has a power series expansion), and so on. All those things that you might have found boring at school were giving me a tremendous joy.

Then this teacher, Monsieur Nijs, put me in contact with professor Jacques Tits at the University of Brussels. I could follow some of his courses and seminars, though I still was in high school.

It is quite amazing to hear that you studied Bourbaki, which is usually considered quite difficult, already at that age.
Can you tell us a bit about your formal school education? Was that interesting for you, or were you rather bored?
I had an excellent elementary school teacher. I think I learned a lot more in elementary school than I did in high school: how to read, how to write, arithmetics and much more. I remember how this teacher made an experiment in mathematics which made me think about proofs, surfaces and lengths. The problem was to compare the surface of a half sphere with that of the disc with the same radius. To do so, he covered both surfaces with a spiralling rope. The half sphere required twice as much rope. This made me think a lot: how could one measure a surface with a length? How to be sure that the surface of the half sphere was indeed twice that of the disc?

When I was in high school, I liked problems in geometry. Proofs in geometry make sense at that age because surprising statements have not too difficult proofs. Once we were past the axioms, I enjoyed very much doing such exercises. I think that geometry is the only part of mathematics where proofs make sense at the high school level. Moreover, writing a proof is another excellent exercise. This does not only concern mathematics, you also have to write in correct French – in my case – in order to argue why things are true. There is a stronger connection between language and mathematics in geometry than for instance in algebra, where you have a set of equations. The logic and the power of language are not so apparent.

You went to the lectures of Jacques Tits when you were only 16 years old. There is a story that one week you could not attend because you participated in a school trip…?
Yes. I was told this story much later. When Tits came to give his lecture he asked: Where is Deligne? When it was explained to him that I was on a school trip, the lecture was postponed to the next week.

From left to right: Pierre Deligne, Martin Raussen, Christian Skau. (Photo: Anne-Marie Astad)

He must already have recognised you as a brilliant student. Jacques Tits is also a recipient of the Abel Prize. He received it together with John Griggs Thompson five years ago for his great discoveries in group theory. He was surely an influential teacher for you?
Yes; especially in the early years. In teaching, the most important can be what you don't do. For instance, Tits had to explain that the centre of a group is an invariant subgroup. He started a proof, then stopped and said in essence: "An invariant subgroup is a subgroup stable by all inner automorphisms. I have been able to define the centre. It is hence stable by all symmetries of the data. So it is obvious that it is invariant."

For me, this was a revelation: the power of the idea of symmetry. That Tits did not need to go through a step-by-step proof, but instead could just say that symmetry makes the result obvious, has influenced me a lot. I have a very big respect for symmetry, and in almost every of my papers there is a symmetry-based argument.

Can you remember how Tits discovered your mathematical talent?
That I cannot tell, but I think it was Monsieur Nijs who told him to take good care of me. At that time, there were three really active mathematicians in Brussels: apart from Tits himself, professors Franz Bingen and Lucien Waelbroeck. They organised a seminar with a different subject each year. I attended these seminars and I learned about different topics such as Banach algebras, which were Waelbroeck's speciality, and algebraic geometry.

Then, I guess, the three of them decided it was time for me to go to Paris. Tits introduced me to Grothendieck and told me to attend his lectures as well as Serre's. That was an excellent advice.

This can be a little surprising to an outsider. Tits being interested in you as a mathematician, one might think that he would try to capture you for his own interests. But he didn't?
No. He saw what was best for me and acted accordingly.

Algebraic geometry

Before we proceed to your career in Paris, perhaps we should try to explain to the audience what your subject algebraic geometry is about.
When Fields medalist Tim Gowers had to explain your research subjects to the audience during the Abel Prize announcement earlier this year, he began by confessing that this was a difficult job for him. It is difficult to show pictures that illustrate the subject, and it is also difficult to explain some simple applications. Could you, nevertheless, try to give us an idea what algebraic geometry is about? Perhaps you can mention some specific problems that connect algebra and geometry with each other.
In mathematics, it is always very nice when two different frames of mind come together. Descartes wrote: "La géométrie est l'art de raisonner juste sur des figures fausses" (Geometry is the art of correct reasoning on false figures). "Figures" is plural: it is very important to have various perspectives and to know in which way each is wrong.

In algebraic geometry, you can use intuitions coming both from algebra – where you can manipulate equations – and from geometry, where you can draw pictures. If you picture a circle and consider the equation $x^2+y^2=1$, different images are evoked in your mind, and you can try to play one against the other. For instance, a wheel is a circle and a wheel turns; it is interesting to see what the analogue is in algebra: an algebraic transformation of x and y maps any solution of $x^2+y^2=1$ to another. This equation describing a circle is of the second degree. This implies that a circle will have no more than two intersections points with a line. This is a property you also see geometrically, but the algebra gives more. For instance, if the line has a rational equation and one of the intersection points with the circle $x^2+y^2=1$ has rational coordinates, then the other intersection point will also have rational coordinates.

Algebraic geometry can have arithmetical applications. When you consider polynomial equations, you can use the same expressions in different number systems. For instance, on finite sets on which addition and multiplication are defined, these equations lead to combinatorial questions: you try to count the number of solutions. But you can continue to draw the same pictures, keeping in mind a new way in which the picture is false, and in this way you can use geometrical intuition while looking at combinatorial problems.

I have never really been working at the centre of algebraic geometry. I have mostly been interested in all sorts of questions that only touch the area. But algebraic geometry touches many subjects! As soon as a polynomial appears, one can try to think about it geometrically; for example in physics with Feynman integrals, or when you consider an integral of a radical of a polynomial expression. Algebraic geometry can also contribute to the understanding of integer solutions of polynomial equations. You have the old story of elliptic functions: to understand how elliptic integrals behave, the geometrical interpretation is crucial.

Algebraic geometry is one of the main areas in mathematics. Would you say that to learn algebraic geometry requires much more effort than other areas in mathematics, at least for a beginner?
I think it's hard to enter the subject because one has to master a number of different tools. To begin with, cohomology is now indispensable. Another reason is that Algebraic geometry developed in a succession of stages, each with its own language. First, the Italian school which was a little hazy, as shown by the infamous saying: "In Algebraic geometry, a counterexample to a theorem is a useful addition to it". Then Zariski and Weil put things on a better footing. Later Serre and Grothendieck gave it a new language which is very powerful. In this language of schemes one can express a lot; it covers both arithmetical applications and more geometrical aspects. But it requires time to understand the power of this language. Of course, one needs to know a number of basic theorems, but I don't think that this is the main stumbling block. The most difficult is to understand the power of the language created by Grothendieck and how it relates to our usual geometrical intuition.

Apprentice in Paris

When you came to Paris you came in contact with Alexander Grothendieck and Jean-Pierre Serre. Could you tell us about your first impression of these two mathematicians?
I was introduced to Grothendieck by Tits during the Bourbaki seminar of November 1964. I was really taken aback. He was a little strange, with his shaved head, a very tall man. We shook hands, but did nothing more until I went to Paris a few months later to attend his seminar.

That was really an extraordinary experience. In his way, he was very open and kind. I remember the first lecture I attended. In it, he used the expression "cohomology object" many times. I knew what cohomology was for abelian groups, but I did not know the meaning of "cohomology object". After the lecture I asked him what he meant by this expression. I think that many other mathematicians would have thought that if you didn't know the answer, there wouldn't be any point to speak to you. This was not his reaction at all. Very patiently he told me that if you have a long exact sequence in an abelian category and you look at the kernel of one map, you divide by the image of the previous one and so on…. I recognized quickly that I knew about this in a less general context.

He was very open to people who were ignorant. I think that you should not ask him the same stupid question three times, but twice was all right.

I was not afraid to ask completely stupid questions, and I have kept this habit until now. When attending a lecture, I usually sit in front of the audience, and if there is something I don't understand, I ask questions even if I would be supposed to know what the answer was.

I was very lucky that Grothendieck asked me to write up talks he had given the previous year. He gave me his notes. I learned many things, both the content of the notes, and also a way of writing mathematics… This both in a prosaic way, namely that one should write only on one side of the paper and leave some blank space so he could make comments, but he also insisted that one was not allowed to make any false statement. This is extremely hard. Usually one takes shortcuts; for instance, not keeping track of signs. This would not pass muster with him. Things had to be correct and precise. He told me that my first version of the redaction was much too short, not enough details… It had to be completely redone. That was very good for me.

Serre had a completely different personality. Grothendieck liked to have things in their natural generality; to have an understanding of the whole story. Serre appreciates this, but he prefers beautiful special cases. He was giving a course at Collège de France on elliptic curves. Here, many different strands come together, including automorphic forms. Serre had a much wider mathematical culture than Grothendieck. In case of need, Grothendieck redid everything for himself, while Serre could tell people to look at this or that in the literature. Grothendieck read extremely little; his contact with classical Italian geometry came basically through Serre and Dieudonné. I think Serre must have explained him what the Weil conjectures were about and why they were interesting. Serre respected the big constructions Grothendieck worked with, but they were not in his taste. Serre preferred smaller objects with beautiful properties such as modular forms, to understand concrete questions, for instance congruences between coefficients.

Their personalities were very different, but I think that the collaboration between Serre and Grothendieck was very important and it enabled Grothendieck to do some of his work.

You told us that you needed to go to Serre's lectures in order to keep your feet on the ground?

Yes, because it was a danger in being swept away in generalities with Grothendieck. In my opinion, he never invented generalities that were fruitless, but Serre told me to look at different topics that all proved to be very important for me.

The Weil Conjectures

Your most famous result is the proof of the third – and the hardest – of the so-called Weil conjectures. But before talking about your achievement, can you try to explain why the Weil conjectures are so important?

There were some previous theorems of Weil about curves in the one-dimensional situation. There are many analogies between algebraic curves over finite fields and the rational numbers. Over the rational numbers, the central question is the Riemann hypothesis. Weil had proved the analogue of the Riemann hypothesis for curves over finite fields, and he had looked at some higher-dimensional situations as well. This was at the time where one started to understand the cohomology of simple algebraic varieties, like the Grassmannians. He saw that some point-counting for objects over finite fields reflected what happened over the complex numbers and the shape of the related space over the complex numbers.

As Weil looked at it, there are two stories hidden in the Weil conjectures. First, why should there be a relation between apparently combinatorial questions and geometric questions over the complex numbers. Second, what is the analogue of the Riemann hypothesis? Two kinds of applications came out of these analogies. The first started with Weil himself: estimates for some arithmetical functions. For me, they are not the most important. Grothendieck's construction of a formalism explaining why there should be a relation between the story over the complex numbers, where one can use topology, and the combinatorial story, is more important.

Secondly, algebraic varieties over finite fields admit a canonical endomorphism, the Frobenius. It can be viewed as a symmetry, and this symmetry makes the whole situation very rigid. Then one can transpose this information back into the geometric world over the complex numbers, it yields constraints on what will happen in classical algebraic geometry, and this is used in applications to representation theory and the theory of automorphic forms. It was not obvious at first that there would be such applications, but for me they are the reason why the Weil conjecture is important.

Grothendieck had a program on how to prove the last Weil conjecture, but it didn't work out. Your proof is different. Can you comment on this program? Did it have an influence on the way you proved it?

No. I think that the program of Grothendieck was, in a sense, an obstruction to finding the proof, because it made people think in just a certain direction. It would have been more satisfying if one had been able to do the proof following the program, because it would have explained a number of other interesting things as well. But the whole program relied on finding enough algebraic cycles on algebraic varieties; and on this question one has made essentially no progress since the 70's.

I used a completely different idea. It is inspired by the work of Rankin and his work on automorphic forms. It still has a number of applications, but it did not realize the dream of Grothendieck.

We heard that Grothendieck was glad that the Weil conjecture was proved, of course, but still he was a little disappointed?

Yes. And with very good reason. It would have been much nicer if his program had been realized. He did not think that there would be another way to do it. When he heard I had proved it, he felt I must have done this and that, which I hadn't. I think that's the reason for the disappointment.

You have to tell us about the reaction of Serre when he heard about the proof.
I wrote him a letter when I did not have a complete proof yet, but a test case was clear. I think he got it just before he had to go to the hospital for an operation of a torn tendon. He told me later that he went into the operation theatre in a euphoric state because he knew now that the proof was roughly done.

Several famous mathematicians have called your proof of the last Weil conjecture a marvel. Can you describe how you got the ideas that led to the proof?
I was lucky that I had all the tools needed at my disposal at the same time and that I understood that those tools would do it. Parts of the proof have since been simplified by Gérard Laumon, and a number of these tools are no more needed.

At the time, Grothendieck had ideas for putting into a purely algebraic framework the work of Solomon Lefschetz from the 20s about families of hyperplane sections of an algebraic variety. Of particular interest was a statement of Lefschetz, later proved by William Hodge, the so-called hard Lefschetz theorem. Lefschetz' approach was topological. In contrast to what one might think, if arguments are topological there is a better chance to translate them into abstract algebraic geometry than if they are analytic, such as the proof given by Hodge. Grothendieck asked me to look at the 1924 book *L'analysis situs et la géométrie algébrique*" by Lefschetz. It is a beautiful and very intuitive book, and it contained some of the tools I needed.

I was also interested in automorphic forms. I think it is Serre who told me about an estimate due to Robert Rankin. I looked carefully at it. Rankin was getting some non-trivial estimates for coefficients of modular forms by proving for some related L-functions what was needed to apply results of Landau, in which the location of the poles of an L-function gave information on the poles of the local factors. I saw that the same tool, in a much less sophisticated way, just using that a sum of squares is positive, could be used here because of the control the work of Grothendieck gave on poles. This was enough. The poles were much easier to understand than the zeros and it was possible to apply Rankin's idea.

I had all these tools at my disposal, but I cannot tell how I put them together.

A little bit about subsequent work

What is a motive?
A surprising fact about algebraic varieties is that they give rise not to one, but to many cohomology theories. Among them the l-adic theories, one for each prime l different from the characteristic, and in characteristic zero, the algebraic de Rham cohomology. These theories seem to tell the same story, over and over again, each in a different language. The philosophy of motives is that there should exist a universal cohomology theory, with values in a category of motives to be defined, from which all these theories could be derived. For the first cohomology group of a projective non-singular variety, the Picard variety plays the role of a motivic H^1: the Picard variety is an abelian variety, and from it the H^1 in all available cohomology theories can be derived. In this way, abelian varieties (taken up to isogeny) are a prototype for motives.

A key idea of Grothendieck is that one should not try to define what a motive is. Rather, one should try to define the category of motives. It should be an abelian category with finite dimensional rational vector spaces as *Hom* groups. Crucially, it should admit a tensor product, needed to state a Künneth theorem for the universal cohomology theory, with values in the category of motives. If only the cohomology of projective non-singular varieties is considered, one speaks of pure motives. Grothendieck proposed a definition of a category of pure motives, and showed that if the category defined had a number of properties, modelled on those of Hodge structures, the Weil conjectures would follow.

For the proposed definition to be viable, one needs the existence of "enough" algebraic cycles. On this question almost no progress has been made.

What about your other results? Which of those that you worked on after the proof of the Weil conjecture are you particularly fond of?
I like my construction of a so-called mixed Hodge structure on the cohomology of complex algebraic varieties. In its genesis, the philosophy of motives has played a crucial role, even if motives don't appear in the end result. The philosophy suggests that whenever something can be done in one cohomology theory, it is worthwhile to look for a counterpart in other theories. For projective non-singular varieties, the role played by the action of Galois is similar to the role played by the Hodge decomposition in the complex case. For instance, the Hodge conjecture, expressed using the Hodge decomposition, has as counterpart the Tate conjecture, expressed using the action of Galois. In the l-adic case, cohomology and action of Galois remain defined for singular or non-compact varieties.

This forces us to ask: what is the analogue in the complex case? One clue is given by the existence, in l-adic cohomology, of an increasing filtration, the weight filtration W, for which the i-th quotient W_i/W_{i-1} is a subquotient of the cohomology of a projective non-singular variety. We hence expect in the complex case a filtration W such that the i-th quotient has a Hodge decomposition of weight i. Another clue, coming from works of Griffiths and Grothendieck, is that the Hodge filtration is more important than the Hodge decomposition. Both clues force the definition of mixed Hodge structures, suggest that they form an abelian category, and suggest also how to construct them.

What about the Langlands program? Have you been involved in it?

I have been very interested in it, but I have contributed very little. I have only done some work on $GL(2)$, the linear group in two variables. I tried to understand things. A somewhat remote application of the Weil conjecture has been used in Ngo's recent proof of what is called the fundamental lemma. I didn't do a lot of work myself, though I had a lot of interest in the Langlands program.

French, American and Russian mathematics

You have already told us about the two institutions you mainly have worked for, namely the IHÉS in Paris and then, since 1984, the IAS in Princeton. It would be interesting for us to hear what your motives were for leaving IHÉS and moving to Princeton. Moreover, we would like to hear what unites the two institutions and how they differ, in your opinion.

One of the reasons I left, was that I don't think it's good to spend all of one's life in the same place. Some variation is important. I was hoping to have some contact with Harish-Chandra who had done some beautiful work in representation theory and automorphic forms. That was a part of the Langlands program that I am very interested in, but unfortunately Harish-Chandra died shortly before I arrived at Princeton.

Another reason was that I had imposed on myself to give seminars, each year on a new subject, at the IHÉS in Bures. That became a little too much. I was not really able to both give the seminars and to write them down, so I did not impose the same obligation on myself after I came to Princeton. These are the main reasons why I left the IHÉS for IAS in Princeton.

Concerning the difference between the two institutions, I would say that the Institute for Advanced Study is older, bigger and more stable. Both are very similar in the way that there are many young visitors that come there. So they are not places where you can fall asleep since you will always be in contact with young people who will tell you that you are not as good as you think you are.

In both places there are physicists, but I think the contact with them was more fruitful for me in Princeton than it was in Bures. In Princeton, there have been common seminars. One year was very intense, with both mathematicians and physicists participating. This was due mainly to the presence of Edward Witten. He has received the Fields Medal even though he is a physicist. When Witten asks me questions, it's always very interesting to try to answer them, but it can be frustrating as well.

Princeton is also bigger in the sense that it has not only maths and physics, but also the School of Historical Studies and the School of Social Sciences. There is no real scientific interaction with these Schools but it is pleasant to be able to go and hear a lecture about, for instance, ancient China. One good feature about Bures which you do not have in Princeton is the following: In Bures, the cafeteria is too small. So you sit where you can and you don't get to choose the people you are sitting with. I was often sitting next to an analyst or a physicist and such random informal interactions are very useful. In Princeton, there is one table for the mathematicians, another for the astronomers, the ordinary physicists and so on. You will not be told to go away if you sit down at the wrong table, but still there is segregation.

The Institute for Advanced Study has a big endowment, while the IHÉS had none, at least when I was there. This didn't affect the scientific life. Sometimes it created instability, but the administration was usually able to hide the difficulties from us.

Apart from your connections with French and US mathematics, you have also had a very close contact with Russian mathematics for a long time, even from long before the fall of the iron curtain. In fact, your wife is the daughter of a Russian mathematician. How did your contact with Russian mathematics develop?

Grothendieck or Serre told Manin, who was in Moscow at the time, that I had done some interesting work. The Academy invited me to a conference for I. M. Vinogradov, a terribly anti-Semitic person, by the way. I came to Russia, and I found a beautiful culture for mathematics. At that time mathematics was one of the few subjects where the communist party could not meddle, as it did not understand it at all, and this turned it into a space of freedom.

We would go to somebody's home and sit by the kitchen table to discuss mathematics over a cup of tea. I fell in love with the atmosphere and this enthusiasm for mathematics. Moreover, Russian mathematics was one of the best in the world at that time. Today there are still good mathematicians in Russia, but there has been a catastrophic emigration. Furthermore, among those wanting to stay, many need to spend at least half of the time abroad, just to make a living.

You mentioned Vinogradov and his anti-Semitism. You talked to somebody and asked whether he was invited?

It was Piatetskii-Shapiro. I was completely ignorant. I had a long discussion with him. For me it was obvious that someone like him should be invited by Vinogradov, but I was explained that that was not the case.

After this introduction to Russian mathematics, I still have some nostalgia for the beautiful memories of being in Moscow and speaking with Yuri Manin, Sergey Bernstein or being at the Gelfand seminar. There was a tradition, which still exists, of a strong connection between the university and the secondary education. People like Andrey Kolmogorov had a big interest in secondary education (perhaps not always for the best)

They have also the tradition of Olympiads and they are very good at detecting promising people in mathematics early on in order to help them. The culture of seminars is in danger because it's important that the head of the seminars is working full time in Moscow and that is not always the case. There is a whole culture which I think it's important to preserve. That is the reason why I used half of the Balzan Prize to try to help young Russian mathematicians.

That was by a contest that you arranged.

Yes. The system is falling apart at the top because there is no money to keep people, but the infrastructure was so good that the system continues to produce very good young mathematicians. One has to try to help them and make it possible for them to stay somewhat longer in Russia so that the tradition can continue.

Competition and collaboration in mathematics

Some scientists and mathematicians are very much driven by the aim to be the first to make major discoveries. That seems not to be your main driving force?
No. I don't care at all.

Do you have some comments on this culture in general?
For Grothendieck it was very clear: he once told me that mathematics is not a competition sport. Mathematicians are different and some will want to be the first, especially if they are working on very specific and difficult questions. For me it's more important to create tools and to understand the general picture. I think mathematics is much more a collective enterprise of long duration. In contrast to what happens in physics and biology, mathematical articles have long and useful lives. For instance, the automatic evaluation of people using bibliographic criteria is particularly perverse in mathematics, because those evaluation methods take only account of papers published during the last three or five years. This does not make sense in mathematics. In a typical paper of mine, I think at least half of the papers cited can be twenty to thirty years old. Some will even be two hundred years old.

You like to write letters to other mathematicians?
Yes. Writing a paper takes a lot of time. Writing it is very useful, to have everything put together in a correct way, and one learns a lot doing so, but it's also somewhat painful. So in the beginning of forming ideas, I find it very convenient to write a letter. I send it, but often it is really a letter to myself. Because I don't have to dwell on things the recipient knows about, some short-cuts will be all right. Sometimes the letter, or a copy of it, will stay in a drawer for some years, but it preserves ideas and when I eventually write a paper, it serves as a blue-print.

When you write a letter to someone and that person comes with additional ideas, will that result in a joint paper?
That can happen. Quite a lot of my papers are by me alone and some are joint work with people having the same ideas. It is better to make a joint paper than having to wonder who did what. There are a few cases of genuine collaborations where different people have brought different intuitions. This was the case with George Lusztig. Lusztig had the whole picture of how to use l-adic cohomology for group representations, but he did not know the techniques. I knew the technical aspect of l-adic cohomology and I could give him the tools he needed. That was real collaboration.

A joint paper with Morgan, Griffiths and Sullivan was also a genuine collaboration.

Also with Bernstein, Beilinson and Gabber: we put together our different understandings.

Work style, pictures, and even dreams

Your CV shows that you haven't taught big classes of students a lot. So, in a sense, you are one of the few full-time researchers in mathematics.
Yes. And I find myself very lucky to have been in this position. I never had to teach. I like very much to speak with people. In the two institutions where I have worked young people come to speak with me. Sometimes I answer their questions, but more often I ask them counter-questions which sometimes are interesting, too. So this aspect of teaching with one-to-one contact, trying to give useful information and learning in the process, is important to me.

I suspect it must be very painful to teach people who are not interested, but are forced to learn math because they need the grade to do something else. I would find that repulsing.

What about your mathematical work style? Are you most often guided by examples, specific problems and computations, or are you rather surveying the landscape and looking for connections?
First I need to get some general picture of what should be true, what should be accessible and what tools can be used. When I read papers I will not usually remember the details of the proofs, but I will remember which tools were used. It is important to be able to guess what is true and what is false in order not to do completely useless work. I don't remember statements which are proved, but rather I try to keep a collection of pictures in my mind. More than one picture, all false but in different ways, and knowing in which way they are false. For a number of subjects, if a picture tells me that something should be true, I take it for granted and will come back to the question later on.

What kind of pictures do you have of these very abstract objects?
Sometimes very simple things! For instance, suppose I have an algebraic variety, and hyperplane sections, and I want to understand how they are related, by looking at a pencil of hyperplane sections. The picture is very simple. I draw it in my mind something like a circle in the plane and a moving line which sweeps it. Then I know how this picture is false: the variety is not one-dimensional, but higher dimensional and when the hyperplane section degenerates, it is not just two intersection points coming together. The local picture is more complicated, like a conic which becomes a quadratic cone. These are simple pictures put together.

When I have a map from some space to another I can study properties it has. Pictures can then convince me that it is a smooth map. Besides having a collection of pictures, I also have a collection of simple counter-examples, and statements that I hope to be true have to be checked against both the pictures and the counter-examples.

So you think more in geometric pictures than algebraically?
Yes.

Some mathematicians say that good conjectures, or even good dreams, are at least as important as good theorems. Would you agree?
Absolutely. The Weil conjectures, for instance, have created a lot of work. Part of the conjecture was the existence of a cohomology theory for algebraic systems, with some properties. This was a vague question, but that is all right. It took over twenty years of work, even a little more, in order to really get a handle on it. Another example of a dream is the Langlands program which has involved many people over fifty years, and we have now only a slightly better grasp of what is happening.

Another example is the philosophy of motives of Grothendieck about which very little is proved. There are a number of variants taking care of some of the ingredients. Sometimes, such a variant can be used to make actual proofs, but more often the philosophy is used to guess what happens, and then one tries to prove it in another way. These are examples of dreams or conjectures that are much more important than specific theorems.

Have you had a "Poincaré moment" at some time in your career where you, in a flash, saw the solution of a problem you had worked on in a long time?
The closest I have been to such a moment must have been while working on the Weil conjecture when I understood that perhaps there was a path using Rankin against Grothendieck. It took a few weeks after that before it really worked, so it was a rather slow development. Perhaps also for the definition of mixed Hodge structures, but also in this case, it was a progressive process. So it was not a complete solution in a flash.

When you look back on fifty years of doing mathematics, how have your work and your work style changed over the years? Do you work as persistently as you did in your early years?
I am not as strong as I was earlier, in the sense that I cannot work as long or as intensively as I did. I think I have lost some of my imagination but I have much more technique that can act as a substitute to some extent. Also the fact that I have contact with many people, gives me access to some of the imagination I am lacking myself. So when I bring my technique to bear, the work can be useful, but I'm not the same as when I was thirty.

You have retired from your professorship at IAS rather early…
Yes, but that's purely formal. It means I receive retirement money instead of a salary; and no School meetings for choosing next year's members. So that's all for the best, it gives me more time for doing mathematics.

Hopes for the future

When you look at the development of algebraic geometry, number theory and the fields that are close to your heart, are there any problems or areas where you would like to see progress soon? What would be particularly significant, in your opinion?
Whether or not it's within reach in ten years, I have absolutely no idea; as it should be… but I would very much like to see progress in our understanding of motives. Which path to take and what are the correct questions, is very much in the air. Grothendieck's program relied on proving the existence of algebraic cycles with some properties. To me this looks hopeless, but I may be wrong.

The other kind of question for which I would really like to see some progress is connected with the Langlands program, but that is a very long story…

In yet another direction, physicists regularly come up with unexpected conjectures, most often using completely illegal tools. But so far, whenever they have made a prediction, for instance a numerical prediction on the number of curves with certain properties on some surface - and these are big numbers, in the millions perhaps - they were right! Sometimes previous computations by mathematicians were not in accordance with what the physicists were predicting, but the physicists were right. They have put their fingers on something really interesting, but we are, so far, unable to capture their intuition. Sometimes they make a prediction and we work out a very clumsy proof without real understanding. That is not how it should be. In one of the seminar programs that we had with the physicists at IAS, my wish was not to have to rely on Ed Witten but instead to be able to make conjectures myself. I failed! I did not understand enough of their picture to be able to do that, so I still have to rely on Witten to tell me what should be interesting.

What about the Hodge conjecture?
For me, this is a part of the story of motives, and it is not crucial whether it is true or false. If it is true, that's very good and it solves a large part of the problem of constructing motives in a reasonable way. If one can find another purely algebraic notion of cycles for which the analogue of the Hodge conjecture holds, and there are a number of candidates, this will serve the same purpose, and I would be as happy as if the Hodge conjecture were proved. For me it is motives, not Hodge, that is crucial.

Private interests – and an old story

We have the habit of ending these interviews by asking questions that are outside of mathematics. Could you tell us a little bit about your private interests outside your profession? We know about your interest in nature and in gardening, for example.
These are my main interests. I find the earth and nature so beautiful. I don't like just to go and have a look at

a scenery. If you really want to enjoy the view from a mountain, you have to climb it by feet. Similarly, to see the nature, you have to walk. As in mathematics, in order to take pleasure in nature – and the nature is a beautiful source of pleasure – one has to do some work.

I like to bicycle because that's also a way to look around. When distances are a little bigger than what is convenient by feet, this is another way of enjoying the nature.

We heard that you also build igloos?
Yes. Unfortunately, there's not enough snow every year and even when there is, snow can be tricky. If it's too powdery, it's impossible to do anything; likewise if it's too crusty and icy. So there is maybe just one day, or a few hours each year when building an igloo is possible, and one has to be willing to do the work of packing the ice and putting the construction together.

And then you sleep in it?
And then I sleep in the igloo, of course.

You have to tell us what happened when you were a little child.
Yes. I was in Belgium at the sea-side for Christmas, and there was much snow. My brother and sister, who are much older than me, had the nice idea to build an igloo. I was a little bit in the way. But then they decided I might be useful for one thing: if they grabbed me by my hands and feet, I could be used to pack the snow.

Thank you very much for granting us this interview. These thanks come also on behalf of the Norwegian, the Danish and the European mathematical societies that we represent. Thank you very much!
Thank you.

Martin Raussen is associate professor of mathematics at Aalborg Univesity, Denmark. Christian Skau is professor of at the Norwegian University of Science and Technology at Trondheim. They have together taken interviews with all Abel laureates since 2003.

Speech in Honour of the Abel Prize Laureate

Hendrik Lenstra (Mathematisch Instituut, Universiteit Leiden, The Netherlands)

Your majesty,
Excellencies,
Prize Winner,
Distinguished guests.

My name is Hendrik Lenstra, from the Universiteit Leiden in the Netherlands, and it is my great pleasure to say a few words in honour of the Abel Prize laureate Pierre Viscount Deligne. I hope that the rest of you will share, if not in the honour, then at least in the pleasure.

Tomorrow, the praises of the laureate will be sung by a team of expert mathematicians, for an audience of mathematicians. Tonight, I will mainly address the non-mathematicians, and I want to start by explaining how Deligne is viewed by his fellow mathematicians. That is actually quite simple to state: many believe that Deligne is God (so that God is a viscount). Others go a bit further: to them, Deligne is greater than God because God knows what is true but Deligne also knows why. In this context of "mathematical theology" I would like to quote Jean-Pierre Serre, who should be known to many of you, as he was the first Abel Prize winner ten years ago. Serre said that while the other sciences search for the rules that God has chosen for this Universe, we mathematicians search for the rules that even God has to obey. That is what Serre said, and it should give you an idea of the standing of the Abel Prize relative to certain other prizes that are awarded in Scandinavia.

Coming back to the nature of Deligne, let me tell you something that happened to me about ten years ago, when I was quietly sitting in my office at Leiden. Suddenly, there was an enormous fuss in the hallway, and one of my colleagues stormed into my office, exclaiming: "Deligne made a mistake, Deligne made a mistake!" Of course, I did not believe him, but he produced one of those thick yellow volumes that have the name of Grothendieck on the cover but were mostly written by Deligne, and sure enough, there it was: a mistake by Deligne, a genuine mathematical mistake by Deligne. I am sure that the prize winner appreciates that I tell this story in Oslo only after he has received the prize. It shows the dual nature of Deligne: he is both divine and human. We all make mistakes all the time. But the difference is: if I make a mistake, nobody shouts it through the hallways.

I discovered another way in which Deligne is like most of us that is relevant for the present occasion.

Most of you are from Norway, and Norway is a kingdom. Deligne is from Belgium, and Belgium is a kingdom. I am from the Netherlands, and the Netherlands are a kingdom – in fact, the oldest of the three; we have been an independent kingdom since the time of Abel (and in those days that kingdom comprised Belgium as well). And if you are a citizen of a kingdom then you can profit from royal wisdom (which is second only to the divine wisdom that I discussed previously). To explain what I mean by royal wisdom, I want to read a few lines that Gauss wrote in his big number theory book, which appeared in 1801 (one year before Abel was born). The lines are taken from the dedication, in the beginning of the book, to the Duke of Brunswick, who had supported Gauss's entire education. I am certain that you would prefer not to listen to Gauss's original Latin and, since I could not find an English translation, you will forgive that I made my own. So here is how Gauss addressed the Duke:

"…I believe that nobody is unaware of the considerable extent of Your munificence towards everybody who appears to cultivate the highest modes of learning, and that not even those sciences that are generally believed to be more abstract and further removed from usefulness in daily life are excluded from Your protection because in Your profound wisdom, keen on profiting from anything that benefits the well-being of human society, You have Yourself grasped the intimate and intrinsic unity of all sciences."

Those are Gauss's words, and with the same words I like to praise the profound wisdom of the Norwegian people, as represented by their wise King and his wise ministers, in establishing the Abel award. One of the stated objectives of the Abel Prize is to draw more young people into mathematics and we all know that a world with more mathematicians is a happier world, a more peaceful world – in short: a better world.

Ladies and gentlemen! Two thousand, three hundred years ago there was another wise king, King Ptolemy of Egypt, who founded the Academy of Alexandria. The wisdom of this king and his ministers was so great that their academy lasted for 900 years. Earlier today, Professor Piene, while discussing the laureate's work, referred to Euclid and to Diophantus. Both Euclid and Diophantus were active at Alexandria but few people realise that there were 500 years between them. This is yet to be equalled by any modern institution. Thus, I want to finish this speech by drinking to the health of the Abel Prize, that it may live to an age of 900 years, to the benefit and the well-being of human society.

I thank you for your attention.

Hendrik Lenstra,
Akershus Castle, 21 May 2013

Algebraic Geometry and the Ongoing Unification of Mathematics: Explaining Deligne to a Broad Audience

Ravi Vakil (Stanford University, Stanford, USA)

Rien n'est plus fécond, tous les mathématiciens le savent, que ces obscures analogies, ces troubles reflets d'une théorie à une autre, ces furtives caresses, ces brouilleries inexplicables ; rien aussi ne donne plus de plaisir au chercheur. Un jour vient où l'illusion se dissipe ; le pressentiment se change en certitude ; les théories jumelles révèlent leur source commune avant de disparaître ; comme l'enseigne la Gita *on atteint à la connaissance et à l'indifférence en même temps. La métaphysique est devenue mathématique, prête à former la matière d'un traité dont la beauté froide ne saurait plus nous émouvoir.*

As every mathematician knows, nothing is more fruitful than these obscure analogies, these blurred reflections of one theory into another, these furtive caresses, these inexplicable disagreements; also nothing gives the researcher greater pleasure. The day dawns when the illusion vanishes; intu-

ition turns to certitude; the twin theories reveal their common source before disappearing; as the Gita *teaches us, knowledge and indifference are attained at the same moment. The metaphysics has become mathematics, ready to form the subject matter of a treatise, whose cold beauty no longer has the power to move us.*

A. Weil, [11, par. 2]

Making the case for mathematics

On 21 May 2013, Pierre Deligne of the Institute for Advanced Study was awarded the Abel Prize at an award ceremony at the University Aula in Oslo, [1]. In conjunction with Deligne's award, there were several mathematical lectures, including the Prize Lecture by Deligne himself, and distinguished mathematical lectures by Nick Katz of Princeton University and Claire Voisin of the École Polytechnique and the CNRS. I gave the "Science Lecture", which was intended for a broad audience. This gave me the opportunity to collect my thoughts on what we as a community can and should and must do to explain and motivate mathematics to the wider public. In this short note, I want to explain what I said, and first to explain what I was trying to do.

My goal when speaking to the public is to get across mathematics: what it is we do – why mathematics is so beautiful, and why it is so powerful, and how those two qualities are so closely related.

We are used to speaking to those in our own field – not just mathematicians but those in our particular research area. If we are not careful, it can become difficult to speak even to those in other parts of mathematics. This would be sad because many of us become mathematicians because we want to think large thoughts, not because we want to think narrow thoughts.

When speaking to a broader audience (even a colloquium of mathematicians), our message must necessarily be universal and not require the knowledge and experience we have gained through years of contemplation.

For example, learning mathematics changes the way that one thinks. We learn how to think well about certain things: size, shape, number, chance and more. We become humble about what we know and how well we know it. We learn to think carefully, precisely and rigorously.

We must remember what it was like to be attracted to mathematics in the first place, when we were younger. Since we are unusual people, we must also remember why many others are also attracted to mathematics. For me, it was the beauty, power and generality of mathematics. I love the sudden unexpected powerful connections between disparate, seemingly unrelated subjects.

We should deliberately use metaphor. (Mathematicians – or at least pure mathematicians, as I can speak only for my own tribe – are trained to be scrupulously precise and are reluctant to use metaphor. But a metaphor is not a lie.) We should follow the central dictum of speaking and writing: *'Know your audience'*. We should speak to them on their own terms and appeal to the reasons they already love mathematics, or at least are ready to love mathematics. We often miss this opportunity.

On the other hand, it is counterproductive to pander. Speaking for areas near my own, number theorists and algebraic geometers sometimes lie about applications, vaguely invoking cryptography. When we don't believe what we are saying, and our heart isn't in it, the audience knows it. We have such an interesting story to tell that there is no need to stretch the truth.

Of course, genuine applications are worth discussing, especially if they are genuinely interesting. There are fantastically appealing ideas in cryptography. In my field of algebraic geometry, there are many applications, as made clear by this summer's huge conference on applied algebraic geometry [8] put on by the Society for Industrial and Applied Mathematics (SIAM). But most pure algebraic geometers are not that aware of the most exciting developments in this area and shouldn't fake it. We must tell the story that only we can tell and not pretend to be someone we are not.

How, then, should we talk to others? When children are five years old, what do they like? They like (among other things) puzzles (certainly a form of mathematical thinking), dinosaurs and stars. The North American popular press (I cannot speak for other countries) is full of articles about dinosaurs, astronomy and fundamental physics, and none of them stretch our credulity about how these advances will help us build a cheaper microwave. We are interested in these discoveries for the right reasons – because of the central human virtue of curiosity. Why, then, do we not think of mathematics in the same way? The public is primed for this.

When movies are made about spectacularly brilliant people, they are never about petroleum engineers or economists. The archetypal geniuses are mathematicians (and musicians and theoretical physicists). We should take advantage of the romance of mathematics and not turn people off, while of course converting audiences to our particular religion.

Deligne in his gracious acceptance speech, and in his subsequent interview [3], was absolutely eloquent on these points. He was direct, honest and very human. He did not ask us to understand the details of his work; instead he showed who he was and why he was led to think the way he did. He made the case for the usefulness of basic research, of the quest for "useless knowledge", [4]. He made the case for mathematics.

While knowing we cannot be as articulate as Deligne, it is important that we still do not shy away from trying. So I now give some version of the ideas I discussed in Oslo in May. I should be clear that I was not speaking to mathematicians, and so I hope those near to my field are not bothered by the fact that what I say may be considered "trivial" (a word we sadly abuse). My main hope is to get across some reason of why we do what we do, and why we find it so terribly compelling.

Impossible connections

Of all the fields of mathematics, perhaps the one with the most fearsome reputation is algebraic geometry. This is the field that Deligne works in, and this is the field that I work in (in a much more modest way). On one hand, the abstraction is so extreme that it is hard to explain even to people in other parts of mathematics. But, on the other hand, it deals with ideas so fundamental and basic that I want to get across some of its magic. I also want to get across what it is I do on a day-to-day basis, and the kinds of things I think about.

Mathematics carries a heavy burden. Mathematics is very powerful and ever-present. Because it is so useful, because it is so necessary, most people only have a chance to learn it in terms of what they need to know in order to do something else. They only see the output of mathematics – a bunch of recipes to learn and to memorise. But what is often lost is what is behind these rules, the reasons for such wonderful structure to exist and how to discover it.

An important aspect of the discovery of mathematics is the dramatic unexpected connection, when you find a relation between two things that seem to have no right to be related. For this reason, mathematicians love coincidence and we love paradox. That is when you discover something new. The "aha!" moment you get when you suddenly see a connection, when something complicated suddenly becomes simple, when two things that are radically different suddenly become the same – this is really the best experience. It has been described as being in a dark room, feeling your way around, finding the pieces of furniture one by one, when someone abruptly turns on the light, and all of a sudden you recognise where you are. At this point you know you have discovered something potentially very deep that will change how you see the world.

This is what I want to tell you about today: the ongoing grand unification and simplification of mathematics. The more we understand, the simpler things become. You can get a glimpse of this in the textbooks we use in universities. The introductory classes have huge textbooks with many colourful pictures. As the courses get more advanced, the books get smaller and smaller, until the most advanced courses often have very slim texts. PhDs in mathematics tend to be much shorter than those in other subjects because their main contributions are very clever ideas, rather than reams of evidence.

This is why the history of mathematics is a constant story of unification of different ideas from different sources, from science, from the real world, from pure thought and, more recently, from computer science.

Π

Deligne's work is all about making sense of a dramatic connection between different parts of mathematics. Before I get to that, I want to give you an example of another dramatic connection that is about something you already know, to show you that these mysteries are right under our noses, if we know how to pay attention.

You may think that you know what π is. Some people are taught that $\pi = 3.14159\ldots$ but this is no answer – how does this sequence of numbers continue? More precisely, π is often defined as the ratio of the circumference of a circle to the diameter. Already we have a mystery that teachers tend not to mention – why does this not depend on the size of the circle? But in any case, π is often introduced as a constant of nature, like the speed of light c, which is approximately $300,000$ km/s.

Now we know the speed of light up to roughly .02 parts per billion, so to around 11 digits, [9]. By comparison, we know π up to 10 trillion digits, [14]! What accounts for this huge difference? There must be some fundamental difference in our "ways of understanding" π and c.

Knowing the speed of light is (at least currently) an empirical question, one of measurement. It is amazing that we know it to so much accuracy and to do this requires a kind of experimental genius.

Knowing π is not a question of measurement; there is a non-empirical way of understanding π, which is purely "formal" and which exists purely in our own heads. (I do not intend to get into subtle philosophical questions here, although I do not mind acknowledging their existence.) For example, one way of understanding π purely formally is by the Gregory-Leibniz series

$$\pi = 4 - 4/3 + 4/5 - 4/7 + \cdots .$$

This is an inefficient way of computing π – after half a million terms, it produces only five correct decimal digits of π. But it clearly makes no reference to the external world. It is thus, in principle, possible to really know π to great accuracy in a non-experimental way.

You should be clear on the fact that there is a great mystery here — why is it that the empirical "version" of π is connected to something purely in our own heads? This is less obvious than it might at first appear. The Nobel Prize winning physicist Eugene Wigner described this in a remarkable essay [12] as "the unreasonable effectiveness of mathematics in the natural sciences".

I want to point out further that not only is there a mystery to puzzle through here, but that we feel compelled to figure it out. This is a fundamental part of what it means to be human.

Before returning to Deligne, let me tell you a few more remarkable facts about π that should make you feel compelled to want to understand more.

If you wish to know a physical constant to great accuracy – to know more and more digits – then you necessarily determine the digits in order. With π, this needn't be the case – if you are willing to work in base 16 then there is a means of computing individual digits of π without having computed the earlier ones. This method uses the Bailey–Borwein–Plouffe (BBP) formula [2]:

$$\pi = \sum_{i=0}^{\infty}\left(\frac{1}{16^i}\left(\frac{4}{8i+1} - \frac{2}{8i+4} - \frac{1}{8i+5} - \frac{1}{8i+6}\right)\right).$$

(Similar ideas work in base 10, see [7].)

Secondly, the mathematically experienced reader will be well aware that π is not rational, and is not even algebraic. How can we know this and not want to know *how* we know it? Again, this fundamentally human curiosity to understand structure is the reason why we discover (or, depending on your point of view, create) mathematics.

Here is another appearance of π, which may suggest that π should not be thought of as a number purely in geometry. What is the probability that a random integer is square-free, i.e., has no square factor (for the pedants: greater than 1)? We should initially think of this as an empirical question, as mathematics is an empirical science. Of the first 10 numbers, 70 % are square-free. If you check more and more numbers, you will notice the percentage converging to around 60 %. Even the fact that it should converge should not be obvious and is clearly interesting. But more amazing is what it converges to: $6/\pi^2$. There are (seemingly) no circles here! So, is π fundamentally a geometric notion or an arithmetic notion? Which one is the more important? Which is logically prior? The answer must be that π is simultaneously a geometric and arithmetic notion (and much more), and that this is some sign of the unity of mathematics. Each part of this unity only becomes apparent by an appropriately broad perspective, by an appropriately deep understanding.

Our noodling around with π may seem only a tenuously distant metaphor for Deligne's work but this is not the case; this unification of geometry and arithmetic, and even the use of π, is precisely part of the many insights provided by Deligne. (And of course, Deligne's contribution is less a collection of individual insights than a web of connected insights that lead to a deep understanding of how and why many hitherto unrelated ideas in mathematics are connected.)

As a hint that $6/\pi^2$ should be seen as a clue to even more beautiful insights, $6/\pi^2$ can be understood in terms of Riemann's zeta-function, as $1/\zeta(2)$. Seen from an advanced standpoint, this $1/\zeta(2)$ is a clue that we should think of the integers somehow as forming a "smooth curve", which, from the right point of view (of algebraic geometry), they do.

Before returning to geometry and arithmetic, I will leave you with one last mystery about the speed of light, in which I deliberately obfuscate in order to provoke you into figuring it out. I mentioned that we know c to about .02 parts in a billion and that it is roughly 300,000 km/s (in fact, slightly less). Nonetheless, there is a pure thought algorithm to work out any digit in the speed of light! How is this possible? (And once you realise that this is a trick question, you will realise that it is not obvious what it means to know c to .02 parts in a billion ...)

Pythagorean triples

The link between geometry and arithmetic is ancient and we now consider one of its first instantiations.

The Pythagorean Theorem was known to the classical Greeks but also much earlier to the Chinese and Indian civilizations. It was known earlier still to the Babylonians. (A wise ten-year-old once asked me, were we to meet aliens, if their mathematics would be recognisable to us. Because many things seem to have been discovered independently at different times in human history, it seems to me that the answer would be yes and that, in particular, they would also have something which we would recognise as the Pythagorean Theorem.)

You have likely thought about this at great length but I encourage you to put yourself in the shoes of a young person, to see that the themes here are really very naive and fundamental. (For a mathematician, "naive" is no insult and is, in fact, high praise!)

The Pythagorean Theorem is about *lengths* a, b and c. For some reason, we feel compelled to ask about *integral* solutions – Pythagorean triples. (Once again, this happened multiple times in human history – this shouldn't be seen as some random whim of one person, happening for no good reason. If many people are led to the same question, there must be some reason, even if we cannot put it convincingly into words.)

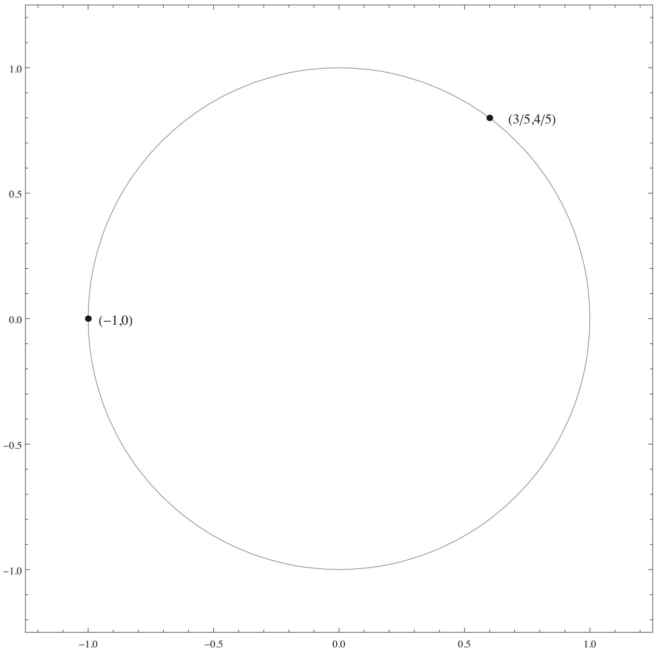

Figure 1. Pythagoras forces us to draw a circle

Once you realise that $3^2 + 4^2 = 5^2$, you should feel compelled to look for more Pythagorean triples and then you will find: $5^2 + 12^2 = 13^2$, $6^2 + 8^2 = 10^2$, ... Then you will inevitably notice some patterns and wonder if they continue. Some are easy (for example, you can "scale up" Pythagorean triples and thus quickly define the notion of a "primitive Pythagorean triple", where a, b and c have no common factor). Some are not so easy. You might notice that $b + c$ is often a perfect square (especially for primitive triples) but not always and realise that nature is trying to tell you something. Similarly, you might notice that the average of a and c is often a perfect square and that this tends to happen at the same time that $b + c$ is a perfect square.

You will inevitably be led to the question: "What are the Pythagorean triples?", and (as with all scientific questions) it begins as a vague, ill-defined question, which later becomes structural once you know more. (Similarly, science went through the stages of "what are the animals" in the process of understanding how and why animals were interrelated.) The answer to this question comes perhaps most naturally from geometry. Rather than finding integral solutions to $a^2 + b^2 = c^2$, by dividing by c^2 it suffices to find *rational* solutions to $A^2 + B^2 = 1$. When we see this, we feel compelled to draw a circle $x^2 + y^2 = 1$ (see Figure 1). When considering questions about the *rational* numbers, we feel compelled to think about them in terms of *real* numbers.

We then use the geometry of the circle. If (A, B) is a solution (i.e., lies on the circle) then consider the line connecting (A, B) to $(-1, 0)$. (There is the special case $(A, B) = (-1, 0)$ to consider but, as we often do in science, we leave consideration of special cases to the end.) We write the line as usual as $y = mx + b$. Then we can compute m and b. Note that without actually doing the computation – but by knowing "how we would do it" – it is clear that m is rational, and then that b is rational.

The remarkable thing is that this is reversible.

Suppose we have a line $y = mx + b$ through $(-1, 0)$, where

m and b are rational. We know that a line meets a circle in two points so we will solve for the other point and will discover that the other point also has rational coordinates. (Implicit here are some ideas which grow up to be rather magical, such as Bézout's Theorem.) This potentially requires doing some annoying algebra but, by thinking about it cleanly, we can avoid doing any algebra by just understanding well how we *would* have done it. We would plug $mx+b$ for y in the equation of the circle:

$$x^2 + (mx + b)^2 = 1.$$

We would expand this out and collect terms and the result would look like this:

$$?x^2+?x+? = 0.$$

We already know one solution ($x = -1$) so we would factor out $(x + 1)$:

$$(x + 1)(?x+?) = 0. \tag{1}$$

But if you think through how you would factor, you would realise that the two question marks in (1) are rational numbers so the zero of the linear form $(?x+?)$ is rational and thus y (which is $mx + b$) is rational too. If you have not done this before, you should feel the urge to test this out with a pencil in the margins.

(For another problem that can be pleasantly solved by pure thought in the same way, show that four distinct points (a, a^2), (b, b^2), (c, c^2) and (d, d^2) on the parabola $y = x^2$ are concyclic if and only if $a + b + c + d = 0$.)

Thus the rational points in the circle are essentially in bijection with lines $y = mx + b$ through $(-1, 0)$ (where the word "essentially" needs to be thought through). Hence they are "essentially" in bijection with slopes m. Now we can do the algebra and we know that we can't lose; all of the creative thought has now taken place. (You can check that $x = (1 - m^2)/(1 + m^2)$ and $y = 2m/(1 + m^2)$.)

There are many things to notice about this solution.

First of all, we started with a fact in geometry (the Pythagorean Theorem), from which we felt compelled to create a question purely in arithmetic (the classification of Pythagorean triples). This second question could have been posed without any reference to geometry. Yet the right way to see it (or at least *a* right way to see it) is in terms of geometry once again.

Second, in answering this question well, we also get tools to answer other mysteries. (For example, you can think through why and when $b + c$ is a perfect square, when (a, b, c) is a primitive Pythagorean triple.)

Third, a sign of the quality of our understanding is the fact that we can solve a much wider family of questions. For example, you can use this to find all rational solutions to

$$x^2 + 5y^2 = 1 \tag{2}$$

(sketched in Figure 2).

For a higher-degree problem, you can also find all solutions to

$$y^4 = x^3 - x^2y, \tag{3}$$

sketched in Figure 3. (What plays the role of $(-1, 0)$ in this latter problem? Why does geometry provide you the clue?)

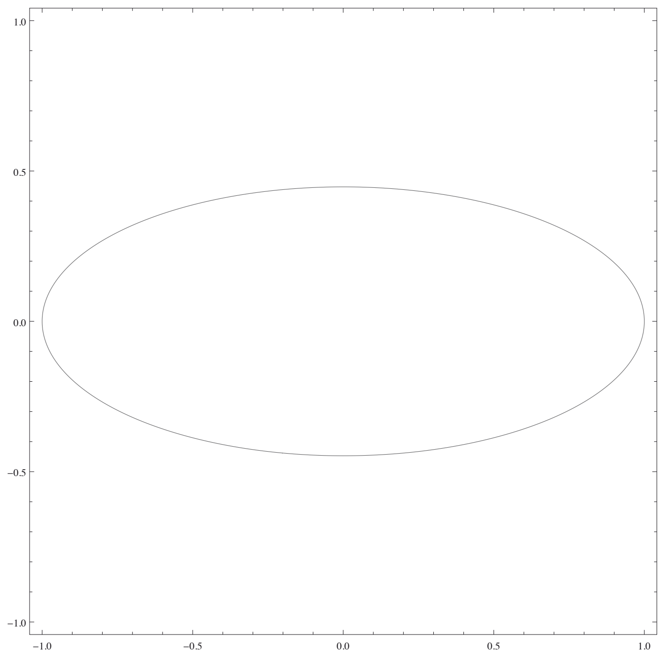

Figure 2. Finding rational points on the ellipse $x^2 + 5y^2 = 1$

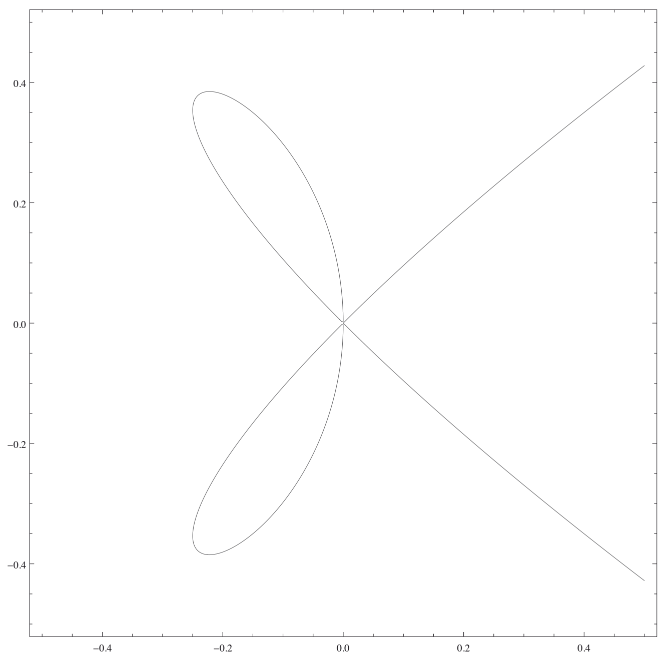

Figure 3. Finding rational points on the special quartic $y^4 = x^3 - x^2y$

The Mordell Conjecture

Then these further solutions lead to even further questions: what kinds of Diophantine problems (basically, equations for which we seek rational solutions) can be solved in this way? Which ones can't? And why?

To get a glimpse of what structure may be out there, we look to one of the most famous Diophantine problems of all time, Fermat's Last Theorem, which is suggested by the problem of Pythagorean triples. What are the integral solutions to $a^n + b^n = c^n$ (where n is a positive integer)? Essentially, what

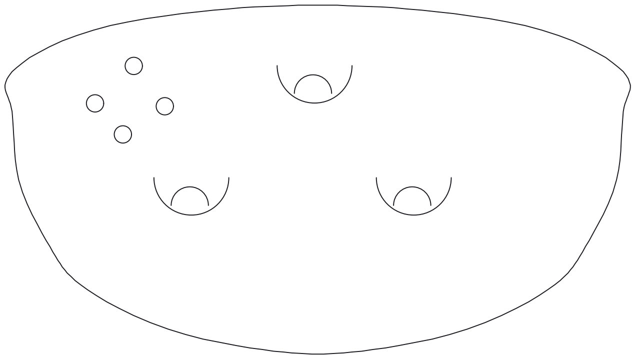

Figure 4. The Riemann surface $x^4 + y^4 = 1$

are the rational solutions to

$$x^n + y^n = 1?$$

There are obviously lots of solutions when $n = 1$ (due to the "simple geometry" of lines) and we have found lots of solutions when $n = 2$ (due to the fairly simple geometry of lines and circles) and empirically it seems hard to find nontrivial solutions when $n > 2$. What is different about $n > 2$? What explains this phenomenon?

As in the case $n = 2$, in considering this question about the rational solutions, we feel compelled to draw the *real* solutions, even though in some logical sense there seems to be nothing to help us here; we are simply drawing a curve (a one-dimensional geometric shape) through our rational solutions. But more amazingly, it can be helpful to consider the *complex* solutions. These form, in a natural way, a surface and one can show that the surface has $\binom{n-1}{2}$ "holes" (in the sense that a torus has one hole), with n points "missing" (because, in some sense, they are "at ∞" – which is nature's way of telling us that we should consider projective space). Figure 4 is a sketch of the case $n = 4$.

Notice that the *real* solutions form a curve that has nothing to do with the holes. Furthermore, the *rational* solutions we care about lie on the curve of real solutions.

More generally, suppose we had a Diophantine problem involving a bunch of polynomial equations and that the *complex* solutions turn out to be a single ("irreducible") surface. Again, the *real* solutions form a curve that has nothing to do with the holes. And the *rational* solutions we care about lie on the curve of real solutions, far from the complex holes.

The amazing thing – conjectured by Mordell and proved by Faltings [6] – is that these holes precisely control the rational solutions, despite being nowhere near them. More specifically, if there is more than one hole (note: the "missing points" do not count as holes) then there can only be a finite number of solutions. Thus, long before Wiles [13] and Taylor-Wiles [10] proved Fermat's Last Theorem (via the Taniyama-Shimura Conjecture), we knew that for each $n > 3$, there were a finite number of counterexamples to Fermat's Last Theorem, and the reason was fundamentally geometric. (And algebraic geometry is the language in which Fermat's Last Theorem was finally proved.)

There is even more in the philosophy behind Mordell's Conjecture. For example, if there are no holes and if you know one solution then you can find them all. (There might not be any solutions – witness

$$x^2 + y^2 = -1, \tag{4}$$

an equation we will return to shortly.) This explains the importance of our "first solution" $(-1, 0)$ in our determination of Pythagorean triples. We are also led to reconsider other Diophantine equations such as (2) and (3). In the first case, we have a sphere (minus 2 points), so this fits into our picture. In the second case, we find a sphere, with four points missing and three points "glued together". Again, the philosophy surrounding Mordell's conjecture applies. Again, the real pictures of Figures 2 and 3 hide the geometry of the complex picture (which is perhaps best left to the reader's four-dimensional imagination). Our satisfactory but ad hoc solutions to both of these problems now fit into a larger and more beautiful and satisfying picture.

All good solutions suggest further questions. Notice that if $g \geq 2$, we have some sort of strong information (which again suggests further questions). If $g = 0$, this even gives an algorithmic way of finding all solutions. But what about $g = 1$? What happens in this borderline case? Border regions are always the most interesting (if often the most turbulent) and here we enter the theory of elliptic curves, perhaps the richest part of arithmetic geometry.

The Weil Conjectures

The Mordell Conjecture gives a startlingly beautiful perspective on Diophantine problems where the complex solutions form surfaces. But we cannot help but wonder about the more general situation. What if the complex solutions form things ("varieties") of other dimensions? It is a fact of complex geometry that the dimensions are necessarily even (this is not obvious to those new to the area). This leads us to the Weil conjectures, one of the central stories of mathematics in the previous century. Given its importance, it is relatively little-known because the precise statement requires serious background to understand. But the central idea is stunning and I will attempt to get across some of its magic.

We first consider the reasons why we might not have any solutions to a Diophantine equation and try to extract some commonality from them.

Simplest: (4) has no integer solutions because perfect squares (the summands of the left side) are non-negative and the right side is negative. This obstruction is about the real numbers and, in particular, the ordering of the real numbers.

Naively and somewhat stupidly, $2x^2 + 4y^2 = 2013$ has no integer solutions because the left side is even and the right side is not. As a more sophisticated example, $x^2 + y^2 = 2015$ does not have integer solutions because perfect squares leave a remainder of 0 or 1 upon division by 4 and the right side leaves a remainder of 3. Both examples are about prime numbers or prime powers. This obstruction is seemingly quite different from that of (4); primes have to do with arithmetic, while the real numbers have to do with ordering and with shape. But this is a clue that they should be seen as somehow related.

There are many directions to go from here and I will describe just one. The complex solutions seem to have more to do with real solutions than with the solutions modulo the prime numbers, i.e. in $\mathbb{Z}/p$ (or their mild generalisation, their solutions in finite extensions of $\mathbb{Z}/p$, the *finite fields*). But the Weil conjectures say something very strong: the holes (of different dimensions) have essentially exactly the same information as the solutions in finite extensions of $\mathbb{Z}/p$. Making this precise requires some language, so I will just give some informal implications. If you want to work out the number of holes of various dimensions in the space of the complex solutions, you could (perhaps with a computer) just count solutions in $\mathbb{Z}/p$ (and in "finite extensions", which are just as easy to work with) – the arithmetic drives the geometry (and topology). Put rather bluntly: you can detect holes in their complex solutions (and the dimensions of the holes) by just counting solutions of equations.

And conversely, if you knew the number of holes of various dimensions, you would have strong information on the number of solutions in all finite fields – the geometry/topology drives the arithmetic. Furthermore, facts on one side of the arithmetic/geometry mirror correspond to very different-looking facts on the other – for example, Poincaré duality for manifolds on one side essentially translates into the functional equation of the zeta function on the other. Making these ideas precise requires the development of powerful mathematics. To be very clear: we do not create heavy mathematical machinery for machinery's sake. Nature forces us to build these machines and then teaches us that they are not so complicated after all.

Just as Fermat's Last Theorem motivated the development of algebra and number theory in the 19th century, the Weil conjectures were a key motivation for the development of algebraic geometry in its modern form. (Our debts to Grothendieck and to Serre cannot be overstated.)

The Weil conjectures have a number of different parts. The proofs of the different parts of the Weil conjectures started in 1960 and went on for a long time. The final, hardest part of the proof was by Deligne and this was central to his being awarded the Fields Medal in 1980, [5].

Conclusion

The Weil conjectures exemplify the unity of mathematics and the call nature makes to us to understand mathematics from a broad enough vantage point that we can see it as a single, highly interconnected subject. For the mathematician, such epiphanous relisations are cause for great joy.

The history of human mathematical understanding of the universe has, for millennia, been a story of unification and synthesis, connecting many different parts of the world. This grand unification is expressed in a rather pure sense in the work of Pierre Deligne. This story is what convinced me, and many like me, to pursue a life of pure mathematics and, on this occasion, I want to express gratitude to Professor Deligne and also wish him congratulations on the occasion of the Abel Prize.

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Matching in Marriage and Markets

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What is a good match, and how can a good match be implemented in practice? For answering these questions, Alvin Roth and Lloyd Shapley received the *2012 Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel*. We review some classic results and new developments in matching theory.

1 Two-Sided Matching: The Marriage Problem

The Marriage Model

Gale and Shapley (1962) studied two examples of *two-sided matching*: a *college admissions problem* and a *marriage problem*. For ease of exposition, we focus on the marriage problem, where (unlike the college admissions problem) the matching is one-to-one. Two finite and disjoint sets of agents are to be matched: the men $M = \{m_1, m_2, \dots, m_n\}$ and the women $W = \{w_1, w_2, \dots, w_p\}$. Each man (woman) has complete and transitive preferences over the women (men). It is convenient to assume all preferences are strict (no indifference). Thus, $w \succ_m w'$ indicates that man m strictly prefers woman w to woman w'. Woman w is said to be *unacceptable* to man m if m prefers to remain single ("married to himself") rather than marry w, and we write $m \succ_m w$. The preferences of man m can be summarised by listing all the women he finds acceptable, in order of preference; so if he thinks w and w' are the only acceptable women, and $w \succ_m w'$, then his preference is summarised as (w, w'). If *no* woman is acceptable to him then his preference is $(\emptyset)$. Similar conventions and definitions are used for the women's preferences.

A *matching* μ is a one-to-one correspondence from the set $M \cup W$ onto itself that is of order two (that is, $\mu^2(x) = x$), where $\mu(m) \in W \cup \{m\}$ for all $m \in M$ and $\mu(w) \in M \cup \{w\}$ for all $w \in W$. If $\mu(m) = w$ then man m is matched with woman w. If $\mu(m) = m$ then man m is "matched with himself", i.e. single. If no agent is matched with someone they consider unacceptable then the matching is *individually rational* – a minimum requirement indeed! More subtly, Gale and Shapley (1962) argued that a pair m and w would *block* μ if they are not matched with each other but they would prefer each other to their assigned partners. That is, m and w form a "blocking pair" if $w \succ_m \mu(m)$ and $m \succ_w \mu(w)$. A matching is *stable* if it is individually rational and there is no blocking pair. A matching is *Pareto-optimal* if it is not possible to make any agent strictly better off without making any other agent strictly worse off. It is easy to show that all stable matchings are Pareto optimal. A matching is *Pareto-optimal for the men* if it is not possible to make any *man* strictly better off without making any one other *man* strictly worse off. Marriages will be arranged by a *matching algorithm*.

Man-proposing Deferred Acceptance algorithm (MDA) (Gale and Shapley, 1962): Each man proposes to the woman he likes best (or makes no proposal, if no woman is acceptable). Each woman peruses the proposals she has received, if any, and holds on to the one she likes best (assuming it is acceptable); the remaining proposals are rejected. Each rejected man makes a new proposal, this time to the woman he considers second best (or makes no proposal, if the second best is not acceptable). Any woman who receives new proposals peruses the new proposals together with the one she was holding from before (if any). She holds on to the one she likes the best (if it is acceptable) and rejects the rest. The process is repeated until no man wishes to make another proposal, in which case the women accept the proposals they hold. The final matching is denoted μ_{MDA}.

Theorem 1 (Gale and Shapley, 1962) The matching μ_{MDA} is stable. Indeed, it is the *man-optimal* stable matching, in the sense that all men prefer this matching to any other stable matching.[1]

The MDA always stops in finite time (since the number of men and women is finite) and it provides a proof of the existence of stable matchings in the marriage problem.[2] The next algorithm is a version of the *top trading cycles algorithm* introduced by Shapley and Scarf in 1974, which they attributed to David Gale.

Man-biased Top Trading Cycles algorithm (MTTC) (Abdulkadiroğlu and Sönmez, 2003): Each man points to the woman he likes most (or to himself, if he prefers to be single). Each woman points to the man she likes most. There exists at least one cycle. Match every man in the cycle with the woman he is pointing to and remove these pairs from the market. Next, the procedure is repeated among the remaining agents. Find a cycle, match every man in the cycle with the woman he is pointing to and repeat until all agents have been matched. The final matching is denoted μ_{MTTC}.

Example 1 There are two men $M = \{m_1, m_2\}$ and two women $W = \{w_1, w_2\}$. Both men (women) find both women (men) acceptable and

$$w_1 \succ_{m_1} w_2\ ,\ \ w_2 \succ_{m_2} w_1\ ,\ \ m_2 \succ_{w_1} m_1\ ,\ \ m_1 \succ_{w_2} m_2\ .$$

In the MDA, m_1 starts by proposing to w_1, and m_2 to w_2. Since the women find these proposals acceptable, the algorithm stops and each man marries his favourite woman. But the women do *not* marry their favourites: the right to propose gives the men a definite advantage! In the MTTC, $m_1 \to w_1 \to m_2 \to w_2 \to m_1$ is a cycle. Again, each man marries his favourite: the woman he points to.

Example 1′. Example 1 is modified so now w_1 finds m_1 unacceptable. In the MDA, she will reject m_1 and he will then propose to w_2. Since w_2 prefers m_1 to m_2, she will reject m_2, who will then propose to w_1. The algorithm stops: m_1 marries w_2 and m_2 marries w_1. In MTTC the cycle is still $m_1 \to w_1 \to m_2 \to w_2 \to m_1$, so m_1 marries w_1 and m_2 marries w_2. Notice that, while both μ_{MDA} and μ_{MTTC} are Pareto-optimal, μ_{MDA} is not Pareto-optimal for the men: both men strictly prefer μ_{MTTC} to μ_{MDA}. The matching μ_{MTTC} violates individual rationality: w_1 finds m_1 unacceptable but is assigned to him anyway.

Example 2 (Abdulkadiroğlu, 2012) There are three men $M = \{m_1, m_2, m_3\}$ and three women $W = \{w_1, w_2, w_3\}$. They all find all members of the opposite sex acceptable. The rankings are:

m_1	m_2	m_3	w_1	w_2	w_3
w_2	w_1	w_1	m_1	m_2	m_2
w_1	w_3	w_2	m_3	m_1	m_1
w_3	w_2	w_3	m_2	m_3	m_3

This means $w_2 \succ_{m_1} w_1 \succ_{m_1} w_3$, etc. Both MDA and MTTC match m_1 with his favourite, w_2. Man m_2 marries w_3 under MDA but he marries w_1 under MTTC, and vice versa for man m_3. The matching μ_{MTTC} is not stable because w_1 and m_3 is a blocking pair.

It must be said that this is not a good model of real life romantic relationships. In such relationships, preferences are incomplete, probably non-transitive, and certainly endogenously determined (a man may like a woman more or less depending on whether she likes him). Dating is sequential; we do not all get married at the same time. However, there are other situations (such as school choice) where the model has proved remarkably useful.[3] But successful application depends on the following question: will the participants reveal their preferences truthfully?

The truth and nothing but the truth?

An important application is school admissions: let the students correspond to the "men" and the schools to the "women". A non-profit organisation or government agency uses a *revelation mechanism* to assign students to schools. Each student submits their preference ordering, i.e. their personal ranking of the schools. The "preferences" of schools over students are given by an exogenous *priority ordering*. (For example, state or local regulations may require that students who live close to a school get priority in admissions.) Based on the rankings submitted by the students, and the schools' fixed priority orderings, the agency runs an algorithm to match the students with the schools. Now a student may try to "game" the mechanism by misrepresenting his preferences, hoping to benefit from this by getting into a preferred school, perhaps at the expense of someone else who would be assigned to a less desirable school. To prevent this, the algorithm should be *strategy-proof*: if should never be possible to benefit by submitting false preferences.

Theorem 2 (Dubins and Freedman, 1981, Roth, 1982b)
MDA is strategy-proof for the men.

This result guarantees that it is in the best interests of each "man", i.e. student, to be truthful. If the "women"/schools are not strategic players, but simply represented by the fixed priority ordering, then all is well. But what if the schools are also strategic players? They may have (private or public) information about students that may impact their priority (Abdulkadiroğlu, 2012). Can the schools be trusted to report this information truthfully? Unfortunately, a theorem due to Roth (1982b) says that no algorithm that always produces stable matchings can be strategy-proof for *both* sides of the market. Consider the MDA in Example 1. By Theorem 2, we may suppose the men reveal their preferences truthfully. But by Roth (1982b), since μ_{MDA} is stable, it cannot be strategy-proof for the women. Table 1, from Ma (2010), represents the women's problem as a strategic form game, where w_1 is the row player and w_2 the column player. To be matched with her favourite man is worth 2, to be matched with the second best is worth 1 and to remain single is worth 0.

Table 1. Matching Game when MDA is in use: w_1 the row player and w_2 the column player

	(m_2, m_1)	(m_1, m_2)	(m_1)	(m_2)	$(\emptyset)$
(m_2, m_1)	1, 1	1, 1	2, 2	1, 1	2, 0
(m_2)	0, 1	2, 2	2, 2	0, 1	2, 0
(m_1, m_2)	1, 1	1, 1	1, 0	1, 1	1, 0
(m_1)	1, 1	1, 1	1, 0	1, 1	1, 0
$(\emptyset)$	0, 1	0, 2	0, 2	0, 1	0, 0

Truthtelling would correspond to w_1 reporting (m_2, m_1) and w_2 reporting (m_1, m_2), in which case each woman gets payoff 1. But this is not a Nash equilibrium, i.e. profitable deviations exist. Specifically, if w_1 submits (m_2), i.e. falsely claims that m_1 is unacceptable, or if w_2 submits (m_1), then under the MDA the women get their favourite husbands and the women's payoffs increase from 1 to 2 (see Example 1'). Thus, MDA is not strategy-proof for the women. Indeed, after eliminating dominated strategies in Table 1, the Nash equilibrium matches each woman with her favourite man, the woman-optimal stable matching. Although MDA seems to favour the men by making them the proposers, the women can take back the advantage by misrepresenting their preferences, falsely claiming they would rather stay single than marry anyone but their one true love! The implication for the school choice problem is clear: the schools cannot be trusted to truthfully reveal which students they would like to admit.

Theorem 3 (Abdulkadiroğlu and Sönmez, 1999, 2003)
MTTC is both strategy-proof for the men and Pareto-optimal for the men.

When MTTC is used in Example 1, whatever the women report, each man will get his favourite woman (so the women may as well tell the truth). MTTC is better (for the men!) than MDA because there is less scope for strategic manipulation by the women.[4]

2 One-Sided Matching: The Housing Market

Shapley and Scarf (1974) studied a model of *one-sided matching* called the *housing market*. There is a set of agents $N = \{1, 2, \cdots, n\}$. Each agent $i \in N$ initially owns a house h_i. Let $H = \{h_1, h_2, \cdots, h_n\}$. Agent i's preference $\succ_i$ is a ranking of the n houses. Again, it is convenient to assume strict preferences: no agent is indifferent between two houses. A matching or allocation is a bijection $\mu : N \to H$.

Top Trading Cycles algorithm (TTC) (Shapley and Scarf, 1974): Each agent points to the agent whose house he likes most (which may be himself, if he likes his own house the best). There exists at least one cycle. Give every agent in the cycle the house of the agent he is pointing to, and remove these agents and houses from the market. The procedure is repeated with the remaining agents and houses, until each agent has been assigned a house. The final allocation is always individually rational and Pareto-optimal.

How is this problem related to the marriage problem? A housing market generates an associated marriage problem as follows. The set N of agents in the housing market is identified with the set M of men in the marriage problem, and the set H of houses is identified with the set W of women. The men's preferences over these "women" correspond in the natural way to their rankings of the associated houses. But houses have no preferences over owners so how are the preferences of the "women"to be defined? Let the favourite man of woman h_i be man i (the one who owns the house h_i) – her remaining preferences can be arbitrary. Under this mapping, the MTTC for the marriage problem yields the same matching as the TTC of the housing market. Pápai (2000) called this a fixed endowment hierarchical exchange rule, a special instance of her general hierarchical exchange rules.

Apart from the fact that houses do not have preferences, the initial ownership distinguishes the housing market from the marriage problem. The initial owner of a house has a property right, i.e. the right to either keep or trade his house. The allocation μ is *individually rational* if there is no i such that $h_i \succ_i \mu(i)$. (No agent should be assigned a house that he likes less than the one he owns initially.) More generally, a coalition $T \subseteq N$ of agents *block* allocation μ if they can reallocate among themselves the houses they initially own in such a way that they all become better off. Formally, let $H_T = \{h_j : j \in T\}$ be the subset of houses initially owned by the members of T. A coalition T blocks an allocation μ if there exists a bijection $\mu_T : T \to H_T$ such that $\mu_T(i) \succ_i \mu(i)$ for some $i \in T$, and there is no $i \in T$ such that $\mu(i) \succ_i \mu_T(i)$. The *core* consists of all allocations that cannot be blocked by any coalition.

Theorem 4 (Shapley and Scarf, 1974, Roth and Postlewaite, 1977) The core contains a unique allocation, which is obtained by running the TTC.[5]

As before, the TTC is implemented by running the algorithm based on submitted rankings (a revelation mechanism). The next result shows that each agent has an incentive to be truthful.

Theorem 5 (Roth, 1982a) The TTC is strategy-proof.

Theorem 6 (Ma, 1994) The TTC is the only algorithm that is individually rational, Pareto-optimal and strategy-proof.

There exist a number of alternative axiomatic characterisations of the TTC. Suppose that when the agents submit the preference profile $\succ = (\succ_1, \succ_2 \ldots, \succ_n)$, the algorithm produces allocation μ; but if agent i instead submits $\succ'_i$ then the allocation is μ'. If this manipulation does not change the house assigned to agent i ($\mu(i) = \mu'(i)$) then the *nonbossiness* axiom requires that it should not change any other agent's assignment either ($\mu(j) = \mu'(j)$ for all $j \neq i$). The *anonymity* axiom requires that every agent is treated symmetrically.

Theorem 7 (Svensson, 1999) The TTC is the only algorithm that is strategy-proof, individually rational, Pareto optimal and nonbossy.

Theorem 8 (Miyagawa, 2002) The TTC and the no-trade mechanism (where each agent keeps his initial house) are the only two allocation mechanisms that satisfy the four properties of individual rationality, strategy-proofness, nonbossiness and anonymity.

What happens under a given mechanism if some agents are removed from the market together with their assigned houses? This issue has been addressed by introducing *consistency* axioms, an approach associated in general with William Thomson. See Ehlers and Klaus (2007) for a recent example of such analysis.

3 One-Sided Matching: House Allocation

In a *house allocation problem* (Hylland-Zeckhauser, 1979), there is a set of agents $N = \{1, 2, \cdots, n\}$ and a set of houses $H = \{h_1, h_2, \cdots, h_r\}$, where $n \geq r$. No agent initially owns a house but each agent wants a house and can rank the houses as before. Since there is no initial ownership, the similarity to the marriage problem should be apparent. Yet, because houses do not have preferences, the house allocation problem is studied separately.

For a concrete example, let Al Roth give a lecture. Naturally, the size of the audience, n, is at least as great as the number of available seats, r. How should these seats be allocated? One natural method is *serial dictatorship*: the audience members form a queue and enter the room sequentially until all seats have been occupied, after which the door is closed and Professor Roth gives his lecture. Of course, each person who enters the room chooses the best seat among all remaining unoccupied seats, according to his own preferences. This method is strategy-proof, nonbossy and *neutral* in the sense that all seats are treated in the same way. It is *not* anonymous: it is good to be first in line.

Theorem 9 (Svensson, 1999) Assume $n = r$. Serial dictatorship is the only mechanism which is strategy-proof, nonbossy and neutral.

A house allocation problem generates an associated marriage problem as follows. Let N correspond to the set of men and H to the set of women. The men's preferences over these "women" correspond to their rankings of the houses. Let all women have *the same* ranking over the men. This common ranking corresponds to the order in the queue (Svensson, 1994). Now, the MTTC algorithm for the marriage problem produces the same matching as the serial dictatorship for the given queue. Thus, a characterisation of a serial dictatorship for house allocation is also a characterisation of the MTTC algorithm in the corresponding marriage problem (Pápai, 2000).

The non-anonymity of serial dictatorship is disturbing but anonymity can be recovered by randomising the queue. Equivalently, the preferences of the "women" in the associated marriage problem is uniformly drawn from the set of permutations of N. An alternative method is to generate an "initial ownership" by randomly assigning seats to audience members and then run the TTC as in the previous section. These two methods are equivalent (Abdulkadiroğlu and Sönmez, 1998, Pathak and Sethuraman, 2011).

The real-world importance of the school choice problem has stimulated interest in situations that are more general than the standard housing market and house allocation problems. Abdulkadiroğlu and Sönmez (1999) studied a model where *some* houses have initial owners but some are initially unoccupied. Some "newcomers" own no house but want one. Fix a priority ordering f of the agents in N, where N consists of both house owners and newcomers. The modified TTC is as follows:

Top Trading Cycles algorithm (TTC^f) (Abdulkadiroğlu-Sönmez, 1999): Let all agents point to the house they like the most. Let all occupied houses point to their owners and let all unoccupied houses point to the agent who has the highest priority under f. There will be a cycle. Assign each house in a cycle to the agent who is pointing to it. Remove all agents and houses in the cycles from the market. Next, each remaining agent points to the house he likes the most among those remaining on the market. All occupied houses whose owners are still on the market point to their owners. All unoccupied houses (including any previously occupied house whose owner was removed) point to the agent who has the highest priority under f among those that remain. Find cycles and remove these agents and houses. Repeat the process until no more assignments can be made.

Theorem 10 (Abdulkadiroğlu and Sönmez, 1999) The TTC^f algorithm is strategy proof, individually rational and Pareto-optimal.

Abdulkadiroğlu and Sönmez's (1999) model generates an associated marriage problem as follows. As before, N becomes the set of men and H the set of women. The preferences of these "women" are as follows: an occupied house's top choice is the current owner; the rest are ranked according to priority f. An unoccupied house ranks all agents in N according to f. The MTTC for this marriage problem is equivalent to the TTC^f algorithm for the house allocation problem. Such a hierarchical exchange under the TTC algorithm was studied by Pápai (2000) although she considered a broader class of house allocation problems than Hylland-Zeckhauser (1979) and Abdulkadiroğlu-Sönmez (1999). In her model an agent may initially own a bundle of houses while others may own none. In effect, Pápai (2000) considered that many seats in the lecture room may be "reserved" for a few audience members who each need just one seat. The problem is how to transfer those reserved seats that are not needed by their "owners" to others. Pápai (2000) used an inheritance tree which specifies the property right to others contingent upon the history of the operation of the TTC algorithm. So her method is equivalent to a recursive sequence of marriage problems, where the MTTC algorithm is implemented once for each marriage problem and the women's preferences are modified according to what has been specified under the inheritance tree after a cycle is removed. Pápai (2000) axiomatically characterised this class of rules (see also Svensson and Larsson, 2005).

Consistency axioms have been extensively studied for the house allocation problem, starting with Ergin (2000). There are two kinds of consistency axioms: population consistency and resource consistency (see, for example, Ehlers and Klaus, 2007). Sönmez and Ünver (2006) characterised the TTC^f algorithm using a consistency axiom.

4 Conclusion: Built for Exchange

The algorithms of matching theory have been used to solve important real-world problems (e.g. Sönmez and Ünver, 2011). There is a virtually unlimited potential for further applications. The U.S. Treasury revealed on 23 March 2009 a public-private plan to purchase "toxic assets" from banks after the 2007-2008 crisis. But the first bailout plan failed because the Treasury did not know how to price these assets. We argue that TTC could help trade money for assets. Suppose there are five banks, A, B, C, D and E, each of which has one bundle of toxic assets, also denoted A, B, C, D and E. The five bundles do not need to have the same par value. A public-private entity F has a bundle F with cash amount $2x + y$, where federal and private each provide cash x and FDIC provides loan amount y. This bundle F of cash is only used to buy just one bundle among the five. Each asset bundle can only exchange with one asset or the cash bundle. It is possible that bank A likes cash bundle F more than its asset bundle A. But it is also possible that bank A likes bundle B more than cash bundle F, both of which are preferred by bank A to its own bundle A. So each bank's preference over all five asset bundles and the cash bundle F can be represented by a rank order. The public-private entity F also has such a ranking. If F believes all bundles are overpriced in comparison with cash bundle F then F can simply put cash bundle F at the top of its ranking. We can consider each of these bundles as a "house" and the TTC algorithm can be used to trade them. But the TTC algorithm is just the second stage. In the first stage, each bank can build its bundle from the toxic assets it holds. The cash bundle can be put on the table first and then banks build their bundles for exchange. Once bundles are built, they become indivisible goods just like houses. Why does this mechanism discover the right price? Consider the case where all asset bundles are overpriced with respect to the cash bundle F. Then there is no trade between asset and cash bundles since F will put the cash bundle at the top of its rank order. On the other hand, if all asset bundles are underpriced with respect to the cash bundle F, then banks will either trade asset bundles among themselves or keep their own bundles. Again there is no trade between asset and cash bundle. Thus, if the public-private entity F publicly announces a cash bundle F, banks must create asset bundles that are worth the same as F (from F's perspective) in order to have trade between the cash bundle and an asset bundle. This mechanism is individually rational. For those who do not receive the cash bundle, there is no harm in participating; at worst, they receive their own asset bundles.

Notes

1. Of course, the symmetric Woman-proposing Deferred Acceptance algorithm (WDA) is stable and *woman-optimal*. To simplify the exposition, we focus on the MDA.

2. The existence of stable matchings is not to be taken for granted. For example, it is not true for *same-sex* marriages. Consider an example due to Gale and Shapley (1962). Four men all find each other acceptable. The preference rankings of the first three men are as follows:

$$m_2 \succ_{m_1} m_3 \succ_{m_1} m_4, \quad m_3 \succ_{m_2} m_1 \succ_{m_2} m_4,$$
$$m_1 \succ_{m_3} m_2 \succ_{m_3} m_4.$$

Man m_4 ranks the other three in some arbitrary way. Suppose m_1 is matched with m_4, and m_2 with m_3. This is unstable because m_1 and m_3 strictly prefer each other to their assigned partners. A similar argument rules out matching either m_2 or m_3 with m_4. So there is no stable matching.
3. For recent applications, see Alvin Roth's blog http://marketdesigner.blogspot.com.
4. The way we defined the MTTC, the women are not allowed to "point to themselves". Even if this were allowed, the undominated Nash equilibrium of the strategic game among the women in Example 1 (a table similar to Table 1) would be truthtelling.
5. The uniqueness was proved by Roth and Postlewaite (1977). One of us would like to apologise to these authors for the erroneous attribution of this result in Ma (1994). The strict preference assumption is important for Theorem 4; the core may be empty if indifferences are allowed (Roth and Postlewaite, 1977). Bogomolnaia, Deb and Ehlers (2005) characterised strategy-proof rules when indifferences are allowed.

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There is a Projective Dynamics

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From planar to spherical dynamics

From 1609 to 1619, Kepler published three laws concerning the motion of a planet around the Sun. They indeed describe mathematically all the bounded motions of a single planet considered as a massless point particle around a Sun, which is a fixed point.

K1 The trajectory is an ellipse. The Sun is a focus.

K2 While the planet is moving, the vector from the Sun to the planet sweeps out equal areas in equal times.

K3 The square of the period is, up to a factor which is the same for all the possible bounded motions, the cube of the diameter of the ellipse.

Newton then published more general laws from which Kepler's laws may be deduced. The context of this deduction is the above, with a fixed Sun and a single small planet. This context allows us to state a simple case of Newton's laws, which was discovered first in 1679.

N1 The law of motion specifies the *force* $\ddot{q}$, second derivative with respect to time of the position q of the planet.[1] The motion itself is selected by the choice of an initial position q and an initial velocity vector $\dot{q}$.

N2 The force is a function of the position of the planet only: the law of motion is given by a vector field, the force field, which does not vary with time.

N3 The force at a point q is the unit vector pointing at the Sun $q_\odot$ divided by the square of the distance to the Sun and multiplied by the mass M of the Sun.

This defines an ordinary differential equation which is vectorial, autonomous and of second order:

$$\ddot{q} = -\frac{M}{r^3}(q - q_\odot), \qquad r = \|q - q_\odot\|. \tag{1}$$

Our 20th century notation reminds us that the above laws presuppose a kind of axiom:

K0 The Sun and planet are points of a 3-dimensional Euclidean space.

The 19th century was the time for clarifying the position of the fifth postulate of Euclidean geometry, by producing the Lobachevsky plane and then comparing it with Euclidean and spherical geometries. As expected, geometers considered the possibility of changing K0 into:

SK0 The Sun and planet are unit vectors q and $q_\odot$ in a 4-dimensional Euclidean vector space E.

The usual space is changed into a 3-dimensional sphere. Curved Keplerian dynamics was indeed first studied on the 2-dimensional sphere. We will present only this familiar dimension, observing simply that laws K1 to N3 are also true in the planar case. There is very little to change if we want more dimensions. Constant negative curvature may also be presented in the same way.

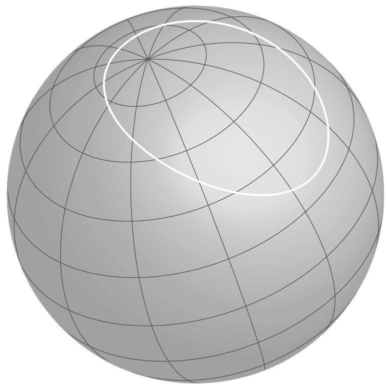
The list of laws mainly survives when we curve the plane into a sphere. This discovery of the 19th century is purely mathematical and has nothing to do with the observation of the sky. For motion on the sphere, there is an analogue of Newton's law which implies an analogue of Kepler's laws.

SN1 The law of motion specifies the second derivative $\ddot{q}$ of the position of the planet, which splits into a vector tangent to the sphere and a vector normal to the sphere. The motion itself is selected by the choice of an initial position q and an initial velocity $\dot{q}$, tangent to the sphere.

SN2 The tangential component of $\ddot{q}$ is a function of the position of the planet only: the law of motion is given by the tangential force field, which does not vary with time.

SN3 The tangential force at a point is the tangent unit vector pointing at the fixed Sun divided by the square of the sine of the angular distance to the Sun and multiplied by the mass of the Sun.

SK1 The trajectory is a spherical ellipse. The Sun is a focus.



SK2 While the planet is moving, the triangle formed of the Sun, planet and centre of the sphere sweeps out in E equal volumes in equal times.

SK3 The period depends only on the angular diameter of the ellipse.

The law of force SN3 was discovered by Paul Serret (1827–1898) in 1859 as the law that produces SK1. He described this deduction in a single page and did not come back to this matter.[2]

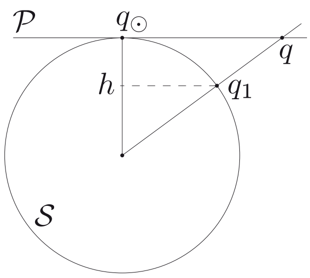
From spherical to projective dynamics

A careful inspection of the new list of laws induces many questions and suggests looking for more analogies. We will rather concentrate on the above limited material and already risk the main question: why is there such a similarity between the plane and the sphere? In 1891, Paul Appell (1855–1930) claimed he had an answer.

> The analogy between the motions of a point on a sphere and those of a point in a plane has been reported for a long time, in particular by M. Paul Serret in his thesis: *On the geometrical and mechanical properties of the lines with double curvature*. One finds the explanation of this analogy in a transformation similar to the homographic transformation that we studied before. This

new transformation associates any motion of a point in a plane under the action of a force depending only on the position with the motion on a sphere under the action of a force depending only on the position; and reciprocally.

Given a sphere $\mathcal{S}$ of radius 1 and a tangent plane $\mathcal{P}$ to this sphere, we associate the point q_1 of the sphere with the projection q of this point on the plane $\mathcal{P}$, made through the radius drawn from the centre to the point q_1: this is the well-known projection which is called "central" in the theory of geographic maps; it associates all the straight lines of the plane $\mathcal{P}$ with the great circles of the sphere $\mathcal{S}$ and reciprocally. [...] That the straight lines of the plane correspond to the geodesic lines of the surface requires, according to a theorem by Beltrami, that the surface has *constant curvature*.



Appell refers here to his previous paper "On the homography in Mechanics", published in the previous year. He had opened this paper by quite striking words:

"The discovery of the *principles of the central projection* marks without contestation an important epoch in the history of modern geometry. The methods founded on these principles possess an intuitive and systematic character, which makes them equally appropriate to reveal new properties of the figures and to join all a set of propositions to a unique general truth."

We found interesting to show that the same principles may be applied, in mechanics, to the motion of one or several free points under the solicitation of forces that only depend on the positions of the points. One can, for example, using the homographic transformation, join one another apparently distinct mechanical questions, such as the motion of a point attracted by a fixed centre proportionally to the distance and the motion of a point attracted by a fixed plane inversely as the cube of the distance.

The path from geometry to mechanics is described by Newton in 1687, in the preface of his famous *Principia*:

Therefore *geometry* is founded on mechanical practice and is nothing other than that part of *universal mechanics* which reduces the art of measuring to exact propositions and demonstrations. But since the manual arts are applied especially to making bodies move, *geometry* is commonly used in reference to magnitude, and *mechanics* in reference to motion. In this sense *rational mechanics* will be the science, expressed in exact propositions and demonstrations, of the motions that result from any force whatever and of the forces that are required for any motions whatever." [New]

The "principles of the central projection" form the axioms of projective geometry. Appell claims here that there is a natural extension of projective geometry, which Newton would call rational projective mechanics. We rather speak of *projective dynamics*, as dynamics is the part of mechanics that concerns motion.

This striking and easily remembered message was soon forgotten. Peter W. Higgs, the famous author of [Hi1], considered again the law SN3 in 1978, while developing computations by Schrödinger about quantum mechanical analogues of this law. And he happened to give precisely the same message, although Appell's strong words had certainly never reached his ears.

For the purposes of this paper another projection is more useful, for reasons which will shortly be made clear: this is (in cartographers' jargon) the gnomonic projection, which is the projection onto the tangent plane from the centre of the sphere in the embedding space. [...] The advantage of this projection over all others for the analysis of the motion of a particle on a sphere stems from the fact that free particle motion (uniform motion on a great circle) projects into rectilinear, but non-uniform motion on the tangent plane. That is, the projected free particle orbits are the same as in Euclidean geometry: the curvature affects only the speed of the projected motion. It will now be shown that this feature persists in the presence of a central force derived from a potential $V(r)$.

We extract three laws from Appell's and Higgs' computations and we then state the founding theorem.

GN1 To get the position $q_1 \in \mathcal{S}$ we project the position $q \in \mathcal{P}$ by central (also called gnomonic) projection. Let h be the positive q-dependent proportionality factor: $q_1 = hq$.

GN2 To get the velocity of q_1 we push forward the velocity of q by the [differential of the] central projection and divide the resulting vector by h^2.

GN3 To get the tangential force at q_1 we push forward the force at q by the central projection and divide the resulting vector by h^4.

Theorem. *Consider the central projection from a finite dimensional affine space $\mathcal{P}$ into a sphere $\mathcal{S}$ of the same dimension. Define a domain of $\mathcal{P}$ and a force field on it. Use laws GN to build the corresponding domain and tangential force field on $\mathcal{S}$. Choose initial position and velocity in $\mathcal{P}$ and the corresponding condition in $\mathcal{S}$ through GN. Then GN sends the trajectory in $\mathcal{P}$ onto the trajectory in $\mathcal{S}$.*

Note that if law GN2 was just "push forward the velocity" then the time parametrisations of a trajectory and its projection would correspond to each other. But GN2 has a second step, the division by h^2, which changes the time parameter, as Higgs warned us. One should be careful with a notation such as $\dot{q}$, as there are two time parameters.

We will leave to the reader the elementary proof of the theorem and rather try to check that laws SN and SK agree with it. Of the three laws SN, only SN3 requires an elementary computation. We denote by θ the angular distance to the Sun. The main proportionality factor is $h = \cos\theta$. The push-forward of the unit vector pointing at the Sun on the flat space is the unit vector pointing at the Sun on the sphere, multiplied by h^2. We should divide it by $r^2 = \tan^2\theta$, according to N3, and by h^4, according to GN3. We get the inverse sine square law SN3.

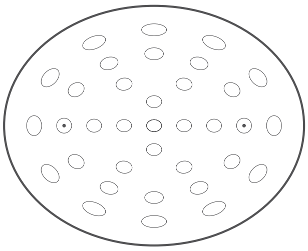
SK1 as a striking success of Appell's explanation

We shall now deduce SK1 from the theorem. This Kepler law has two parts. That the trajectory is a spherical conic section is obvious from GN1 and K1. It is enough to recall that a spherical conic section is the intersection of a homogeneous quadric with the sphere.

The second part of SK1 requires some familiarity with the focal theory of conics. Here is the convenient definition. Consider the plane $\mathcal{P}$ and the sphere $\mathcal{S}$ disposed in the 3-dimensional real vector space E as in the second figure. Define a conic section B on $\mathcal{P}$ or on $\mathcal{S}$ as the intersection of $\mathcal{P}$ or $\mathcal{S}$ with a cone of equation $\mathcal{B}(q, q) = 0$, where $\mathcal{B}$ is an indefinite symmetric bilinear form. Consider any point q on $\mathcal{P}$ or $\mathcal{S}$. Consider the quadratic form:

$$\mathcal{B}_q : v \longmapsto \mathcal{B}(q, q)\mathcal{B}(v, v) - \mathcal{B}(q, v)^2.$$

We draw on the tangent plane at q of $\mathcal{P}$ or $\mathcal{S}$ a typical level curve of the function $v \mapsto \mathcal{B}_q(v)$, where v is a tangent vector at q. We call this level curve the tangent conic.



If the tangent conic is a circle, the point q is a focus of the conic section B. This is our definition of a focus. According to law K1, when $q = q_\odot$, the tangent conic associated with the planar trajectory is a circle. But at $q_\odot$, the tangent planes at $\mathcal{P}$ and at $\mathcal{S}$ coincide, and the equation defining the tangent conic is the same. Thus SK1 is proved.

Bertrand's problems

That Kepler's law K1 is a consequence of Newton's law N3 is not only a geometrical surprise but also a topological surprise: the planet returns to its position after one turn and reaches it with the initial velocity vector. The trajectory is a closed curve. This fact was "unbelievable" for Kepler, according to page 591 of his *Epitome*. It certainly misguided him in his research of a law of dynamics. Newton expressed with nice propositions this singularity of N3 as opposed to the other homogeneous central force fields. He concluded his *Principia* by wondering if the aphelia of the distant planets are really at rest.

Bertrand put this in 1873 in the form of a problem: find all the central force fields with rotational symmetry around the Sun such that all the trajectories starting with a small enough velocity are closed. The "small enough" hypothesis is there to exclude the parabolic and hyperbolic trajectories. We can also assume that the orbit is bounded or that the planet makes a full rotation around the Sun. The solutions of Bertrand's problem are law N3 and the harmonic oscillator. Bertrand presents a proof where some technicalities require further examination. An earlier computation by Lagrange, published in 1765, gives in any case a complete proof.

Several authors, such as Liebmann, Ikeda & Katayama and Kozlov & Harin, raised Bertrand's problem on the sphere and solved it by a short computation. Higgs observed that no computation is needed, as the central projection gives all the solutions, which are simply the projections of the force fields that solve the planar case. Indeed, the centrality of the force field and its rotational symmetry are respected by GN, as well as the topology of the orbits.

Bertrand proposed in 1877 a second similar problem. One hypothesis is stronger: he required that the trajectories are conic sections. Another is weaker: he only assumed N2 and no particular symmetry of the force field. A remark by Halphen about the solution of this problem led Appell to projective dynamics. Appell also considered the spherical version of Bertrand's second problem and immediately reduced it to the planar version by using GN.

SK2 and an example of "double explanation"

The algebraic way to state the law of areas K2 is: the bivector $C = (q - q_\odot) \wedge \dot{q}$ is constant on any trajectory. This is true if and only if the force $\ddot{q}$ is central, i.e., proportional to $q - q_\odot$, as shown by the expansion $\dot{C} = \dot{q} \wedge \dot{q} + (q - q_\odot) \wedge \ddot{q}$. To address SK2 is straightforward. We consider the trivector $q_\odot \wedge q \wedge \dot{q}$ and argue in the same way with the centrality property on the sphere: $\ddot{q}$ belongs to the plane $(q_\odot, q)$.

This "explanation" of K2 and SK2 is complete but let us try another one. We all know that C is called the angular momentum and that the conservation of the angular momentum is "explained" by the rotational symmetry of the mechanical problem. The force field N3 has rotational symmetry. The corresponding first integral is given in Lagrangian dynamics by the Euclidean inner product $\langle w, \dot{q}\rangle$, where w is the vector field which generates the rotation. If $q_\odot = (0, 0)$, $q = (x, y)$ then $w = (-y, x)$ and the angular momentum is $x\dot{y} - y\dot{x}$, which is also $(q - q_\odot) \wedge \dot{q}$, as we are in dimension 2.

We now address the spherical case, where the tangential force field also has rotational symmetry. We get a formula for the angular momentum: $\langle w_1, \dot{q}\rangle$, where the inner product is given by the metric of the sphere, and w_1, push-forward of w under central projection, generates a rotation on the sphere. Fortunately, we easily see that this angular momentum is the same as in the "explanation" in term of central forces.

This double explanation is however uncomfortable, as we use two distinct hypotheses to predict the same result, central force in the first case and rotational symmetry in the second case. Moreover, we can observe what happens if the Sun is no longer placed at the tangency point of $\mathcal{P}$ and $\mathcal{S}$ but is centrally projected on $\mathcal{S}$ from any other point of $\mathcal{P}$. The first explanation still predicts an angular momentum for the spherical dynamics. But the rotational symmetry is broken by law GN3.

Bad news and good news

As long as we remain inside the Newtonian conception of rational mechanics, the rotational symmetry is not necessary, as we just said, for the conservation of angular momentum. And it is not sufficient either. For example, the vector field w on $\mathcal{P}$ that we defined above has rotational symmetry. But if we consider the dynamics of $\ddot{q} = w$, the force is not central and thus the angular momentum is not conserved.

Our second explanation for SK2 presupposes we turn to Lagrangian concepts and consider properties that Lagrange discovered 90 years after the publication of the *Principia*. A fundamental hypothesis is necessary to step into this conservative dynamics: that *the forces are derived from a potential.*

Recall that this means that there is, on $\mathcal{P}$ or $\mathcal{S}$, a function $U(q)$, the potential, such that the tangential force at q is the gradient of U at q. A law of dynamics N2 or SN2 satisfies this property if and only if the quantity $H = \|\dot{q}\|^2/2 - U(q)$ is a first integral, which is called the energy.

Projective dynamics consists first of elementary remarks about the forces, which induce, as we shall see, less elementary remarks about the potential. The concept of potential requires a "metric" or more precisely a non-degenerate quadratic form. The concept of force field does not. We find here again the structure of geometry: the most basic geo-"metric" properties are not metric. They concern the intersection of lines. They are stated without endowing the space with any quadratic form. We should first master the "non-quadratic" properties and only after this consider the quadratic forms and their transformations. The impatient geometer is instinctively attracted by the magic and intuitive theorems of Euclidean geometry and tends to skip this tedious part which includes Desargues' Theorem and the cross ratio. When discovering projective dynamics, one can similarly consider what follows as bad news.

Even starting from a force field that is derived from a potential, the result of GN3 is not in general a tangential force field derived from a potential.

In our case, the projected force field is, however, derived from a potential, like any central force field with rotational symmetry. But there is something surprising: the potential is the same on the plane and on the sphere. The good news is that we shall clearly understand this by simply analysing the transformation of the energy.

First integrals that are polynomial in velocity are sent by GN on first integrals of same type and same degree, with similar decomposition into monomials.

Let us examine some first integrals. We wrote the formula $C = (q - q_\odot) \wedge \dot{q}$ on $\mathcal{P}$. But in the picture of the central projection, $\mathcal{P}$ should be considered, like $\mathcal{S}$, as embedded in the vector space E with one dimension more. So, we can prefer the formula $C = q_\odot \wedge q \wedge \dot{q}$ even in the planar case. Then, we have the same formula for the angular momentum on $\mathcal{P}$ and $\mathcal{S}$, and we have more: C, and even indeed $q \wedge \dot{q}$, are actually *invariant* by the central projection GN. Here is the proof. The position q is sent on hq by GN1, while GN2 may be decomposed into three steps: $\dot{q}$ is first sent on $h\dot{q}$, then projected on the tangent plane to $\mathcal{S}$, then divided by h^2. The second step does not change $q \wedge \dot{q}$ and the final factor is $h \cdot h/h^2 = 1$.

Next, we consider the energy. We write the classical formula for the planar Kepler problem and express it in terms of the invariant $q \wedge \dot{q}$ we just discovered:

$$\begin{aligned} H_{\mathcal{P}}(q,\dot{q}) &= \frac{1}{2}\|\dot{q}\|^2 - \frac{M}{\|q - q_\odot\|} \\ &= \frac{1}{2}(\|q \wedge \dot{q}\|^2 - \|q_\odot \wedge q \wedge \dot{q}\|^2) - \frac{M}{\|q - q_\odot\|}. \end{aligned}$$

The identity is obtained from the planar constraints $\|q_\odot\|^2 = \langle q_\odot, q\rangle = 1$ and $\langle q_\odot, \dot{q}\rangle = 0$ and the expansion of

$$C^2 = \|q_\odot \wedge q \wedge \dot{q}\|^2 = \begin{vmatrix} \|q_\odot\|^2 & \langle q_\odot, q\rangle & \langle q_\odot, \dot{q}\rangle \\ \langle q_\odot, q\rangle & \|q\|^2 & \langle q, \dot{q}\rangle \\ \langle q_\odot, \dot{q}\rangle & \langle q, \dot{q}\rangle & \|\dot{q}\|^2 \end{vmatrix}. \quad (2)$$

Rules GN1 and GN2 associate a state $(q, \dot{q})$ of the dynamics on $\mathcal{P}$ with a state (q_1, q_1') of the dynamics on $\mathcal{S}$. Here we use q_1' rather than $\dot{q}_1$ to distinguish between both time parameters. The first integral $H_{\mathcal{P}}$ is also a first integral for the spherical dynamics, which can be expressed as:

$$H_1(q_1, q_1') = \frac{1}{2}(\|q_1 \wedge q_1'\|^2 - \|q_\odot \wedge q_1 \wedge q_1'\|^2) - \frac{M}{\|q - q_\odot\|}.$$

The variable q in the last term should be expressed as a function of q_1. If we use the (positive) spherical distance θ, this term becomes $-M \cot\theta$. Is this term a potential for the dynamics on $\mathcal{S}$? Indeed,

$$H_{\mathcal{S}} = H_1 + \frac{1}{2}C^2 = \frac{1}{2}\|q_1 \wedge q_1'\|^2 - M\cot\theta$$

is a first integral and is also sum of the natural kinetic energy on the sphere and a function of q alone. Then, $H_{\mathcal{S}}$ is the energy and $-M\cot\theta$ is the potential energy, which thus appears to be invariant by central projection.

The structure of our argument is quite typical of projective dynamics. The forces and the first integrals are easily transformed by central projection. They are the basic concepts. Energy and potential are secondary concepts, that we reach by manipulations of the basic concepts. In the Kepler problem, angular momentum and potential energy happen to be invariant by Appell's projection. But the energy is not.

This hierarchy of concepts may induce negative feelings about the theory. We have to deal first with a mechanics where we ignore the conservation of energy. This is at least as difficult as trying to get an intuition about affine geometry: we can only start from our intuition of the Euclidean space, and we should convince ourselves that what we see as a circle is nothing but an ordinary ellipse among the other ellipses, and that what we see as a square is nothing but an ordinary parallelogram.

SK3 and the energy

The third Kepler law K3 can be decomposed into two statements. The first is that the period is the same for all the ellipses with the same diameter. The second tells us how the period varies when the diameter varies. As the law of force is homogeneous, there is nothing extraordinary in the final formula: the period is proportional to some power of the diameter. And there is nothing surprising that such a power law disappears when the plane $\mathcal{P}$ is curved and replaced by the sphere $\mathcal{S}$.

We will focus on the surprising analogy: the first part of K3 becomes SK3. The period is the same for all the spherical ellipses with the same angular diameter. In order to prove this, we shall better replace the period by the energy, through the following classical result ([Win], [Gor]).

Proposition. *If all the orbits of a Hamiltonian vector field X_H on a symplectic manifold $\mathcal{M}$ are periodic and define a circle bundle structure on $\mathcal{M}$ then, on any connected component of a level set of the Hamiltonian H, all the orbits of X_H have the same period.*

Proof. Choose a periodic orbit p. Consider local coordinates on the base space of the circle bundle $\mathcal{M}$ as functions defined on a neighbourhood of p in $\mathcal{M}$. They commute with H. We can form a composition of their Hamiltonian flows that carries p on any neighbouring periodic orbit in the same level set of H. As these flows all commute with X_H, the time parameter is respected and the period is unchanged. □

Here is a classical proof of K3. The so-called "apsides", extrema for the distance $r = \|q - q_\odot\|$, are determined by the condition $\langle q - q_\odot, \dot{q}\rangle = 0$. Both extremal distances thus satisfy $r^2\|\dot{q}\|^2 = C^2$ or $2H_{\mathcal{P}}r^2 + 2Mr - C^2 = 0$. The sum of both roots is the diameter of the ellipse and is $-M/H_{\mathcal{P}}$. This is, by a standard application of the proposition to the Kepler problem, a function of the period only.

The same argument gives SK3. The apsis condition is $\langle q_\odot, q'\rangle = 0$, to be expressed through expression (2), where we also make $\|q_\odot\|^2 = \|q\|^2 = 1$ and $\langle q, q'\rangle = 0$. We get $\sin^2\theta\|q'\|^2 = C^2$ or $2H_{\mathcal{S}}\sin^2\theta + M\sin 2\theta = C^2$, as the potential is now $M\cot\theta$. The average $\bar{\theta}$ of both roots in $[0,\pi]$ is half of the diameter of the spherical ellipse. As the graph of $2H_{\mathcal{S}}\sin^2\theta + M\sin 2\theta$ has a vertical axis of symmetry in this interval, this axis is $\theta = \bar{\theta}$ and $\bar{\theta}$ satisfies $H_{\mathcal{S}}\sin 2\bar{\theta} + M\cos 2\bar{\theta} = 0$. The diameter of the ellipse is $2\bar{\theta} = \arctan(-M/H_{\mathcal{S}})$.

Three quantities are involved in the proof of SK3: the period, the energy and the diameter of the ellipse. None of them are invariant by central projection. The relation between period and energy is a known fact in conservative mechanics but their relation with the diameter was not easy to predict. Appell's projection explains many analogies between planar and spherical Kepler's problems but not this one.

A moving centre of force or several fixed centres

When considering multiparticle dynamics on a space of constant curvature, we should carefully distinguish between three problems which are nearly identical in the flat theory: the one fixed centre problem, or Kepler problem, which we discussed above; the restricted 2-body problem, with again a Sun and a zero mass planet but where the Sun has a free motion on the curved space; and the 2-body problem, where both masses are positive. One quickly realises that the nice properties of the flat case have already disappeared in the restricted 2-body problem, for reasons that are clearly explained in [BM1]. So, things are getting worse as soon as the Sun is moving. As we are only concerned with the research of analogies, we will concentrate on fixed centres.

We have in mind the two fixed centres problem, which was set and integrated by Euler and which is defined in the plane (or in the space) by the equation:

$$\ddot{q} = -\frac{M_\odot}{\|q - q_\odot\|^3}(q - q_\odot) - \frac{M_\star}{\|q - q_\star\|^3}(q - q_\star). \tag{3}$$

All the presentations we know of the integrability of this problem require identities which may be easy to check but which one cannot simply infer from the form of the equation. We will prove the integrability and understand a generalisation that Lagrange discovered, without further computation than what we need in the study of the spherical one fixed centre problem. The non-invariance of the energy by central projection, which we tend to consider *a priori* as "bad news", will appear as the most effective remark from projective dynamics.

We start with the spherical two fixed centres problem, which was set and integrated by Wilhelm Killing (1847–1923) in 1885, using a separation of variables that Joseph Liouville (1809–1882) had presented in 1846. We will not use this integrability result but just observe that the energy is conserved in this problem: just as the tangential force field produced by a centre is derived from a potential, so is the sum of two such fields, which defines the attraction by two centres.

The idea is now to project the spherical problem on the plane and obtain again a two fixed centres problem. The projected problem would have its own first integral of energy while the projected energy of the spherical problem would be another first integral. We would get two independent first integrals, which, as well-known, would give the integrability.

Back to one centre and the key remark by Halphen

We shall study projections that are more general than that presented in the second figure. The centre of force $q_\odot$ may be centrally projected on a plane $\mathcal{P}$ containing $q_\odot$ but not tangent to $\mathcal{S}$. In such a case we decompose the central projection into two parts. First, we project from $\mathcal{S}$ to the tangent plane at $q_\odot$. Then we project from this tangent plane to $\mathcal{P}$. We know the result of the first projection: the centrality and the rotational symmetry of the force field are preserved. It can be shown that the second projection produces disappointing force fields in all cases except one: the Newtonian force field (1). The following lemma is a corollary of an already mentioned remark by Halphen in 1877.

Lemma. *Suppose that two hyperplanes G and G_1 of a vector space F have a common point $q_\odot$ and do not contain the origin. Suppose that there is on $G \setminus \{q_\odot\}$ a central force field, positively homogeneous of degree -2, with $q_\odot$ as the centre of force. Then the force field obtained as in GN3, by push-forward through central projection on G_1 followed by division by h^4, is the restriction to the image of this projection of a central and positively homogeneous of degree -2 vector field on $G_1 \setminus \{q_\odot\}$.*

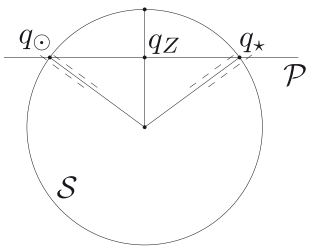
The proof is an elementary computation and the general case is simply deduced from the case $\dim F = 2$. The computation gives a bit more. Consider again both vector fields in the lemma and define their *unit loci* as the sets of points q where the force is $q_\odot - q$. Then *the unit locus of the vector field on G is sent on the unit locus of the vector field on G_1 by parallel projection with direction the vector $q_\odot$.*

When $\dim G = 2$ and the force law on G is (1), the unit locus is the circle with equation $r = M^{1/3}$. This locus becomes an ellipse on G_1. A first reaction is disappointment. The ellipse is not a circle and the new force field is not Newtonian and not even potential. But this is again the same mistake: we are only able to visualise the picture described in the lemma in a Euclidean space. However, there is nothing Euclidean in the statement: F is just a vector space. We got an ellipse on G_1. Well, we shall simply decide that this ellipse is a circle, by endowing G_1 with a convenient Euclidean structure. The central projection of a Kepler problem is indeed a Kepler problem.

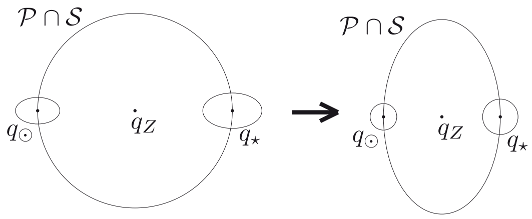
Two centres: Integrability without computation

The subtle part and the good surprise come when we put two centres of force $q_\odot$ and $q_\star$ on a sphere $\mathcal{S}$. By projecting both on the same plane, we obtain for each centre a unit locus, which is an ellipse. Now, we cannot find a Euclidean structure that makes both ellipses a circle, except if the ellipses may be superposed by translation and rescaling. And this is the case, due to an obvious symmetry, if we choose a plane $\mathcal{P}$ which contains both centres $q_\odot$ and $q_\star$ and which is orthogonal to $q_Z = (q_\odot + q_\star)/2$. The equation of motion is then exactly (3), the Euclidean norm in the denominators being such that the unit loci of $q_\odot$ and $q_\star$ are circles.

We have completed our project: we know a first integral of (3), the energy, and another first integral, the projection of the energy of the spherical two fixed centres problem. We can see that one is not a function of the other: the level curves of their respective restrictions to the tangent plane at q_Z are centred ellipses of different kinds.



Can we add terms to (3), corresponding to additional centres of force, while keeping the integrability? Can we consider other attractions than the Newtonian? A third fixed centre of type SN3 on the sphere would not give on $\mathcal{P}$ a unit locus that can be superposed by translation and rescaling on the already drawn unit ellipses. We would not get on $\mathcal{P}$ a resulting force field which is derived from a potential. A law of forces different from SN3 would lose its conservative character as soon as we project it from a plane to a non-parallel plane.



But let us try the parallel case. Appell's projection from a plane to a parallel plane is a mere rescaling of the dynamics. The tangent plane of $\mathcal{S}$ at the point $q_Z/\|q_Z\|$ is parallel to $\mathcal{P}$. A central force field on $\mathcal{S}$ with rotational symmetry around this point would project on the tangent plane, and then on $\mathcal{P}$, to a central force field with rotational symmetry around the vector q_Z. There is just one problem: this rotation is not the good one. It preserves $\mathcal{S}$ while we endowed $\mathcal{P}$ with a Euclidean form for which $\mathcal{P} \cap \mathcal{S}$ is not a circle. And our assertion that a central force field with rotational symmetry is derived from a potential requires that the same Euclidean form is used to define the rotations and the gradient vector field.

Can we choose the law of force such that the additional force is *also* invariant by the rotations defined by our new Euclidean form on $\mathcal{P}$? Yes, indeed, as there exists a force field that is invariant by all the affine transformations of $\mathcal{P}$ fixing the centre of force q_Z. This is $M_Z(q - q_Z)$, where M_Z is a parameter. If we add this term to the right hand side of (3), this right hand side is still derived from a potential and still comes from a force field on the sphere that is derived from a potential. Both energies are conserved and we have the integrability. This generalisation of the two fixed centres problem was proposed by Lagrange in 1767. Here, we reduced Lagrange's discovery to elementary geometrical and dynamical remarks, while previous authors could only argue with unclear algebraic manipulations. Our argument proves at the same time the integrability of the spherical version of Lagrange's extension of the two fixed centres problem, which was previously known to Kozlov & Harin.

Divergence-free force fields

The inverse sine square law of forces on the unit sphere, which Serret pointed out as producing sphero-elliptic trajectories, was not unknown to previous geometers. Lobachevsky and Bolyai had ideas about what should be the universal law of gravitation on a 3-dimensional space of constant curvature. These ideas also single out the same law. They may be translated into a familiar formula: the divergence of the force field is zero. Serret's force field happens to have this property, when considered on a 3-dimensional sphere. Clearly, this central field is uniquely determined by this property and the rotational symmetry.

Killing was the first to claim that SN3 has this double property: to be divergence-free and to produce SK1. We already cited his remarkable paper of 1885, where SK3 is also presented as well as many other fundamental facts of the dynamics in constant curvature spaces.

This coincidence again calls for an explanation. We cannot see any logical relation between both properties. Indeed, K1 is still there in any dimension $n \geq 2$, while the divergence is zero only if $n = 3$. Appell's central projection GN provides a fundamental key, if not a complete explanation.

If $\mathcal{P}$ and $\mathcal{S}$ are 3-dimensional, GN3 sends a divergence-free force field on a divergence-free force field.

Finally, the inverse sine square force field is divergence-free because the usual Newtonian force field is.

Let us conclude by trying to count the fundamental coincidences which are behind the nice properties of the Kepler problem. The dimension n of the space is an integer parameter. There is a power $k = 3$ in the denominator of (1) and a power $m = 4$ in law GN3. Halphen's lemma works because $k = m - 1$, while the above divergence statement requires $n = m - 1$. A corollary is $k = n$, which means a divergence-free force field. This last property is often considered as defining the gravitational law in higher dimension. However, due to $k \neq m - 1$, such a law would not fit as deeply with the geometry of the ambient space as Newton's does.

Further developments

There are relatively few works directly concerned with the central projection in dynamics. The reader can quickly access most of them through [Alb] and [BM2]. In contrast, there are many related topics belonging to very classical geometrical or

mechanical subjects. I warmly thank Julian Barbour, Alexey Borisov, Alain Chenciner, Hans Lundmark, Ivan Mamaev, Stefan Rauch and Christian Velpry for the precious information they gave me about constant curvature spaces, Newtonian dynamics, classical integrable systems, etc. Projective dynamics seems promising as a simplifying tool, which also opens the way to numerous and profound investigations.

Notes

1. Properly speaking, the force should include the mass of the planet as a factor. The correct Newtonian terminology is "accelerating force".
2. Paul Serret was born in Aubenas, South East of France. He is not closely related to the academician Joseph-Alfred Serret (1819–1885), who nevertheless has ancestors, according to Albin Mazon, from the surroundings of Aubenas.

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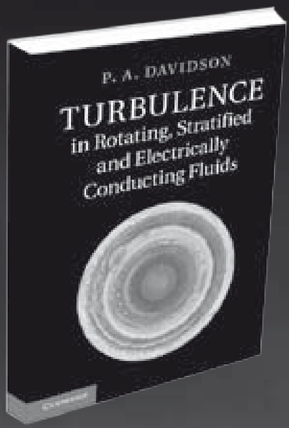

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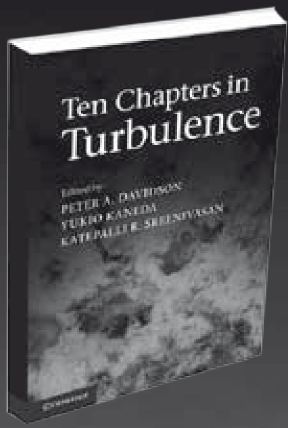

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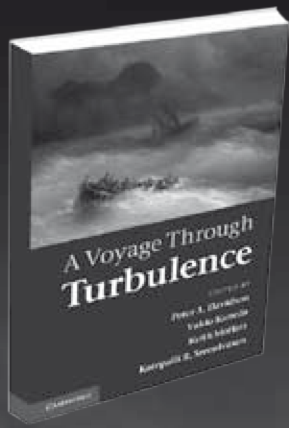

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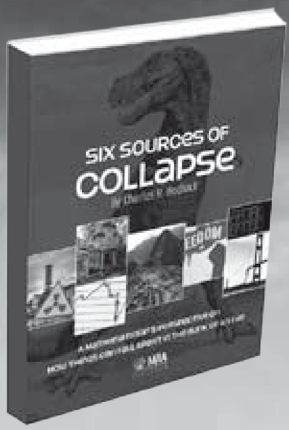

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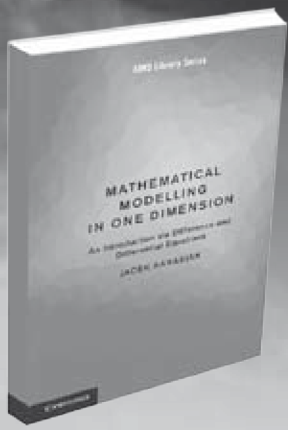

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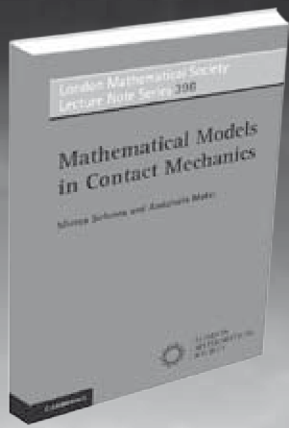

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Institute for Computational and Experimental Research in Mathematics

Jill Pipher (Brown University, Providence, USA)

The Institute for Computational and Experimental Research in Mathematics (ICERM) is a recently funded National Science Foundation (NSF) Mathematics Institute in Providence, Rhode Island. Its mission is to support and broaden the relationship between mathematics and computation in a variety of pure and applied mathematical fields: all of the institute's activities and programmes have a strong experimental and/or computational component. The NSF grant was awarded to five principal investigators at Brown University in August 2010 and ICERM opened its doors one year later, following a start-up period that included a major renovation on the beautiful space provided by Brown University.

The Institute for Computational and Experimental Research in Mathematics (ICERM) is located on the top two floors of 121 S. Main Street.

Activities

ICERM organises two international semester research programmes each academic year, along with several independent topical workshops, a summer undergraduate research programme, an early career researcher IdeaLab, public lectures and a variety of special events including conferences and activities designed to increase the participation of under-represented groups in mathematics. Even though the 2012–2013 academic year was only ICERM's second year in operation, more than 1200 people participated in institute activities, and about 1000 received some financial support to do so.

Semester Research Programs

hese themed semester programmes and their associated workshops bring together approximately 20 scientists in residence for most of the semester, together with postdoctoral fellows, graduate students and hundreds of weekly/monthly visitors. ICERM develops, and also seeks, external proposals for semester research programmes. These proposals are reviewed and selected by the Science Advisory Board, chaired by Andrea Bertozzi (UCLA), at its annual meeting in November. Each semester programme has three associated workshops that draw speakers and attendees to the institute from an international roster of both leading experts and junior faculty. The organising committee of a semester programme arranges tutorials in advance of the workshops and weekly seminars. Postdocs and graduate students run a peer-to-peer seminar and attend regularly scheduled, professional round-table discussions run by directors (see below).

During the Spring semester of 2013, ICERM hosted a special programme devoted to common themes in automorphic forms, combinatorial representation theory and multiple Dirichlet series. L-functions — vast generalisations of the Riemann zeta function — are fundamental objects of study in number theory. In the 1980s, the idea emerged that it could be useful to tie together a family of related L-functions in one variable to create a "double Dirichlet series", which could be used to study the average behaviour of the original family of L-functions.

Early career IdeaLab participants discuss cutting edge research ideas with both peers and leaders in their field.

'Moduli Spaces Associated to Dynamical Systems' workshop participants problem solve at the board.

Participants in ICERM's 'Complex and *p*-adic Dynamics' workshop

Double Dirichlet series soon became multiple Dirichlet series. It gradually emerged that the local structure of these multiple Dirichlet series shows a rich connection to combinatorial representation theory. Researchers in this programme explored this interface between automorphic forms and combinatorial representation theory, and developed computational tools for facilitating investigations.

The upcoming Fall 2013 programme focuses on the recent impact of computation and experiment on the study of the pure mathematics sides of topology, geometry and dynamics. Specific areas include 3-dimensional topology, the study of locally symmetric spaces, low-dimensional dynamics and geometric group theory. In Spring 2014, researchers will convene to explore computational problems on graphs, a central area of research in computer science. Recent years have seen qualitative changes in both the problems to be solved and the tools available to do so, while application areas such as computational biology, the web, social networks and machine learning give rise to large graphs and complex statistical questions that demand new algorithmic ideas and computational models. The semester research programmes at ICERM have been planned through to May 2015; the Fall 2014 semester programme focuses on High-dimensional Approximation and Spring 2015 brings mathematicians and physicists to ICERM to explore Emergent Phenomena and Phase Transitions.

Training and mentoring

A special focus of this institute is the training and mentorship of younger and early career mathematicians, through specific outreach programmes and directed opportunities for connections between mathematicians at different stages of their careers. This includes ICERM's postdoctoral programme, integration and support of graduate students in the context of semester programmes, summer research programmes for undergraduates (Summer@ICERM) and early career researcher IdeaLabs.

ICERM's postdoctoral programme brings recent PhDs to the institute in order to support and expand their research and to create lasting career collaborations and connections. ICERM supports postdoctoral researchers in two different ways: postdoctoral fellows, who participate in a single semester programme and are supported by a stipend, and a smaller number of institute fellows, who stay at ICERM for one year and are supported by a salary for nine months with the possibility of additional summer support.

The research semester programme budget includes partial support for a cohort of graduate students. A housing allowance and travel to the institute is provided to about 10–14 graduate students, each of whom applies to be in residence for the entire semester. Applicants include graduate students working with visitors to the programme, as well as students who intend to come without an advisor. To prepare graduate students and postdocs better for their future careers, the institute also organises regular round-table discussions that, in the course of each semester, cover the following topics: applying for academic positions, writing and submitting papers and grant proposals, ethics in research and job opportunities in industry and government.

Topical workshops

ICERM organises three to five independent workshops per year: most are chosen from among a group of external proposals and some are externally funded. The formats of these workshops vary according to the goals, and the themes are quite varied. Examples follow of some topical workshop themes (past and future).

The first event at ICERM in August 2011 was a workshop on geometric complexity theory, highlighting the nascent approaches to P vs. NP from arithmetic geometry. Many of the members of the organising committee and participants have helped organise a long programme at the new Simons Institute for the Theory of Computing around these themes to occur in Fall 2014.

A December 2012 workshop tackled the challenge of reproducibility in computational and experimental mathematics. The conference gathered researchers from pure and applied mathematics from academia and other settings, together with interested parties from funding agencies, national laboratories, professional societies and publishers. There were "lightening talks" by many participants, a few longer talks and dedicated time for breakout group discussions on the current state of the art and the tools, policies and infrastructure needed to improve the situation. A Wiki and outcome paper is archived on the ICERM website.

An upcoming June 2014 workshop will gather experts in applied and computational mathematics working on the development of robust and efficient numerical

Participants in the 'Moduli Spaces Associated to Dynamical Systems' workshop gather around a poster presentation

Summer@ICERM 2013 students and faculty leaders during a field trip to the Providence headquarters of the American Mathematical Society.

ICERM's state-of-the-art lecture hall

schemes essential for solving complex coupled nonlinear systems of PDEs and advancing the development of fast and efficient linear and nonlinear solvers that are scalable and optimal.

Summer programs and special events

In addition to the topical workshops, ICERM organises and hosts a variety of events and activities. A summer undergraduate research programme (Summer@ICERM) brings outside faculty leaders, teaching assistants and undergraduates to the institute for an intensive eight-week research programme on different topics each year. ICERM hosts two to four public lectures per year and co-sponsors an annual undergraduate research conference (SUMS). ICERM is part of a consortium of North American institutes, including the NSF Mathematics institutes, working together to support the participation of under-represented groups in the mathematical sciences, including women, under-represented racial and ethnic minorities and people with disabilities.

Facilities and infrastructure

ICERM inhabits the top two floors of a Brown University-owned office building located on the edge of campus at the downtown riverfront. The lecture hall and common space on the 11th floor of 121 S. Main St. features 20 ft floor-to-ceiling windows with spectacular views of the city and campus. The 10th floor houses the visitor offices – 45 desks in single and multi-person offices – and common areas, as well as a fully equipped seminar room. Visitors can enjoy the many writeable surfaces, including painted wall-size chalkboards and the towering glass walls on both sides of the lecture hall. The lectures at workshops are captured, streamed live and made available on the website (see the Resources page at icerm.brown.edu).

ICERM provides thin-clients for visitor use in all offices as well as in the common areas. The thin clients run a thin version of Debian Linux and provide open access to a web browser, SSL terminal and printing capability. Visitors also have access to virtual Linux and Windows desktops via Virtual Bridges. Most visitors bring their own devices, and wireless access and printing access is available. Long-term visitors may take advantage of an internal social connection platform, Atrium. Access to Brown University's high performance computing cluster at the Brown Center for Computing and Visualization is available to long-term visitors upon request. One visitor to the Spring 2013 semester programme at ICERM noted: "ICERM is the first institution that is able to provide sufficiently powerful high performance computers with sufficiently recent compiler."

One of several collaborative spaces at ICERM

How to participate

ICERM announces calls for proposals for research programmes, workshops and summer undergraduate programmes at various times of the year. The institute invites applications to participate, with or without funding, in all programmes and activities, invites applications for postdoctoral positions and sends regular updates and a newsletter to those on the mailing list.

Interview with the ICMI president Ferdinando Arzarello (University of Turin, Italy)

Mariolina Bartolini Bussi (Università di Modena e Reggio Emilia, Italy)

From 1 January 2013 to 31 December 2016 the ICMI has a new President: Ferdinando Arzarello, a full professor at the Department of Mathematics, University of Turin, Italy. Here follows an interview with the new president, to capture his feelings and plans for the ICMI.

First, I want to thank you for this interview. I know that you have many obligations but I believe that it is important to share your ideas with our readership. What are, in your opinion, the main projects of the ICMI? What are your aims for your term?

The main projects of the ICMI are well illustrated on the ICMI website (http://www.mathunion.org/ICMI/) and in ICMI News, to which people can freely subscribe (http://www.mathunion.org/icmi/publications/icmi-news/). I just wish to mention:

- The quadrennial International Conference on Mathematics Education (ICME), held in 2012 in Seoul (South Korea) and announced for 2016 in Hamburg.
- The permanent programme of ICMI Studies, started in the 1980s in order to have a better understanding and resolution of the challenges that face multidisciplinary and culturally diverse research and development in mathematics education. So far, 19 studies have been completed; three studies are in progress and one study, the first ICMI study on primary schools, has just been launched.
- The Klein project, inspired by Felix Klein's famous book Elementary Mathematics from an Advanced Standpoint, which was published one century ago. It is intended as a stimulus for mathematics teachers, to help them make connections between the mathematics they teach, or can be asked to teach, and the field of mathematics, while taking into account the evolution of this field over the last century.
- The Capacity & Networking Project (CANP), The Mathematical Sciences and Education in the Developing World. It is a major development focus of the international bodies of mathematicians and mathematics educators (the International Mathematical Union, IMU, and the International Commission on Mathematical Instruction, ICMI) in conjunction with UNESCO and the International Congress of Industrial and Applied Mathematics, ICIAM. The project is a response to Current Challenges in Basic Mathematics Education (UNESCO, 2011).

I know that short notes about these projects have already been published in the EMS Newsletter.

The Klein and CANP projects have been developed in cooperation with the International Mathematical Union and bear witness to the extremely good relationships between the ICMI and the IMU. In this effort of cooperation with developing countries, the Commission for Developing Countries of the IMU (http://www.mathunion.org/cdc/about-cdc/members-cdc/) and the International Centre for Pure and Applied Mathematics - Centre International de Mathématiques Pures et Appliquées (CIMPA) are involved.

This network of activities is very demanding and exploits the competences of both the members of the present executive committee of the ICMI and of what we could call "friends of the ICMI", that is, members of the ICMI community at large, who, after working on ICMI activities in the past, are still willing to cooperate with the ICMI. In this very rich community I hope to be able to give a personal contribution on the process of "culture melange", in order to foster the exchanges between different cultures and traditions. I am especially worried about and interested in Africa. When I submitted my CV to be elected for President I quoted a sentence by Publius Terentius Afer (Terence, the Latin playwright born in Africa in the second century BC): *Homo sum, humani nihil a me alienum puto* (I am a human being, so nothing human is strange to me). Two of the series of CANP programmes actually involve Africa (Mali and Sub-Saharan Africa; Tanzania and Eastern Africa).

The role played by non-European scholars in the science renaissance after the Middle Ages is well-known to historians, although not so popular with the general public. An Italian merchant Leonardo Fibonacci is credited with introducing to the West and to Europe the important arithmetic and algebraic knowledge developed by Arab scholars. This cultural melange is well presented, in a snappy way, by the exhibition "Un ponte sul Mediterraneo" (A bridge over the Mediterranean: http://php.math.unifi.it/archimede/archimede/fibonacci/mostra.php). This influence changed European and World history.

We have already published (Issue 87) the names of the members of the executive committee (2013–16). They are: Ferdinando Arzarello (President), Cheryl E. Praeger and Angel Ruiz (Vice-Presidents), Abraham Arcavi (Secretary-General), the Members-at-Large Catherine P. Vistro-Yu, Jean-Luc Dorier, Roger Howe, Yuriko Yamamoto Baldin and Zahra Gooya, the ex-officio Members Bill Barton (past President of the ICMI), Ingrid Daubechies (President of the IMU) and Martin Grötschel (Secretary of the IMU). Would you like to say some words about them? Have you already met them?

I was already acquainted with Abraham Arcavi (the current Secretary General of the ICMI), Bill Barton (the past President), Yuriko Baldin, Jean Luc Dorier and Zahra Goya. Apart from their scientific output, I became acquainted with them on different occasions. For example, I worked with Jean Luc at the first CERME (Osnabrueck, Germany, 1998) in the working group about the teaching of algebra; I was in Auckland many times and I became a friend of Bill and his wife Pip; I met Abraham at a Winter school on mathematics education that I organised in Italy some years ago; I am working with Yuriko in the Klein project; I worked with Zahra when we were both in the IPC of the PME some years ago. Now I am starting to know all the members of the EC through the frequent emails we are exchanging because of our charge and during our meetings. The working climate is excellent and we have started to share some responsibilities. But another person is also very important for our work in the EC: Lena Koch, who manages all the administrative business of the ICMI from her bureau in Berlin, where the offices of the IMU and the ICMI have been for some years.

You already had substantial international experience as an outstanding researcher in mathematics education, as a member of the International Committee of the International Group for the Psychology of Mathematics Education (IGPME) and as the President of the European Society for Research in Mathematics Education (ERME). I have no doubt that your past experience will be very useful for your presidency. What similarities and differences do you envisage between these contexts?

IGPME and ERME are both associate organisations of the ICMI: IGPME since 1976 and ERME since 2010. The IGPME is a research community, whose main focus is not so much on the mathematical content of the teaching and learning of mathematics but rather on the conditions (psychological, sociological and so on) where this process occurs. ERME is mainly a European association, although in ERME conferences we have the pleasure of hosting outstanding researchers from all over the world. In principle, ERME has the potential to be attractive for many non-European researchers because of the spirit of fostering communication, cooperation and collaboration. The ICMI has no individual members as the members are countries. In this way, the ICMI has created, in a sense, a much larger community, larger in time (ICMI was founded in 1908), in space (more than 90 countries), in focus (the mathematics to be taught, in connection with the IMU traditions), and in the issues to be discussed, which address not only research questions in the traditional sense but also political questions, related to the diffusion of mathematics education all over the world. This project surely draws on the expertise of Western researchers but has to take into account the culture, the traditions, the needs and constraints of many different countries, most of which are not even able to guarantee primary school education to every child. This requires a change of perspective, as far as the relevance of projects is concerned.

As you say, the ICMI now has more than 90 member states from all the continents. The executive committee includes members from several continents. What might be, in your opinion, the contribution of European culture and tradition in mathematics education to the future development of ICMI activities?

A good question. Education, including mathematics education, has been in Europe and in other Western countries the main tool for developing critical thinking and the very sense of democracy. Europe can contribute with a reflection on the ways this process was developed, not offering ready-made solutions but rather fostering the development of similar processes in other parts of the world, taking into account local cultures and traditions. I like to mention here a quotation from Yuri Lotman, the founder of the Tartu-Moscow Semiotic School at Tartu University in Estonia: "culture is not a repository of ready-made ideas and texts, but a living mechanism of collective conscience."

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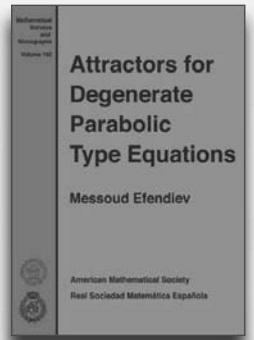

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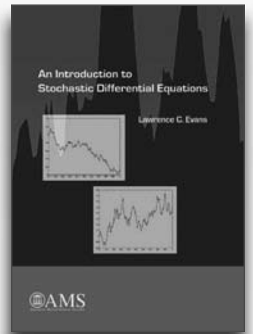

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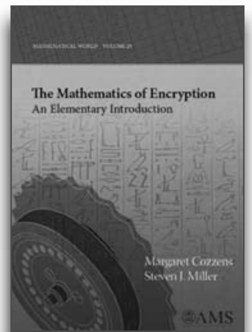

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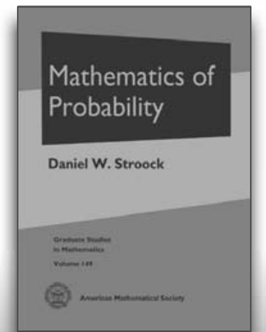

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Solid Findings on Students' Attitudes to Mathematics

Rosetta Zan (University of Pisa, Italy) on behalf of the Education Committee of the EMS

Episodes from ordinary school life

Episode 1:

Alicia (7th grade) has to find the perimeter of a rectangle whose length is 12 cm and whose width is 8 cm. She multiplies 12 by 8. The teacher says: "Why did you multiply? You have to find the perimeter, not the area…!" And Alicia says: "Shall I divide?"

Episode 2:

Nicholas (11th grade) has to solve the inequality:

$$-7x^2 < \sqrt{7}$$

He multiplies both sides by –1/7, obtaining:

$$x^2 > -\frac{\sqrt{7}}{7}$$

Then he multiplies by 7 and transposes the right member to the left side:

$$7x^2 + \sqrt{7} > 0$$

At this point he stops and refuses to continue in spite of the teacher's encouragement.

Most people – teachers and mathematicians too – will probably describe Alicia's and Nicholas' behaviours as effects of a 'negative' or even 'wrong' attitude toward mathematics, which leads the students to answer randomly or to refuse answering. But what does 'negative attitude' really mean? And how can this diagnosis help mathematics educators in planning remedial actions? In other words, how can mathematics teachers modify a student's negative attitude toward mathematics?

Negative / positive attitude towards mathematics

The construct *attitude* finds its origin in social psychology (Allport, 1935), in connection with the problem of predicting an individual's behaviour in contexts that involve choices based on simple preferences like buying goods or voting. In these studies, attitude is generally described as a predisposition to respond to a certain object either in a *positive* or in a *negative* way. In mathematics education, early studies about attitude had already appeared in the second half of the 20th century, moved by the belief that something called 'attitude' plays a crucial role in learning mathematics (Neale, 1969): they tried to highlight a causal relationship between (positive) attitude toward mathematics and school mathematics achievement.

The characterisation of attitude in these early studies is that typical of the social sciences, seeing attitude toward mathematics as the emotional disposition toward the discipline, thus identifying a positive/negative attitude toward mathematics as a positive/negative emotional disposition ('I like/dislike mathematics'). In this early period, some important results have been obtained, in particular about the relationship between attitude towards mathematics and the choice of mathematical courses (Aiken, 1970) and about gender differences (Fennema & Sherman, 1977). But, in actual fact, research on attitude failed in reaching its main goal: a clear correlation between attitude towards mathematics and mathematical achievement does not emerge (Aiken, 1970; Ma & Kishor, 1997). For example, McLeod (1992) refers to data from the Second International Mathematics Study, which indicate that Japanese students had a greater dislike for mathematics than students in other countries, even though Japanese achievement was very high.

The acknowledgment of this failure and the attempts of interpreting it contributed to point out the need for a theoretical debate about research on attitude. Researchers highlight as critical points in previous research the lack of a suitable definition of positive/negative attitude and the inappropriateness of the instruments used to measure it (obviously also 'success' and therefore 'achievement' in mathematics can be defined and measured in different ways).

As regards the definition, characterising attitude simply as the emotional disposition toward mathematics can be useful in dealing with issues such as the choice of mathematics courses or the comparison between different groups of individuals but it seems inadequate in dealing with complex issues such as success in mathematics, which involves the decisions made by an individual during problem solving activity. In this case, a 'positive attitude' toward mathematics cannot be reduced to a positive emotional disposition but should be linked with 'positive' beliefs about the discipline, i.e. with an epistemologically correct view of it. A suitable characterisation is made by Richard Skemp (1976), who identifies an *instrumental* vision of mathematics, according to which mathematics is a discipline made of fixed rules without reasons to be memorised and applied, as opposed to a *relational* vision of mathematics, characterised by a focus on the processes and their relations rather than on products.

In fact, in the early period, the instruments used to measure attitude, even when attitude was simply seen as the emotional disposition towards mathematics, were mainly questionnaires constituted of items that refer not only to emotions ('I like mathematics') but also to beliefs about mathematics ('Mathematics is useful') and to behaviours ('I always do my homework in maths'). In some way therefore they seem to take into account the vision of mathematics. But most choices appear to be questionable about the evaluation of positive/negative (that results in the assignment of a score). For example, with regard to beliefs about mathematics, agreement with the items 'Mathematics is useful' or 'Mathematics is easy', which are most used in questionnaires on attitudes, is considered 'positive' but many mathematicians might question this choice.

These critical points also influence the approach to the crucial issue of promoting a positive attitude toward mathematics. Is the goal of developing a positive emotional disposition toward mathematics, if this disposition is not associated to a 'positive' view of the discipline, a significant goal in mathematics education? Considering only the emotional aspects has a great risk: it can lead teachers to avoid complex tasks in order to promote a positive emotional disposition.

A new period for research on attitude

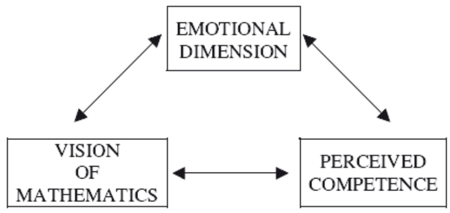
A new period for research on attitude toward mathematics arose from the debate about the critical issues described above, characterised by the need for a theoretical framework and for new methods of inquiry. This need involves research on affect, a new field in mathematics education that, in addition to attitude, also includes constructs such as emotions and beliefs (see McLeod, 1992; Törner, 2013). Recent research on affect takes into account a radical critique that at the end of the 1980s arose within the social sciences and later involved mathematics education: the limits of a normative approach, i.e. the attempt to explain behaviour through measurements or general rules based on a cause-effect scheme. The awareness of the high complexity of human behaviour gradually led to the affirmation of an alternative paradigm: the interpretive one, aimed at understanding – rather than explaining through universal laws – an individual's actions.

Recent studies therefore abandon questionnaires in favour of narratives (essays, diaries, interviews) and also of the observation of behaviour in natural settings or in structured situations. In particular, the use of narratives makes it possible to take into account the subjectivity of an individual's attitude towards mathematics.

An example of a study that uses narratives to describe attitude toward mathematics is that carried out by Di Martino and Zan (2010), who collected more than 1800 autobiographical essays with the title '*Maths and me: my relationship with maths up to now*' written by students of all grade levels, trying to identify how students describe their relationship with mathematics. From this study emerges that when students describe their own relationship to mathematics, nearly all of them refer to one or more of these three dimensions:

- Emotions.
- Vision of mathematics.
- Perceived competence.

These dimensions and their mutual relationships therefore characterise a student's relationship with mathematics, suggesting a Three-dimensional Model for Attitude (TMA):



The TMA model for attitude (Di Martino & Zan, 2010)

Interestingly enough, the teacher emerges as a crucial mediating factor with respect to these three dimensions and it is the most recurrent factor linked to changes in a student's attitude toward mathematics.

The multidimensionality highlighted in the model suggests the inadequacy of the positive/negative dichotomy for attitude that refers only to the emotional dimension. In particular, the model suggests considering an attitude as negative when at least one of the three dimensions is negative (identifying, according to Skemp, as negative an *instrumental* vision of mathematics, and as positive a *relational* one). In this way, it is possible to outline different *profiles* of negative attitude towards mathematics.

With this model in mind, let's go back to Alicia and Nicholas, protagonists of the episodes described. Actually, we know more about them than the crude description made above: we have further information obtained through the use of observational tools, aimed at better interpreting their actions.

Alicia has written in her autobiographical essay: "At elementary school I was not a genius in mathematics, so in the third class I realized that I was not good and therefore closed my head, saying that mathematics was not for me."

Nicholas was involved in an interaction with a young researcher, who intervenes in front of his block, saying: "Why don't you try to solve this inequality by reasoning, instead of remembering the correct way to follow?"

Nicholas answers: "Mathematics is done by fixed and precise rules that have to be respected and applied: you can not invent anything. To solve problems you have to follow them, and in this moment I don't remember the rules to solve inequalities."

This further information allows us to understand the apparently irrational behaviour of Alicia and Nicholas. Alicia's words reveal that she is convinced of not being able to do mathematics, highlighting her low perceived competence in that field. Nicholas seems to view mathematics as a discipline made of fixed rules without reasons, to be memorised and applied; in other words, he seems to have an instrumental vision of mathematics instead of a relational one.

Even with this information, we continue to see the two students' behaviours as a consequence of a negative attitude towards mathematics. But the theory on attitude developed in mathematics education gives us new instruments to interpret these kinds of behaviour and then to intervene effectively. Alicia's *profile* of negative attitude appears to be different from that of Nicholas, thus requiring different didactical actions: in the case of Alicia, the negative component is her low perceived competence in mathematics and the primary goal of the teacher should be to convince her that she 'can' do mathematics; in the case of Nicholas, the negative component seems to be his instrumental vision of mathematics and the goal of remedial action should be to overcome this vision, in favour of a relational one.

Some conclusions

Although the debate about some critical issues in research on attitude towards mathematics still continues, this research has produced some solid findings and we will highlight them here.

The most important such solid finding is, in our opinion, that non-cognitive factors have a crucial role in learning mathematics; this 'belief' has been the starting point of research on attitude, has motivated interest for the construct of attitude and has become a finding, since research has highlighted the deep interaction between cognition and affect in the context of mathematics.

But in its evolution, research on attitude has also contributed to highlight another general and significant issue in mathematics education: the need to adapt constructs and tools borrowed from other fields in order to face problems that are specific to mathematics education. This position characterises mathematics education as a discipline problem-led rather than method-led; in other words, research in mathematics education is led by problems, which influence the search for suitable methods, and not by the methods available, which impose what kind of problem can be dealt with.

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 Tommy Dreyfus, Ghislaine Gueudet, Bernard Hodgson, Celia Hoyles, Konrad Krainer, Mogens Niss, Juha Oikonnen, Núria Planas, Despina Potari, Alexei Sossinsky, Ewa Swoboda, Günter Törner, Lieven Verschaffel and Rosetta Zan.

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ERME Column

Behiye Ubuz (Middle East Technical University, Ankara, Turkey) and Maria Alessandra Mariotti (University of Siena, Italy)

CERME8, Side-Manavgat Antalya, Turkey
6–10 February 2013
http://www.cerme8.metu.edu.tr/

The 8th Congress of the European Society for Research in Mathematics Education (CERME8) was held at the Starlight Convention Center, Thalasso & Spa Hotel, in Manavgat-Side, Antalya, Turkey, 6–10 February 2013, chaired by Prof. Dr. Behiye Ubuz (Local Organiser Chair) and Prof. Dr. Maria Alessandra Mariotti (International Programme Committee Chair).

At CERME8, considering ERME policy, we wanted to allow groups of researchers in a particular scientific area to really work together on their area of research, with sufficient time to get to know each other, to share and discuss their research and to engage in deep scholarly debate. At the same time, we wanted to support the scientific development of young researchers fostering their active participation in our research community. Therefore, at CERME8, participants spent most of the time in discussion and debate within the thematic Working Groups (WGs) over seven working sessions of 90 minutes each. The leaders team organised the peer review process among the members of the groups according significantly devolved and distributed responsibility in criticising but also supporting the elaboration of the single contributions. This process was aimed not only at raising the quality of the papers but also at developing a sense of belonging to a community, for all participants. At the end of this first phase, all the accepted paper were posted on the website of the conference and participants were expected to read all the papers related to their own WGs, before attending the conference. This corpus of papers constituted the first working material for the WG activities and a great deal of time and intellectual effort was spent by the leaders in outlining the structure of the working sessions, where the different contributions were fully discussed and related to the other contributions. The particular format of the CERME conference gives the participants the opportunity of getting fruitful feedback that can enlarge and enrich their own perspectives. Thus, after the conference, the authors have the possibility of further revising their papers, integrating significant elements emerging from their WG discussions. This will be the form in which the papers will pass through the final review process and, when accepted, will appear in the proceedings. The double review process that is used at CERME congresses – papers are firstly accepted for discussion in the WGs and than their final version has to be accepted for publication in the proceedings – not only aims at raising the quality of the papers but also at assuring a fair balance between quality and inclusion, two goals that seem to pull in different directions and may create tension and sometimes frustration. However, the attainment of a good balance between *quality* and *inclusion* constitutes the main challenge of our community according to our main objective: to ensure the ERME spirit of communication, cooperation and collaboration.

The number of WGs has increased over the years and since CERME7 we have had 17 WGs. Except for WG 15 and WG 17, the number of participants in each has been around 25–30, including about 4 WG leaders. For CERME8, 375 research papers and 90 poster proposals were submitted, with 310 research papers and 57 poster proposals accepted for publication in the proceeding.

In addition to the WG activities, the congress was enriched with a number of plenary scientific activities and a varied social and cultural program. The opening session included a plenary address by Paolo Boero, who proposed a deep reflection on how to deal, as researchers, with the unavoidable complexity of big problems concerning the teaching and learning of mathematics in our societies. On the basis of a long personal elaboration, strictly and functionally interwoven with the evolution of the experimental activity in a school carried out with the Genoa research group since the 1970s, Boero offered us some answers to those big questions emerging from complex phenomena, particularly those concerning societal needs and values and related educational choices.

As at previous CERMEs, two other plenary talks were given by former WG leaders. Alain Kuzniak presented a vivid account of what are today the core items and the contributions of research in the didactics of geometry, and he did it in the light of the rich discussions which have been occurring in the CERME Working Group on Geometry since its beginnings in 1999. Candia Morgan delineated a superb survey of the complex field of the study of language in mathematics education. As she said, she offered her map, her personal and critical account on previous studies in this field, and especially a theoretical elaboration as it emerged from the active discussion that has taken place at the CERME Working Group on Language and Mathematics over the years.

As is a tradition at CERMEs, one day before the opening, another fundamental event took place: the YERME (Young European Researchers in Mathematics Education) day. This is now a constant appointment where young researchers – doctoral students or post-doctoral researchers – meet expert scholars in thematic discussion groups. This event, together with the YERME Summer School (YESS), is based on the volunteering of some members of the society. At CERME8 the organisation of the YERME day (http://cerme8.metu.edu.tr/yerme.html) was coordinated by João Pedro da Ponte, Ferdinando Arzarello and Behiye Ubuz, and the activities were led by Professors Paolo Boero, Behiye Ubuz, Uffe Thomas Jankvist, Barbara Jaworski, Ester Levenson, Maria Alessandra Mariotti, João Pedro da Ponte, Susanne Prediger, Mario Sanchéz and Susanne Schnell.

The success of the ERME Conferences is witnessed by the constantly increasing number of participants and presentations. In Manavgat, around 520 participants attended the congress, from 45 countries within and beyond Europe. Many participants from European countries were there: UK (31), Portugal (29), Germany (98), Italy (22), Greece (10), Finland (6), Spain (30), Netherlands (8), Sweden (54), Cyprus (2), Denmark (16), Norway (24), Austria (1), Czech Republic (5), France (41), Ireland (5), Romania (1), Russia (4), Belgium (4), Iceland (4), Estonia (2), Latvia (1), Poland (2) and Switzerland (4). Moreover, there were 34 researchers from Turkey, 12 from Israel, 20 from the US, 16 from Canada, 1 from Australia, 1 from the Far East (Japan), 27 from Latin America (Brazil, Chile, Colombia and Mexico) and several others from non-affluent countries, e.g. Iran (2), South Africa (1), Saudi Arabia (3), Algeria (1), Kuwait (1), Tunisian (2), Lebanon (1) and Zaire (1).

A view of the Starlight Convention Center, Thalasso & Spa Hotel

A view from Side-Antalya (taken from http://www.resim11.com/Antalya.html)

CERME8 must surely be regarded as a great opportunity for teachers, mathematics educators, teacher educators and policymakers around the world and in Turkey who are interested in mathematics education and its development. The proceedings of CERME 8 will appear very soon; we are certain that the reader will appreciate the richness of the contributions collected in that text that we hope will offer the opportunity to share with us something of the exciting experience of our congress, and encourage interested researchers to meet us at the next CERMEs.

Behiye Ubuz [ubuz@metu.edu.tr] (left) is professor of Mathematics Education at the Faculty of Education of the Middle East Technical University in Ankara, Turkey. She is involved in several National Project in Turkey. Recently she has organized the 35th Conference of the International Group for the Psychology of Mathematics Education (Ankara, 2011) and CERME 8 (Antalya, 2013).

Maria Alessandra Mariotti [mariotti21@unisi.it] (right) obtained a degree in mathematics at the University of Pisa and a PhD in mathematics education at the University of Tel Aviv. She is a professor at the Department of Information Engineering and Mathematics of Siena University. Her research field is that of mathematics education, with particular interest in geometrical thinking and proof and a specific focus on the use of new technologies in school practice.

Mathematical Formula Search

Michael Kohlhase (Jacobs University Bremen, Bremen, Germany), Helena Mihaljevic-Brandt (Zentralblatt MATH, Berlin, Germany), Wolfram Sperber (Zentralblatt MATH, Berlin, Germany) and Olaf Teschke (Zentralblatt MATH, Berlin, Germany)

Have you ever in a paper you are reading encountered an unfamiliar symbol and immediately wanted to know more about the object it denotes? Or an expression in a calculation for which you would like to analyse relevant literature? Or have you gotten stuck in a proof and wanted to know which identities are applicable so that you can progress?

A traditional approach to such situations would be to consult an expert in the field, and this is certainly still a good idea in many cases. But you may not know of the right person, and even with an expert available one can hardly be sure that they will cover the vast complexity of modern development in mathematics. In particular, the retrieval of non-English literature remains a real difficulty here. You could also post your question on a forum like mathoverflow.net but, again, you have to hold to luck that the right person comes across this. Maybe you would not even be desperate enough to try to employ a search engine like Google or Bing even though you know that they are optimised for finding word occurrences in documents. But formulae are not words and so results from traditional search engines are erratic. What we really need in the situations described above is a *formula search engine*.

MathSearch project

To remedy this lack and to support mathematics research, the German Leibniz Association has funded a collaborative research project by Zentralblatt MATH (zbMATH) and a group of computer scientists from Jacobs University Bremen. The goal of the three year MathSearch project, which started in March 2012, is to develop tools for information retrieval and literature access for mathematics. A first prototype is already available at zbmath.org/formulae/ and is ready to be explored by mathematicians (see Figures 1 and 2); later, improved versions will be permanently integrated into the new zbMATH interface as an additional facet.

In situations where we partially remember a formula – e.g., the energy of a signal $s(t)$ has something to do with squaring $s(t)$ and integrating over it – we would like to search for formula schemata like

$$\int_{?a}^{?b} ?s(t)^2 \mathrm{d}t,$$

where $?a$, $?b$ and $?s$ are query variables (wildcards that can be instantiated by the search engine – those are the parts we do not remember or do not care about). Similarly, if we are stuck in a proof, e.g., needing an approximation of the integral $\int_D |\sin(x)\cos(x)|\mathrm{d}x$, queries such as the one in Figure 1 could give inspiration.

Since such search requests are part of the daily work of a mathematician, infrastructure services have already started to integrate formula search engines within their facilities. The

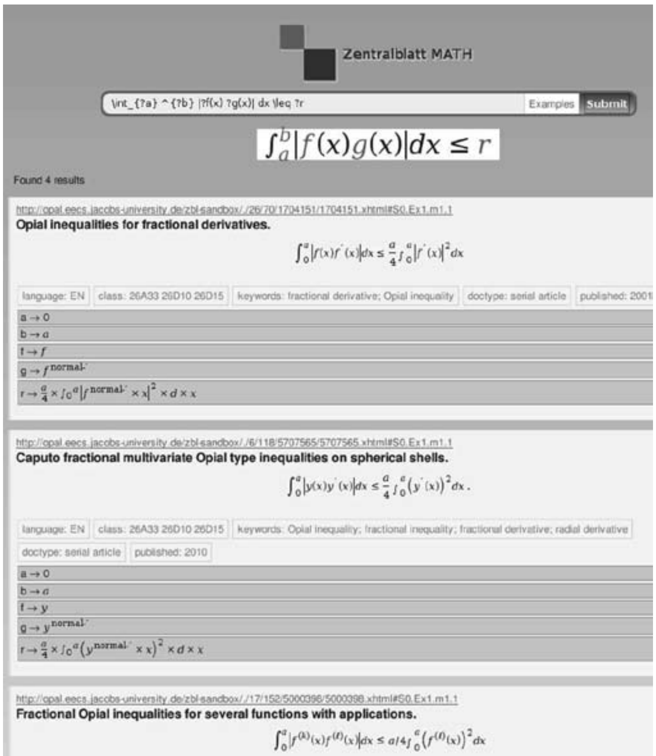


Figure 1. Searching for an applicable theorem

NIST Digital Library of Mathematical Functions (dlmf.nist.gov/), for instance, offers a formula search for their content, which is highly standardised. The European Digital Mathematics Library EuDML (eudml.org/) also offers a formula search with matching based on similarity. However, none of the existing search engines employ a thoroughly *semantic* approach, trying to encode the complete mathematical *meaning* of the entered query; instead, the matchings are displayed according to their *structural similarity*. Within the MathSearch project we are trying to combine the expertise of the mathematical knowledge management group at Jacobs University together with the broad knowledge of the zbMATH editorial board in order to build an *intelligent* search facility for mathematicians. The zbMATH database with its comprehensive and carefully edited content is certainly a very good source for such a service.

Mathematical knowledge retrieval

The problem of mathematical information retrieval and literature access has three parts: (*i*) *digitisation* (only digital documents can be searched); (*ii*) *content extraction*; and (*iii*) *search*. The MathSearch project sidesteps the first by re-

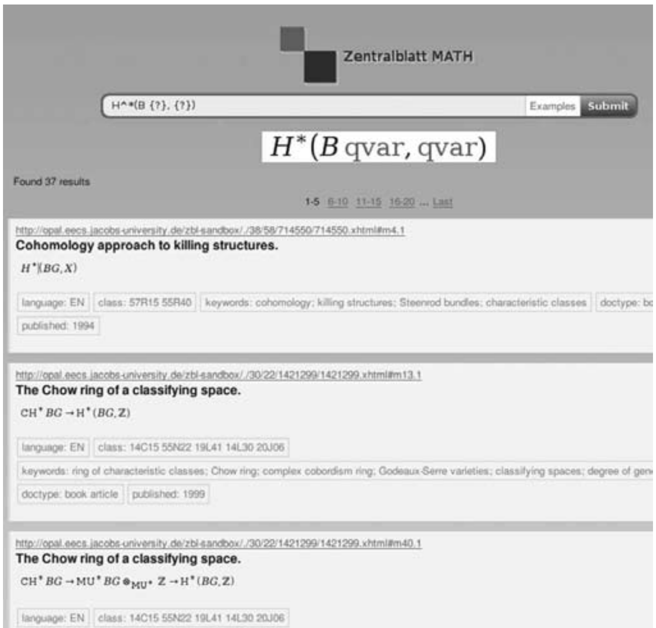


Figure 2. Searching for the ‘homology of a classifying space’ as a text string can be quite unsatisfactory, e.g., due to the various phrasings

stricting itself to born-digital (LaTeX) documents: primarily the zbMATH database and the arXiv.org corpus, which leaves two remaining problems for the MathSearch project.

For (*ii*) note that mathematical documents are written in formats (usually LaTeX) optimised for formatting (visual layout) of formulae, not their functional structure. In our first example above, we want to find Parseval’s theorem,

$$\frac{1}{T}\int_0^T s^2(x)\mathrm{d}x = \sum_{k=-\infty}^{\infty} |c_k|^2,$$

even though the bound variables have been renamed. Generally, we want to search for the functional structure of a formula, e.g., for a “binomial coefficient n choose k” modulo notation conventions like $\binom{n}{k}$ or C^n_k. Of course, this content extraction problem is highly non-trivial, since it is riddled with ambiguities which can only be resolved from context. This problem rears its ugly head even for very simple formulae: $f(a+b)$ can be the product of a scalar f with a sum $a+b$ or the application of a function f to $a+b$ (invisible operator ambiguity), $\sin x/y$ can be $\frac{\sin(x)}{y}$ or $\sin(\frac{x}{y})$ (scope ambiguity) and finally B_n can be the n-th Bernoulli or Bessel number (lexical ambiguity).

For (*iii*) we note that we need sub-linear processing algorithms for the approximately 10–100 billion formulae occurring in the mathematical literature; anything less efficient would not lead to acceptable answer times. In the MathSearch project we currently employ substitution tree indexing, a technique borrowed from automated theorem proving which has essentially constant answer times ranging from 3 to 70 ms (average = 11 ms). Unification queries seem to support most formula retrieval needs but result ranking and combination with keyword and metadata search are still open problems.

Ultimately, the development of mathematics information retrieval systems will be less a problem of devising efficient search algorithms or disambiguation strategies and more a problem of cataloguing notation conventions, understanding the use of context and identifier scoping in mathematical documents and engineering query languages that mathematicians feel comfortable expressing their information needs in. Therefore we encourage the mathematical community to use our formula search engine prototype at zbmath.org/formulae/ and to give feedback on the search service in general and the search results in particular. This, and the analysis of the queries posed by the community, will allow the MathSearch project to improve and calibrate the service.

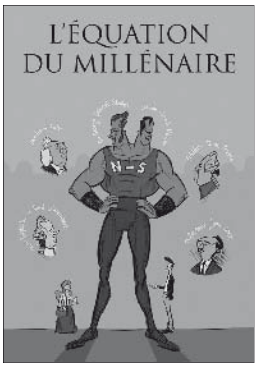
Michael Kohlhase [m.kohlhase@jacobs-university.de] is a full professor for computer science at Jacobs University Bremen and an associate adjunct professor at Carnegie Mellon University. He studied pure mathematics at the Universities of Tübingen and Bonn (1983–1989) and continued with computer science, in particular higher-order unification and automated theorem proving (PhD, 1994, Saarland University). His current research interests include knowledge representation for mathematics, inference-based techniques for natural language processing and computer-supported education.

Helena Mihaljević-Brandt was born in 1982 in Sarajevo and is currently working on the editorial board of zbMATH. She studied mathematics in Göttingen and obtained her PhD from the University of Liverpool in the field of complex dynamics.

Dr. Wolfram Sperber [wolfram.sperber@fiz-karlsruhe.de] is working on the editorial staff of Zentralblatt MATH. He has been engaged in diverse projects in the field of specialised information for many years; one of them is the swMATH project. He is involved in the development of the concept of this new service.

Olaf Teschke [teschke@zblmath.fiz-karlsruhe.de] is a member of the Editorial Board of the EMS Newsletter, responsible for the Zentralblatt Column.

Book Reviews



Camille Bouvard et al.

L'Equation du Millénaire (The Millennium Equation)

Fondation Science Mathématiques de Paris
35 Pages

Reviewer: Roberto Natalini

The Millennium Equation and the fun of mathematics

In the last few years, a number of graphic novels have dealt with physics and mathematics. Some of these books have enjoyed worldwide success, like *Logicomix* [1] and the *Feynman* biography [2]. Other interesting works only had a local circulation, and I mention here two amazing Italian works: *Gottinga* [3], a delicate watercolour book, stages the last lecture about infinity given by an old German professor; and *Enigma* [4] is about the personal and scientific life of Alan Turing. Going beyond comics, science was the main theme in the world-popular sitcom "The Big Bang Theory", where young scientists are forced to deal with real life problems, with frequent allusions to high mathematics and physics. And we should not forget the recent narrative efforts by two Fields Medallists: Cédric Villani, with his "Théorème Vivant", and Alain Connes, with "Le Théâtre Quantique", almost-non-fictional (the former) and fictional (the latter) works delivering a strong scientific message. The common motivation shared by all these initiatives is to promote science to people of all sorts (including young people) in a fun and attractive way by using simple and direct language and giving an inside look at scientific activity. This approach is usually intended not only as a device to promote allegedly boring matter disguised as something cool but as a way to accomplish a bigger goal: sharing and promoting the true spirit of science, the enthusiasm and the joy animating all real scientists around the world. The life of scientists is indeed very often full of jokes, self-irony and curiosity, while, in contrast, the usual – supposedly effective – educational approach boils down to depicting only the final form of the scientific results, with motivations and passion and fun eventually expunged. Fiction, TV comedies and comics move in the opposite direction. Moreover, it is also quite natural to rely on comic books in order to promote the popularisation of mathematics, given how one of the main features of mathematical communication has always been the use of signs and drawings. Words are usually not enough to communicate mathematical ideas and a simple drawing can often be more effective when it comes to explaining ideas. Comics take this simple idea to its natural endpoint by adding entertainment and fun.

Below, we review a comic book proposed by the "Fondation Science Mathématiques de Paris", with the title *L'Equation du Millénaire* (The Millennium Equation), by Camille Bouvard et al., under the scientific supervision of two mathematicians Claude Bardos and Jean-Yves Chemin, who, during their careers, have been deeply involved with fluid dynamics. The subject of this comic book is the system of equations which rules the dynamics of fluids. The motion of fluids has been one of the main problems in physics even during ancient times. Actually, Greek scientists were more concerned with hydrostatics and buoyancy (the Archimedes' principle) and, with the notable exception of Leonardo Da Vinci's prophetic drawings and remarks, the main problems concerning the dynamics and the evolution of fluids have been addressed by modern mathematics. The first set of equations describing fluid flows was written down

by Euler in 1757. They consisted of partial differential equations describing the conservation of mass and momentum, although the form of the pressure was not well determined, except for an incompressible fluid. However, at the same time, D'Alembert proposed his famous paradox, which consists of showing that the total of the forces on a body vanish when the body is surrounded by an inviscid, irrotational and incompressible flow, a result which is quite far from our usual experience since a body moving in a fluid is subjected to drag forces. These problems did not really matter at that time and they were just the object of some theoretical and controversial studies made by a small group of scientists. Only a century later, the solution to this and other paradoxes was proposed, somewhat independently, by Claude-Louis Navier and George Gabriel Stokes. They observed how in the Euler

equations the internal friction between different parts of the fluid is neglected, and so they proposed adding the internal stress forces to the momentum equation, which in turn adds a diffusion term to the resulting system of differential equations. In the case of incompressible flows, this system is now called the system of Navier–Stokes equations, and it is the crucial ingredient of all current simulation studies on fluid motions, from aircraft to boats, and in thousands of different applications (blood flows, river flows, car design, weather forecasting). Besides the difficulty of producing reliable simulations in some cases, the theoretical framework still remains quite open. After the seminal works of Jacques Leray in the 1930s establishing the global existence of weak solutions in two and three space dimensions, some partial and quite technical results were proved about the existence of strong solutions in three dimensions. But still, the whole picture is not at all complete and some important questions remain without a precise answer. For this reason, in 2000, the Clay Mathematics Institute included the full solution of the Navier–Stokes equations in the list of the seven Millennium Problems, which are awarded with a prize of 1 million US dollars each for a correct solution.

The graphic novel displays all these historical developments by using some clever narrative devices. We start, in present day, with Gaspard (a young poet) and Ingrid (a young mathematician), who are in the gardens of the Sanssouci palace in Potsdam, near Berlin, built by Frederick the Great, King of Prussia in the 18th century. Suddenly, they are surprised by the arrival of Frederick the Great himself, who is complaining about the great mathematician Euler, whose mathematical theory was unable to make the great fountain in the park work. And Euler himself also arrives at the same time, fiercely replying to the King. Starting from this heated and somewhat surprising discussion, the main historical developments of the mathematical theory of fluids are presented, and all the mathematicians quoted above are sooner or later introduced as characters. The cartoonists are good enough to mix the point of view of Gaspard and Ingrid with a well-balanced blend of historical facts and scientific explanations. The tone is akin to comedy and the art is lively and nice-looking. Clearly – and luckily – the presentation is somewhat free from strict historical constraints and a major role is played by the use of clever "winks", like the old Euler character, who is given some funny, popular French expressions during a sort of trial involving the validity of his equations.

In conclusion, this book stands as a serious, but funny, attempt to use new language to popularise mathematics. It is cast in a direction to be explored in the future, possibly by strengthening the collaboration between mathematicians and cartoonists

"L'Equation du Millénaire" (The Millennium Equation)
Produced by the Fondation Science Mathématiques de Paris
Script: Nicolas Rougerie and Gaël Octavia; Drawing: Camille Bouvard; Storyboard: Camille Bouvard and Léonidas Herrera; Colours: Camille Bouvard, Léonidas Herrera and Véronique Prothée. Scientific supervision: Claude Bardos and Jean-Yves Chemin.
Download the comic book (in French) at: http://www.sciencesmaths-paris.fr/fr/bd-462.htm.

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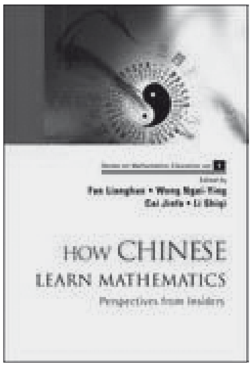
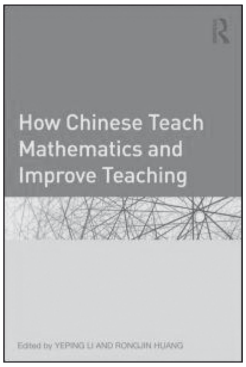
Roberto Natalini, born in 1960, got his PhD in mathematics from the University of Bordeaux (France) in 1986. He has been research director in Rome at Istituto per le Applicazioni del Calcolo "Mauro Picone" of the Italian National Research Council since 1999. He has published more than 90 scientific papers in international journals and his research themes include: theory and numerics of hyperbolic problems, fluid dynamics, road traffic, chemical damage of monuments and biomathematics. In 2012, he received a Google Research Award for the project "Multi-population Models for Vehicular Traffic and Pedestrians" and he is the scientific head of the Mathematical Desk for Italian Industry. He coordinates the website Maddmaths!, the main Italian site devoted to the diffusion of mathematics, which is supported by two Italian mathematical societies, SIMAI and UMI.



Fan L., Wong N., Cai J. & Li S.

How Chinese Learn Mathematics. Perspective from Insiders

World Scientific, Singapore, 2004
592 pages
ISBN 978-9812704146



Li Y. & Huang R.

How Chinese Teach Mathematics and Improve Teaching

Routledge, New York, 2013
264 pages
ISBN 978-0415895002

Reviewer: Mariolina Bartolini Bussi

How Chinese learn mathematics? How Chinese teach mathematics? Review of two recent books

Around the year 2000, a substantial change happened in the international perception of cultural aspects of mathematics education and research on mathematics education: the dominance of European and North American centred literature began to be undermined. Two major events happened:

- ICME 9 (2000) was held in Japan (Tokyo) offering Western researchers a direct perspective of mathematics education in the Far East.
- The ICMI Study 13 on *Mathematics Education in Different Cultural Traditions: A Comparative Study of East Asia and the West* was held in Hong Kong (2002) after two years of work by the International Programme Committee (Leung, Graf, Lopez-Real, 2006).

What produced the strongest impact in the West was the evidence offered by many international comparative studies (e.g. large-scale projects such as TIMSS and PISA) which showed high quality performances of students from the Far East, in spite of conditions of work in schools (very crowded classrooms, teacher-centred instruction, poor equipment and so on) that were perceived as negative by Western researchers and teachers.

During ICME 12 (Seoul, South Korea – see the EMS Newsletter, Issue 85) the presence and cultural influence of mathematics teachers and teacher educators from the Far East was evident in all the scientific activities, especially in the plenary panel chaired by Frederick Leung (Hong Kong) on *Math education in East Asia (Korea-China-Japan)*. It reported on the social and educational context, teacher education and development, and classroom practices of East Asian countries, which belong to the so-called Confucian Heritage Culture (CHC). In CHC, students are requested to have a strong commitment and to make a strong effort (and this is a part of the cultural context, where school is highly appreciated and considered a means of changing social status). Yet there are also curricular choices and methodologies for teaching and for teacher education that are quite different from the ones recommended in the West. Intercultural comparison is not easy at all. It is not possible to focus only on the evident differences in either the implemented or the attained curriculum to understand the deep reasons for the effective teaching. Each curriculum is constructed upon a deep structure of holistically integrating presuppositions about the nature of the human self, society, learning processes, language, concepts, human development, freedom, authority and the epistemology and ontology of mathematical knowledge (see Bartolini Bussi & Martignone, 2013).

Two books (and a forthcoming one) can help Westerners (and Europeans) understand what is happening in China as far as mathematics education is concerned and to exploit the provocation from Chinese teachers and students as a prompt to discuss some shared hidden ideologies typical of Westerners.

The two books are:

How Chinese Learn Mathematics. Perspective from Insiders by Fan L., Wong N., Cai J. & Li S., referred to below as HCL2004 or simply HCL; and

How Chinese Teach Mathematics and Improve Teaching by Li Y. & Huang R., referred to below as HCT2013 or simply HCT.

A forthcoming book: *How Chinese Teach Mathematics. Perspective from Insiders*, planned for February 2014, is the twin of the former with the same editorial team.

This review gives a brief analysis of the two published books, putting them within the context of the existing literature.

The structures of the two published books are similar.

HCL2004 contains 20 chapters (plus an introduction), divided into four sections:

1) Overview and International Perspectives (6 chapters).
2) Context and Teaching Materials (5 chapters).
3) Pedagogy and Learning Processes (7 chapters).
4) Inspiration and Future Directions (2 chapters).

HCT2013 contains 14 chapters (plus a foreword and a preface to each part), divided into five parts:
1) Introduction and Perspectives (2 chapters plus a preface).
2) Chinese Teachers' Regular Practices for Developing and Improving Classroom Instruction (3 chapters plus a preface).
3) Mathematical Instruction Practices and Classroom Environment in China (4 chapters plus a preface).

4) Selected Approaches and Practices for Improving the Quality of Teachers and their Teaching (3 chapters plus a preface).
5) Commentary.

From this list, it is clear that whilst HCT is about teaching, HCL is not only about learning. Actually, in the Chinese culture and tradition, the process of learning cannot be separated from the process of teaching. The figure

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represents the traditional Chinese character *xué* that can be translated "to study", "learning" or "school" depending on the context. The bottom part represents the learner (the sketch of a child with crossed legs) whilst the top part of the character represents the teacher's hands which chase away darkness from the learner's head. Hence learning is a part of a dialectic teaching-learning process.

Before giving some details about the contents of the two books, it is worthwhile describing other features of both.

The two books have different, large authoring teams (30 authors for HCL; 34 authors for HCT), which are, in both cases, the result of blending Chinese insiders, Chinese outsiders (i.e. those living in the West) and Westerner insiders (who are well-prepared scholars in the multicultural field). Two Chinese insiders appear in both teams (Li Jun and Ma Yungpen). The proportions of authors who are Chinese insiders, Chinese outsiders and Westerner insiders are similar in both books:

HCL: total authors (30); Chinese insiders (21); Chinese outsiders (5); Westerner insiders (4).

HCT: total authors (34); Chinese insiders (20); Chinese outsiders (7); Westerner insiders (7).

There is, however, a difference that is not only dependent on the intervening decade between the starts of the two projects. HCL claims on the cover to offer a "Perspective from insiders" and the few Westerners co-author chapters with Chinese authors, whilst HCT gives to Westerners (with only one exception) the role of commenting on the sections authored by Chinese scholars.

This choice seems to be related to the original design of each book and to the publishing programme.

HCL is the outcome of a special event of the ICME9 held in Tokyo (Japan) in 2000, i.e. the two sessions *Forum of All Chinese Math Educators*, that drew much interest from an unexpectedly large audience not only from mainland China but also from Hong Kong, Singapore, Taiwan, Russia, Japan, the United States and so on. The introduction reads: "With the momentum and encouragement we gained from the event and all the interested scholars and researchers […] we started our journey of more than three years, from the initial discussion of the main theme and structure to the organization of peer-reviewing for all the contributions, and finally to the completion of the book." In a sense, the book aimed to offer to the international readership a provocative perspective from insiders about the (then) unknown phenomena of Chinese mathematics education. All the books of the same series of *Mathematics Education*[1] address the presentation of approaches from countries (e.g. Russia, Singapore, South Korea, Japan, Thailand) that are less known in the West because of the publication language.

HCT, on the contrary, is supposed to be a Westerner enterprise, to exploit the richness of the Chinese tradition of mathematics teaching in order to improve the teaching and learning of mathematics in the West and, especially, the US. This is particularly clear if one reads either the foreword (authored by Alan Schoenfeld, one of the most famous US scholars and recipient in 2011 of the Felix Klein medal for lifetime achievement in mathematics education research) or the final commentary (*This book speaks to us*) authored by Stigler, Thompson and Li (from the US). In the foreword, Schoenfeld criticises the standard attitude of superficially importing the best practices from other countries to the US as routes to failure, as the attempt to use artefacts or practices from another culture without understanding the original cultural context cannot work. Rather, Schoenfeld emphasises two kinds of lessons to be studied and learned from success in another nation: the first is "to challenge our assumption – especially tacit assumptions"; and the second is to understand the principles of a working innovation in order to understand which alternative classroom practices may be designed anew in order to match our culture and, at the same time, to live up to the principles. This perspective, emphasised in all the commentaries to the sections, makes HCT more easily usable (i.e. less "risky") for Western researchers, who are often warned not to fall into the trap of simply copying Chinese practices. In the same series on mathematics education, there are more than fifty volumes edited by Schoenfeld, with nearly all the books from the US and only three books addressing the issue of different cultures.[2] HCL, on the contrary, addresses a mature researcher who knows the pitfalls of Eurocentrism.

The two books HCL and HCT are, in a sense, complementary to each other. It is impossible to summarise in short the many interesting contributions. I have identified some main issues to encourage the reader to look inside. Below, I have clustered the chapters of both books around five different issues.

The *rationale* for the interest of Westerners in Chinese mathematics education is summarised in both books (HCL: Chapters 1, 19 and 20; HCT: Chapter 1).

The *history of mathematics education in China* appears in both books with a major focus on Ancient China (HCL: Chapter 6) and a major focus on the 20th century, where the influence of Russian tradition played a major role (HCT: Chapter 2).

Chinese curriculum and textbooks are analysed in both books with a comparison between the mathematics curricula of major Western and Far Eastern regions (HCL: Chapter 2) and the analysis of textbooks (HCL: Chapters 8 and 10). The traditional principle of the "two basics", i.e. "basic knowledge and basic skills" is described

1 http://www.worldscientific.com/series/sme.
2 http://www.routledge.com/books/series/LEASMTLS/.

in both books (HCL: Chapter 7; HCT: Chapter 3).

Mathematics teacher education and development in China is addressed mainly by HCT (Chapter 5 on the intensive studies of textbooks as an element of teacher professional development; Chapters 4, 6, 11, 12 and 13 on different aspects of the Chinese version of "lesson study"). The Chinese version of "lesson study" is briefly mentioned also by HCL (Chapter 15).

Typical Chinese *classroom practices* are analysed in both books. The teacher's *coherence* of speech and behaviour is addressed by HCL (Chapter 4) and HCL2013 (Chapter 8). *Teaching with variation* is identified as one characteristic feature of the Chinese classroom (see below) and addressed by HCL (Chapters 12 and 13) and by HCT (Chapter 7). Different classroom practices are compared in HCL (Chapters 9, 14, 15, 16 and 17) and in HCT (Chapters 9 and 10).

The issue of *learners* is specifically addressed only by HCL according to the title: with comparative studies between US and Chinese students (mathematical thinking in Chapter 3; the cultural model of learning in Chapter 5); with analysis of the effects of different representation on mathematics learning of Chinese 4th graders (HCL: Chapter 18); and with analysis of the effects of Cram schools (i.e. schools with extra-curricular programmes that provide supplementary education in service of people with special needs) on children's mathematics learning (HCL: Chapter 11).

I am now considering three issues which are considered by both insiders and outsiders peculiar to the Chinese mathematics education and discussed in both books, although with different emphasis:

- The "two basics" principle.
- Teaching with variation.
- The Chinese lesson study.

HCT (Chapter 3) reads: "'*Two basics*' literally refers to basic mathematics knowledge and skills. Basic mathematics knowledge includes mathematical concepts, rules, formulas, axioms, theorems, and their embedded 'mathematical ideas and methods'. Basic mathematics skills include computation, data processing (including the use of calculators), simple reasoning and drawing tables and figures that follow specific procedures. Basic skills are primarily acquired through practice." (p. 29) In this spirit, memorisation and diligence are also essential for the Chinese students. This position is controversial for Western scholars, who have developed theoretical approaches where mathematics education is learner centred and where motivation and affective issues play a larger function. Yet if one goes to the comparison of beliefs of US students versus Chinese students (HCK2004: Chapter 5), one sees that memorisation, diligence, humility, morality and similar are much more important in China than in the US. This simply means that there is not a "universal" attitude towards learning and that Westerners must also avoid any ideological defence of their model as universal.

Teaching with variation (*biànshì* in Chinese) is addressed in many chapters, e.g. in Chapter 7 of HCT. It is introduced in a broad way, emphasising that in the West the idea of variation is also considered important to develop discernment and that discernment is essential in concept formation. Simple examples are given: for instance, students will be able to discern the concept of an isosceles triangle by looking at different types of triangles (e.g. scalene triangles, non-equilateral isosceles triangles, equilateral triangles and so on). This idea is not new but in China it is used in a systematic way to design curricula according to a format that is well defined and applied in most areas of mathematics. In the same chapter the theory of *biànshì* is presented, distinguishing several kinds of *biànshì*. A very simple example concerns the understanding of the concept of speed in 4th grade. This concept is analysed according to a conceptual framework, emphasising that the understanding of the concept of speed involves an understanding of the following formulas:

Speed = Distance ÷ Time
Distance = Speed x Time
Time = Distance ÷ Speed.

Then a set of 6 lessons is designed (and later tested) focusing on the following issues:

- A review of background knowledge (division as both sharing and grouping, changing time units, changing length units and so on).
- The use of division as sharing to build up the formula Speed = Distance ÷ Time (*inductive biànshì*).
- Strengthening students' numeracy skills in the formula Speed = Distance ÷ Time (*broadening biànshì*).
- Applying to different situations to distinguish the different units in the formula Speed = Distance ÷ Time (*broadening and deepening biànshì*).

And so on (HCT: p. 111).

The attention is always focused on the relationships between magnitudes rather than on numbers, hence pushing the students to approach algebraic reasoning.

The *Chinese lesson study* is presented thoroughly in HCT (Chapters 4, 6, 11, 12 and 13) as the most relevant method for in-service teacher education. The basic idea is to have not isolated classrooms but "permeable" classrooms, in the sense that teacher practices are open for inspection by other teachers. The most famous example in the West is the Japanese lesson study, although similar practices are popular in China, Korea and many other countries of eastern Asia. In short, working in a small group, teachers collaborate with one another, meeting to discuss learning goals, to plan an actual classroom lesson (called a "research lesson"), to observe how it works in practice and then to revise and report on the results so that other teachers can benefit from it. This practice is made easier in the Chinese classrooms as teachers spend the whole day at school but less than one half with students. In the remaining part they sit in the same room (the same room for all the mathematics teachers of the same grade), have time not only to mark homework but also to cooperate with colleagues in designing a lesson, to go to another classroom to observe a lesson, to analyse in small groups the observed lesson and to design a new one.

The two books reviewed in this paper open windows on mathematics education in China. All over the world mathematics educators are paying attention to the Confucian Heritage Culture, not only for the high performances of high school students in the international comparison but for other reasons: the demographic extension of the Far East area that, with India, includes more than one half of the world population; and the high correlation between more and more rising scores in mathematics education and the clear and fast development of economy. The books like the ones reviewed here cope with the issue in a culturally attentive way and offer hints to reconsider policies on mathematics education in the Western world. Hence, they should be present in the library of each education department.

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Li Y & Huang R. (eds.), 2013, *How Chinese Teach Mathematics and Improve Teaching.* New York: Routledge.

Mariolina Bartolini Bussi is a Member of the Editorial Board) of the EMS Newsletter.

Solved and Unsolved Problems

Themistocles M. Rassias (National Technical University of Athens, Greece)

I have not failed. I've just found 10,000 ways that won't work.
Thomas A. Edison

I Six new problems – solutions solicited

Solutions will appear in a subsequent issue.

115. Prove that for every $x, y > 0$, the following inequality holds

$$x2^y + y2^{-x} > x + y.$$

(*Dorin Andrica, Babeş-Bolyai University of Cluj-Napoca, Romania*)

116. Prove that for every positive integer $n \geq 3$, the following inequality holds

$$n\sqrt[n]{n} + \frac{n+1}{\sqrt[n+1]{n+1}} > 2n + 1.$$

(*Dorin Andrica, Babeş-Bolyai University of Cluj-Napoca, Romania*)

117. Let f be a real-valued function defined on an open interval I of the real line. Prove or disprove the following statements:

(a) If for every $t \in I$ we have

$$\lim_{h\longrightarrow 0}(f(t+h) - f(t-h)) = 0$$

then f is continuous on I.

(b) If for every $t \in I$ we have

$$\lim_{h\longrightarrow 0}\frac{f(t+h) - f(t-h)}{h} = 0$$

then f is constant on I.

(c) If f is continuous on I and for every $t \in I$ we have

$$\lim_{h\longrightarrow 0}\frac{f(t+h) - 2f(t) + f(t-h)}{h^2} = 0$$

then f is a linear function.

(*Richard A. Zalik, Auburn University, USA*)

118. Let $[a, b]$ be a closed bounded interval of the real line. Assume that f is a continuous function of bounded variation and that g is a strictly increasing continuous function, both defined on $[a, b]$. For $a \leq \alpha < \beta \leq b$, let $V(f, \alpha, \beta)$ denote the total variation of f on $[\alpha, \beta]$. Let $c \in [a, b]$ be arbitrary but fixed, and define $v(f, t)$ to equal $V(f, c, t)$ on $[c, b]$ and $-V(f, t, c)$ on $[a, c]$. Finally, let $q(t) := g(t) + v(f, t)$ and $h(t) := f[q^{-1}(t)]$. Prove that $h(t)$ is absolutely continuous on $[q(a), q(b)]$.

(*Richard A. Zalik, Auburn University, USA*)

119. Let $h : [0, \infty) \to \mathbb{R}$ be a continuous function and let $f : [0, \infty) \to \mathbb{R}$ be a twice differentiable function which satisfies the inequality

$$f''(x) - 5f'(x) + 6f(x) \geq h(x) \quad \text{for} \quad x \geq 0,$$

with initial conditions $f(0) = f'(0) = 0$.
Prove that

$$f(x) \geq \int_0^x \left(e^{3(x-t)} - e^{2(x-t)}\right) h(t)dt, \quad x \geq 0.$$

(*Ovidiu Furdui and Dorian Popa, Technical University of Cluj-Napoca, Romania*)

120. Let $f : [a, b] \to \mathbb{C}$ be a function of bounded variation on $[a, b]$. Show that

$$\begin{aligned}
&\left|\int_a^b f(t)\,dt - f(x)(b-a)\right| \qquad (1)\\
&\leq \int_a^x \left(\bigvee_t^x (f)\right) dt + \int_x^b \left(\bigvee_x^t (f)\right) dt\\
&\leq (x-a)\bigvee_a^x (f) + (b-x)\bigvee_x^b (f)\\
&\leq \begin{cases} \left[\frac{1}{2}(b-a) + \left|x - \frac{a+b}{2}\right|\right] \bigvee_a^b (f), \\ \left[\frac{1}{2}\bigvee_a^b (f) + \frac{1}{2}\left|\bigvee_a^x (f) - \bigvee_x^b (f)\right|\right](b-a), \end{cases}
\end{aligned}$$

for any $x \in [a, b]$, where $\bigvee_c^d (f)$ denotes the total variation of f on the interval $[c, d]$.

(*Sever S. Dragomir, Victoria University, Melbourne, Australia, and University of the Witwatersrand, Johannesburg, South Africa*)

II Two new open problems

Let $p(z) = \sum_{\nu=0}^n a_\nu z^\nu$ be a polynomial of degree n. Then a polynomial $p(z)$ is called self-inversive if it satisfies the condition $z^n\overline{p(\overline{1/z})} \equiv p(z)$ and self-reciprocal if it satisfies the condition $z^n p(1/z) \equiv p(z)$.

The self-inversive polynomials are interesting partly because of their close relationship to real trigonometric polynomials, and self-reciprocal polynomials due to the property that for any polynomial of degree n, the polynomial $z^n p(z + 1/z)$ is always a self-reciprocal polynomial of degree $2n$.

The following problems are unsolved and have been open for a long time.

121*. Let $p(z)$ be a polynomial of degree n having no zeros in $|z| < K$, where $K \leq 1$. What is the sharp upper bound for

$$\frac{\max_{|z|=1} |p'(z)|}{\max_{|z|=1} |p(z)|} ? \tag{2}$$

Remark 1. When $K = 1$, the sharp bound is $n/2$, which was conjectured by Paul Erdős and proved by P. D. Lax [Bull. Amer. Math. Soc. 50 (1944), 509-513]. The above problem, which we believe is due to R. P. Boas, Jr., was told to us by Q. I. Rahman and has been open for a long time. The problem in the case $K \geq 1$ was solved by M. A. Malik [Jour. London Math. Soc. 1(1969), 57–60] (also, see [G. V. Milovanović, D. S. Mitrinović and Th. M. Rassias, Topics in Polynomials: Extremal Problems, Inequalities, Zeros, World Scientific, Singapore, 1994; Theorem 3.1.4, p. 675] and [N. K. Govil, Q. I. Rahman and G. Schmeisser, On the derivative of a polynomial, Illinois Jour. Math. 23(1979), 319-329]).
Remark 2. One might expect the bound in (2) to be $\frac{n}{1+K^n}$ but this is far from being true, as is shown by the example

$$p(z) = \left(z - \frac{1}{2}\right)\left(z + \frac{1}{3}\right),$$

given by E. B. Saff. (*N. K. Govil, Auburn University, USA*)

122*. Let $p(z)$ be a self-reciprocal polynomial of degree n. What is the sharp upper bound for

$$\frac{\max_{|z|=1} |p'(z)|}{\max_{|z|=1} |p(z)|} ? \tag{3}$$

Remark 3. The above problem was proposed by Q. I. Rahman and has been open for more than 40 years. The problem has been solved for some subclasses of self-reciprocal polynomials (see, for example, [N. K. Govil, G. Labelle and V. K. Jain, Inequalities for polynomials satisfying $p(z) \equiv z^n p(1/z)$, Proc. Amer. Math. Soc. 57 (1976), 238–242] and [Q. I. Rahman and Q. M. Tariq, An inequality for self-reciprocal polynomials, East J. Approx. 12 (2006), 43–51]) but the problem in the general case is still open.
Remark 4. C. Frappier, Q. I. Rahman and St. Ruscheweyh (see [C. Frappier, Q. I. Rahman and St. Ruscheweyh, New inequalities for polynomials, Trans. Amer. Math. Soc. 288 (1985), 69–99; p. 97] or [Q. I. Rahman and G. Schmeisser, Analytic Theory of Polynomials, Clarendon Press, Oxford, 2002; p. 529]), by constructing a polynomial

$$p(z) := \{(1 - \mathrm{i}z)^2 + z^{n-2}(z - \mathrm{i})^2\}/4$$

of degree n which satisfies $p(z) = z^n p(1/z)$ but for which

$$\frac{\max_{|z|=1} |p'(z)|}{\max_{|z|=1} |p(z)|} \geq (n-1), \tag{4}$$

show that the bound in (3) is at least $(n - 1)$, which is quite surprising since half of the zeros of a self-reciprocal polynomial lie in $|z| \geq 1$.
Remark 5. It might be remarked that the problem of obtaining a sharp upper bound in (3) for self-inversive polynomials is solved (see [Q. I. Rahman and G. Schmeisser, Analytic Theory of Polynomials, Clarendon Press, Oxford, 2002; Theorem 14.3.1, p. 527] or [N. K. Govil, Proc. Amer. Soc. 41 (1973), 543–546, Lemma 4]) and, in fact, for self-inversive polynomials we have

$$\frac{\max_{|z|=1} |p'(z)|}{\max_{|z|=1} |p(z)|} = \frac{n}{2}. \tag{5}$$

(*N. K. Govil, Auburn University, USA*)

III Solutions

107. Find all differentiable functions $f : \mathbb{R} \to \mathbb{R}$ with continuous derivative f' such that the following properties hold:
1. $f(x + f'(x)) = f(x)$ for all $x \in \mathbb{R}$;
2. The derivative f' vanishes at a single point.

(*Dorin Andrica, Babeş-Bolyai University of Cluj-Napoca, Romania*)

Solution by the proposer. Let a be the point with $f'(a) = 0$. Consider the function $g : \mathbb{R} \to \mathbb{R}$ defined by $g(x) = x + f'(x)$. We shall prove that $g(x) = -x + 2a$ for all $x \in \mathbb{R}$, so $f(x) = -x^2 + 2ax + b, b \in \mathbb{R}$, for $x \in \mathbb{R}$.

To begin with, note that f restricts injectively to either side of a. We now show that g is an involution on $\mathbb{R}$, that is $g(g(x)) = x$ for all $x \in \mathbb{R}$. Clearly, this holds at $x = a$. For $x \neq a$, injectivity of f on either side of a shows that x and $g(x)$ always fall on opposite rays: either $x < a < g(x)$ or $g(x) < a < x$. Consequently, x and $g(g(x))$ always fall on the same ray. Since $f(x) = f(g(x)) = f(g(g(x)))$, the conclusion follows by injectivity of f on the ray containing both x and $g(g(x))$.

Next, we show that g is differentiable at any $x \neq a$, and $g'(x) = -1$ for all $x \neq a$. To this end, fix any such x and let $0 < h < |x - a|$. Then

$$\lim_{h\to 0} \frac{g(x+h) - g(x)}{h} = \lim_{h\to 0} \frac{\frac{f(x+h)-f(x)}{h}}{\frac{f(g(x+h))-f(g(x))}{g(x+h)-g(x)}} = \frac{f'(x)}{f'(g(x))} \tag{6}$$

$$= \frac{g(x) - x}{g(g(x)) - g(x)} = \frac{g(x) - x}{x - g(x)} = -1, \tag{7}$$

and we are done. Note that all quotients above make sense.

Finally, continuity at $x = a$ yields $g(x) = -x + 2a$.

Also solved by Mihály Bencze (Brasov, Romania), Soon-Mo Jung (Chochiwon, Korea), S. E. Louridas, (Athens, Greece)

108. For a positive integer n denote by a_n the number of linear functions defined by $f(x) = ax + b$, where $a, b \in \{1, 2, \cdots, n\}$, having an integer root. Prove that

$$\lim_{n\to\infty} \frac{a_n}{n \ln n} = 1.$$

(*Dorin Andrica, Babeş-Bolyai University of Cluj-Napoca, Romania*)

Solution by the proposer. The following formula holds.

$$a_n = \sum_{k=1}^{n} \left[\frac{n}{k}\right], \tag{8}$$

where $[x]$ denotes the integer part of the real number x. Indeed, write $f(x) = a(x + x_1)$, where x_1 is a positive integer. We have $b = ax_1 \in \{1, 2, \cdots, n\}$, hence considering successively $x_1 = 1, x_1 = 2, \cdots, x_1 = n$, we get $[\frac{n}{1}], [\frac{n}{2}], \cdots, [\frac{n}{n}]$ possibilities to choose the coefficient a, and the formula (1) follows.

Using the inequalities $x - 1 < [x] \leq x$ from formula (8), we obtain

$$\sum_{k=1}^{n} \frac{n}{k} - n < a_n < \sum_{k=1}^{n} \frac{n}{k}.$$

According to the well-known asymptotic result

$$\sum_{k=1}^{n} \frac{1}{k} = \ln n + \gamma + O\left(\frac{1}{n}\right),$$

we get

$$\lim_{n\to\infty} \frac{a_n}{n \ln n} = 1.$$

Also solved by Mihály Bencze (Brasov, Romania), Soon-Mo Jung (Chochiwon, Korea), P. Krasopoulos (Athens Greece)

109. Find all continuous functions $f, g : (0, 1) \to \mathbb{R}$ that satisfy the functional inequality

$$x f(x) + (1 - x) g(1 - x) \geq x f(y) + (1 - x) g(1 - y) \qquad (9)$$

for all $x, y \in (0, 1)$.

(*Prasanna K. Sahoo, Department of Mathematics, University of Louisville, USA*)

Solution by the proposer. Let $f, g : (0, 1) \to \mathbb{R}$ be continuous functions that satisfy (9). It follows that if f and g are solutions of (9) so also are $f + a_1$ and $g + a_2$ for some arbitrary constants a_1 and a_2. Replacing x by $x + \frac{1}{2}$ and y by $y + \frac{1}{2}$ in (9), we obtain

$$\left(\frac{1}{2} + x\right) f\left(\frac{1}{2} + x\right) + \left(\frac{1}{2} - x\right) g\left(\frac{1}{2} - x\right) \geq \left(\frac{1}{2} + x\right) f\left(\frac{1}{2} + y\right) + \left(\frac{1}{2} - x\right) g\left(\frac{1}{2} - y\right) \qquad (10)$$

for all $x, y \in I_{\frac{1}{2}}$, where $I_{\frac{1}{2}}$ denotes the open interval $(-\frac{1}{2}, \frac{1}{2})$. By defining

$$G(x) = \frac{1}{2}\left\{ f\left(\frac{1}{2} + x\right) + g\left(\frac{1}{2} - x\right)\right\} \qquad (11)$$

and

$$H(x) = \left\{ f\left(\frac{1}{2} + x\right) - g\left(\frac{1}{2} - x\right)\right\} \qquad (12)$$

and putting (11) and (12) into (10), we obtain

$$G(x) - G(y) \geq -x\{H(x) - H(y)\}, \qquad (13)$$

for all $x, y \in I_{\frac{1}{2}}$. It can be shown that f and g are increasing. Thus, H is also increasing on the interval $I_{\frac{1}{2}}$. Further, H and G are continuous on the interval $I_{\frac{1}{2}}$ since f and g are continuous. Interchanging x and y in (13), we have

$$G(y) - G(x) \geq -y\{H(y) - H(x)\}. \qquad (14)$$

From (13) and (14), we obtain

$$-x\{H(x) - H(y)\} \leq G(x) - G(y) \leq -y\{H(x) - H(y)\}. \qquad (15)$$

If $H(x) = H(y)$ for all $x, y \in I_{\frac{1}{2}}$ then $G(x) = G(y)$, which in fact shows that G is identically a constant. Hence f and g are constant functions by (11) and (12). Suppose now that there exist x and y such that $H(x) \neq H(y)$. We define $\Omega = \{H(x) \,|\, x \in I_{\frac{1}{2}}\}$ and $\phi : \Omega \to \mathbb{R}$ by $\phi(\omega) = G(x)$, where x is such that $H(x) = \omega$. The map ϕ is well-defined. Let

$$\omega_1 = \omega_2.$$

Hence we get from the definition,

$$H(x_1) = H(x_2),$$

which in turn implies

$$G(x_1) = G(x_2)$$

and thus

$$\phi(x_1) = \phi(x_2).$$

Hence ϕ is well-defined. Notice that Ω is an interval since it is the image of $I_{\frac{1}{2}}$ under the continuous map H. The end points of the interval Ω are $\inf_x H(x)$ and $\sup_x H(x)$. Define

$$x_*(\omega) = \inf\{x \,|\, H(x) = \omega\}, \qquad (16)$$

$$x^*(\omega) = \sup\{x \,|\, H(x) = \omega\}. \qquad (17)$$

Using (16), (17) and the definitions of ϕ in (15) we get

$$-x_*(\omega_2) \leq \frac{\phi(\omega_2) - \phi(\omega_1)}{\omega_2 - \omega_1} \leq -x^*(\omega_1) \qquad (18)$$

for $\omega_2 > \omega_1$ and $\omega_1, \omega_2 \in \Omega$.

Now we want to show that under the assumption $\omega_2 > \omega_1$,

$$\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2) = x^*(\omega_1).$$

From (18) we get

$$x_*(\omega_2) \geq x^*(\omega_1) \qquad (19)$$

for $\omega_2 > \omega_1$. Note that since H is increasing $x_*(\omega)$ is also increasing. Thus the limit $\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2)$ exists and

$$\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2) \geq x^*(\omega_1). \qquad (20)$$

On the other hand,

$$H(x_*(\omega_2)) = H\Big(\inf_x \{x \,|\, H(x) = \omega_2\}\Big) = \omega_2. \qquad (21)$$

Thus

$$\lim_{\omega_2 \to \omega_1^+} H(x_*(\omega_2)) = H(\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2)) \qquad (22)$$

since H is continuous. Putting (22) and (21) together we get

$$H(\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2)) = \lim_{\omega_2 \to \omega_1^+} \omega_2 = \omega_1. \qquad (23)$$

Consider

$$\begin{aligned} x^*(\omega_1) &= \sup\{x \,|\, H(x) = \omega_1\} \qquad (24)\\ &= \sup\Big\{x \,|\, H(x) = H(\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2))\Big\} \\ &= \sup\{x \,|\, x = \lim_{\omega_2 \to \omega_1^+} x_*(\omega_2)\}. \end{aligned}$$

Hence

$$x^*(\omega_1) \geq \lim_{\omega_2 \to \omega_1^+} x_*(\omega_2). \qquad (25)$$

Thus by (20) and (25) we get

$$\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2) = x^*(\omega_1). \qquad (26)$$

Returning back to (18), that is,

$$-x_*(\omega_2) \leq \frac{\phi(\omega_2) - \phi(\omega_1)}{\omega_2 - \omega_1} \leq -x^*(\omega_1)$$

with $\omega_2 > \omega_1$ and taking the limit $\omega_2 \to \omega_1^+$, we get

$$-\lim_{\omega_2 \to \omega_1^+} x_*(\omega_2) \leq \lim_{\omega_2 \to \omega_1^+} \frac{\phi(\omega_2) - \phi(\omega_1)}{\omega_2 - \omega_1} \leq -\lim_{\omega_2 \to \omega_1^+} x^*(\omega_1). \qquad (27)$$

By (26), (27) becomes

$$-x_*(\omega_1) \leq \lim_{\omega_2 \to \omega_1^+} \frac{\phi(\omega_2) - \phi(\omega_1)}{\omega_2 - \omega_1} \leq -x^*(\omega_1).$$

Hence, we get

$$\lim_{\omega_2 \to \omega_1^+} \frac{\phi(\omega_2) - \phi(\omega_1)}{\omega_2 - \omega_1} = -x_*(\omega_1). \qquad (28)$$

Thus

$$\begin{aligned} \phi(\omega) &= -\int_0^{\omega} x_*(t)\, dt + c \qquad (29)\\ &= -\int_0^{\omega} x^*(t)\, dt + c, \end{aligned}$$

since $x^*(\omega) = x_*(\omega)$ almost everywhere in Ω. Thus, we get

$$G(x) = \phi(H(x)) \tag{30}$$
$$= -\int_0^{H(x)} x_*(t)\,dt + c$$
$$= -\left\{x\,H(x) - \int_0^x H(t)\,dt\right\} + c.$$

The last line follows from the fact that the integral $\int_0^{H(x)} x_*(t)\,dt$ is equivalent to $\int_0^x t\,H'(t)\,dt$. Hence, we get

$$G(x) = \phi(H(x)) = -\,xH(x) + \int_0^x H(t)\,dt + c, \tag{31}$$

where c is an arbitrary constant. From (11) and (12), we see that

$$f\left(\frac{1}{2} + x\right) = G(x) + \frac{1}{2}\,H(x) \tag{32}$$

and

$$g\left(\frac{1}{2} - x\right) = G(x) - \frac{1}{2}\,H(x). \tag{33}$$

Hence, from (31), (32) and (33), we get

$$f\left(\frac{1}{2} + x\right) = \left(\frac{1}{2} - x\right)H(x) + \int_0^x H(t)\,dt + c$$

and

$$g\left(\frac{1}{2} - x\right) = -\left(\frac{1}{2} + x\right)H(x) + \int_0^x H(t)\,dt + c.$$

These can be rewritten as

$$f(z) = (1-z)H\left(z - \frac{1}{2}\right) + \int_0^{z-\frac{1}{2}} H(t)\,dt + c, \quad z \in (0,1) \tag{34}$$

and

$$g(z) = -(1-z)H\left(\frac{1}{2} - z\right) + \int_0^{-(z-\frac{1}{2})} H(t)\,dt + c, \quad z \in (0,1), \tag{35}$$

where H is an arbitrary continuous and increasing function. From (34) and (35), we obtain

$$f(z) = (1-z)\,H\left(z - \frac{1}{2}\right) + \int_0^{z-\frac{1}{2}} H(t)\,dt + c_1, \qquad z \in (0,1)$$

and

$$g(z) = (z-1)\,H\left(\frac{1}{2} - z\right) + \int_0^{\frac{1}{2}-z} H(-t)\,dt + c_2, \qquad z \in (0,1),$$

where $H : I_{\frac{1}{2}} \to \mathbb{R}$ is an arbitrary continuous and increasing function and c_1, c_2 are arbitrary constants.

Also solved by Mihály Bencze (Brasov, Romania), Soon-Mo Jung (Chochiwon, Korea)

110. Let $f : (1, +\infty) \to \mathbb{R}$ be a continuously differentiable function. It is known that the condition $\lim_{x\to\infty} f'(x) = 0$ is not sufficient to imply $\lim_{x\to\infty} f(x) = l \in \mathbb{R}$. But what about if we use another condition of the form $\lim_{x\to\infty} x^\alpha f'(x) = 0$ $(\alpha > 0)$?
Find for what values of α $(\alpha > 0)$ the following statement is true: *If* $\lim_{x\to\infty} x^\alpha f'(x) = 0$ *then* $\lim_{x\to\infty} f(x) = l \in \mathbb{R}$.

(*Panagiotis T. Krasopoulos, Athens, Greece*)

Solution by the proposer. For $\alpha \le 1$, the statement is false. Consider the function $f : (1, +\infty) \to \mathbb{R}$, $f(x) = ln(ln(x))$. We have $\lim_{x\to\infty} x^\alpha f'(x) = \lim_{x\to\infty} \frac{1}{x^{1-\alpha} ln(x)} = 0$ and $\lim_{x\to\infty} f(x) = +\infty$.
For $\alpha > 1$, the statement is true. Let an $\varepsilon > 0$ and let $\bar{\varepsilon} = \frac{\varepsilon(\alpha-1)}{2} > 0$. Then there is an $x_0 > 1$ such that for every $x \ge x_0 > 1$ we have $|x^\alpha f'(x)| < \bar{\varepsilon}$. Thus,

$$-\bar{\varepsilon} < x^\alpha f'(x) < \bar{\varepsilon} \quad \text{or} \quad -\bar{\varepsilon}x^{-\alpha} < f'(x) < \bar{\varepsilon}x^{-\alpha}$$

and by integrating from x_0 to x we get

$$-\bar{\varepsilon}\left(\frac{x^{-\alpha+1}}{-\alpha+1} - \frac{x_0^{-\alpha+1}}{-\alpha+1}\right) < f(x) - f(x_0) < \bar{\varepsilon}\left(\frac{x^{-\alpha+1}}{-\alpha+1} - \frac{x_0^{-\alpha+1}}{-\alpha+1}\right).$$

For $x \ge x_0 > 1$, we have $\frac{\bar{\varepsilon}}{\alpha-1}x^{-\alpha+1} \le \frac{\bar{\varepsilon}}{\alpha-1}x_0^{-\alpha+1} < \frac{\bar{\varepsilon}}{\alpha-1}$. Thus,

$$f(x) - f(x_0) < \bar{\varepsilon}\left(\frac{x^{-\alpha+1}}{-\alpha+1} - \frac{x_0^{-\alpha+1}}{-\alpha+1}\right) < -\bar{\varepsilon}\frac{x_0^{-\alpha+1}}{-\alpha+1} < \frac{\bar{\varepsilon}}{\alpha-1}$$

and

$$f(x) - f(x_0) > -\bar{\varepsilon}\left(\frac{x^{-\alpha+1}}{-\alpha+1} - \frac{x_0^{-\alpha+1}}{-\alpha+1}\right) > \bar{\varepsilon}\frac{x_0^{-\alpha+1}}{-\alpha+1} > -\frac{\bar{\varepsilon}}{\alpha-1}.$$

By using $\bar{\varepsilon} = \frac{\varepsilon(\alpha-1)}{2}$ we get $-\frac{\varepsilon}{2} < f(x) - f(x_0) < \frac{\varepsilon}{2}$. Now, let $x_1, x_2 \ge x_0$. We have that $-\frac{\varepsilon}{2} < f(x_1) - f(x_0) < \frac{\varepsilon}{2}$ and $-\frac{\varepsilon}{2} < -f(x_2) + f(x_0) < \frac{\varepsilon}{2}$. Adding these two inequalities gives $|f(x_1) - f(x_2)| < \varepsilon$, which means that the Cauchy criterion for convergence is satisfied. Thus there is an $l \in \mathbb{R}$ such that $\lim_{x\to\infty} f(x) = l$. This completes the proof. □

Also solved by Mihály Bencze (Brasov, Romania), Soon-Mo Jung (Chochiwon, Korea)

111. Let a, b be integers such that $2 \le a < b$. Prove that

$$\sum_{n=1}^{\infty}\left(\frac{(a-1)(b-1)\lfloor n\log_a b\rfloor}{b^n} + \frac{b-1}{a^{\lfloor n\log_a b\rfloor}} - \frac{1}{a^{\lfloor\log_a n\rfloor}b^{\lfloor\log_b n\rfloor}}\right) = 1,$$

where $\lfloor x\rfloor$ denotes the largest integer not greater than x.

(*Carlo Sanna, Università degli studi di Torino, Turin, Italy*)

Solution by the proposer. Since $a^{\lfloor\log_a n\rfloor}b^{\lfloor\log_b n\rfloor} > n^2/ab$ for any positive integer n, the series $\sum_{n=1}^{\infty}\frac{1}{a^{\lfloor\log_a n\rfloor}b^{\lfloor\log_b n\rfloor}}$ is (absolutely) convergent and we can associate its terms

$$\sum_{n=1}^{\infty}\frac{1}{a^{\lfloor\log_a n\rfloor}b^{\lfloor\log_b n\rfloor}} = \sum_{k=0}^{\infty}\sum_{n=b^k}^{b^{k+1}-1}\frac{1}{a^{\lfloor\log_a n\rfloor}b^{\lfloor\log_b n\rfloor}} = \sum_{k=0}^{\infty}\frac{1}{b^k}\sum_{n=b^k}^{b^{k+1}-1}\frac{1}{a^{\lfloor\log_a n\rfloor}}.$$

For all nonnegative integers k we have $k\log_a b - 1 < \lfloor k\log_a b\rfloor \le k\log_a b$ and $\lfloor k\log_a b\rfloor < \lfloor(k+1)\log_a b\rfloor$ because $a < b$. So $b^k \le a^{\lfloor k\log_a b\rfloor+1} - 1$, $a^{\lfloor k\log_a b\rfloor+1} \le a^{\lfloor(k+1)\log_a b\rfloor}$ and $a^{\lfloor(k+1)\log_a b\rfloor} \le b^{k+1}$. It follows that

$$\begin{aligned}\sum_{n=b^k}^{b^{k+1}-1}\frac{1}{a^{\lfloor\log_a n\rfloor}} &= \sum_{n=b^k}^{a^{\lfloor k\log_a b\rfloor+1}-1}\frac{1}{a^{\lfloor k\log_a b\rfloor}} + \sum_{j=\lfloor k\log_a b\rfloor+1}^{\lfloor(k+1)\log_a b\rfloor-1}\ \sum_{n=a^j}^{a^{j+1}-1}\frac{1}{a^j} \\ &\quad + \sum_{n=a^{\lfloor(k+1)\log_a b\rfloor}}^{b^{k+1}-1}\frac{1}{a^{\lfloor(k+1)\log_a b\rfloor}} \\ &= \frac{a^{\lfloor k\log_a b\rfloor+1} - b^k}{a^{\lfloor k\log_a b\rfloor}} + \sum_{j=\lfloor k\log_a b\rfloor+1}^{\lfloor(k+1)\log_a b\rfloor-1}(a-1) \\ &\quad + \frac{b^{k+1} - a^{\lfloor(k+1)\log_a b\rfloor}}{a^{\lfloor(k+1)\log_a b\rfloor}} \\ &= C_{k+1} - C_k,\end{aligned}$$

where $C_k := \frac{b^k}{a^{\lfloor k \log_a b \rfloor}} + (a-1)\lfloor k \log_a b \rfloor$ (note that we assume $\sum_{i=c}^{c-1} := 0$ by convention). In conclusion,

$$\begin{aligned}\sum_{n=1}^{\infty} \frac{1}{a^{\lfloor \log_a n \rfloor} b^{\lfloor \log_b n \rfloor}} &= \sum_{k=0}^{\infty} \frac{C_{k+1} - C_k}{b^k} \overset{\star}{=} \sum_{k=0}^{\infty} C_{k+1} \left(\frac{1}{b^k} - \frac{1}{b^{k+1}} \right) - C_0 \\ &= \sum_{n=1}^{\infty} \left(\frac{(a-1)(b-1)\lfloor n \log_a b \rfloor}{b^n} + \frac{b-1}{a^{\lfloor n \log_a b \rfloor}} \right) - 1,\end{aligned}$$

where ($\star$) is valid since $\lim_{k\to\infty} \frac{C_{k+1}}{b^k} = 0$. The claim follows.

Also solved by Mihály Bencze (Brasov, Romania), Soon-Mo Jung (Chochiwon, Korea), S. E. Louridas (Athens, Greece)

112. Let $\alpha > 0$ be a real number. Find the value of

$$\lim_{n\to\infty} n \int_{-1}^{0} \left(x + \frac{x^2}{2} + e^{\alpha \cdot x} \right)^n dx.$$

(*Ovidiu Furdui, Technical University of Cluj-Napoca, Romania*)

Solution by the proposer. The limit equals $1/(1+\alpha)$. Let $f : [-1, 0] \to \mathbb{R}$ be the function defined by $f(x) = x + x^2/2 + e^{\alpha \cdot x}$. It is easy to see that f is strictly increasing so it has an inverse denoted by f^{-1}. Let $\beta = e^{-\alpha} - 1/2$. We have, by using the substitution $y = x + x^2/2 + e^{\alpha \cdot x}$, that

$$\int_{-1}^{0} \left(x + \frac{x^2}{2} + e^{\alpha \cdot x} \right)^n dx = \int_{\beta}^{1} y^n (f^{-1})'(y) dy.$$

Now, we integrate by parts and we get that

$$\begin{aligned}\int_{\beta}^{1} y^n (f^{-1})'(y) dy &= \frac{y^{n+1}}{n+1} (f^{-1})'(y) \Big|_{\beta}^{1} - \frac{1}{n+1} \int_{\beta}^{1} y^{n+1} (f^{-1})''(y) dy \\ &= \frac{1}{n+1} (f^{-1})'(1) - \frac{\beta^{n+1}}{n+1} (f^{-1})'(\beta) \\ &\quad - \frac{1}{n+1} \int_{\beta}^{1} y^{n+1} (f^{-1})''(y) dy.\end{aligned}$$

It follows that

$$\begin{aligned}n \int_{-1}^{0} &\left(x + \frac{x^2}{2} + e^{\alpha \cdot x} \right)^n dx \\ &= \frac{n}{n+1} (f^{-1})'(1) - \frac{n}{n+1} \cdot \beta^{n+1} \cdot (f^{-1})'(\beta) \\ &\quad - \frac{n}{n+1} \int_{\beta}^{1} y^{n+1} (f^{-1})''(y) dy.\end{aligned}$$

On the other hand,

$$\left| \int_{\beta}^{1} y^{n+1} (f^{-1})''(y) dy \right| \le M \cdot \int_{\beta}^{1} |y|^{n+1} dy \le M \cdot \int_{-1}^{1} |y|^{n+1} dy \le \frac{2 \cdot M}{n+2},$$

where $M = \sup_{y \in [\beta, 1]} |(f^{-1})''(y)|$. It is not hard to check that $(f^{-1})''(x)$ is a continuous function. In fact, one can check that $(f^{-1})''(f(x)) = -f''(x)/(f'(x))^3$. Thus,

$$\lim_{n\to\infty} n \int_{-1}^{0} \left(x + \frac{x^2}{2} + e^{\alpha \cdot x} \right)^n dx = (f^{-1})'(1),$$

since $\beta \in (-1, 1)$. Since $(f^{-1})'(f(x)) = 1/f'(x)$ and $f(0) = 1$ we have that $(f^{-1})'(1) = 1/f'(0) = 1/(1+\alpha)$ and the problem is solved.

Also solved by Mihály Bencze (Brasov, Romania), Rodolphe Garbit (Université d'Angers, LAREMA, UMR CNRS, France), Soon-Mo Jung (Chochiwon, Korea)

Remark. Problem 102 was also solved by G. C. Greubel, Department of Physics, Old Dominion University, Norfolk, VA, USA. Problem 98* was solved by Carlos M. da Fonseca, University of Coimbra, Portugal.

We wait to receive your solutions to the proposed problems and ideas on the open problems. Send your solutions both by ordinary mail to Themistocles M. Rassias, Department of Mathematics, National Technical University of Athens, Zografou Campus, GR 15780, Athens, Greece, and by email to trassias@math.ntua.gr.
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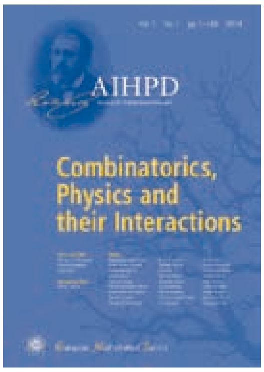

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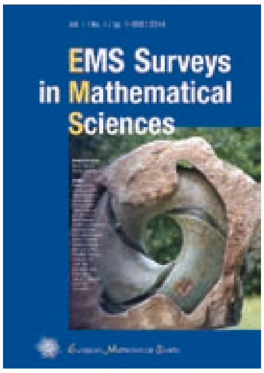

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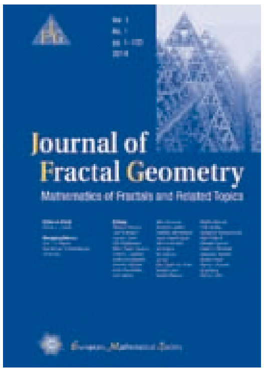

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