NEWSLETTER

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Karen Uhlenbeck (photo courtesy of Peter Badge)

Feature
Fourier, One Man, Several Lives

Interviews
Karen Uhlenbeck
Eva Miranda

Society
The Italian Society for Industrial and Applied Mathematics

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EMS Executive Committee

EMS Agenda

2019

11–13 October
EMS Executive Committee Meeting, Yerevan, Armenia

EMS Scientific Events

2019

25–28 September
Morse Theory and its Applications
Kiev, Ukraine

30 September–4 October
Mathematical Modelling in Biomedicine
Moscow, Russia

4–7 October
Analysis and PDE 2019
Hannover, Germany

7–1 October
Mathematical and Computational Aspects of Machine Learning
Pisa, Italy

7–11 October
Dirac Operators in Differential Geometry and Global Analysis
Bedlewo, Poland

21–25 October
New Trends in Asymptotic Methods for Multiscaled PDEs
Karlstad, Sweden

28–30 October
Algebraic Transformation Groups: The Mathematical Legacy of Domingo Luna
Roma, Italy

25–31 October
Workshop on Birational geometry
Moscow, Russia

25–28 November
Recent Trends in Geometric Analysis and Applications
Pisa, Italy

2020

5–11 July
8th European Congress of Mathematics
Portorož, Slovenia

Report from the Executive Committee Meeting in Berlin, 22–23 March 2019

Richard Elwes, EMS Publicity Officer

Technische Universität Berlin is an important place in the current life of the EMS: it was the site of the 2016 European Congress, and is the home institution of the new EMS President Volker Mehrmann. (This is no coincidence, as Volker was also the Chair of the ECM's local organising committee.) It was to TU Berlin that the EMS Executive Committee returned in the spring.

The meeting was generously hosted by the International Association of Applied Mathematics and Mechanics (GAMM) and the German Mathematical Society (DMV), and on Friday evening Heike Faßbender and Friedrich Götze, respective Presidents of the two societies, welcomed the assembled company to Germany, and told us about them. Founded in 1890, the DMV can boast Georg Cantor, Felix Klein, David Hilbert and Hermann Minkowski among its former Presidents. With 4500 current members (including maths students, schoolteachers, and mathematicians in industry) the DMV is a large and very active society with many international connections and regular meetings, often jointly with other countries. It awards several prizes including the biennial Cantor Medal and (jointly with the IMU) the quadrennial Carl Friedrich Gauss Prize for Applications of Mathematics.

Meanwhile GAMM (the International Association of Applied Mathematics and Mechanics) was founded in 1922 by Richard von Mises and Ludwig Prandtl, with a stated purpose of promoting "scientific research in all branches of mechanics, mathematics and physics, which are among the foundations of engineering, primarily through the organization of scientific meetings." It now boasts over 1300 members, 17 Activity Groups, 6 Student Chapters, and an annual meeting of over 1200 participants.

Officers' reports

The meeting was opened by Volker Mehrmann, acting as Chair for the first time. He presented a report on his activities since the start of his Presidency in January. He also drew the committee's attention to FAIRmat, a proposal in which the EMS has participated regarding Mathematical Data for the European Open Science Cloud, based on the FAIR principles: Findability, Accessibility, Interoperability, and Reusability. For more details see www.opendreamkit.org/2019/01/29/FAIRmat/.

EMS Treasurer Mats Gyllenberg then presented his report on the society's income and expenditure for 2018. The society's finances remain healthy, with expenditure on scientific projects in line with the allocated budget. The committee approved his report and proposal to transfer funds into the EMS portfolio.

After Secretary Sjoerd Verduyn Lunel had delivered his report, the EMS' new Vice President Betül Tanbay led a discussion on the role of the Executive Committee members within the EMS. It was agreed that handbooks should be created for all EC posts, including the President, to make explicit the expectations of the roles and to help build institutional memory.

Membership and scientific meetings

The Executive Committee was pleased to approve a list of 68 new individual members and one new institutional member: The Department of Mathematics of the Institute for Mathematics, Astrophysics and Particle Physics of Radboud University. The EC discussed those members in arrears, whose membership may be terminated at the 2020 EMS Council meeting if they fail to respond to EMS correspondence. The EMS is also slowly accumulating individual lifetime members, following the introduction in 2018 of that system.

For an update on the 8th European Congress of Mathematics in Portorož, see the report from the Presidents' meeting on page 5. Looking ahead to 2024, the EMS has received two preliminary bids for the 9th ECM, both of which the committee agreed to invite to develop into full bids to be presented to the EMS Council in 2020.

This is a busy time for EMS-sponsored events, with 14 EMS supported Summer Schools planned over 2019. There was also discussion of broadening the range of meetings that occur under the EMS banner to include recurring thematic conferences.

Standing committees and projects

Stéphane Cordier, the Chair of the Applied Mathematics Committee in attendance as a guest, presented his report. For more details of the AMC's activities including ESSAM (EMS Summer Schools in Applied Mathematics), see its webpage: https://euro-math-soc.eu/committee/applied-mathematics.

The committee then discussed reports from the committees on Developing Countries, Education, Publication and Electronic Dissemination, Raising Public Awareness of Mathematics, and Women in Mathematics.

The committee discussed a number of projects the EMS is involved in, including the European Digital Mathematics Library (http://www.eudml.org), the online Encyclopaedia of Mathematics (www.encyclopediaofmath.org), EU-MATHS-IN (the European Service Network Of Mathematics For Industry And Innovation), and plans for a future Global Digital Mathematics Library.

Publicity and publishing

The EMS Publicity Officer Richard Elwes presented his report, on publicity both on- and offline. The EMS's social media presence continues to grow, with over 4000 followers on Twitter and approaching 3000 on Facebook.

The President then presented a report on the future of the EMS Publishing House and its move to Berlin, with the successful establishment of a new limited company under German Law owned by the EMS. The committee welcomed the progress that has been made.

The EC discussed other matters relating to publications, including a report from the Editor-in-Chief of the EMS Newsletter Valentin Zagrebnov, the EMS's quarterly e-news (www.euro-math-soc.eu/e-news), and Zentralblatt (www.zbmath.org).

Relations with funding organisations and political bodies

The President reported on recent developments around Horizon 2020 and its successor framework, Horizon Europe. He reiterated the importance of our community speaking with one voice to enhance the prominence of mathematics in the political sphere. The President then gave an update on European Research Council, particularly a letter to encourage the mathematics community to submit proposals (this can be read in the latest e-news: www.euro-math-soc.eu/news/19/06/12/ems-e-news-31-june-2019).

The next ESOF (European Open Science Forum) will be in 2020 in Trieste (overlapping with, and close to, the next ECM). It is expected that the committee for Raising Public Awareness of Mathematics will deliver a session there.

The President led a discussion on "Plan S", the initiative for open-access science publishing proposed by Science Europe. The EC expressed its thanks for the actions of the committee for Publishing and Electronic Dissemination, particularly the EMS's response to the open consultation on Plan S. This can be read at www.euro-math-soc.eu/news/19/02/8/feedback-ems-implementation-plan-s.

Relations with mathematical organisations

The President provided an update on the International Mathematical Union (of which the EMS is an adhering organisation) under the new Presidency of Carlos Kenig. The committee agreed to appoint Vice President Betül Tanbay as liaison officer with the IMU, with the aim of improving communication between the two bodies. There was a lively discussion about the appropriate roles of the ICM and ECM in the mathematical calendar. This is expected to continue at future meetings.

The committee discussed the EMS's joint work with other mathematical organisations, including ICIAM (the International Council for Industrial and Applied Mathematics), ECMI (European Consortium for Mathematics in Industry), the Bernoulli Society, CIMPA (the International Centre of Pure and Applied Mathematics), TICMI (Tbilisi International Centre of Mathematics and Informatics), the Banach Centre, the Abel Prize, and the Gordin Prize.

Conclusion

The committee expressed its thanks to the local organisers at TU Berlin and to Presidents Heike Faßbender (GAMM) and Friedrich Götze (DMV) for the excellent hospitality and organisation we have become used to in Berlin. However, in contrast to previous EC meetings, the assembled company did not then disperse, as the annual Meeting of Presidents of EMS Member Societies followed immediately (see below).

The next Executive Committee meeting will be 11–13 October in Yerevan (Armenia).

Report from the Meeting of Presidents of EMS Member Societies in Berlin, 23–24 March 2019

Richard Elwes, EMS Publicity Officer

This is a slightly abbreviated report, as several of the topics discussed were also covered in the immediately preceding Executive Committee meeting – see above.

The meeting of Presidents of EMS member societies has been an important annual tradition since 2008. This year, in a break from previous practice, it was held immediately following an Executive Committee meeting. This brought two advantages: a reduction in the society's travel, and an increase in the number of Executive Committee members and guests present. So it was that 38 representatives of European Mathematical Societies gathered in Berlin in March, along with 12 EMS officers and committee-representatives.

As with the Executive Committee meeting, it was held at Technische Universität Berlin, on the kind invitation of GAMM and the DMV, whose respective Presidents Heike Faßbender and Friedrich Götze offered warm words of welcome to the assembled company.

EMS round-up

After a tour-de-table in which everyone introduced themselves and their society, EMS President Volker Mehrmann opened the meeting, for his first time as Chair. He emphasised the theme of mutual respect for his presidency: respect between different mathematical and geographical areas, and between mathematicians of different ages, nationalities, genders, races, sexualities, and identities. He then updated the group on his recent activities, and on developments within the EMS and in the broader political and scientific context. (See EC report on page 3.) He invited member societies to make nominations for EMS Prizes, and to submit proposals for scientific events such as Joint Mathematical Weekends.

Presentations

Over the two days, the meeting enjoyed several presentations. Elena Resmerita, Deputy Convenor of the society European Women in Mathematics (EWM) discussed her society's work. Since 1986, the EWM's aims have been to encourage women to pursue mathematics at every level, to support women in their mathematical careers, to shape research and university policies, as well as providing a meeting place for like-minded people, promoting scientific communication, and cooperating with other groups with similar goals. The EWM has long had a close relationship with the EMS through its standing committee on Women in Mathematics, but since 2018 it has been an EMS member society, providing a new opportunity for collaboration. In particular, common goals include getting more female students into STEM, stopping the so-called "leaky pipeline" in women's career progression, improving the gender balance on committees, editorial boards, among invited speakers, etc., but without increasing the burden on the most active women, and increasing respect for the achievements of women with interrupted careers. Elena also made several more concrete proposals for ways in which the two societies can work together.

Next, Klavdija Kutnar provided an update on preparations for the 8th European Congress of Mathematics in Portorož (Slovenia), 5–11 July 2020. In particular, she announced that the Scientific Committee chaired by Maria Esteban had finalised the list of Plenary and Invited speakers, which can be viewed at http://8ecm.si/program, and also that Vaughan Jones (Fields Medal, 1990) will deliver a public lecture. She also announced that the meeting will be held under the honorary patronage of His Excellency Mr. Borut Pahor, President of the Republic of Slovenia.

Lucian Beznea presented a report on the 9th Congress of Romanian Mathematicians, held 28–3 July 2019, in Galati (Romania), which hosted over 350 participants from more than 25 countries.

Hrvoje Kraljević, President of the Croatian Mathematical Society, delivered a presentation on its work. Its history dates to 1885 with the founding of the Croatian Society of Natural Sciences, and it now boasts around 700 individual members (of whom over 100 work in universities), along with 7 institutional members. It has four sections: scientific, educational, engineering, and professional, each of which is active in organising meetings and special events.

Next, Ivan Fesenko provided an update on preparations for the next International Congress of Mathematicians to be held in 2022 in St Petersburg, noting that all registered ICM participants are promised visa-free entry into Russia.

Ivan Fesenko then delivered a second report "On advancement of young mathematicians' research and a proposal for a Mathematical Research Institute of new type", regarding a plan for a new top-tier international institute with branches in several countries.

Tomás Chacón, representing the Sociedad Española de Matemática Aplicada (SEMA), presented a report on preparations for the International Congress on Industrial and Applied Mathematics (ICIAM), 15–19 July 2019, in Valencia (Spain).

Stéphane Seuret, President of the Société Mathématique de France (SMF), presented a report on his society, drawing attention to the major expansion underway at CIRM (Centre International de Rencontres Mathématiques) in Marseilles. (Delegates will be able to enjoy the improvements next year, as it is the venue for next year's Presidents' Meeting.) He also announced the completion of the SMF's digital transformation, including its new website www.smf.emath.fr/.

Finally, Thierry Horsin, President of the French Société de Mathématiques Appliquées et Industri-

elles (SMAI), reported on his society. SMAI's activities include political engagement on education (jointly with SMF) and human rights, the creation of the Jean Jacques Moreau Prize in Optimization (jointly with the SMF), an annual mathematics careers fair, plus a range of publications, meetings, and mathematical outreach.

Discussion and close

At every Presidents' meeting, a generous amount of time is set aside for free discussion. Of course, it is not possible to summarise these sessions in detail. On this occasion, this focussed initially on improvement of gender balance within the EMS, with several contributions regarding what can be done at a national level and what at a European level, and how can we learn from each other's successes. Several concrete proposals were made, regarding how the EMS can serve as a platform for the exchange of best practices. The Chair urged the Presidents of the National Societies to become more active and more diverse in their nominations for prizes and speakers, and to consider not only candidates from their own country. This is the way to enrich our lists of nominees.

The Chair continued the discussion by asking the presidents for ideas of how to involve more young mathematicians in our societies, noting that GAMM has a successful programme of "GAMM Juniors".

The Chair closed the discussion with an appeal to work together and speak with one voice. It is the responsibility of national societies to lobby at the national level, and of the EMS to unify these initiatives to create a strong and clear European voice to stress the importance of mathematics.

On behalf of all the participants, the Chair thanked GAMM and the DMV, and their Presidents Heike Faßbender and Friedrich Götze, for the generosity and smooth running of the two meetings at the Technical University of Berlin. The next Presidents' meeting will be held at CIRM in Luminy (France) in 2020.

Andrei Okounkov to Deliver a Public Lecture at 8ECM

Tomaž Pisanski (University of Primorska, Koper, Slovenia)

We are happy to announce that Fields medalist Andrei Okounkov will deliver a public lecture at the 8th European Congress of Mathematics!

Andrei Okounkov is a Russian mathematician who works in mathematical physics and neighbouring areas of representation theory and algebraic geometry. Enumerative geometry lies at the crossroads of all of these fields of mathematics, and a lot of Okounkov's recent work focuses on K-theoretic generalisations of classical questions in enumerative geometry. In particular, a K-theoretic generalisation of the Donaldson–Thomas-style counting of curves in algebraic threefolds is an exciting area at the forefront of current research with a conjectural relation to counting membranes of M-theory put forward by Nekrasov and Okounkov, and a geometric representation theory description of its basic building blocks obtained by Okounkov and A.Smirnov. Earlier conjectures of Maulik–Nekrasov–Okounkov–Pandharipande, connecting cohomological DT counts with Gromov–Witten theory of algebraic threefolds, in many ways shaped the developments in both fields. The proof of the MNOP conjectures for toric varieties by Maulik-Oblomkov-Okounkov-Pandharipande and the work that followed extends, among other things, the representation theoretic understanding of the Gromov-Witten theory of curves (and also of the point) obtained in the early 2000s by Okounkov and Pandharipande.

In 2004, Okounkov was awarded an EMS prize for work that "contributed greatly to the field of asymptotic combinatorics." In 2006, at the 25th International Congress of Mathematicians in Madrid, Spain, he received the Fields medal "for his contributions to bridging probability, representation theory and algebraic geometry."

Andrei Okounkov is a professor at the Columbia University in the city of New York and at the Skolkovo Institute of Science and Technology in Moscow. He also serves as the academic supervisor of HSE International Laboratory of Representation Theory and Mathematical Physics. His previous positions include the University of Chicago, University of California at Berkeley, and Princeton University.

We look forward to welcoming Professor Okounkov at the 8th European Congress of Mathematics in Portorož, Slovenia!

Further details will be posted on our website, please follow us on 8ecm.si for more news.

The photo and CV of Tomaž Pisanski can be found in previous Newsletter issues.

The EMS Publishing House – Farewell to Thomas Hintermann

Valentin Zagrebnov (Editor-in-Chief EMS Newsletter)

This issue is the last one published under the aegis of the founder and Managing Director of the EMS Publishing House, Thomas Hintermann, who retired at the end of August.

The idea of founding a publishing house associated with the EMS had first been discussed in 1999. By 2002, Thomas had successfully created a publishing enterprise which operated from Zürich and which was to grow over the years into an internationally renowned and well-running undertaking.

During its many successful years, Thomas systematically followed the editorial philosophy he had declared upon the publishing house's foundation in 2001 and which he summarised on the occasion of its ten-year jubilee in 2012 in the Newsletter (issue 83, page 5):

"The collection and presentation of scientific results belong in the hands of people who can do it effectively […] However, commercialism should not determine or influence the way this process is being conducted. The EMS Publishing House, together with a number of other publishers with a similar philosophy, is here to ensure that this is not going to happen."

We are confident that these principles, coupled with new impulses, will be carried forward by the new EMS Publishing House administration in Berlin under the leadership of André Gaul and Vera Spillner.

We would like to express our deep gratitude to Thomas for all the work he has carried out during these years managing the EMS Publishing House in Zürich. We are thankful to him for his help and assistance: always competent, constructive and efficient. We shall strive to keep this friendly and productive atmosphere alive in the EMS Newsletter, and wish Thomas all the best for his future!

The EMS Publishing House – Welcome to the New Team

Volker Mehrmann (President of the EMS)

After many years of publishing, the EMS can be proud to say that the philosophy declared by the founder of the Publishing House, Thomas Hintermann (see above), has been very successfully followed. With Thomas' retirement approaching, the decision was taken to move the EMS publishing business to Berlin and invite a new leadership team to carry it into the future. André Gaul, mathematician and founder of the startup PaperHive, which brings researchers together to discuss publications, is the new CEO and Managing Director of the EMS Publishing House. Vera Spillner, theoretical physicist and experienced editor, will be the Editorial Director and contact for our authors and editors.

We are looking forward to leading the valuable content of the EMS Publishing House into the future of publishing and to create new publishing opportunities for the mathematical community", says André Gaul. Open Access will be a major focus of the EMS Publishing House going forward. The leadership team is working in close cooperation with Volker Mehrmann, thus actively participating in current Open Access developments such as the ICIAM Open Access Manifest. "We are very interested to hear from our authors and the community how they want mathematical content to be published in the future", says Vera Spillner, "everyone is invited to get in touch with us and share their ideas". Thomas Hintermann, who has worked closely with the new leadership team over the summer, adds: "I am happy to see the European Mathematical Society Publishing House and our authors getting this chance to grow further into the world of modern publishing, and I wish the new leadership team and the mathematical community the very best of success."

We are looking forward to the future developments of our EMS Publishing House and I would like send best regards and good wishes to its founder as well as the new leadership team.

Fourier, One Man, Several Lives

Bernard Maurey (Sorbonne Université, Paris, France)

Fourier was born 250 years ago, twenty-one years before the French Revolution in 1789. The events of those troubled times turned his life into an adventure novel: the Revolution with its mortal dangers; Bonaparte's expedition to Egypt with its discoveries; later a political career as prefect of Isère at Grenoble, where Fourier wrote the first versions of the *Théorie analytique de la chaleur*, when he was not busy with the construction of the road from Grenoble to Turin or the drainage of marshland at Bourgoin; and finally, his academic role at the very heart of the Parisian scientific community during the years 1820–1830. While relating a variety of aspects which are not all of scientific concern, we shall, of course, dedicate an important space to the theory of heat, Fourier's major work, as well as to the Fourier series, which are a crucial element of his mathematics.

Some books about Fourier

Numerous authors have written about Fourier, especially from the second half of the 20th century onwards, when several new works were published. Jean Dhombres and Jean-Bernard Robert have done a colossal work [D–R] , which I did not hesitate to exploit, although often at the price of regrettable simplifications. A new work, under the direction of Dhombres, is to appear this year [Fo-R]; it aims at a broader public, contains a great number of illustrations and as yet unpublished archive material. Ivor Grattan-Guinness was the first to publish, in his 1972 book [GraF], the content of the essay *Sur la propagation de la chaleur* presented by Fourier at the *Académie des Sciences* in December 1807. Umberto Bottazzini [Bott] dedicated two sections[1] to the study of the heat problem in the years 1800–1830. Another book I am going to refer to is the one by Jean-Pierre Kahane and Pierre Gilles Lemarié-Rieusset [K–L], the first part of which, written by Kahane, presents a history of Fourier series. The treatise on harmonic analysis by Thomas Körner [Körn] masterly sets out the mathematics attached to the name of Fourier. It also contains two small chapters on Fourier's life[2], which are based mainly on the very informative book by John Herivel [Heri].

1 The Revolution, the Egypt campaign

Jean-Baptiste Joseph Fourier was born on 21 March 1768 in Auxerre. The baptism certificate bears *Jean-Joseph* as his first name. He was born into a family of craftsmen in social ascension: his father, a tailor, had about ten employees. At the age of ten, Jean-Joseph was orphaned and the clergy of Auxerre took care of him. There was a remote – and uncertain – family relationship with a beatified priest, so the abandonment of the boy was out of the question. He received a good education at the *École Royale Militaire d'Auxerre* , which was run by monks. After finishing school, at the age of 19, he applied for admittance to the entrance examination to the artillery, which was curtly refused him. "Not being noble" , it was impossible for Fourier to become an artillery officer. So he turned to the religious orders and became a novice at the Benedictine Fleury Abbey in Saint-Benoît-sur-Loire. He lived there for two years, from 1787 to 1789, and could have become *Father Fourier*, but the French Revolution broke out and the constituent *Assemblée nationale* issued decrees suspending the pronunciation of religious vows just before Fourier would pronounce his own in November 1789. Early in 1790, he returned to his former school in Auxerre, this time as a teacher. It was called "Collège National–École Militaire" at that time. He stayed there for four years, taught different subjects such as history, philosophy, eloquence and also mathematics, and became a "civil servant teacher".

Initially reserved towards the French Revolution, Fourier engaged in the *Comité de surveillance* of Auxerre at the beginning of '93 and even became its president in June '94. He witnessed violent scenes of desecration of churches during the wave of dechristianising of 93–94, although we do not know what his feelings about it were. From September '93 onwards, the Comité d'Auxerre found itself in charge of executing the decisions of Maximilien de Robespierre and the *Comité de Salut Public*. Fourier, being rather moderate, might have been jeopardised by his lack of zeal in supporting the head cutters. Victor Cousin, his successor at the *Académie française* in 1831, reported in the *Notes additionnelles à l'éloge de M. Fourier* – years after the events – that Fourier had deliberately spoiled the arrest, in the town of Tonnerre, of a man sentenced to the scaffold.[3] Nevertheless, Fourier signed a certain number of arrest warrants in the context of his competence in the *Comité d'Auxerre*. One event was to have led to his imprisonment: the "affaire d'Orléans", which is reported in great detail by Herivel.[4]

Early in October '93, Ichon, a member of the *Convention*, was despatched to collect weapons, equipment and horses in the Yonne and six surrounding *départements*, preparatory to certain operations in the Vendée. With this aim, he named six citizens of Auxerre – among them Fourier – for a one-month mission in Orléans from mid-October onwards. The city was troubled by the conflicts between sans-culottes and bourgeois. Laplanche, also a member of the *Convention*, had been sent there from Paris at the beginning of September. He first took revolutionary measures supposed to satisfy the sans-culottes, but then he did not resist the pressure from the richer classes and clashed with the leaders of the sans-culottes. Fourier opposed himself to Laplanche and, clearly exceeding the scope of his mission, supported the "left wing", as we would call it today. As a result, Laplanche and the authorities of Orléans requested Fourier's recall to Auxerre and denounced his be-

1 [Bott, 2.3, 2.4]

2 [Körn, part VI, ch. 92 and 93]

3 [D–R, ch. III, p. 94]

4 [Heri, 2.2]

haviour. Their complaint was transmitted to Paris. A decree from Paris relieved Fourier of all his duties on 29 October '93: "The Commission conferred [...] to the citizen Fournier [*sic*] is revoked; he is no longer authorised to receive such Commissions", and he was not permitted to carry out any more public functions. Ichon, who was responsible for Fourier's dispatch to Orléans, felt part of the blame reflecting on him; in his fury, he issued an arrest warrant against Fourier, who fortunately had not returned to town yet. As things had calmed down a bit by his return to Auxerre, he was left in peace. Meanwhile, the Orléans affair ended up reappearing in Paris. With Robespierre fighting to his left as well as to his right, the agitators from Orléans were targeted, and so was Fourier. On 19 June '94, the *Comité de Sûreté Générale* ordered his arrest (it was the very June the "Grande Terreur" law was adopted). We know today [Fo-R] that Fourier was not imprisoned, he benefitted from privileged treatment and was put under house arrest on 4 July at his home in Auxerre. Robespierre fell at the end of July and Fourier was "freed" on 11 August.

At the end of '94, Fourier was selected as one of the young teachers to be trained at the newly established *École Normale*, the "École Normale de l'an III". The institution lasted only one semester, from January to June '95. Fourier was a distinguished student, but because of political changes, his former participation in the Jacobin committee got him into trouble. In times of the Thermidorian Reaction, a hunt for "terrorists" was taking place. The new authorities in Auxerre wanted Fourier to be expelled from the *École Normale*; they reproached him with his past in an address to the *Convention Nationale*:[5]

> We, the Representatives, shudder when we consider that the pupils of the *Écoles Normales* have been chosen under the rule of Robespierre and by his protégés; it is only too true that Balme and Fourrier [*sic*], pupils from the Yonne department, have for a long time uttered the appalling principles and the infernal maxims of the tyrants.

At the beginning of June '95, Fourier was imprisoned. After a few days he obtained a conditional release order, but the order was not followed and he stayed in prison for a month or more. At the end of August he was freed, his judicial troubles finally settled and all his civil rights restored.

The first years of the Revolution were certainly dangerous, though undoubtedly exciting, too. Kahane[6] cites a passage from a letter Fourier wrote:

> As the natural ideas of equality were developing it became possible to conceive the sublime hope to establish among us a free government without kings nor priests and to take this double yoke away from the European soil that had been usurped for so long.

And yet it was thanks to the education he received from the Benedictines at the *École Royale Militaire* of Auxerre that he was able to write beautiful sentences like the one we just cited, and the institution made a teacher out of him. The above extract is taken from a long letter written in the summer of '95 to Edmé-Pierre-Alexandre Villetard, deputy of the Yonne (reproduced by Dhombres and Robert[7]), under the circumstances mentioned above, when Fourier tried to justify his behaviour in the years '93–'94, his integrity being questioned.

As an outstanding student of the École Normale, he attracted the attention of Gaspard Monge, he attended lectures of Pierre-Simon Laplace and of the eminent Joseph-Louis Lagrange, "the first among Europe's scholars", as Fourier wrote in his *Notes sur l'École Normale*.[8] Laplace, an acknowledged scientist under the Ancien Régime, had to seclude himself during the Terreur; in '95, he reappeared on the scene and quickly became very influential. Fourier mentioned in his *Notes* that he also attended lectures on physics by René-Just Haüy, on chemistry by Claude-Louis Berthollet as well as the lectures by the – very old – naturalist Louis Jean-Marie d'Aubenton (I am citing only the most well known). When the *École Normale* was closed, Fourier became a teacher at the *École Polytechnique* (which we are going to refer to simply as "the École" in the following; before September '95 it was called *École Centrale des Travaux Publics*). Recommended by Monge, he became *substitut* at the *École* at the end of May '95 – his mission consisted of supervising the students' works –, then assistant teacher in October '95. For over two years, he deeply committed himself to his duties as a mathematics teacher.

One can get an impression of the lectures held by Fourier from 1796 to 1797 from the notes taken by students,[9] which are kept at the *Institut de France* and at the *École des Ponts et Chaussées*.[10] These lectures were not based on the manuals from the 18th century (like the treatise by Étienne Bézout), but rather inspired by Lagrange and Laplace's lectures given at the *École Normale*; also the geometric spirit of Monge is discernible. From November '95 onwards, Fourier was in charge of the lectures on *Algebraic Analysis*, which prepared for the lectures on differential calculus. In January '96 he took over part of Prony's Analysis lectures, including the calculus of variations. In March '96, he showed the students the existence of complex roots of polynomials by means of the method presented by Laplace at the *École Normale* [Éc-N]. Laplace's proof applies to the case of real coefficients; it puts the degree n of the polynomial into the form $n = 2^i s$ where s is odd, and proceeds by a subtle recurrence on i, the case $i = 0$ being determined by the property of the intermediate values – taken as evident. Fourier simplified and generalised a bit: if we suppose that the polynomials with complex coefficients of odd degree have a complex root, we can factor them into complex factors of first degree. In May '96, he treated differential and integral calculus. In '97, he succeeded Lagrange in the chair of Analysis and Mechanics. He could have occupied it for many years, like several others did. However, political events were to divert the course of his existence.

The expedition to Egypt was a pivotal episode in Fourier's life. Early in '98, the authorities of the *Directoire exécutif* enjoined him to take part in an operation that was surrounded by secrets: by then, only few of its members knew the exact destination. At the end of March '98, Fourier left Paris, as did some

5 [D–R, ch. IV, p. 150]
6 [K–L, ch. 1, p. 8]
7 [D–R, Annexe IV, p. 709]
8 [D–R, Annexe II]
9 [D–R, ch. IV, p. 158]
10 [GraF, ch. 1, p. 6–7]

forty current and former students of the *École*, out of the graduates '94 (the first year) to '97, whose teacher Fourier might have been. Among them, Jean-Baptiste Prosper Jollois and Édouard de Villiers du Terrage ("Devilliers" at the *École*, a tribute to the Revolution), engineer and future engineer of the *Ponts et Chaussées* corps, aged 22 and 18. They would write many pages of the monumental work *Description de l'Égypte*, which, published from 1809 on, would record the discoveries of the expedition through the texts and illustrations from numerous contributors; Fourier would contribute with a long preface. Among the illustrators are Vivant Denon and Henri-Joseph Redouté (painter, brother of Pierre-Joseph Redouté, who is known for his watercolour paintings of roses). Scientists and engineers like Monge, Berthollet, Étienne Geoffroy Saint-Hilaire, Nicolas-Jacques Conté and Pierre-Simon Girard also took part in the journey. Back in France, Girard, chief engineer of the *Ponts et Chaussées* corps, would direct the construction of the Ourcq canal; under his orders in 1809: the young Augustin Louis Cauchy, 20 years old, aspiring engineer at *Ponts et Chaussées*.

Fourier boarded in Toulon in mid-May. An expeditionary corps of over 30,000 men set off from France and Italy. The Egypt campaign was not easy: among many other victims, 7 of 42[11] young polytechnicians would never come back. The expedition landed at Alexandria, early in July. At the beginning of August, in Rosetta (the place where the famous *stone* was found in July '99, at about 50 km from Alexandria), Fourier became responsible for the *Courrier de l'Égypte*, a newspaper with the mission to promote the engagement of the general-in-chief Napoléon Bonaparte. At the end of August '98, he was named permanent secretary of the *Institut d'Égypte* created in Cairo by Bonaparte. He played an administrative as well as a political role, especially when it came to negotiations with the local authorities. Dhombres and Robert point out that when Bonaparte engaged in Syria (February-June '99), Fourier found himself as the de facto governor of Lower Egypt, without officially holding the title. When Bonaparte (and Monge) returned to France in August '99, he remained the principal civil authority, in particular after the death of General Jean-Baptiste Kléber who had been assassinated in Cairo in June 1800 and whose eulogy had been given by Fourier (he knew how to write speeches and was a good orator). He ensured the link between the civilians and the servicemen of the expedition. He negotiated again when the adventure came to an end when General Menou surrendered in September 1801, this time with the English who held the Egyptian harbours, in order to obtain for the French scientists the right to leave under the best possible conditions, keeping the essential parts of their notes and discoveries. Nevertheless, the Rosetta stone would be sent to England, where it is still kept today.

Fourier's activity in Egypt was not limited to administration and politics. In October '98, he acted as examiner of the *École Polytechnique*: together with Monge, he questioned students who graduated in '96 and came to Egypt. He participated in scientific and archeological expeditions, namely in Upper Egypt in September-October '99. He led mathematical research, presented several communications at the *Institut d'Égypte* on algebraic subjects, rather minor works which were not published, and also a *Mémoire sur l'analyse indéterminée*, judged more convincing by Dhombres–Robert [D–R] and Grattan-Guinness [GraF] , who understand it as a forerunner of what we call *linear programming*. Fourier would pick up this question again, much later, in communications at the *Académie des Sciences* in 1823 – in order to simplify, we name *Académie des Sciences* the institution which has also been called *Académie Royale des Sciences* or *Classe des Sciences de l'Institut* – as well as in an article from 1826 in the *Bulletin des Sciences, par la Société philomathique*.

11 [Mass, annexe]

2 Grenoble, Paris, the work

On his return from Egypt, Fourier landed in Toulon in November 1801 and returned to Paris in early January 1802, where he briefly went back to the *École Polytechnique*. However, Napoleon then sent him to Grenoble as prefect of the Isère department in 1802 after the death of the previous prefect, Gabriel Ricard. Fourier accepted the position and arrived in mid-April. It is possible that this was partially an aggravation, but there was also a need to fill the role with a capable and dependable person: qualities that Fourier demonstrated in Egypt. In Grenoble, he began work on the Theory of Heat and in 1805, he wrote an unpublished essay that was a sort of first draft of the theory. At the end of 1807, he presented a first essay on the propagation of heat to the *Académie*. The four "examiners" recorded in the minutes of the meeting on 21 December were Lagrange, Laplace, Monge and Sylvestre Lacroix. The text was not well received by Lagrange,[12, 13] and had a slightly better reception with Laplace who, in a memoir of 1809–1810 [Lapl], attributed to Fourier the discovery of the heat equation.

Fourier's 1807 essay, still unpublished, was published and commented on by Grattan-Guinness in 1972 [GraF]. It was kept at the *École nationale des Ponts et Chaussées*, where Claude Louis Marie Henri Navier, a friend of Fourier's, was a professor. Navier was the "executor" of Fourier's manuscripts. Gaston Darboux, the editor of Fourier's *Œuvres* (Works – published in 1888 and 1890), discovered the essay at the end of the 1880s, but did not make use of it. Attached to the "Essay" were documents sent by Fourier to the *Académie* in 1808 and 1809; these showed that he had been made aware of the objections of the examiners and that he had responded. Included in these documents were an *Extrait* submitted in 1809 (only the first ten pages have been preserved) that is a short non-mathematic presentation of the essay's content, and a ten-page collection of *Notes* responding to the objections.[14]

The *Académie* remained silent on the work presented by Fourier in 1807. A rather cold summary by Siméon Denis Poisson, published in the *Bulletin des Sciences* in March 1808, mentioned the heat equation, but not the processing by means of "Fourier series". In 1809, Fourier finished writing the *Préface Historique* to the *Description de l'Égypte* (Historic Introduction to the Description of Egypt). This com-

12 [GraF, p. 24, end of ch. 1]

13 [Bott, Note(^{5}) for ch. 2]

14 [GraF, ch. 1, p. 24]

position hung over him at a time when his mind was occupied with heat and he wanted to see his 1807 essay recognised. The *Préface*, an impressive document of 90 pages, was checked over by Napoleon; Fourier "travelled up" to Paris to present his work. He had to be a historian to report on the history of Egypt, both ancient and contemporary, a stylist to deliver a text that he considered flawless and a diplomat to know how he had to describe the actions taken in Egypt by the man who was now the Emperor. Körner states that an Egyptologist of his acquaintance considers this *Préface* to be "a masterpiece and a turning point in the subject",[15] and that this Egyptologist was surprised to learn that the author was also equally well known as a mathematician! In order to carry out his task, Fourier was assisted by Jacques-Joseph Champollion-Figeac, who was passionate about Egyptology. His younger brother, Jean-François, who was born in 1790 and was a pupil at the *lycée impérial* in Grenoble in 1804, the same year it was established, was an enthusiast of ancient languages, and had a small part in preparing the *Préface*. Jean-François Champollion began deciphering hieroglyphics in 1822. After his death in 1832, he was buried – in accordance with his wishes – near to Fourier (who was also buried not far from Monge) in the Père-Lachaise Cemetery in Paris.

In 1811, Fourier significantly reworked his 1807 text and was finally awarded a prize by the *Académie* in January 1812. Lagrange continued to oppose him (he died the following year). The report awarding this prize was not without its reservations, "[...] the way in which the author reaches his equations is not without its difficulties and [...] his analysis to integrate them leaves something to be desired, as regards the level of generality or even on the side of rigor". Although honoured by the prize, Fourier was offended, he protested to the permanent secretary for mathematical sciences, Jean-Baptiste Joseph Delambre, but there was not much to be done. The following years brought major political upheaval that occupied and affected the prefect of Isère: 1814 and 1815 saw Napoleon's first exile, then his return from the island of Elba and his downfall.

After Napoleon's defeat in Russia, it was French territory that was threatened from the end of 1813 by a coalition primarily made up of Britain, Austria, Prussia and Russia. Henry Beyle, 30 years old and not yet known as the writer Stendhal, was attached to the *Conseil d'État* (Council of State) during the war. He was sent to Dauphiné in November 1813 in order to assist the special commissioner responsible for the measures to be taken to protect the region. In January 1814, Grenoble feared the arrival of the Austrian forces that had taken Geneva. The prefect had to organise the defence with the help of the military and Stendhal. Stendhal did not like Fourier, who, in his opinion, delayed and hindered military action; he had particularly contemptuous words for the prefect: "One of the causes for my trouble in Grenoble was the little intellectual scientist with practically no character and the low manners of a decorated servant, named Fourier".[16] Paris fell on 31 March and Napoleon abdicated on 6 April. On 12 April, he signed the Treaty of Fontainebleau and departed for his new kingdom, the island of Elba. With Austrian troops in Grenoble, Fourier and the majority of his prefecture rallied behind the First Restoration on 16 April. Napoleon's route took him close to Grenoble, to the great discomfort of Fourier, who was to have almost another year in his role as prefect.

In 1815, on his return from Elba, Napoleon entered Grenoble and Fourier left to avoid him. After having suspended him and threatened him with arrest on 9 March, Napoleon reconsidered and named him prefect of the Rhône department on 11 March. Fourier began work again at his new post but it ended with his refusal to apply the purging measures set by Napoleon and his Ministry of the Interior – Lazare Carnot being the Minister of the Interior – and he was dismissed on 3 May 1815.

During the Second Restoration, Fourier's pension was taken away as he was too well known as having served in the Napoleonic regime, particularly for his participation in the Hundred Days. He then received welcome support from the prefect of the Seine, Gaspard Chabrol de Volvic. Chabrol was a former student of the *École* (class of 1794), had had Fourier as a teacher and, furthermore, had been in Egypt. He was already the prefect of the Seine under Napoleon, but did not participate in the Hundred Days and remained in the same role until 1830. Chabrol entrusted Fourier with managing the statistical office for the Seine department. Fourier dedicated himself to this task with great interest and published *Recherches statistiques sur la Ville de Paris et le département de la Seine* (Statistical research on the city of Paris and the Seine department) in four volumes between 1821 and 1829. These were far from the theoretical works on probabilities or statistics by Laplace, but Körner mentions that some demographers know Fourier only as the man who played a significant role in the development of government statistics in France.[17]

In 1817, the political upheavals had abated and Fourier was elected a member of the *Académie* after an initial candidacy and an election in 1816 that was not approved by King Louis XVIII. He became the permanent secretary of the Académie des Sciences five years later, after the death of Delambre. As a leading member of the *Académie*, he had the opportunity to be in contact with Sophie Germain and they exchanged letters regularly between 1820 and 1827; he obtained spaces for her to attend the Institut's public meetings, he supported her against Poisson, who was also working on the theory of elastic surfaces, and she backed him for the post of permanent secretary in 1822. It is thought that Laplace, in his old age (he was 73 years old in 1822), became closer to Fourier and also supported him. Fourier gave a eulogy for Laplace (deceased in 1827), again a fine speech. In 1822, he edited the definitive version of the Analytical Theory of Heat, and his essay from 1811 was finally published in 1824! He was elected to the *Académie française* in 1826, although the decision was not unanimously appreciated, as it is true that his literary work was somewhat meagre.

The end of Fourier's life was difficult due to ill health. He suffered from chronic rheumatism (also whilst in Grenoble) and may have contracted a tropical disease in Egypt; he became extremely sensitive to the cold, as Grattan-Guinness[18]

15 [Körn, end of ch. 92]
16 [D–R, ch. VI, p. 347]
17 [Körn, end of ch. 93]
18 [GraF, end of ch. 22]

comments: "[illness] caused him to discourage the diffusion of heat in his quarters", to the point where he wore thick woollen clothes and ran the heating in all seasons. Throughout these years, he was absent from many of the *Académie*'s meetings. His final months were especially difficult and he spent his days in a special chair[19] from which he was still able to work. The disease may have also diminished his intellectual faculties when the permanent secretary should have taken better care of the famous essay by Évariste Galois, presented in 1829 and then in 1830. Fourier died on 16 May 1830 in Paris at the age of 62.

For us, Fourier is primarily the man of a unique work, the theory of heat. He published several lesser-known works, including essays on statics in 1798 (an article on rational mechanics, including three proofs of the principle of virtual work using the concept of moment) and on statistics between 1821 and 1829. He left a mass of manuscripts, many of which can be found in the National Library of France. One particular topic must be mentioned: for a very long period of time, Fourier conducted research on determining the number of real roots of a polynomial that are in a given interval, and on the methods of calculating values close to these roots. The question had already interested him in 1787 and even throughout his earlier years [Fo-R]. On 9 December 1789, he presented a statement to the *Académie* on this subject, which he also focused on in his lessons at the *École* in 1796 and 1797 and which he worked on in Egypt and then in Grenoble in 1804. These clarifications were given by Navier, see below. Fourier published several articles in the same vein from 1818 and submitted communications to the *Académie* between 1820 and 1830. His research led to a higher limit for the number of roots. In 1829, Jacques Charles-François Sturm discovered the theorem that is now named after him (his essay was published in 1835) and that allowed him to find the *exact* number of roots. Sturm stated that "the theorem that is developed throughout this essay is greatly similar to that of Fourier".

In his final years, Fourier started a work titled "Analyse des équations déterminées" (Analysis of determinate equations) that he was unable to finish; it was meant to bring together in two volumes the algebraic works mentioned above. Navier went on to publish the existing parts in 1831 and he wrote a "Foreword by the editor" of 24 pages that aimed to confirm Fourier's precedence over results that were more than 40 years old. Navier cited the documents in his possession; he paid particular attention to a pre-1789 manuscript *Recherches sur l'algèbre* (Research on algebra) attributed to Fourier (but not by his hand and incomplete, with only the first 28 pages remaining), and mentioned notes taken by a student during Fourier's lessons at the *École* in 1797, then a text written in Grenoble in 1804. He also concentrated on the existence of accounts that made it possible to date each of these documents. Precedence was contested by François Budan de Boislaurent, who became a doctor of medicine in 1803 and general inspector of public instruction in 1808. He was a skilled mathematician, although an "amateur"; he submitted an essay to the *Académie* in 1803, published an article in 1807 and a book in 1822 on the same question of the number of roots.[20]

19 [GraF, end of ch. 22]

20 see Jacques Borowczyk [Boro]

The dispute was very heated, even if it is not as important today. If Fourier's analytical method led to Sturm's result, it was that of Budan, which is combinatorial and of an algorithmic nature, that has had consequences in algebraic computation nowadays.

3 Trigonometric series

It was, of course, not Fourier who invented the trigonometric series: Leonhard Euler, Daniel Bernoulli and many others had used them before him. We may need to go back to Brook Taylor, the man of the *Taylor formula*, one of the first to link, around 1715, the vibration of cords to sinusoidal curves, which at the time were called "companion of the cycloid". But Fourier gave some beautiful examples of such series, and above all, systematised the relation between "function" and "Fourier series". By doing so, he helped to modify and specify the conception of functions in mathematics, a task to be completed about twenty years later by Dirichlet. Fourier calculated a large number of trigonometric series expansions of 2π-periodic, not necessarily continuous functions, some of which already figured in his essay from 1805. He rediscovered the expansion of the function equal to x when $|x| < \pi$, mentioning of course that it was Euler's due, and clearly stating[21] the need to limit its validity to $|x| < \pi$, he expanded in a sine series the odd function which equals $\cos x$ for $0 < x < \pi$ (a fact that shocked Lagrange and even Laplace), also the function which equals $\sinh x$ for $|x| < \pi$, and many others. Reading the book by Grattan-Guiness [GraF] one realises the vastness of the mathematical content in Fourier's works on heat. In the following, I would like to dwell on an example which is undoubtedly the most famous one.

After having explained the physical principles needed to understand the temperature evolution in bodies and having established the *heat equation* inside a solid:

$$\frac{\partial v}{\partial t} = \kappa \Delta v, \qquad \kappa > 0,$$

Fourier proposes[22] to explicitly determine the equilibrium temperature $v(x, y, z)$ in an infinite solid limited by two parallel planes and a third one perpendicular to the two others, supposing a fixed temperature at the edge. The solid is put into equation so that the geometry and the temperature do not depend on the coordinate z: in art. 165, it is restricted to a problem in x, y, namely a rectangular blade which is modelised by the set $\{(x, y) : x \geqslant 0, |y| \leqslant \pi/2\}$. At the edge, the temperature v equals 1 when $x = 0$ and $|y| < \pi/2$, or 0 when $x \geqslant 0$ and $|y| = \pi/2$. The equilibrium equation in the blade is $\Delta v = 0$. The condition at the edge being even in y, Fourier searches for solutions that are even in y: he considers a solution that combines functions $e^{-kx}\cos(ky)$, where the fact that v is zero in the case $|y| = \pi/2$ imposes that k is an odd integer, and where we have $k > 0$, for reasons of physical likelihood.[23] The method of separated variables had already been used by Jean d'Alembert and Euler, the superposition (even of an infinity) of solutions by Bernoulli. So Fourier searches for a v

21 for example, [Fo-C, art. 184]

22 [Fo-C, ch. III, art. 163, p. 159 and next.]

23 [Fo-P, art. 33], to be found in [GraF, p. 138]

of the following form:

$$v(x,y) = a\,e^{-x}\cos y + b\,e^{-3x}\cos 3y + c\,e^{-5x}\cos 5y + \cdots$$

The condition $v = 1$ for $x = 0$ makes him try to find an expansion which satisfies

$$1 = a\cos y + b\cos 3y + c\cos 5y + \cdots \tag{1}$$

when $|y| < \pi/2$. He first determinates the coefficient a of $\cos y$, then finds analogously the following coefficients $b, c, \ldots$ To achieve this, he takes derivatives of equation (1) an even number of times and writes for any integer $j > 0$ the identity

$$0 = a\cos y + b3^{2j}\cos 3y + c5^{2j}\cos 5y + \cdots . \tag{2}$$

To calculate the coefficients, Fourier supposes, as a first step, a limited number of m unknowns $a, b, \ldots, r$, and considers a system of m equations, the first one resulting from (1) while the $m-1$ other ones, i.e., for $j = 1, \ldots, m-1$, are

$$0 = a\cos y + b3^{2j}\cos 3y + c5^{2j}\cos 5y + \cdots + r(2m-1)^{2j}\cos(2m-1)y.$$

Putting $y = 0$, he obtains a Vandermonde system, which he solves in order to find an approximate value $a^{(m)}$ to the solution a, and takes the limit with m using the Wallis product formula that provides $a = 4/\pi$.

Going a bit more into detail, using $x_1, \ldots, x_m$ instead of $a, b, \ldots, r$, and setting $k_i = (2i-1)^2$, $i = 1, \ldots, m$, the m equations considered by Fourier are

$$k_1^j x_1 + k_2^j x_2 + \cdots + k_m^j x_m = \delta_{j,0}, \qquad j = 0, \ldots, m-1,$$

where $\delta_{j,0}$ is the Kronecker symbol. Fourier calculates "by hand", filling four pages, but we can make use of Cramer's rule that expresses x_1, the approximate value $a^{(m)}$ at step m, with the help of a quotient of two Vandermonde determinants,

$$x_1 = \frac{k_2 \ldots k_m \prod_{1<i<\ell\leqslant m}(k_\ell - k_i)}{\prod_{1\leqslant i<\ell\leqslant m}(k_\ell - k_i)} = \frac{k_2 \ldots k_m}{\prod_{1<\ell\leqslant m}(k_\ell - 1)} = \frac{3.3.5.5\ldots(2m-1)(2m-1)}{2.4.4.6\ldots(2m-2)\quad(2m)},$$

which leads us to Wallis. The calculation for $x_2, x_3, \ldots$ is analogous.

Fourier remarks further that the value 1 on the left of equation (1) will change into -1 if we add π to y. This essential remark makes him understand which are the values of the 2π-periodic extension of the sum of his series, constant on the interval $(-\pi/2, \pi/2)$: he has obtained [24] the trigonometric series development of a "crenel function",

$$\frac{\pi}{4}\operatorname{sign}(\cos y) = \cos y - \frac{1}{3}\cos 3y + \frac{1}{5}\cos 5y - \frac{1}{7}\cos 7y + \cdots . \tag{3}$$

The formula already figures in the manuscript from 1805 and the study from 1822 of this problem can also be found in the dissertation from 1807.[25] Further on in the text, Fourier comes to the "Fourier" integral formulas for the calculation of coefficients. He had not used them in the previous example, where he applied the computational method described above. Then, again using the same lines of argument, he establishes the integral formulas, at least initially. Considered a flaw by some, a quality by others, Fourier is not concise: he sets about a long proof. Beginning at art. 207, first article of Section VI, *Développement d'une fonction arbitraire en séries trigonométriques* [Fo-C] he starts from an odd periodic function and writes its development in Taylor series at 0, which is supposed to exist. Equating the Taylor series to the trigonometric (sine) series found for this same function, he calculates the "Fourier" coefficients with the help of equations that look like the ones he gave in the case of the crenel function. This leads to art. 218, the integrals appearing in art. 219. Fourier does not restrict himself to only one proof: in art. 221, he finally proposes to multiply the sum of the trigonometric series by $\sin nx$ and integrate term by term from 0 to π, using the orthogonality which will play such a fundamental role in analysis. He uses this method at least from 1807 on;[26] the issue, though, is not yet the justification of the integration term by term. In his progressive and "pedagogical" approach, he started from a regular function to apply the first proof (which, to a small extent, could prove the *existence* of the development), and he notes in the end that he is now, with the help of the integral formulas, able to analyse "general" functions.

Back to physics, Fourier gives many examples "limited" in space, one of them being the case of the *armilla*, a metal ring (ch. IV). The study of heat in a cylinder of infinite length leads to Bessel functions; they were presented by Friedrich Bessel in 1816–1817 at the Berlin Academy and published in 1819, but Fourier had studied this example since 1807 [GraF, ch. 15 et 16] and written the power series of J_0 long before Bessel's publication (although after Euler, in 1766 and 1784[27]). Fourier solves by power series the differential equation $u'' + u'/x + \kappa u = 0$ ($\kappa > 0$, u is linked to the Bessel function J_0 by $u(x) = cJ_0(\sqrt{\kappa}x)$), and uses this to produce, for the cylinder, eigenmodes – he called them "modes propres" – that are orthogonal. The constant κ is determined by the condition (7) on the surface of the cylinder (given further on), which provides a series of possible values, linked to the solutions $\kappa_i > 0$ of an equation of the form $J_0(\sqrt{\kappa_i}r) + \sqrt{\kappa_i}J_0'(\sqrt{\kappa_i}r) = 0$, $r > 0$ being the radius of the cylinder. Finally, the case of unlimited space, omitted in 1807, reveals the Fourier transformation on the real line: it appears in the awarded dissertation from 1812 (art. 71) and in article 346 of the last chapter of the book from 1822 with its inverse transformation. That chapter IX is simply entitled *De la diffusion de la chaleur* (On the diffusion of heat).

It seems difficult for the amateur historian to evaluate the proof that lead to the equation (3) giving the *crenel function* and which used arguments that might be considered totally wrong according to rigorous criteria: the derived series (2) given by Fourier are grossly divergent; it is comprehensible that mathematicians from the mid-19th century may have not taken his mathematics seriously. Today one can say that those series converge in the sense of distributions, but Fourier used

24 [Fo-C, ch. III, art. 177–180]
25 [Fo-P, art. 32–43]
26 [Fo-P, art. 63]
27 [GraC, 3.4.4, 9.2.8]

the pointwise value of partial sums. Kahane[28] sees it as the search for trigonometric polynomials that become more and more "flat" at 0 and which converge towards the solution. One is tempted to state that Fourier has been lucky in that matter. And even then! As he liked to accumulate concordant evidence, he explicitly calculated the derivative of the partial sum of (1), when one replaces the undetermined coefficients $a, b, c, \dots$ by the obtained values; this derivative is equal to $(-1)^m \sin(2mx)/(2\cos x)$, and he deduced that the antiderivatives, partial sums of (1), were more and more close to constant functions on $(-\pi/2, \pi/2)$.[29]

Niels Henrik Abel [AbeU] and Peter Gustav Lejeune Dirichlet [DirC] soon came to bring more rigour into the processing of function series. Abel had not, strictly speaking, considered trigonometric series in his paper from 1826 (which he wrote in French.[30] It has been translated into German by August Leopold Crelle, the "chief" of the *Journal für die reine und angewandte Mathematik*, see [AbeO, préface p. III], see also Bottazzini [Bott, 3.1] for a review of the article). Abel, on the occasion of studying Newton's binomial series, which he considered not to be sufficiently justified by Cauchy (although he praised the treatise on Analysis by the latter), established principles for the study of function series, in particular for the power series of a complex variable, the continuity of which he proved in the open disc of convergence. Moreover, Abel also wrote the complex number $z = a + \mathrm{i}b$ as $z = r(\cos\varphi + \mathrm{i}\sin\varphi)$ and then obtained Fourier series. Unfortunately, his good principles did not prevent him from making too optimistic statements that turned out to be false.[31]

4 Competition for heat, enmities

The study of heat was a serious subject around 1800, especially with the rise of the steam engine. Jean-Baptiste Biot, a student of the first graduation class in the *École polytechnique* in 1794, and later close to Laplace, was a member of the *Institut* from 1803. He is still known mainly for the Biot–Savart Law (1820), as well as for the law on the rotation of polarised light passing through a liquid (1835). In 1804, he published an essay on the propagation of heat (*Mémoire sur la propagation de la chaleur*) [BioM].[32] In this essay, which is very reverential with regard to "Mr Laplace", he deals with the temperature equilibrium in a bar that is heated at the end, a subject that had already been studied extensively in several European countries, both through experimentation and with attempts at mathematisation. We can cite the book by mathematician and physicist Johann Heinrich Lambert *Pyrometrie oder vom Maaße des Feuers und der Wärme* (Pyrometry, or the measurement of fire and heat), which was published in 1779, two years after his death. This book was printed in a Gothic script, which does not help us, and was therefore little-read in France and had a correspondingly weak impact. It is also necessary to refer back to an (anonymous) article by Isaac Newton in 1701 that sets an initial principle that one can summarise as follows: the temperature of a warm body, cooled in a constant and low-temperature air current, is a decreasing exponential function of time.

Biot described his experiment, stating that it is not possible to noticeably heat the end of an iron bar that is 2 m long by 3 cm in the cross-section if the other end is placed in an intense fire. In the temperature equilibrium, he found an exponential decay in the temperature of points of the bar when one moves away from the source, putting forward a verbal mathematical proof, but he did not write an equation. He explained the equilibrium that occurs at each point of the bar between the heat received from the source, the heat transferred to the further points of the bar and the heat lost at the surface, but without writing a formula. He also did not cite Lambert, even if these considerations were practically identical to those in art. 326 of the latter.[33]

Biot mentioned that the results depend on a second order differential equation (one can think that it takes the form $u'' = \kappa u$, $\kappa > 0$), where the quotient of the *radiance* and *conductivity* of the bar appears, two coefficients that he differentiates between, measuring loss towards the surface and internal conduction. He indicates, without a formula, that the usual solution to the differential equation (in $a\,\mathrm{e}^{\sqrt{\kappa}x} + b\,\mathrm{e}^{-\sqrt{\kappa}x}$) includes only one term here as it must stay bounded when x becomes large (positive). In addition, he highlights using only words that the mathematical process leads, outside the equilibrium state, to a second order partial differential equation involving time. In order to evaluate the temperature of a very hot source, Biot also suggested applying the exponential law discovered: using a bar that has one end touching the source, too hot for a thermometer, it is possible to measure the temperature of a point of the bar that is suitably far from this end and to therefore deduce the temperature of the heat source.

Fourier's first essay from 1805 already included general equations for the propagation of heat but it was not published. Rather, these were personal notes totalling some 80 pages. Fourier went much further than Biot: he dealt with equilibrium temperatures $v(x, y)$ or $v(x, y, z)$ that depend on several space variables and also looked at the variation with time. However, he wrote the differential equation (4) below, simply in x, for the temperature equilibrium of a bar heated at one end and he politely mentioned[34] Biot's work from 1804. In this essay, the heat equation was not yet in its correct form as Fourier included in the equation inside a solid the $h(v - v_e)$ term of the equation (7) given below. This term should only appear on the surface.[35] Even so, it can be seen in a note added to the margin that he was not sure that this term should be present.[36] The formalisation of the physical phenomenon was still not satisfactory:[37] Biot and Fourier struggled with *differential homogeneity* in the infinitesimal analysis of the problem, an "analytical difficulty" that Fourier circumvented then with an artificial contortion. On the other hand, the essay includes accomplished mathematical sections. There are sev-

28 [K–L, 2.4]
29 [Fo-P, art. 43]
30 [AbeO, XIV, p. 219]
31 [Bott, 3.5]
32 [Bott, 2.3.a]
33 [Heri, 8.1, p. 163]
34 [GraF, end of ch. 8, p. 186]
35 [Bott, end of 2.3.a, p. 65–66]
36 [GraF, ch. 5, p. 111]
37 [Heri, 8.1, p. 164–165]

eral developments in trigonometric series[38] that Fourier will present again later on, including the crenel function and the sawtooth function.

In his essay submitted to the Académie [Fo-P] in 1807, Fourier gave for the temperature equilibrium v of the "Biot bar" the equation

$$\frac{\partial^2 v}{\partial x^2} = \frac{2h}{K\ell} v \tag{4}$$

which involved the width $\ell/2$ of the bar. And Biot's name disappeared, for reasons I was unable to discover. At that time, Fourier applied a physical analysis that he (almost) did not change later, by presenting his concept of *heat flow* (which resolved his problem of homogeneity). Biot's analysis took into account conductivities h, K – in the *quotient* mentioned by Biot and referenced above –, but it did not account for ℓ. Later, Fourier, feeling mistreated by Biot, took pleasure in insisting on several occasions in his correspondence on "the mistake" of the latter: it was incorrect to claim that a 2-metre-long iron rod heated at one end could not be heated at the other end if it has a small cross-section.

Biot was an excellent scientist but Fourier often treated him with disdain. They were not on the same side, either politically or ideologically – Biot was a conservative Catholic –, and on several occasions, Biot disparaged Fourier's work. This opposition could have started with this essay of 1807, copies of which Fourier sent to Biot and Poisson. Rightly or wrongly, Biot believed that Fourier borrowed from his 1804 article, without now citing him, and was insulted. Poisson also attacked Fourier's mathematics. Biot and Poisson were both ambitious and talented young men who were influenced by Laplace; it seems that the "patron" stayed above this clash.

For his part, Laplace wrote on the propagation of heat in 1809 in an essay that dealt with plenty of other subjects in physics, as the title indicates [Lapl]. For heat, he adopted[39] the principle of transmission through action at a short distance. He discovered the heat equations, though he accepted Fourier's priority: "I must remark that Mr. Fourier already got to these equations", he added however, "of which the real foundations seem to me to be those that I have presented". In October 1809, Biot published in *Mercure de France*, a literary magazine, an article [BioC] summarising *Du calorique rayonnant* (Of radiating heat) by Pierre Prévost. In this article, he cited a number of scientists, such as Laplace, Lavoisier (1784), Pictet, Rumford and Leslie and explained Prévost's perspective on radiation, describing specific examples to grasp the phenomenon. Until this point, there was not much here to anger Fourier, who, at the time, was not particularly concerned with radiation. But Biot continued:

> This is what led a major geometrician (2) [*this (2) refers to a footnote of Biot's article, see below*] to extend radiation even to the interior of solid bodies; [...] These considerations immediately provided the mathematical laws of transmitting heat in accordance with phenomena and they have the advantage of removing an analytical difficulty that, until this point, has stopped all those who wanted to calculate the propagation of heat through bodies.

38 [GraF, end of ch. 8, p. 184]

39 [Lapl, Note, p. 290 in *Œuvres de Laplace*, t. XII]

Fourier's name appeared not once in the dozen pages of Biot's article. The note (2) was phrased as follows: "(2) Mr Laplace. What has been related here has been gathered from his conversations and form the subject of a work on heat that he has not yet published". Actually, Laplace had already "read" a text at the *Académie* during the meeting of Monday 30 January 1809, which was the prelude to the 1810 essay [Lapl]. Biot actually credited Laplace with all the recent discoveries on the theory of heat and he implicitly contested the validity of Fourier's results, without citing him: "an analytical difficulty [...] that has [...] stopped *all* who [...]". This passage in particular shocked and nettled Fourier. He responded and compiled very sharp criticisms of Biot in letters to several correspondents.[40, 41] Even if he was loath to cause controversy in scientific reviews, Fourier was also a politician with his supporters: to advance his cause, he knew to write to those with influence (he also learned that silence is most effective in certain circumstances). He also communicated with Laplace in highly civil terms, although he still held a grudge that led him to forget to cite Laplace throughout the entirety of his major work [Fo-C].[42]

Biot opposed Fourier, but he was quite quick to leave his research on the theory of heat, unlike Poisson. Nevertheless, Biot discussed heat in his large, four-volume work *Traité de physique expérimentale et mathématique* (Treaty on experimental and mathematical physics) in 1816.[43] In a lengthy footnote on page 669 in volume 4, he claimed to have been the first to establish the correct equation for the stationary state in his 1804 essay. Fourier had no difficulty in contradicting this claim of precedence.[44] In the same footnote, Biot also cited Laplace as having discovered the general heat equations, whereas Fourier only "rediscovered" them: omitting that of 1807, he mentioned Fourier's award-winning essay of 1812, which followed Laplace's essay. To conclude, Biot highlighted the works by Poisson, in which he praised the handling of the problem of heat as being superior to that of Fourier's, which used trigonometric series. No trace of the controversy can be found after 1816, at least in Fourier's lifetime. However, at 68 years old, Biot still had some venom to let out: in an article in the *Revue des Savants* in 1842, which was dedicated, according to the title, to the publication started in 1836 of the *Comptes Rendus Hebdomadaires de l'Académie*, he lashed out at the leadership of permanent secretary Fourier, the quality of his eulogy for Laplace, etc.

5 Parisian Life

Kahane[45] talks about competitors of Fourier, namely Cauchy, and especially Poisson, whose mathematics is rehabilitated by him (if this were necessary); he may want to balance Grattan-Guinness, who said very negative things about Poisson in one of his books [GraF]. Poisson has been a competitor, if not an opponent of Fourier. In a seminar in 2018, I heard

40 [D–R, ch. VI, p. 340]

41 [Heri, Appendix, letters XVII and XVIII]

42 [D–R, ch. VIII, p. 479])

43 [GraC, 7.7, 9.4.2]

44 [Heri, ch. 7, p. 158]

45 [K–L, 3.5]

Gilles Lebeau talking about Poisson as a great man. It is funny to have a look at how he was seen by a future great man, the young Abel, 24 years old, sometimes living in Paris (to be precise, from 10 July to 29 December 1826). Fortunately for Frenchmen like me, a collection of Abel's letters in French translation appeared in [AbeM], published in 1902 to the centenary of his birth (part of these letters already appeared in 1881, in French with slightly different translations, see [AbeO]). Abel was hoping to get into contact with French mathematicians, but the summer was not the best period to do so. He writes:

> I have only seen Poisson on a promenade; he looked quite self-satisfied. It is said, though, that he is not. (Lettre XVI, to Hansteen, 12th of August 1826).

and later:

> Poisson is a short man with a nice little stomach. He carries himself with dignity. Like Fourier. (Lettre XVIII, to Holmboe, 24th of october 1826).

As regards physical aspects, Abel certainly had a preference for young Parisian girls, who are mentioned in the same letter of the 24 October to his Norwegian friend. Abel's letters contain several expressions in French, reproduced below in italics. By their private nature, these letters heavily contrast with Fourier's severity,[46] whose emotional life is not really known (although we know that he was jovial by nature). After having said he likes to see Miss Mars in the theatre, and having talked about the funeral of the great actor Talma, Abel adds the following:

> I go sometimes to the Palais Royal which is named by people of Paris as *un lieu de perdition*. As a large number, there are *des femmes de bonne volonté*. They are absolutely not indiscreet. All we hear is *Voulez-vous monter avec moi mon petit ami; petit méchant*. [...] Lots of them are quite beautiful.

Abel ensures meanwhile that, being engaged in Norway, he stays very reasonable. He also notices the following meeting:

> [...] Herr Le-jeune Dirichlet, a Prussian who went to talk to me, considering me as a compatriot.

This "Prussian" was born in 1805 in Düren, located at that time in Napoléon's France, between Cologne and Aix-la-Chapelle, but Düren came back to Prussia after 1815; his grandfather was born in Verviers.[47] In May 1822, the young German came to Paris in order to study there. In 1825 he showed one case – among two – of the "Fermat's great theorem" for $n = 5$, and presented his results to the *Académie*; the other case was rapidly completed by Adrien-Marie Legendre (and later, by Dirichlet himself, in a paper published in 1828 in the journal "de Crelle"). At the end of 1825, the general Foy, who had given him a comfortable position as preceptor since the summer of 1823, died and Dirichlet considered leaving France. Dirichlet belonged to a circle of Fourier's "supporters", including Sturm, Sophie Germain, Navier and, a little bit later, Joseph Liouville, about 20 years old. From the editors' comments on Abel's letters,

46 [D–R, épilogue, p. 683]

47 see Jürgen Elstrodt [Elst]

Fourier recommended Dirichlet for his first position at Breslau (named today Wrocław in Poland) in 1827. It is probably under his influence that the arithmetician Dirichlet turned out to study trigonometric series. In his celebrated article [DirC] published in 1829 in French on that subject, he reproduces identically, without explicitly mentioning Fourier's name but by citing "*Théorie de la chaleur*, No. 232 et suiv.", the equation for the coefficients which can be found at the end of the article 233 of Fourier's book [Fo-C],

$$\frac{1}{2\pi}\int \varphi(\alpha)\,\partial\alpha$$
$$+\frac{1}{\pi}\left\{\begin{matrix}\cos x\int\varphi(\alpha)\cos\alpha\,\partial\alpha+\cos 2x\int\varphi(\alpha)\cos 2\alpha\,\partial\alpha\ldots\\ \sin x\int\varphi(\alpha)\sin\alpha\,\partial\alpha+\sin 2x\int\varphi(\alpha)\sin 2\alpha\,\partial\alpha\ldots\end{matrix}\right\}.$$

Of course, Fourier's work has come close to Dirichlet's kernel D_n and to its use: he wrote[48] indeed – using the outdated function *sinus verse* – the sum $D_n(x) = \sum_{j=-n}^{n}\cos(jx)$ in the equivalent form $\cos(nx)+\sin(nx)\operatorname{cotan}(x/2)$ and he additionally had an heuristics reasoning to a "Riemann's lemma", and also to the convergence towards $\varphi(x_0)$ of integrals of φ multiplied by the translation by x_0 of these "kernels". Dirichlet made this "reasoning" into proofs.

Does the absence of the name "Fourier" in the paper [DirC] mean that Fourier was such a great man for Dirichlet that naming him was useless? Bearing this in mind, one can nowadays understand his first paragraph:

> [...] This property had not escaped the attention of the celebrated geometer who has opened a new field of applications of analysis by introducing ways of expressing arbitrary functions; they are given in the Memoir that contains all his first researches on heat.

Dirichlet's article in the journal "de Crelle", after the title, was introduced in this way:

> (By Mr. *Lejeune - Dirichlet*, prof. de mathém.)

and dated on the last page: "Berlin, Janvier 1829", one month before his 24th birthday. Later, in German, in an article [DirD] published in 1837 to the mathematical physicists, Dirichlet mentions Fourier and makes explicit his high esteem of him. On the other hand, Dirichlet [DirC] criticises Cauchy, who proposed proofs concerning Fourier series (*Mémoire sur le développement des fonctions en séries périodiques*, 1827). Bad memories of Paris, from May 1822 to 1826? Coming back to the letter of 24 October 1826, Abel wrote:

> Legendre is a very nice man but unfortunately "old as stones" [*steinalt*, *in original German*]. Cauchy is *fou*, and one would have nothing to do with him,

but he also adds the following: "although he is nowadays the mathematician who knows how to tackle mathematics." Later, about a memoir entitled *Sur une certaine classe d'équations transcendantes*, which he had just finished and wanted to present to the *Académie*, Abel confides:

> I have shown it to Cauchy; but he barely had a look at it. And without undue immodesty I dare say that it is good. I am curious to know the opinion of the *Institut*.

48 [Fo-C, ch. IX, art. 423, p. 562]

It was precisely the permanent secretary Fourier who would deal with that manuscript, but it was not as good as it could have been. Legendre (who was, albeit an expert, already 74 years old) and Cauchy were appointed as referees in the meeting of 30 October 1826. Then the process got stuck. Two years later, Carl Jacobi wrote to Legendre, from Königsberg[49] on the 14 March 1829, in order to obtain news from this memoir, one month before Abel's death. Legendre answered on the 8 April from Paris. He explained that "the memoir was almost not readable, being written in very white ink with badly done characters", and Cauchy and he agreed on asking the author to hand in a more readable copy, something that Abel did not, and the matter did not move forward. According to Legendre, Cauchy bears the greatest responsibility for that:

> Mr. Cauchy kept the manuscript without taking care of it [...]. However, I asked Mr. Cauchy to give me the manuscript which I never have had and I will check what I can do to right, if possible, the lack of care that he gave to a work which would have certainly deserved better.

Kahane,[50] speaking of Fourier, says that Cauchy "was not his friend", which is a nice understatement. It is often written though that Cauchy acknowledged Fourier's authorship of the notation $\int_a^b$ for the definite integral, taken between a and b: it is as if Georg Cantor, or Karl Weierstrass after 1885, insisted on acknowledging Leopold Kronecker's authorship of his δ symbol...

6 Reception of His Work: Riemann

One can read in several documents that Fourier remained unknown, badly discussed in France – even Victor Hugo had his opinion about that![51] – in spite of his (slow) recognition by the *Académie*. His collected works ("ses Œuvres") were belatedly published, in 1888 and 1890. Nevertheless, Dirichlet celebrated him, in French, only seven years after the publication of the book of 1822, and he certainly passed on his high assessment of Fourier's works to Bernhard Riemann. The historical part of Riemann's habilitation thesis [Riem], written in 1854 and published in 1867 after his death, is given by Kahane[52] in both German and French (translation by L. Laugel [R–L], 1873). From the first page, Riemann states the following:

> The trigonometric series, as named by Fourier [...] have a pivotal role in the part of Mathematics dealing with arbitrary functions.

Later, after having recalled d'Alembert, Euler, Bernoulli and Lagrange:

> Even after almost fifty years, no decisive progress on the problem of the possibility of the analytic representation of arbitrary functions had been done until Fourier's remark, which gave a new viewpoint on this problem. This has marked the coming of a new era for this part of Mathematics, which soon came to light in a brilliant way via the great developments of the Mathematical Physics. Fourier noted that, in the trigonometric series [...] the coefficients are given by the formulas
>
> $$a_n = \frac{1}{\pi}\int_{-\pi}^{\pi} f(x)\sin nx\,dx, \qquad b_n = \frac{1}{\pi}\int_{-\pi}^{\pi} f(x)\cos nx\,dx.$$
>
> [*Riemann writes* $a_n \sin nx + b_n \cos nx$, *in contrast with what is done nowadays*] He saw that these equations can also be used when the function $f(x)$ is arbitrary.

Riemann then refutes Poisson's viewpoint, who, each time he cited these formulas (Riemann takes, as an example, *Traité de mécanique* from 1833, art. 323, p. 638), referred to a publication by Lagrange in *Miscellanea Taurinensia* (t. III, 1762–1765). In this long manuscript, Lagrange solves a certain number of equations and differential systems and comes back in the art. 38 to his solution to the problem of vibrating strings (wave equations), where his reasoning is based on N identical masses situated at equidistant points of the string, letting N go to infinity afterwards. The formula cited by Poisson appears in the article 41. Lagrange raises a question there of interpolation on the interval $[0, 1]$ by a trigonometric polynomial that is a sum of sine functions.

Given a "curve" $Y(x)$ such that $Y(0) = Y(1) = 0$, Lagrange looks for another curve $y(x) = \alpha\sin(x\pi) + \beta\sin(2x\pi) + \cdots + \omega\sin(nx\pi)$, for large but fixed n, which equals the initial curve Y at the points $x_k = k/(n+1)$, $k = 1, \ldots, n$. He writes his solution (up to some change of notation) as

$$y(x) = \sum_{j=1}^{n} 2Z_j \sin(jx\pi) \quad \text{where}$$
$$Z_j = \frac{1}{n+1}\sum_{k=1}^{n} Y(x_k)\sin(jx_k\pi), \quad j = 1, \ldots, n;$$

Lagrange's reasonings in the previous pages yield the "inverse" equation $y(x_k) = Y(x_k)$, for any $k = 1, \ldots, n$. One recognises the direct and inverse transformations of "Fourier", on the group $\mathbb{Z}/(2n+2)\mathbb{Z}$, restricted to "odd" functions (one could extend the function Y as an odd function on $[-1, 1]$). Then, Lagrange *decides to set* $n + 1 = 1/(dX)$ and $x_k = k/(n+1) = X$. He thus rewrites the equation for Z_j as an "integral from $X = 0$ to $X = 1$"; doing this replacement in $y(x)$, he gets a kind of *Fourier's integral equation* (for odd functions, and restricted to a finite degree n), which, in modern notation, reads as

$$y(x) = 2\sum_{j=1}^{n}\Big(\int_0^1 Y(X)\sin(jX\pi)\,dX\Big)\sin(x\pi). \tag{5}$$

Lagrange emphasises that he has found a function $y(x)$ in this way which *equals* $Y(x)$ at the points $x_k = k/(n+1)$, $k = 1, \ldots, n$ (and also $k = 0, n+1$).

There is still one issue: to agree with Poisson's viewpoint against the precedence of Fourier, one has to read a true *integral*. However, in order to succeed in the above interpolation, Lagrange *must* keep a finite sum. To Poisson's expected bias with respect to Fourier, even after Fourier's death, Riemann replies with a little lack of sincerity, by refusing to acknowledge, at least in these "Riemann sums", the partition mesh of which tends to 0, the beginnings of Fourier's integral equations! He writes:

49 [JacW, p. 436]
50 [K–L, end of ch. 1]
51 [K–L, end of ch. 1]
52 [K–L, 5.9]

> This formula has the same form as Fourier's series, in such a way that, at first glance, confusion can easily be possible; but, this perception only results from the fact that Lagrange used the symbol $\int dX$, where he would today have used the notation $\sum \Delta X$. [...] If Lagrange would have taken the limit with n going to infinity in this equation, he would have arrived at Fourier's result; [...].

Although introduced by Euler in 1755, the notation $\sum$ for finite sums (without bounds, like for the integral at that time) was not common before 1800; Lagrange needed a notation in order to write the double sum in the formula (5) for $y(x)$ in only two lines, the sum in $X = x_k$ (thus expressed in terms of integrals from 0 to 1), and the one in j which is written as $s_1 + \cdots + s_n$. To this end, he would have used the notation $\int$. Riemann adds that Lagrange *did not believe* in the possibility of representing arbitrary functions by trigonometric series and therefore, he did not arrive at a derivation of Fourier's formulas: "Of course, nowadays, it seems to be scarcely conceivable that Lagrange did not obtain Fourier's series from his sum formula". He goes on:

> It is Fourier who has first understood in a complete and exact way the nature of trigonometric series.

He then proceeds with the first general proofs of Fourier's theorem, i.e., with Dirichlet's article [DirC].

7 Mathematical Physics or Pure Mathematics?

It is beyond my expertise to comment on the obvious seminal character, affirmed in the title of Dhombres and Robert's book [D–R], of Fourier's work with respect to mathematical physics. It is clear that Fourier wanted to develop the understanding of the world and derive equations for an extraordinarily important natural phenomenon, as Newton did for the gravitational attraction. His ambitions are high, and the mathematical-physics viewpoint is already affirmed in the first lines of the preliminary discussion (*Discours préliminaire*) of the *Théorie analytique* [Fo-C]:[53]

> Like gravity, the heat penetrates all substances of the universe [...] The aim of our manuscript is to state the mathematical laws of such a phenomenon. This theory will be one of the most important field of general physics.

and in the middle of the preliminary discussion:

> The thorough study of nature is the most fertile source of mathematical discoveries.

Fourier stresses at the beginning of the *Discours* that he himself had taken numerous measurements in support of his theory, with the most precise instruments. It was not his intention to take into account the particular aspects that can characterise heat; he avoided having to distinguish between the different forms of propagation – by contact, diffusion or radiation. Biot shared this point of view in 1804 [BioM]:

> I will not examine here whether heat is a body or if it is nothing but the result of the internal motion of material's particles, but rather, assuming that its effects are measurable by the thermometer, once they become noticeable, I will search the laws of its propagation.

53 cited in [D–R, Annexe V, p. 717] and [K–L, 2.5]

In his essay of 1807, and more definitely since the award-winning essay of 1812,[54, 55] Fourier based his approach on the notion of *heat flux*, which may seem natural today but is in fact his invention. Let there be a point P inside a homogeneous solid, a time t and a direction given by a unitary vector u. Consider an infinitesimal circle $d\sigma$ with centre P and contained in an affine plane that is orthogonal to u. Let dS be the area of $d\sigma$ and dq the quantity of heat that crosses $d\sigma$, in the direction of u and in a duration dt after the date t. The heat flux at the point P, at the time t and in the direction u is the limit ϕ_u of the quotient $dq/(dS\ dt)$. In modern terms, Fourier's fundamental law indicates that this flux is expressed as a scalar product $\phi_u = -\kappa \nabla v \cdot u$, where v is the temperature and where the coefficient $\kappa > 0$ depends on the solid. He put it in other words[56] in the articles 96 and 97 of the section *Mesure du mouvement de la chaleur en un point donné d'une masse solide* (Measuring the movement of heat at a given point of a solid mass). In fact, there are no vectors in Fourier's text, only rectangular coordinates. The flux in the general case is determined by three values, the fluxes in the directions of increasing x, y and z. In his book, he gets there very progressively, starting with uniform movements of heat, and at first even uniform in the direction of a coordinate axis (ch. I, sec. 4 and sec. 7). Fourier returns to the flux in art. 140, before deducing from it the heat equation in art. 142.

Of course, Fourier did not write the heat equation without including the characteristic physical constants of the given body. So, for the equation that governs the temperature v inside a solid, he writes

$$\frac{\partial v}{\partial t} = \frac{K}{CD}\Big(\frac{\partial^2 v}{\partial x^2} + \frac{\partial^2 v}{\partial y^2} + \frac{\partial^2 v}{\partial z^2}\Big), \tag{6}$$

where D is the density, K the inner conductibility and C the specific heat. Furthermore, he is among the first ones to pay attention to the *dimension equations* involving positive or negative powers of physical dimensions, the length, the time and, for him, the temperature.[57] Today we would have the mass instead of the temperature, expressing heat by a mechanical equivalent.

Fourier defines the boundary condition for his partial differential equation (6): the equation at the border of the solid is, in modern notation,

$$\nabla v \cdot n = -\frac{h}{K}(v - v_e), \tag{7}$$

where n is the outgoing normal vector, h the exterior conductibility and v_e the temperature outside of the solid[58] (Fourier supposed $v_e = 0$).

Dhombres and Robert[59] point out that still at the present time, teaching of heat propagation follows Fourier's approach. They remark:

> [...] the practically unchanged manner in which we formulate, present and demonstrate today the fundamental results that Fourier enounced [...],

54 [Heri, ch. 9]
55 [Fo-P, art. 18 and next]
56 [Fo-C, ch. I, sec. VIII, p. 89]
57 [D–R, ch. VIII, p. 515–518]
58 [Fo-C, art. 146 p. 138 and art. 147]
59 [D–R, ch. IX, p. 626]

stating that in the major manuals of physics from the middle of the 20th century (Georges Bruhat, Richard Feynman and others) the calculations given for a metal plate or a ring, for example, are essentially similar to Fourier's. They complement that today, we do not demonstrate the law of heat diffusion in solids any more, partly because we do not know how to do it from the first principles of atomic physics, while Fourier's reasoning seems not to be atomistic enough nowadays.

After the consecration, Fourier published between 1817 and 1825 his "contributions à l'étude de la chaleur rayonnante" (contributions to the study of radiating heat), the phenomenon of radiation by which heat (or cold) can propagate over a distance without any contact. But this subject had to wait for certain progress in physics to take place at the end of the 19th century (Stefan's law in 1860, rediscovered by Boltzmann in 1879), before more complete answers could be obtained. In 1824, Sadi Carnot, son of Lazare Carnot, published his *Réflexions sur la puissance motrice du feu* (Reflections on the driving power of fire), but Fourier did not get familiar with this research – and he was not the only one in the 1825–1830 years. Carnot's publication, however, has contributed to the birth of thermodynamics.

Kahane has written several articles on Fourier. He mentions the opposed viewpoints of Fourier and certain "pure" mathematicians. He cites[60] a famous extract from a letter that Jacobi wrote to Legendre, sent on 2 July 1830, a little after Fourier's death in mid-May 1830. Jacobi addressed Legendre in French, excusing himself here and there for the possible incorrectness of his language use. Jacobi's letters were transcribed by Joseph Bertrand [JacL]. We have to trust Bertrand and his editor for the exactitude of the transcription: the letters were burned during the Paris Commune in 1871, as did Bertrand's house in the Rue de Rivoli.

Jacobi writes:[61] "I was delighted to read Mr. Poisson's report on my work, and I think I can be very pleased with it; he seems to have presented [*my work*] very well. But Mr. Poisson should not have reproduced in his report the not very suitable statement of the deceased Mr. Fourier, reproaching Abel and me for not having paid prime attention to the movement of heat." He added:

> It is true that Mr. Fourier was of the opinion that the main aim of mathematics was its public utility and the explanation of natural phenomena; but a philosopher like him should have known that the sole purpose of science is the honor of the human mind, and that in this regard, a question about numbers is as worthy as a question about the system of the world.

Jacobi continued by expressing his regret at Fourier's death: "Such men are rare today, even in France, they cannot be replaced that easily." He closed by asking Legendre to give his "regards to Miss Sophie Germain whose acquaintance I had the good fortune to make, and let me know about her condition". Sophie Germain suffered from a "long disease", she died the following year.

Four years earlier, Abel had written to Holmboe (24 October 1826) that he regretted Fourier's and other French mathematicians' commitment to applied sciences:

> [Cauchy] is by the way the only one to work on pure mathematics at present. Poisson, Fourier, Ampère etc. focus on nothing else but magnetism and other subjects of physics.

Poisson, Fourier, André-Marie Ampère: three professors at the École Polytechnique. We could ask ourselves if the scientific pre-eminence of the *École* in France during the first half of the 19th century, with its mission to educate mainly engineers, could be one of the reasons for the decline of French mathematics in the middle of the same century, when it is surpassed by the German University. Joseph Ben-David [B-Da] rather incriminated the teaching practice at the *École*, which did not keep up with the progress of science and forgot one of the institution missions fixed by the founding fathers – the second term of the grandiose maxim of 1804: "Pour la Patrie, les Sciences et la Gloire" (For the Country, the Sciences and the Glory).

Fourier's mathematical fame suffered an eclipse in France in the second half of the 19th century, but harmonic analysis, Fourier series and the Fourier transform have found their place in the "very pure" French mathematics of the 20th century. Kahane has contributed to this by his articles and books dealing with most specialised subjects regarding *thin sets*, that result from an exclusively mathematical study of Fourier series. A little paradoxically, the same Kahane turned himself into a defender of Fourier's mathematical physics. Regarding the temporary "eclipse", he observes, in his article [KahQ] of 2014, a forceful return of Fourier's standpoints on the occasion of a mathematics-physics convergence in our days:

> This underestimation of Fourier does now belong to the past. It could only maintain itself in France thanks to a divorce between mathematics and physics, which is completely overcome today. One of the biggest French universities, in Grenoble of course, carries the name of Joseph Fourier.

We conclude with Kahane, who writes in the same text:

Chronology

Joseph-Louis Lagrange 1736–1813
Gaspard Monge 1746–1818
Pierre-Simon Laplace 1749–1827
Adrien-Marie Legendre 1752–1833
Lazare Carnot 1753–1823
François Budan de Boislaurent 1761–1840
Sylvestre-François Lacroix 1765–1843
Joseph Fourier 1768–1830
Napoléon Bonaparte 1769–1821
Jean-Baptiste Biot 1774–1862
Marie-Sophie Germain 1776–1831
Jacques-Joseph Champollion-Figeac 1778–1867
Siméon Denis Poisson 1781–1840
Henri Beyle (Stendhal) 1783–1842
Friedrich Wilhelm Bessel 1784–1846
Claude Louis Marie Henri Navier 1785–1836
Augustin Louis Cauchy 1789–1857
Jean-François Champollion 1790–1832
Nicolas Sadi Carnot 1796–1832
Niels Henrik Abel 1802–1829
Jacques Charles Sturm 1803–1855
Carl Gustav Jacobi 1804–1851
Gustav Lejeune-Dirichlet 1805–1859
Joseph Liouville 1809–1882
Évariste Galois 1811–1832
Bernhard Riemann 1826–1866

60 [K–L, 4.6]
61 [JacW, vol. 1, p. 454]

> When I was young, and it is still the same among the young people, “the honor of the human mind” sounded more glorious than “the thorough study of nature”. However, Fourier’s philosophy seems to me to be closer than ever to the actual evolution of mathematics and their – sometimes termed “unreasonable” – impact on the natural sciences.

Most of the “historical” references below, like Fourier’s *Théorie analytique de la chaleur* or the *Mémorial* compiling Abel’s letters, are nowadays easy to access, thanks to websites like EuDML, Gallica, archive.org and many others.

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Bernard Maurey [bernard.maurey@imj-prg.fr]. Born in 1948, student of the École Polytechnique in 1966. Professor at the University Paris 7-Denis Diderot until his retirement in 2011. Has been working mainly on the Theory of Banach spaces, then on geometric and functional inequalities.

Interview with Abel Laureate Karen Uhlenbeck

Bjørn Ian Dundas (University of Bergen. Norway) and Christian Skau (Norwegian University of Science and Technology, Trondheim, Norway)

Karen Uhlenbeck, The Abel Prize Laureate 2019. © Andrea Kane/ Institute for Advanced Study

Professor Uhlenbeck, firstly we want to congratulate you on being awarded the Abel Prize 2019 for your pioneering achievements in geometric partial differential equations, gauge theory and integrable systems, and for the fundamental impact of your work in analysis, geometry and mathematical physics. You will receive the prize tomorrow from His Majesty the King of Norway.
I'm greatly honoured, thank you!

You spent your childhood in New Jersey, and you described yourself both as a tomboy and as a reader. That sounds contradictory, but perhaps it isn't?
I don't believe it is exactly. I think now you would just say that I was interested in sports and the outdoors – "tomboy" is an old-fashioned word – and also, everyone in my family read, so our favourite time during the week was our trip to the library.

Your mother was an artist, and your father was an engineer?
Yes.

Were there strong expectations as to what you and your siblings were to do later in your lives?
Yes, there were strong expectations that we should be able to support ourselves. My parents married in the middle of the Great Depression, and the difficulties with having enough money to live were very present to them. So they were mostly concerned that we would actually have jobs. And I think they had expectations of my brother actually getting an engineering degree, engineering being a good profession. As with me they didn't care so much what I did.

You say that you were interested in everything, but you also mentioned that Latin was the only hard course in high school. How did you then end up with mathematics? We would have thought you would have chosen Latin then?
Well, I don't really know myself! It was only lately I got the explanation for myself. But Latin was the only hard subject I had. It was not something you could do right away, you really had to work at translating Latin. You know, to be in this tradition of years and years and years of knowledge, and actually be reading something that was written so long ago was exciting even when I was a youngster. In my last year in high school I signed up for the honours maths course which was calculus. It conflicted with the Latin course, so I signed up for something like Spanish instead. However, after one or two classes in Spanish I changed my mind and I went back to Latin and took the regular maths course, which did not conflict with the Latin course.

Then you enrolled at the university as a physics major?
That's right. I had been turned on to physics. My father was a very intellectual person even though it had nothing to do with his job, and he got books out of the library, and I remember particular books written by Fred Hoyle. I read all those books and I think he also read them. I have to confess that I didn't do all the mathematics in them, but I saw all the mathematics in them. I also remember books by George Gamow that I found in the library. There weren't many books on maths and science in the library at all, so my resources were somewhat limited, but I was fascinated by the physics. Of course, I didn't even know that you could be a mathematician, so I enrolled as a physics major.

So you had some experience with mathematics when you started at the university?
Right. I tell the story all the time, this was three years after the Sputnik[1] went up, and so there were programmes all over the country in integrated maths and science, encouraging students to study maths and science. So there were honours courses in maths. I took a unified course in which I had an honours course in maths and an honours course in physics and chemistry, and I just really took to the mathematics right from the very beginning. I enjoyed it and I was caught up in it, and I was actually very good at it. And, you know, when you're very good at a subject, you're also encouraged to go on studying it. I really enjoyed playing with the ideas.

Did you have an aha moment, where you sort of got enthralled by mathematics? You mentioned something about the derivative.
That's right! The first time I really saw a derivative, it was actually not with a professor, but with a teaching assis-

[1] The first artificial satellite, launched on the 4th of October 1957 by the Soviet Union.

tant for the course, who was doing problem sessions. We hadn't got to taking derivatives in the class, but he showed us how to take a derivative, and he showed us how to take a difference quotient and take a limit. And I still remember that I turned to my fellow student and said: "Are you allowed to do that?" I was very excited to be able to do that. Also, I still remember when I was understanding the proof of the Heine–Borel theorem. I just remember, you know, arguing by using little boxes and things like that. And I was very excited by the experience.

This was at the University of Michigan?
Yes, I was a first year student there.

The experience at Michigan you describe as sort of special. I suppose you could have gone to other places, but you went to Michigan, and did that turn out to be a good choice?
Yes, it turned out to be a very good choice. Well, they had this honours programme. I also met the right people and the right things happened to me. In my first year at the University of Michigan I earned pocket money by waitressing in the dining hall. I lived in New Jersey so I didn't go home for the break, and I was around. During one of the breaks I was in an art museum. Well, my mother was an artist and I had essentially been going to art museums since I was in the womb; anyway, I was in the art museum, and I bumped into a professor next to me, and it turned out that he was a maths professor. His name was Dan Hughes. He found out who I was and what I did and the first thing I knew – I think it was the next semester, it might have been my second semester, I can't remember when it was – but the first thing I knew was that I was grading linear algebra without having ever taken it! So I was just taken in and, you know, I didn't think about it as anything special. To me I was just somebody who didn't know what was going on and wanted to learn things. But I think I got very good treatment by my maths professors.

So you were actually seen and you were recognized?
Yes. In fact, I think I took my first graduate course when I was a sophomore. I took the graduate course in algebra and I remember we did the Wedderburn lemmas. I remember that I didn't understand the course, but three years later when I did come to study for the preliminary exams I looked at the material, and I could actually pull it up and understand it. It's amazing what your brain actually does – learning is not linear at all. Anyway, I was already in advanced maths classes as a sophomore. Then I spent my junior year abroad, in München, and I had beautiful lectures. I took lectures from a Professor Rieger and a Professor Stein.

Socially, was that a very different experience than what you were used to from an American university?
The programme that I was in was from the Wayne State University, and there were students from all over the US in that programme. And I remember realising at the time how really good my education at Michigan was. I can tell you, there were students from Princeton, Yale, Columbia and so forth, and I was as well educated, or better educated. Certainly, my mathematical background was much better than the few others in the programme with maths majors. It was also interesting to have rubbed shoulders with American students from all different universities. To your question, the life of a German student was nothing like the life of an American student. You know, I went to the opera, I became enamoured with the theatre when I was there, I learned to ski, and of course I had a German boyfriend at some point. We went for long walks in the Englischer Garten, because it was romantic, and I learned German. Well, I can't say that I know it still but I was pretty good at German at the time that I was there, even though I don't have an ear for language at all.

After the University of Michigan you decided to go on with a PhD-program in mathematics. You spent one year at the Courant Institute in New York and then you moved to Brandeis in Boston because your husband at that time was accepted to Harvard University ?
He was a graduate student in biophysics accepted at Harvard, that's right.

But you decided on Brandeis University, even though you may have got into both Harvard and MIT?
I didn't apply. I was already aware of the fact that there were tensions around being a woman in mathematics. And I really wasn't interested in them. NYU (New York University) had a very special record for women. Lipman Bers had been there and had trained a whole generation of women students. So NYU had a very good reputation towards women. Brandeis hadn't much of a reputation at all. I had a NSF-postdoc and I probably would have got into MIT and Harvard, but some inner radar said not to do that.

You chose Richard Palais as your thesis advisor. Can you tell us why you chose him, and what the theme of your thesis was?
I took a course from him in my first year there. I was a second year student and I was already being noticed, since I came in and passed my preliminary exams. I think maybe only one of the students that had been there for a year did so at the time, the rest all took longer. So whatever feelings there were about having women students disappeared very rapidly at Brandeis. Richard Palais had given this beautiful course on infinite-dimensional topology the year before, but that year he taught a course on the calculus of variations, which is the basis for his book on the calculus of variations and global analysis. I was just excited by this new field. I understood immediately what global analysis was like, and Palais was a beautiful lecturer. I still remember the day I went in and asked him about the heat equation, and he told me everything I needed to know for four to five years. I remember just making this conscious decision I wanted to work in this new field instead of doing a special case of some boundary value problem somewhere. I made a conscious decision to jump in, so to speak.

So the theme was calculus of variations?

That's right.

So this was related to what is called global analysis?
That's right. It was really calculus of variations from the global analysis point of view.

What is global analysis, can you describe it to us?
I think that global analysis was the change from viewing an ordinary or partial differential equation as a very complicated object with lots of indices and with lots of formulas as simple equations in an infinite-dimensional space or an infinite-dimensional manifold. Conceptually, it simplifies what you're doing tremendously. It was also discovered that a whole lot of stuff that you could do in finite dimensions you could do – under the right hypotheses – in infinite-dimensions. The typical example would be what the Abel Prize winners Atiyah and Singer did; the two actually proved a theorem at that time about partial differential equations. Loosely speaking, it says that a partial differential equation of a certain type has a kernel and a cokernel, like in finite dimensions, and the difference between the kernel and the cokernel comes from topology. This was a very exciting discovery, and it's foundational to the change in perspective towards these equations.

Here is a quote from your article in the Proceedings at the International Congress of Mathematics in 1990 in Kyoto, where you gave a plenary talk: "In the 1960s, an ambitious subject called "Global Analysis" developed with the explicit goal of solving non-linear problems via methods from infinite-dimensional differential topology… The optimism of the era of global analysis has ultimately been justified, but this did not happen immediately. The problem is essentially as follows: in order to discover properties of solutions of ordinary or partially differential equations which have global significance, it is essential to make estimates." Could you expand on that?
Let me use something that I have thought of since I started doing interviews. It's a little bit like the question of the large and the small. When you paint a picture you have to have an overall perspective and an overall design and an overall point of view, but the whole thing will fall apart if you can't do the details. Saying "that's a person" is not the same thing as actually making a person out of it carefully with all the skill and background and all the teaching that you have. So the inequalities are the fundamental thing that the global picture is made out of, but in order to know exactly the right ones to do you need the global picture.

An example of that would be to find minimal surfaces in higher dimensions?
That's right. Well, in that case the problem of minimal surfaces turns out to be what you call a borderline case. In my thesis, I actually wrote down quite a few problems in the calculus of variations that satisfied a topological condition in Morse theory called the Palais–Smale Condition. The techniques of manifold theory go through for analysing the gradient flows, and so forth. But the problem is, those are made-up problems. So what happens when you come down to a problem that you really want to solve?

Karen Uhlenbeck receives the Abel Prize from H.M. King Harald. Photographer: Trygve Indrelid/NTB

In the case of geodesics, the Palais–Smale condition and all the infinite-dimensional stuff go over beautifully just like that, like clockwork. But, of course, we knew how to do geodesics: we just approximated it by broken curves, and reduced it to a finite dimensional problem. So, the question is, what good is it if it doesn't solve the problem we want to solve? My observation was that if you took the equation that you need to minimize to get a minimal sphere, and you add a small term to it, then it suddenly satisfies the Palais-Smale condition, and Morse theory is true. Now you look at the solutions of that equation and you let the perturbation go to zero, and then you can see what is happening to those solutions. What happens is that those solutions approach a solution, which could be trivial. But there's a place in the surface that you're studying where, as the perturbation goes to zero, all the information collects over that point. So if you take a microscope and look around that point, and you make the area around that point bigger and bigger and bigger, in the limit, you can actually get a solution on the whole plane. And you, lo and behold, notice this in fact solves the whole problem for you because the point at infinity can be added; that's a technical theorem. The point at infinity can be added and you suddenly have found your minimal sphere. You certainly discover that not all the solutions persist, but enough of them persist so you can say something about the problem.

John Nash, who shared the Abel Prize with Louis Nirenberg in 2015, intimated to us in the interview that we had with him that his paper titled "Continuity of solutions of parabolic and elliptic equations" from 1957–58 might have been decisive in him getting the Fields Medal in 1958, except for the fact that De Giorgi, an Italian mathematician, had independently proved that same result at about the same time. In 1977 you published a paper in* Acta Mathematica *– a prestigious math journal – with the title "Regularity for a class of non-linear elliptic systems". In the introduction you say that the results in that paper are an extension of the De Giorgi–Nash–Moser result. Could you tell us about this paper

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and its genesis? Also, the mathematical community took notice of you when your paper appeared. Do you consider this paper to be your first great paper?

Yes, and I still consider it to be my best paper. In fact, it's a very difficult paper and I wonder how easy it would be for me to understand it now. It's a long time ago, it's more than 40 years ago. But the fact is that I had found some calculus of variations problems in my thesis that satisfied Morse theory, so they had a lot of critical points. But the problem is that the geometrically simple cases of them led to integrals that were of a not completely standard sort. It turns out that you could find minima, but these minima were not necessarily smooth; these minima could have singularities. I faced the fact that I needed to show that these minima were actually regular, real solutions, not just what they called weak solutions. I learned enough about the background of the theory when I was a graduate student to be able to show that the derivatives were bounded, but on the other hand I couldn't carry it any further. I worked and fussed with this problem for a long time. In fact, if it had just been one function that you minimized, the De Giorgi–Nash–Moser result would have given that the solutions were regular. By the way, Moser's name has been tacked on to the De Giorgi–Nash theorem since he simplified their proofs. But I had a system, that is many functions, and those techniques didn't a priori carry over. I had actually met Jürgen Moser and he sent me some of his reprints, which I read very carefully. At some point I was able to use his Harnack inequalities to actually prove the fact that those solutions are regular. Actually, they have critical points where the derivative of the function vanishes, but I was able to see that the functions were smooth enough, as smooth as you would expect of them. I remember S.-T. Yau having me come to California to see him and talk to him about that paper. There I also met Leon Simon and Richard Schoen.

This particular paper was on partial differential equations and had very little to do with geometry, right?

Actually, it had nothing to do with geometry.

The techniques you developed in this paper, were they important for you when you wrote papers later?

Actually, no! Well, I'm afraid that's a little bit of the story of my mathematical career. I could have pursued it and made extensions of it and carried it further. There were some non-trivial points about putting more variables in there, and I kind of figured out how to do it. I saw you could do it but I didn't know if it was going to be useful, and I still don't know whether that problem is useful. So I didn't pursue it, but someone else did – Martin Fuchs did, actually.

But then we are entering a different phase in the beginning of the 1980s, where you published a series of highly influential papers. Those must have been amazing years? What were the conditions that made this possible?

I don't really know, because I was getting a divorce. I moved from the University of Illinois at Urbana-Champaign to the University of Illinois in Chicago. I got together with a new boyfriend, and I taught at the University of Illinois in Chicago. I taught two courses a term for three terms, three quarters a year, and I think I travelled a lot. So, how I did this I have no idea! I find it amazing!

They sound like horrible conditions!

I like to tell this, because young people really think that you have to be at Oxford or some other prestigious place to actually do good work, and I think there's no evidence that that's really true.

One of the first papers in this remarkable series is coauthored with Jonathan Sacks and is titled: "The existence of minimal immersions of the 2-spheres". There you develop a series of techniques, both with respect to regularity and with respect to compactness. We couldn't find the term in the paper, but is it here the "bubbling" idea starts?

Well, I asked Jonathan Sacks about where that idea comes from, and he thinks that I actually used it in some talks I gave about the theorem. The technique is in the paper but we didn't call it anything, and I think I only used it in talks. But the name caught on.

And there you're studying immersed 2-spheres modulo the action of the fundamental group, and you're saying that you can represent them by particularly nice immersed spheres. There were certain technical things there that you encountered that gave rise to these bubbling effects, right?

Yes, right. The idea is that you add on a small term – you can do this to most problems actually – and then it satisfies the Palais–Smale condition. It allows you to construct a Morse theory. And that gives you lots of solutions, lots of minima, lots of saddle points. But now you really want solutions to the original problem, not to the approximate problem, so you want to take the perturbation away. Now this works best in the scale invariant case, meaning that the problem does not really see scales. So what happens is that you take the limit and get a solution. But the solution might actually be trivial, it might just be a map to a point. But you go back and see what happens to the solution and it actually converges everywhere, except at a finite number of points. And around these points what happens is a scaling invariant problem. So the little region around the point thinks it's just as good as the big plane. And so you have the description of a solution as a "bubble", a sphere, actually happening around a little tiny point. And by looking at it with a microscope, a magnifying glass, and blowing it up – I think I used the term "blowing it up" at this time – you see the bubble that happens at that point and you get a solution on a plane. Then you prove that can put the point at infinity in. That's a regularity thing, namely that you can put the point at infinity in. So that way you could actually construct quite a few of these immersed spheres.

Indeed, it's a generating set for π_2, is it not?
I think someone else proved that. Actually, this is one of the things we saw we could do but we didn't do it.

These bubbles that occur, how do you control that there aren't infinitely many of them?
That's an estimate. Well, actually, the answer is: each bubble needs a certain amount of energy, and you have only a finite energy. If you want you could even make an estimate of how many you can have at most.

It is hard to choose, but many people hold your two papers from 1982 titled, respectively, "Removable singularities in Yang–Mills fields" and "Connections with L^p bounds on curvature" in particularly high esteem. Could you give us a brief overview? Specifically, why are the Yang–Mills equations important, and why is gauge invariance important?
Well, the Yang–Mills equations are important because high energy theoretical physicists told us they were important! Mathematicians could very well have done the whole theory, they just didn't think of doing it. So, it's one of these pieces of evidence that pure mathematics really needs input from outside of itself. Sometimes it's even another branch of mathematics that can give valuable input, but this is an example of ideas outside mathematics that turn out to be important in mathematics. Physicists actually got very excited about mathematics, probably because this was an application of the Atiyah–Singer index theorem to tell you what the dimension of the space of the solutions was.

It's a topological invariant, and it needed ideas from the nascent field of global analysis. They, i.e. the physicists, had explicit solutions of a certain type on the four dimensional sphere, solutions that they could explicitly write down. And they had some more complicated ones that they could write down. However, they knew they didn't know all the solutions of the more complicated sort by the Atiyah–Singer index theorem, and so it becomes a question about what the spaces of solutions of such things look like. And the removable singularity theorem from the first paper comes from the fact that if you take a sequence of these solutions, and if it doesn't converge to a solution, you know that it converges to a solution off a particular point. And at that point the bubbling phenomena happens. So, my first theorem about removable singularity was proving that you can put in that point where the solutions fail to converge.

The second paper is a little bit different. The Yang–Mills equations themselves are not elliptic equations, basically due to the presence of a large symmetry group. They have a coordinate invariance. It's like you're looking at a plane and you're not using Cartesian coordinates, but you're using any arbitrary set of coordinates you want to write down. You still have a plane, but you have all these ways to describe it. So what happens in gauge theory is that you have these physical objects called connections. I think they call them fields in physics; mathematicians call them connections. They have this gauge invariance, which means that they have coordinates that are free. And there are way too many of them – they correspond to a symmetry group – and you have to divide out by them. The problem is that you have to do something rigid, like constructing Euclidean coordinates on them. And what I did, I showed under what circumstances you can actually construct these coordinates. Once you have the right coordinate system you just treat it from standard PDE methods. That's described in the book "Instantons and Four-Manifolds" that I wrote with Daniel Freed, but you need the second variate equation. Someone had to do it. I have to say this is one example of if I hadn't done it, someone else would have done it. I mean it had to be done.

We are now talking about the Yang–Mills equations and gauge theory, which first popped up in physics, but had tremendous influence on mathematics. Many of us are familiar with the article from 1960 by the physicist and the 1963 Nobel Prize recipient, Eugene Wigner, with the intriguing title "The unreasonable effectiveness of mathematics in the natural sciences". Considering what has happened in global analysis we could perhaps turn this on its head and say: "The unreasonable effect that physics has had on mathematics"?
No! Well, I don't know about the Greeks, but certainly there was actually no difference between maths and physics with for example Isaac Newton. In fact, the real division between maths and physics occurred in the 19th century, where people like Weierstrass started putting all sorts of holes in the arguments that people were making. They were saying: "Okay, you take a sequence of things, how do you know that there is a minimum, maybe there isn't any minimum, you physicists are assuming there is". So you get a real division. Mathematics kind of separated itself because it needed the foundation of rigour. I mean, you can see this happening with infinite-dimensional vector spaces becoming very important. In the theory of calculus of variations the most important space is called a Hilbert space, so that would date that for you. And they are absolutely essential in quantum mechanics. Physicists were the ones that introduced Dirac's deltas and so forth. But mathematics had to separate and make all this rigorous before you could actually have a mathematical subject. So you see a real division occurring between maths and physics at this point. Maths kind of separated itself and made things robust and rigorous. The physicists weren't really interested in this, and actually the mathematicians stopped being interested in physics, too. And then I think it came back together at some point.

The example you mention is very interesting. Weierstrass pointed out that Riemann did not have a rigorous proof that the so-called Dirichlet problem had a solution. In fact, Riemann's defective proof relied on a kind of minimizing procedure that he called the Dirichlet principle.

That's right!

Also, Riemann was certainly a more physics-inclined mathematician than Weierstrass was.
I see. I didn't actually realise that. I don't really know so much about that part of the history.

But then, of course, your results from these two papers are taken further. At this time you're an established mathematician, and you're seeing that people like Taubes, Freedman and Donaldson are grabbing hold of the things you are doing and proving remarkable things about four manifolds. We really don't see the connections with what the physicists were originally thinking about. Could you elaborate on that?
Well, Taubes' PhD thesis is in physics, and as a graduate student he wrote a book called *Vortices and Monopoles* with his advisor Arthur Jaffe. Some of that is motivated by the connections with physics and, in fact, one of the hot topics in that subject right now is Higgs bundles. A physicist at my department at the University of Texas, Andrew Neitzke, is studying them just as hard as any mathematician would have done, so I don't know how much they are separated. But certainly they started to have a life of their own in mathematics.

A quite spectacular life at that. Could you give a short outline of the dramatic developments in four manifold theory that ensued and in what parts your contributions were particularly important?
Donaldson's classification of simply connected four manifolds with definite intersection form is based on the construction of the boundary of the moduli space of solutions to the self dual Yang–Mills equations. There are a number of ingredients in this construction. First of all, my theory on bubbling occurring in limits of solutions to the Yang–Mills equations show that the boundary consists of lower dimensional solutions spaces with bubbles attached. Cliff Taubes shows which of these configurations occur as limits of smooth solutions. In the simplest case, the boundary consists of the four manifolds itself, and the moduli space provides a cobordism of the manifold with a neighbourhood of the singular points of the moduli space. Hence, not all the continuous four manifolds constructed by Freedman can be given smooth structures. In fact, none of the exotic examples can be smoothed.

With R. Shoen you prove that any minimizing map from a Riemannian manifold to a compact Riemannian manifold is smooth outside a closed bounded set of codimension three. Could you tell us about this result and why the singular set grows with the dimension?
The work of Schoen and myself on harmonics maps, and in fact all the theorems of this type, is based on monotonicity, which estimates energy in small balls in terms of energy in larger balls. We show that when the scaled energy is sufficiently small, the solution is smooth. So singularities need a certain amount of energy. A counting argument shows that the singularities can only form on a set of Hausdorff codimension two (it is four for Yang–Mills). It can be tricky to actually get this down to codimension three, but the argument depends on both monotonicity and an approximation to the blow-up of the singularity, which is a harmonic map from S^{n-1} into the target manifold.

The conventional picture is often that a good mathematician is a person with really outstanding intellectual power, who solves the problem through his or her superior genius, and the solution comes as a kind of bolt of lightening. We know of course that this is not the typical case…
Well, there's a lot of luck involved. There's a lot of knowledge of how to take advantage of luck!

Right! However, for most of us the most important quality – besides of course a good intellectual capacity – is perseverance and the capacity of concentration. Could you expand on this and, also, have you had moments of epiphanies, where in a flash you saw solutions to problems you had been struggling with?
Let me answer your last question first, and the answer is yes. You struggle with a problem, it can be over a period of years, and you suddenly get some insight. You're suddenly seeing it from a different point of view and you say: "My goodness, it has to be like that". You may think all along that it has to be like that, but you don't see why, and then suddenly at some moment you see why it is true. It could also be a very simple idea that suddenly hits you. I don't remember where I was and what I was doing when I had those moments, but I still remember those moments.

But in all these cases we are talking about a moment, like a bolt of lightening?
There is a moment when you suddenly realise that you see how to do it. And that of course comes after all the struggle you had. Struggle isn't the right word, because you wouldn't do it if it wasn't also a lot of fun, all the time you spent thinking about this problem. Then, of course, you get the problem that is even worse: you have to write it up! But there is this moment right in between when it's really great. I remember these moments, but I have to tell you, suddenly when everything fits together you keep going back and checking if it's right. In fact, I had a similar moment a year or so ago about a problem I'd started working on, and I kept on going back and checking it to be sure that it was right. As to the first part of your question, I think you can't do mathematics without the ability to concentrate. But also, that's where the fun is, the rest of the world fades away and it's you and the mathematics. And I think there isn't any other way to do mathematics.

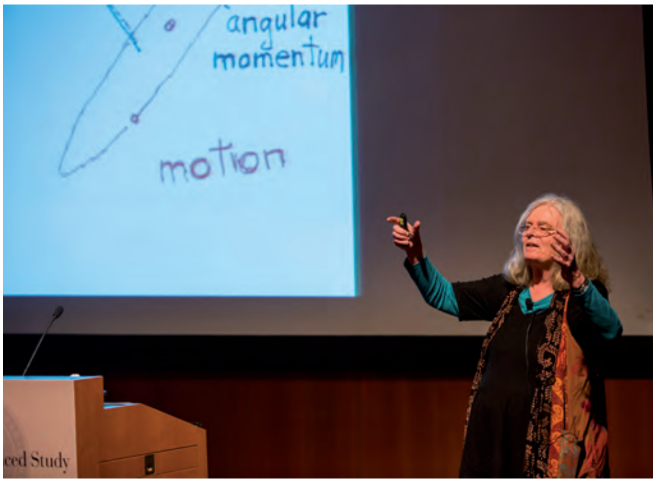
And, of course, that's one of the reasons that there are so few mathematicians. It's a very special endeavour appealing to a small minority whose minds are wired in a special way.
Well, I also think society doesn't have the will to support too many mathematicians. You mentioned perseverance, but you know, it's also an escape! Some of us really see it as an escape.

But there are two sides to this. The community is important. We promised to come back to Yau, who was one of the mathematicians that really believed in you. That kind of recognition can be crucial as well.
Yes, the community is very important. Yau is a brilliant mathematician, but he is also good at inspiring students and other people. He is good at finding mathematical results that he likes and getting other people involved in them.

In 1985 you published a paper together with Yau titled "On the existence of Hermitian–Yang–Mills connections in stable vector bundles". This work had a profound impact on the field of complex geometry as well as in physics. Edward Witten declared that the Hermitian–Yang–Mills is one of the major building blocks of supersymmetric string theory, and it provides a very elegant existence theorem by reducing to a criterion in terms of purely algebraic geometry. How did this work with Yau come about?
I knew from the late 1970s that Yau admired my mathematics. Also, Richard Schoen and Yau, and Jonathan Sacks and I, published essentially the same paper about minimal immersions of Riemann surfaces. These are minimal objects of different shapes like a 2-sphere, a torus, a two-holed torus or something. Anyway, Yau approached me and told me the problem. I didn't know anything about the field at all. One of the problems was to find out what the stability condition means. There is also a different formulation in terms of complex geometry than in real geometry. I was able to absorb this and my contribution was like what I'd done before: I added an epsilon and a term that made the problem solvable. You solved that problem, and then you took the perturbation away and looked at limits. And again you are faced with the same problem as before: you have to know what the limits look like. And that was the hard part of the paper, actually.

After 1989 you produced a series of papers about harmonic maps into symmetric spaces where the action of the loop group features prominently. Could you tell us about this project and also about your later collaboration with L. Terng and L. M. and R. J. Siebner?
I can't really go into the work with Terng in less than ten pages. It is somewhat accidental that this example has to do with harmonic maps, as our work is on a number of different equations: KdV, non-linear Schrödinger and Sine–Gordon equations. I also don't really want to describe the work with Sibner and Sibner. It is based on a mountain pass lemma, and a loop in the space of connections which is not contractible, but which does not have enough energy to allow bubbling under gradient descent.

You coined the expression "linear thinking versus sloppy thinking" to describe two types of mathematicians. One type thinks linearly, step by step, while the other is a more intuitive type of mathematician. One type tends to be a theory builder, while the other tends to be a problem solver. You count yourself as a problem solver, right?



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Yes, I definitely belong to the problem solvers. I'm really not much of a theory builder at all. In fact, I don't even read papers that way, I don't read papers from start to finish. I look at the beginning, I look at the end, I look at the references, I try to find the main theorems, I try to find the definitions, and then I try to find the key lemmas. Then I try to prove the key lemmas, and when I get stuck on the key lemmas I go back and look at the paper. That's a typical scenario for the way I read a paper. So that might give you some idea why I do not build theories!

You have said, and we quote: "I have an addiction to intellectual excitement, and as a consequence I find that I am bored with anything I understand". Could you expand on that? Specifically, does this have as an effect that you have shied away from conventional problems, so to speak, and rather focused on problems arising in new and unchartered territory?
I had the privilege of working in several fields (eigenvalue problems, harmonics maps, gauge theory, integrable systems) when very basic ideas were being developed. I know from going to seminars that these subjects have developed a great deal, with many more examples and details worked out. I find I am not interested or excited by the new results as I was when the subjects were new and basic ideas were being worked out. I regard this as an intellectual failing, and as far as I can identify my thoughts on my career, it is the one single regret I have. Some of my students have suffered, as I gave new problems in new fields to many of them, but then did not help advertise their work among my colleagues or help them develop their ideas as I might have. In an alternative life I might have contributed more to the development of mathematics instead of always looking for new directions and different approaches.

Hilbert in his talk at the ICM congress in 1900 in Paris, where he presented his famous 23 problems, said the following: "As long as a branch of science offers an abundance of problems, so long is it alive; a lack of problems foreshadows extinction". How does global analysis, in particular gauge theory, fare with respect to Hilbert's statement? Are there still big problems around?

From left to right: Bjørn Ian Dundas, Christian Skau and Karen Uhlenbeck. © Eirik Furu Baardsen/DNVA

Well, a lot of the areas in which you really would like to understand the problems are very difficult to access and one is quite stuck. For example, there are lots of problems which have to do with complex gauge groups. The gauge theory that I was talking about all had to do with things like unitary groups and special unitary groups. You can actually look at special linear groups, so there is an imaginary part of the connection which accesses the Higgs field. Clifford Taubes spent a lot of time trying to understand those problems. The two dimensional case of Higgs bundles has actually been a very hot area of research over the last five or six years. It's a big open problem how to think of these things, because the limits of solutions have singularities, and it is always very difficult to understand singularities. So that's a big open problem. My answer to you about what the big open problems are, is that we would all be there if we knew what they were! I haven't been that active mathematically the past decade, so I don't know if I'm the right person to ask. These days I count on other people bringing me good problems.

But in 1988 you did make some predictions.
Oh dear!

In "Instantons and their relatives" you list five key points that you were thinking mathematics was moving towards. One was "Simplicity through complexity". We think that was about moduli?
Yes, that's right!

Do you feel that mathematics has gone in that direction?
Yeah, I think so. There are a lot of topological invariants that are constructed using models that were originally a gauge group and an associated Higgs bundle. They are very complicated because there are a lot of fields and a lot of different terms. But when you actually write down the equations you are trying to solve and look at the space of solutions, the moduli space is actually very simple. So I think it's fair to answer yes to your question.

And you asked, just as a question: "And after theoretical physics?", indicating that you think that in the future inspiration will come not only from physics, but also from other sciences.
Well, I'm thinking it ought to be, because certainly the field of mathematical biology has grown, and the field of computer science has a lot of interesting aspects that must have mathematical connotation, and so forth.

You say another interesting thing: "I hope that no comments on the place of women in mathematics are even relevant by 2038".
Yes, right, I really hope that.

Continuing in that vein for a short moment: reflecting on your own experience, you say in an interview with the New Yorker: "I figure, if I had been five years older, I could not have become a mathematician because disapproval would be so strong". Could you expand on that?
I became a mathematician in the wake of the second wave of feminism; actually as a result of Sputnik and the second wave of feminism. The point is that Betty Friedan's book *The Feminine Mystique* opened up people's eyes to the fact that a lot of life was not open to women. So this is in the early 1960s and I was already at school at this time. By the time that I got into graduate school, and then looked for a job, the fact that women might do something else was actually in discussion. Five years before it probably wasn't really in discussion that women might be doing something like this. And I feel that the combination of Sputnik and the second feminist movement really paved the way and opened the doors for me. Five years earlier I would have missed that.

You have said that you were respected by your immediate mathematical colleagues, who recognized the brilliant mathematics you were doing. But the broader community was less accepting, some being very sceptical. Do we see a parallel here to how your most famous female predecessor, Emmy Noether (1882–1935), was treated way earlier?
Well, something changed, but it changed in the 1960s. Success for women in traditionally male fields is a complex issue.

Emmy Noether was one of the great mathematicians of the 20th century, and she is arguably the greatest female mathematician of all time. She gave a plenary talk at the ICM (International Congress of Mathematicians) meeting in Zürich in 1932. The next time a woman was invited to give a plenary talk at an ICM meeting was in Kyoto in 1990, and that woman was you. You are also the first woman to be a recipient of the Abel Prize – altogether there are now 20 recipients. In 2014 Maryam Mirzakhani became the first woman to get the Fields Medal – 60 Fields medals have been awarded so far. What this dramatically illustrates is that mathematics has been dominated by men. The topic women and mathematics is a many-faced issue involving cultural factors, stereotypes, prejudice and much more, and we will not get into that. However, we would like you to respond and confront directly those beliefs and viewpoints that still linger, and which can be summarized as follows: the rea-

son mathematics is so dominated by men is for a large part due to the fact that men are predisposed to abstract thinking, and, in particular, to mathematics.

Absolutely not! I don't believe that at all. Among other things, it's not even clear what you need to be a good mathematician. The more diverse population you have doing something like mathematics the better it is. So I think that is a completely misguided viewpoint. The fact that there were no women mathematicians was because they couldn't study, they couldn't get jobs and they had a hard time getting respect from all but their immediate colleagues. On top of that, the other women didn't accept them, we even had to struggle with that. That changed over a period of years and there's no question in my mind that things have really improved, at least in the United States. My understanding from talking to people is that it has not improved everywhere. And it's still true that women in their 50s have a tough time. But the younger women seem to have found acceptance and openness. The community has changed. The younger women are more visible, they are more talkative and they are more involved in the community. I like to think that things have really changed, but many people don't realise what it was like in the 1960s.

So you think that the glass ceiling has been broken?

Well, yes… yes and no. I wouldn't say that there aren't problems, it's just a lot better than it was.

You voice concern for minorities in mathematics, as well, and you did that again when you were told that you were going to get the Abel Prize. Is that something you have been concerned about for a long time? Is that related to other initiatives that you've undertaken?

Actually, quite a few people are concerned about it. The fact is that – perhaps not when I was younger, but by the 1990s when I started to help women – I became aware of the difficulties that under-represented minority students have. I also knew Elisa Armendariz very well, who was our chairman at the University of Texas for many years, and who is Hispanic. Many people are concerned about the difficulties that minority mathematicians have. The question is what you can do about it. The problem is, one doesn't know what to do about it.

But you had initiatives like the Park City Mathematics Institute.

The story of Park City is tied up with how I got involved with the women's programme. When I founded it, I thought, this is great, we'll have Park City and there will be a handful of women mathematicians showing up, and we will all get together and know each other. The problem was that there weren't even a handful of women that showed up, it was so predominantly male. So that was when I became involved with women. And basically I got involved because the Institute for Advanced Study gave me money, secretarial support and the prestige to actually try to start a programme. And I had Chun-Lian Terng as collaborator, and we could do maths – or we thought we could do maths at the same time that we did this organisation – and we actually did a little maths throughout this. So, you know, when I see an opportunity, then I'll try to do something.

Could we ask you what you plan to do with the prize money?

When I learned that I had got the prize I was of course amazed, overwhelmed and so forth. But the very next day, before it was even publicly announced, somebody said: what are you going to do with the money? And I said: money? I hadn't thought about that yet. But I thought about it and I realised that I wanted to do something for under-represented minorities. And I wanted to do something that is going to work! I don't want to just go out there and do anything. So I called up my friend Rhonda Hughes, who I knew from the women's programme. She has been running an EDGE-programme (EDGE stands for Enhancing Diversity in Graduate Education). She and Sylvia Bozeman from Spelman College have been running a programme for graduate students who are just starting out, half of which are minority women, and they share excellent ties with the minority maths community. I called her up and talked to her, and I made the decision that I'll give half of the prize money away. One third of it will go to the Institute for Advanced Study and two thirds of it will go to the EDGE-foundation, which gives scholarships to minority students. The Institute for Advanced Study has already matched the money that I'll be giving them for this purpose, so I'm very pleased about that.

That's splendid. We end this interview by asking what interests and hobbies you have outside mathematics?

Walking in the mountains would be at the top of the list. I've started to paint a little bit. Actually, I was not so well for a while, and I started to play the recorder again, and I started doing some painting. At this age I have to keep up my exercise programme and keep up with my friends. I find that life is already very full.

On behalf of the Norwegian and European Mathematical Societies, and the two of us, we thank you for this very interesting interview. And again, congratulations on being awarded the Abel Prize.

Thank you. I am deeply honoured.

Bjørn Ian Dundas is a professor of mathematics at the University of Bergen. His research interests are within algebraic K-theory, homotopy type theory and algebraic topology.

Christian Skau is a professor emeritus of mathematics at the Norwegian University of Science and Technology (NTNU) at Trondheim. His research interests are within C-algebras and their interplay with symbolic dynamical systems. He is also keenly interested in Abel's mathematical works, having published several papers on this subject.*

Interview with Eva Miranda (UPC)

About the Importance of Organizing a Women Workshop

NCCR SwissMAP (University of Geneva, Switzerland)

The Women in Geometry and Topology Workshop this year will be taking place in Barcelona, 25th–27th September and is organised by GEOMVAP. In 2017, the Women in Geometry and Topology Workshop took place in Zurich and was organised by the Swiss National Centre of Competence in Research NCCR SwissMAP.

In this short interview, Eva Miranda (UPC), who is chair of this year's organisation committee, tells us about the importance of organising a women's workshop and what she hopes it will achieve. Eva Miranda is a Full Professor in Geometry and Topology at Universitat Politècnica de Catalunya, Doctor Vinculado at ICMAT and Chercheur affilié at Observatoire de Paris. She is the director of the Laboratory of Geometry and Dynamical Systems at UPC and the group leader of the UPC Research group GEOMVAP (Geometry of Varieties and Applications). Since May 2018 she is a member of the Governing Board of the Barcelona Graduate School of Mathematics.

Can you tell us about your impression of the 2017 Women in Geometry and Topology in Zurich?

I remember that when I was first invited to be a speaker at the 2017 Women in Geometry and Topology Workshop in Zürich, organised by SwissMAP, I must admit that I was initially slightly reluctant about the idea of a workshop highlighting women (or any specific gender) in the title. I was, however, very impressed by the event and its success. I was also particularly surprised by the number of men who attended the talks and actively participated by asking questions. This was indeed unexpected for me.

You say you were initially reluctant to participate, are you convinced now of the benefits of this type of event?

I believe organising this type of workshop is very important, not only for women in mathematics or, in this case geometry and topology, but in other subjects too. As women, we are used to being invited to conferences and finding ourselves to be the only woman or one of the few women speakers. I think it is very pleasant to bring a lot of women to the surface so that people stop thinking that there are no women in mathematics or in geometry and topology.

People are sometimes a bit hesitant about attending a women's conference, as I was. I would like to change this. I want to bring women from different countries together and give them visibility and I also want men to be present. I take this opportunity to encourage people from all genders to come and to participate!

How did you get involved in the organisation of this year's workshop?

During the 2017 workshop in Zurich, SwissMAP member Anna Beliakova (UZH), suggested I organise the next workshop in Barcelona… Initially I was unsure. I remember then having some very interesting discussions during the conference about how to address the gender gap, how the situation in Switzerland was different from the situation in Spain or Italy and about how every country has a diverse scenario and social ingredients. Notwithstanding, there are also many similarities. Being at the conference and seeing the benefits as well as the different conversations encouraged me to organise this year's workshop. On top of that, in 2018 we were awarded a special research project focused on Geometry and Topology SGR932 from Generalitat (GEOMVAP), where gender balance is one of the strategic objectives as well as public engagement, and we decided to include this conference in the list of activities addressing the gender gap.

What are the similarities and new features planned for this year's programme?

The plenary speakers from the last workshop of Women in geometry and topology covered a range of different topics. We are following the same guidelines this year: we have women who work more in topology, women who work more in symplectic geometry, some in algebraic geometry and also others who work in applications to computer science.

This year one of our public lecture speakers will be Carme Torras (CSIC), who works in computer science. We are very honoured as she is an exceptional researcher and has been awarded many different prizes; she is a real role model! In fact, our goal is to put forward different examples of role models. We believe this that is important, as one of the problems that exists is the visibility of women in mathematics. The more we advance, the fewer there are. Although this has been a constant problem, the situation is now desperate. In the master class I am teaching this year on differential geometry, I have only one woman out of 11 students. I believe we really have to do something about this and organising this workshop is a move in the right direction and I'm hopeful it will contribute to making things change.

Who are you expecting will submit applications for the contributed talks?
One of the new features for this year's workshop is that the call for contributions is also open to all genders. The plenary speakers are female mathematicians but the call for contributions is open to all genders. We want this because we believe that things are going to change but for this to happen we need everybody to participate.

Personally, I'd like to encourage all kinds of profiles for the contributed talks. I don't want an all-women event. I want all genders to be present to see how we can improve the situation by talking to each other and cooperating to improve the situation.

Have you also considered having male participants in the panel discussions?
We plan to also have men because it is very important to have all points of view as this is a common effort. We would like our panels to also be composed of people who are not in mathematics. For example, we recently invited someone with a social studies background to be part of the panel discussions as we want to have a wide perspective on the problems and solutions. One of our panel discussions will be "From inequalities to equalities: how to break the glass ceiling in maths". The moderator of the panel will be Marta Casanellas (UPC).

The public lectures are an important part of the programme. Can you tell us about the speakers?
These public talks are open to non-experts and are not only for mathematicians. Both speakers are excellent and have very original profiles. One of the talks will be given by Marta Macho (UPV/EHU), recipient of the Ekamunde Equality Prize and also chair of mathematics and gender at the University of Bilbao. The title of her talk will be "Sesgos de género en la academia: cuando las matemáticas no funcionan" (*Gender bias in academia: When mathematics doesn't work*). Marta gives wonderful talks and I am delighted to have her as a speaker. She has a very different type of approach when she gives talks, I am sure that the participants will not be disappointed with her presentation; she tackles the problems from a different angle and she is very provocative.

We also have a public lecture by Carme Torras, who I mentioned earlier. Her lecture will be entitled "Cloth manipulation in assistive robotics: Research challenges, ethics and fiction" She has a very interesting profile, a mathematician who has reinvented herself. She is now working in robotics with robots to design clothes (ERC Advanced Grant Clothilde) and in this process of designing clothes there is a lot of mathematics. I don't know how she finds the time, but she is also a science fiction writer.

What would you say are the long-term effects of this type of event and what in your opinion is the take-home message?
For the people who participate as speakers, I think the visibility for women is very important. I will give an example, it is something that can happen too often when you are a member of a scientific committee and you're asked to name and propose speakers of the field. In an instinctive way the first names that will cross your mind are people whom you've actually seen giving talks. Perhaps if you've never heard of a particular woman giving a talk you would not necessarily think of her. This type of event is also a good way to promote and give women visibility. People will remember them and know which field they are working on.

It is equally important for the next generations. PhD students or Master's students who find themselves the only woman in their course will be glad to participate in this women's workshop and be inspired by the role models.

In summary, I would say that this type of conference firstly provides great visibility for women researchers. Secondly, it provides role models that can inspire younger generations. Finally, it sends out a message to the grant providers and to society. The grant covering most of this event is from Generalitat and it was awarded to us as part of their strategic objective on gender equality. We want to tell the government to keep moving in this direction and that what we are currently doing is not enough.

I would like to add that one of the footprints that we are going to get from this workshop is that we are going to offer the opportunity to participants who have a contributed talk to publish them in a Springer book of the collection Research Perspectives CRM Barcelona, which our research group is editing in 2019. So the conference will leave a trace behind not only in our minds but also a *printed trace in a book*.

Can you tell us about where the workshop will be taking place?
The three-day workshop will mostly take place at CRM, which is the main research centre in mathematics in Barcelona. We have decided that the public talks will be held in the centre of Barcelona at the Institute of Catalan Studies because CRM is just outside Barcelona. Our motivation to organise it in the centre of Barcelona was mainly to give more visibility to this part of the programme, which is of general interest not only to mathematicians but also to the general public.

I want to take this opportunity to advertise that Barcelona is a wonderful place to visit and also that we have grants available to cover lodgings and registration. Come and visit us! The link to the conference is www.crm.cat/2019/Women_GT.

We look forward to seeing you there!

Two Cases Illustrating the History of Algebraic Expressions

Eleonora Sammarchi (Université Paris Diderot, France)

Introduction

If we consider a polynomial as an algebraic expression composed of two, three or several added and/or subtracted terms, we might imagine that its conception is as old as the theory of equations itself. On the contrary, analysis of medieval and early modern writings dealing with algebraic computations shows that the definition of the mathematical features of this expression is far from being immediately evident: it requires a prior systematisation of the theory of algebraic computations and a deep investigation of the notions of number and operation. Therefore, polynomials seem to be conceived quite late in the history of algebra. From the point of view of mathematical terminology, this is supported by the fact that the first occurrences of the French word *polynôme* and of the Latin word *polinomius* are attested in seventeenth-century writings.[1]

For a long time, algebraists manipulated a generic object which presented its own peculiarities, but also shared several features of what would later become a polynomial. In order to reconstruct the origins of this fundamental object of algebra, research needs to focus on the lexical choices made by some of these medieval and early modern algebraists. What terms did scholars use for their algebraic expressions? And what kind of object did these terms designate?

In the following sections, I present two results of my research into the history of algebraic expressions. The first case considered is that of the school of arithmetician-algebraists inspired by the writings of the mathematician al-Karajī. This school flourished in the eastern part of the Arab-Islamic Empire between the end of the tenth and the thirteenth century. The second concerns the German Cossic tradition, which includes the sixteenth and seventeenth-century generations of *Rechenmeister* (masters of computations).

It was decided to compare these two traditions because, although they belong to two different times and places, they developed the same interest in the relation between algebra and arithmetic, and they seem to have had a common pragmatic attitude towards the way in which they conceived their algebraic expressions. Both the Arabic and the German masters aimed to develop the technical aspects of algebra rather than to investigate the nature of the entities engaged in these techniques.

[1] We find the term "polynomial" in Cyriaque de Mangin's *Cursus mathematicus* (1634); Jacques Ozanam's *Dictionnaire mathématique* (1691); and Fantet de Lagny's *Nouveaux éléments d'arithmétique et d'algèbre* (1697).

Al-Karajī's school of arithmetician-algebraists

One of the major accomplishments of ninth-century algebraists such as al-Khwārizmī and Abū Kāmil was the elaboration of a theory for second-degree equations that could be applied in order to solve both geometrical and arithmetical problems. Once this theory was established, algebraists redirected their interest to new topics. By the end of the tenth century, the mathematician al-Karajī chose to investigate the interaction between arithmetic and algebra, and began to create a coherent and exhaustive system of rules for calculating with algebraic entities. His work gave rise to a new tradition of arithmetician-algebraists, whose aim was to improve algebra with the help of arithmetic and vice-versa. This tradition focused on the notion of operation, and its aim was to make the algebraist able to manipulate unknown quantities as the arithmetician manipulates known ones.

Al-Karajī's research was then improved upon by the twelfth-century scholar al-Samaw'al (d. 1135). In the middle of the thirteenth century, the Persian mathematician al-Zanjānī followed this same tradition, and his *Qisṭās al-mu'ādala fī 'ilm al-jabr wa'l-muqābala* (*Balance of the equation in the science of algebra and muqābala*) accurately recalls and elaborates upon al-Karajī's work.

Algebraic powers

In the presentation of the rules for algebraic operations, unknown quantities are considered as either simple or composed entities. The basic terms are the algebraic powers. These are defined by al-Zanjānī as follows:

> A thing (*shay'*) multiplied by itself is called a root, and the result [of the multiplication] is a *square* […] The product of the root by the *square* is a *cube*, and by the *cube* a *square-square*, and by the *square-square* a *square-cube*, and by the *square-cube* a *cube-cube*, and by the *cube-cube* a *square-square-cube*, and so on. If the root is two, the *square* is four, the *cube* is eight, the *square-square* is sixteen, the *square-cube* is thirty-two, the *cube-cube* is sixty-four and the *square-square-cube* a hundred and twenty-eight.[2]

[2] Al-Zanjānī, *Balance of the equation*, translated from [2], fol. 2r-v. This definition owes its origins to Diophantus, whose arithmetical books were translated into Arabic in the second half of the ninth century by Qusṭā ibn Lūqā and represented an important reference for the algebraic-arithmetical tradition. Al-Zanjānī took the definition and the numerical example cited here from al-Karajī's treatise *al-Fakhrī*.

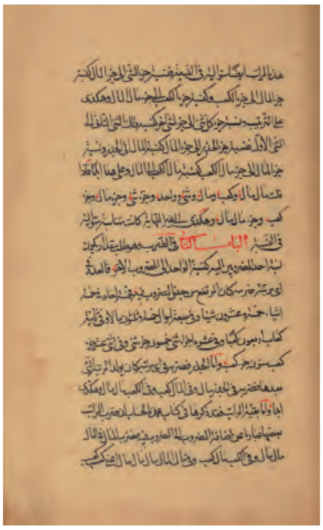

Fig. 1. Al-Zanjānī's *Balance of the equation*, fol. 4r. The word 'ibāra ("expression") appears at line 16.

As the quotation shows, the computations of these texts are expressed in rhetorical form: there are no symbols or abbreviations and numbers are usually written out in letters (see also Fig. 1, which reproduces an extract of al-Zanjānī's treatise).

Once the algebraic powers have been presented, the operations (multiplication, division, ratio, addition, subtraction and extraction of square root) are applied to them. The result of a computation can be simple, or composed of several simple terms.

Simple vs. composite numbers, magnitudes and expressions

If we examine the lexicon of the arithmetician-algebraists' treatises, al-Karajī's school did not have a technical word that specifically designated their algebraic expressions. Since algebra was conceived as an art that allows manipulation of geometrical as well as arithmetical quantities, they sometimes referred to these objects as simple/composite numbers (*'adad*) or as simple/composite magnitudes (*miqdār*).[3]

Another interesting choice made by these authors, especially by al-Karajī and al-Zanjānī, is the use of terminology that comes from the field of linguistics. The implicit analogy is between a sentence as a concatenation of words and an algebraic expression as an aggregate of algebraic terms. Thus, we find in the texts the use of *lafẓ* ("term") or of *jumla mufrad* ("simple sentence") in order to designate a single unknown quantity, and the use of *jumla* or of *'ibāra* ("phrase", "expression") for a multi-terms aggregate. One example of this terminology occurs in al-Karajī's introduction of the algebraic addition:

> *Add two expre*ssions (*jumlataīn*), of one, two or several *genres*. The rule for this is that you juxtapose each *genre* to its *genre*. Example: add five things and four *squares* to three things and three *squares*. Add five things to three things: this makes eight things, and four *squares* to three *squares*: this makes seven *squares*. Hence, the sum is eight things plus seven *squares.*[4]

Another example is found in al-Zanjānī's explanation of the multiplication of two ranks:

> The product of two ranks is an expression (*'ibāra*) obtained by the application of the multiplicand to the multiplicator. The product of the *square* by the *square* is a *square-square*, and by the *cube* it is a *square-cube*, and by the *square-square* it is a *square-square-square*, I mean a *cube-cube*. […] The rule is that you combine the terms (pl. of *lafẓ*) of the multiplicand to the terms of the multiplicator and you first mention the smaller one.[5]

On the mathematical features of the expressions

The lack of specific terminology is probably due to the fact that the algebra of these texts is conceived in ordinary language. Moreover, these scholars concentrated on improving the technical aspects of algebraic computations, and paid very little attention to more epistemological questions about the nature of the objects they were working with. This non-philosophical attitude was quite frequent at the time, and especially in a milieu like that of these arithmeticians. In these texts, there is no explicit definition of what an expression is in the field of the arithmetic of the unknowns. However, if we examine the examples and the problems in which such expressions are employed, we can identify the mathematical peculiarities presented by these aggregates.

Inverses of powers

The first significant difference with regard to the notion of polynomial is that expressions can include the inverses of powers. For instance, in his treatise *al-Bāhir fī 'l-jabr* (*The brilliant in algebra*), al-Samaw'al gives a tabular method for the extraction of the square root of the following composed magnitude:

> 25 *cube-cube* plus nine *squares-square* plus 84 *squares* plus 64 units plus a hundred part of a *square* plus 64 part of a *square-square* minus 30 *square-cube* and 40 *cube* and 116 thing and 48 part of a thing and 96 part of a *cube*.[6]

This can be transcribed as follows: $25x^6+9x^4+84x^2+64+100\frac{1}{x^2}+64\frac{1}{x^4}-30x^5-40x^3-116x-48\frac{1}{x}-96\frac{1}{x^3}$.

Numeral adjectives

As was previously mentioned, the algebra of these texts is conceived in natural language. When we analyse the algebraic expressions, we can see that what we call the coefficient of a term corresponded for al-Karajī's school

[3] In this tradition the term "magnitude" is also applied to the numbers. Hence, it loses its geometrical sense and can be translated as "quantity".

[4] Al-Karajī, *al-Fakhrī fī ṣināʿat al-jabr wa'l-muqābala* (*Book of al-Fakhrī on the art of algebra and muqābala*), translated from [7], p. 118.

[5] Al-Zanjānī, *Balance of the equation*, translated from [2], fol. 4r.

[6] This exercise is discussed by Roshdi Rashed in [1], p. 32–24. As can be seen, al-Samaw'al uses a mixed form for writing the numbers: sometimes they are written in numerals, sometimes they are written out in letters.

to a numeral adjective. Hence, it was interpreted as a counting number. For instance, in the equation: "Three squares plus ten things equal thirty-two units", which we could transcribe as $3x^2+10x=32$, "three" and "ten" are "the number of squares" and "the number of things". They are conceived as a multitude of squares or things, as 32 is a multitude of units.[7] For this reason, irrational numbers are accepted as solutions of the equation, but they are not conceivable as a "number of things". However, it is difficult to determine whether we can speak of coefficients in these texts or not. Indeed, this concept seems to change when these authors use tabular methods in order to solve, for instance, the division of two composed magnitudes or the extraction of square root of an expression, as in al-Samaw'al's exercise. In these cases, numeral adjectives are directly employed and manipulated in a tabular computation. Hence, they seem to share some of the features of modern coefficients, and acquire an autonomous status in comparison to the unknown quantity.

Subtracted quantities

In this arithmetic of the unknowns, although the rule of signs for the multiplication and the subtraction are already known, it is not correct to qualify the quantities as positive or negative: there can only be added or subtracted terms. Indeed, as al-Samaw'al's exercise shows, a simple or composed expression never starts with a minus-term: subtracted terms are always listed at the end of the expression, after the quantity from which they are subtracted. Moreover, since negative arithmetical numbers did not exist, negative solutions are not yet considered as solutions of algebraic problems.

The German Cossic tradition

The *expressions* considered by al-Karajī's tradition can be of *n*-degree. But when two expressions are put together in order to compose an equation, the degree of the latter is never greater than 2. During the Italian Renaissance, algebraists developed a theory of cubic (and quartic) equations solved by arithmetical tools. In this field, Gerolamo Cardano's *Ars Magna* is one of the texts that had a significant impact on the German-speaking arithmetic masters of subsequent generations. Cardano's list of equations and the rules (*Regulae*) that he presented in order to solve them were discussed, although often not correctly understood, by the so-called Cossic tradition. The word *Coß* derives from the Italian *cosa*, which translated the Arabic word *shay'* ("thing"). It denotes the unknown quantity and, by extension, algebra as a theory of equations. The members of this tradition were sixteenth and seventeenth-century *Rechenmeister*, especially those who worked in Ulm and Nuremberg.

As Ivo Schneider has pointed out,[8] the success of the *Rechenmeister* school was directly related to the rise of the new bourgeoisie, who needed private teachers of mathematics, and especially arithmetic, for their children. Christoph Rudolff's *Behend und hübsch Rechnung durch die kunstreichen Regeln Algebre, so gemeinicklich die Coß genennt werden* (1515), Adam Ries's *Die Coß* (1524) and Michael Stifel's *Arithmetica integra* (1544) were three fundamental texts of the sixteenth-century Cossic tradition. Their work was then commented on and improved in the seventeenth century by a new generation of masters, among whom Peter Roth and Johannes Faulhaber are pre-eminent.

Stifel's "Cossic numbers"

For the Cossic tradition, algebra is part of arithmetic. This subordination is clearly stated in Stifel's *Arithmetica Integra*. According to Stifel, there are three types of numbers, which constitute the main topics of the three books of the *Arithmetica Integra*:

- rational numbers are the numbers of the arithmetic of integers and fractions (Book I, *De algorithmo numerorum integrorum et minutiarum*)[9];
- irrational numbers are the numbers of the arithmetic of radicals (Book II, *De essentia numerorum irrationalium*);
- Cossic numbers are the numbers of the arithmetic of unknown quantities (Book III, *De Regula Algebrae*, Section III, *De algorithmo numerorum cossicorum*).

The definition of a Cossic number is given at the very beginning of Section III:

> Cossic numbers are numbers denominated proportional to a geometrical progression.[10]

Stifel designated each algebraic power with a specific symbol (see Fig. 2). This symbolic writing of variables relies on that of Cardano and constitutes a significant innovation in early modern algebra.

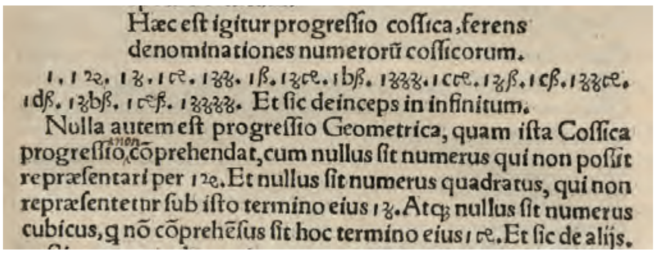

Fig. 2. Stifel's representation of Cossic numbers.

[7] Jeffrey Oaks clarified this point in [5]. He noticed the fact that, in Arabic texts, the term "coefficient" does not exist. Instead, these scholars worked with literal collections of squares, things etc. In his article, Oaks presents what he calls the "aggregations interpretation", according to which "the two sides of an Arabic equation are not linear combinations in the modern sense, but are collections of the algebraic powers, in which all mathematical operations have already been performed". This is also an interesting observation from our present point of view.

[8] See [8].

[9] Sabine Rommevaux-Tani clarified in [6] that, for Stifel, an *algorithmo* is an explanation of how to write the numbers and how to add, subtract, multiply and divide them.

[10] Stifel, *Arithmetica Integra*, translated from [9], fol. 234r.

Simple and composed Cossic numbers

As with the Arabic expressions, Cossic numbers can be simple (*simplices*), like $20x$ and $30x^2$, composed (*compositi*), like $x+x^2$, or diminished (*diminuti*), like $2x-8$. Moreover, since the theory of equations has a primary role in the German texts, simple and composed Cossic numbers are directly conceived in the framework of an equation. Hence, they are either two or three-term aggregates.

Seventeenth-century "Cossic quantities"

In Peter Roth's *Arithmetica Philosophica* (1608) and Johannes Faulhaber's *Academia Algebrae* (1622), the language of arithmetic books shifts from Latin to German. In the new texts, *numero cossico* is replaced by idioms that are less arithmetically denoted, such as "Cossic quantity" (*Cossische Quantitet*) and "Algebraic quantity" (*Algebraische Quantitet*).

Another term which we can already find in Cardano's writings, is *Aggregaten*, or its Latin version *Aggregata* (in both cases, it is always used in the plural). It originally designates the result of a numerical sum. However, since, as we have mentioned, there are several types of numbers for the Cossic tradition, and algebraic numbers are one of these types, *Aggregaten* is also employed in the field of algebra, where it refers to a composed algebraic quantity.

We can find these three lexical choices juxtaposed in one of the problems (*Quaestionen*, see Fig. 3) included in Faulhaber's *Academia Algebrae*:

> There are several aggregates of *D-sursolit* numbers[11] formed by addition, following each other in the right order (so that nothing is left out), and they make together the sum 70322010. How many are they? And what are the algebraic quantities [that] are naturally said equal to this just set number according to a regular computation? The answer of the Aggregates is 4. And [this] is the desired Cossic quantity: $6x^{15}+90x^{14}+525x^{13}+1365x^{12}+819x^{11}-3003x^{10}-3575x^9+6435x^8+9009x^7-9009x^6-12285x^5+6825x^4+7601x^3-2073x^2-1470x$ divided by 1260.[12]

On the mathematical features of the Cossic quantities

Except for Stifel's definition of Cossic numbers, Peter Roth and Johannes Faulhaber, like al-Karajī's school,

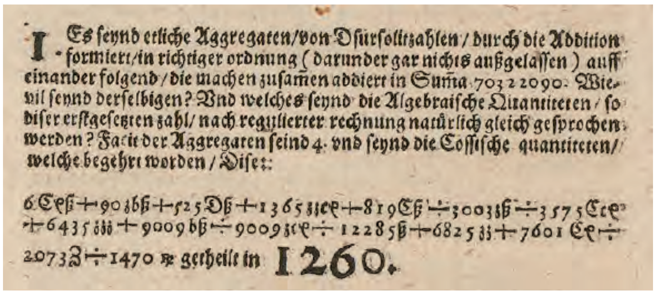
I. Es seynd etliche Aggregaten/von Dsursolitzahlen/durch die Addition formiert/in richtiger ordnung (darunder gar nichts außgelassen) auff einander folgend/die machen zusamen addiert in Suma 70322090. Wievil seynd derselbigen? Vnd welches seynd die Algebraische Quantiteten/so diser erstgesetzten zahl/nach regulierter rechnung natürlich gleich gesprochen werden? Facit der Aggregaten seind 4. vnd seynd die Cossische quantiteten/welche begehrt worden/Dise:

Fig. 3. Faulhaber, *Academia Algebrae*, fol. 15Csi.

lacked an explicit definition of these aggregates. Their focus was on the solution of the equations, and on the collection of problems. Compared to the algebra of sixteenth-century authors, in the seventeenth century the technique for solving algebraic problems became more sophisticated. As shown in the analyses of Schneider and Manders,[13] Roth and Faulhaber combined algebra with the theory of numbers in order to deal with higher-degree equations. Moreover, they presented several problems involving sums of series of algebraic powers, and transposed into algebra several classes of arithmetical problems, especially those dealing with polygonal numbers. Despite the similarities, the notations adopted in these texts suggest that the expressions conceived by Roth and Faulhaber still differ from the modern notion of the polynomial.

Fictive numbers

In these problems, Cossic quantities can be of *n*-degree: they do not include the inverse of powers, and negative quantities are still difficult to conceive. However, a new kind of number is mentioned: a number that is fictive (*ficta*, or *gedicht*). Inherited from Cardano, this concept is also present in the Cossic writings. In his Book II, Stifel wrote that we can imagine numbers that are less than nothing (*finguntur numeri minores nihilo ut sunt 0-3, 0-8, etc.*).[14] In the same way, Roth mentioned the two values of the root –the true one (*waaren*) and the fictive one (*gedicthen*) – in problems like the following one:

> First, $1x^7+584x^4+17680x^3+18416x^2$ are equal to $7x^6+266x^5+158688x+174720$. Given now that the value of one true root of this equation is 10; then the question is what, which and how many other values of the root, true and fictive, will there be? Answer (*Facit*): the others are $10+\sqrt{48}$, $10-\sqrt{48}$, also –4 and –7. Thus you see that this equation admits three true and two fictive values of the root.[15]

Fictive numbers do not have the same status as true (i.e. positive) numbers. However, their presence in these texts marks a significant difference in comparison to the Arabic context, in which these quantities are not even conceivable.

[11] A *D-sursolit* number is an algebraic number of power 13. Indeed, in the Cossic definition of algebraic powers, the solid number is the third algebraic power, and the *sursolit* number is the number whose exponent is the first prime exponent after the solid, i.e. the fifth. As Stifel's notation shows, Cossic symbols are then combined with the alphabet letters B, C, D… in order to designate the other prime numbers.

[12] Faulhaber, *Academia Algebrae*, translated from [3], fol. 15Csi. I have translated the Cossic notation into the modern symbolic language of mathematics. As we can see in Fig. 3, the "minus" symbols are expressed using the symbol of division. For this reason, the authors writes "divided by". I have decided to translate the literal sense, but this actually means that we must subtract (and not divide!) 1260 from the Cossic quantity.

[13] See [8] and [4].

[14] In [9], fol. 48r.

[15] Peter Roth, *Arithmetica Philosophica*, translated in [4], p. 201.

The two case studies presented here show that, as often happens in the history of mathematics, similar questions can arise and similar attitudes can be adopted at different periods. The scholars that we have considered did not focus on the definition of their algebraic expressions because these generic objects were seen as parts of the theory of equations and functional in the resolution of problems. As historians, we can perceive not only that both the Arabic expression and the German Cossic quantity differed from the modern notion of the polynomial, but also that they designated two different objects. In this article, I have only been able to sketch some of their peculiarities: a more detailed analysis will be developed in future work. What already seems clear is that a historical investigation, supported by textual analysis of the sources, of the algebraic expressions that existed before the polynomial contributes to clarifying the process by which the latter was elaborated and bears directly on several significant topics in the history of algebra, such as the resolution of equations through arithmetical methods, the emergence of negative quantities and the introduction of a symbolic language.

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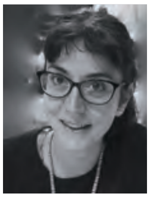

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The UMI Archives – Debates in the Italian Mathematical Community, 1922–1938

Livia Giacardi (University of Turin, Italy) and Rossana Tazzioli (University of Lille, France)*

The Archives of the Unione Matematica Italiana (Italian Mathematical Union, UMI), located at the Dipartimento di Matematica of Bologna University, have recently been reorganised and will soon be opened to scholars.[1] They consist of two parts: a historical one covering the period from 1921 to the mid-fifties, and a modern one reaching from 1967 until today. This paper focuses on the historical part containing two sections: a first one with documents listed in the old inventory of the UMI Archives, concerning the years 1921–1933 and 1939–1943, and a second one kept in a box labelled "Correspondence relating to the Italian Mathematical Union 1938–1950. Do not open before the year 2000". The latter is a non-inventoried archive (sealed files, "fondo secretato") and contains 14 files from the years 1938–1952. It was forbidden to consult this section, most likely to avoid the premature disclosure of documents relating to UMI's unseemly reaction following the Racial Laws. This part mostly consists of the correspondence of Enrico Bompiani, vice president of the UMI from 1938 to 1948 and president from 1948 to 1952. In order to hide evidence that the UMI collaborated with the fascist regime, some documents have most probably been removed.

As we try to show in this paper, the documents of the UMI Archives highlight new significant aspects of the history of the UMI, in particular the attitude of the Italian Mathematical Union towards the fascist regime and the Racial Laws (1938), by enriching or completing the existing literature on the relationships between mathematicians and fascism.[2] They moreover provide useful information on the international context of the inter-war period, when mathematicians tried with difficulty to reconstitute scientific internationalism interrupted by the First World War.

What is the UMI? Its foundation and first years

The history of the UMI begins in 1922. Unlike other national mathematical societies – such as the American, French, or the German mathematical societies (AMS, SMF, and DMV respectively) – the UMI was not born of the will of Italian mathematicians, but was an emanation of an international institution founded in 1920: the International Mathematical Union (IMU).[3]

Immediately after the First World War, in July 1919, the International Research Council (IRC) was set up in Brussels, excluding Germans and their former allies by the statutes. The IMU was officially founded on September 20, 1920 during the International Congress of Mathematicians (ICM) held in Strasbourg and, in accordance with IRC's regulation, excluded the former Central Powers from the organisation. The Belgian Charles-Jean de la Vallée Poussin and the Frenchman Gabriel Koenigs were elected president and secretary of the IMU respectively, while Vito Volterra and Émile Picard were among the honorary presidents.

Then professor at the University of Rome, Volterra was at the peak of his scientific and institutional career. A mathematician of high reputation throughout the world, nicknamed "Mister Italian Science", Volterra was vice president, and later president, of the prestigious Accademia dei Lincei. His role in the foundation of the UMI was significant, as he proposed to the Accademia dei Lincei, which accepted, the creation of a new Italian society of mathematicians. On 18 March 1921 Volterra informed Salvatore Pincherle, a specialist in functional analysis and professor at the University of Bologna, that the Accademia dei Lincei had designated him as president of the new society that would represent Italy in the International Mathematical Union.

With Volterra's agreement, Pincherle sent a circular in which he listed twelve crucial points of the new Union's program; among them we mention the following:

- To bring Italian experts in mathematics closer together;
- To encourage research on pure science;
- To reinforce relationships between pure mathematics and various branches of applied mathematics;
- To nourish interest in questions concerning mathematics teaching;
- To spread works and research of Italian mathematicians throughout foreign countries;

* We thank the CIRM of Trento for supporting our research with the project "Research in Pairs 2019".

[1] See the website of the UMI: http://umi.dm.unibo.it/en/info-3/umi-historical-archive /; on Italian science archives see http://www.archividellascienza.org/en/.
We would like to thank the former UMI president, Ciro Ciliberto, the current president, Piermarco Cannarsa, and the treasurer Veronica Gavagna for allowing us to consult the Archive; we are also grateful to the archivist Alida Caramagno for her useful suggestions.

[2] See for instance (Israel, Nastasi 1998), (Nastasi 1998), (Guerraggio, Nastasi 2005).

[3] For more details on the UMI history see (Giacardi 2016), (Giacardi, Tazzioli 2018), (Giacardi, Tazzioli 2020), and (Pucci 1986), (Bini, Ciliberto 2018).

- To promote exchanges of mathematical books and journals in Italy and abroad;
- To organise national meetings on pure and applied mathematics.

Salvatore Pincherle (1853–1936)

Vito Volterra (1860–1940)

In order to attain these goals, Pincherle founded a new journal, the bi-monthly *Bollettino della Unione Matematica Italiana* (BUMI). In May 1922 Pincherle sent out another circular to the members of the new society and asked them to send brief reports of their work to the UMI, including the origin of their research problems, the problems to be dealt with, their principal results and a few details of their mathematical methods.

Thanks to the rich correspondence and the documents of the UMI Archives we can trace the history of the early years of the Italian Mathematical Union. We learn that at the beginning Pincherle had difficulties in convincing colleagues to join the new society. In fact, some of the most famous Italian mathematicians disliked the foundation of the UMI. As an example, we refer to a letter from the UMI Archives by Tullio Levi-Civita, professor at the University of Rome, arguably the leading player, along with Volterra, in Italian mathematics in the first decades of the twentieth century. Levi-Civita wrote to Pincherle on April 16, 1922:

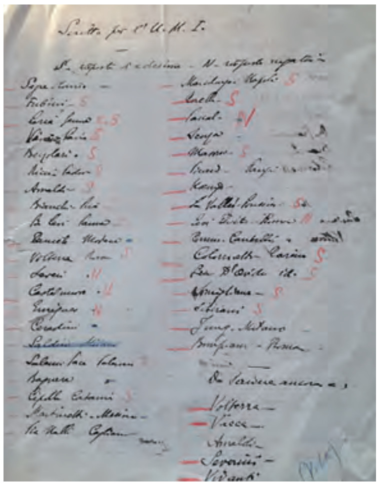
> *"Although all the aims that should have been pursued by the Union [UMI] were not covered by the Statutes of the Circolo Mat.[ematico] di Palermo, yet I cannot escape the impression that the true and desirable analogue of the "Société Math. de France", "American Math. Society", "Deutsche Math. Ver." etc. still is the Circolo, which really honored Italy, when Guccia was alive, it was in full working order. Why kill it or weaken it with a new society? Would it not be much better to invigorate it, and to continue it and exploit its good traditions and indisputable merits?"*

Therefore, according to Levi-Civita, the Circolo Matematico di Palermo (Mathematical Circle of Palermo), an international and highly reputed mathematical society in Italy and abroad, already played the role of an Italian mathematical society. Founded by Giovanni Battista Guccia in 1884, the Circolo had reached its zenith on the eve of the First World War. In the twenties, because of the catastrophe of the war and the death of Guccia in autumn 1914, the Circolo and its journal, the famous *Rendiconti del Circolo Matematico di Palermo*, showed signs of crisis (Brigaglia, Masotto 1982) (Bongiorno, Curbera 2018). Levi-Civita and other mathematicians preferred to attempt to revive the fortunes of the Circolo and its journal rather than create a new society that would, in their view, sink it definitively.

Another issue made Italian mathematicians wary of the UMI. Was this not an emanation of the IMU, an exclusionary organisation par excellence, the enemy of scientific internationalism by the statutes? In the same letter to Pincherle Levi-Civita claimed:

> *"The international union [IMU], to which the circular refers to (see point 6), is not actually international. […] It seems to me that this aspect of the [UMI] program should be clearly proposed in a way that leaves no room for doubt."*

Many other mathematicians – such as Guido Castelnuovo, Umberto Cisotti, Gino Loria, Corrado Segre and Giulio Vivanti – shared the same perplexities, as the correspondence contained in the UMI Archives shows. For example, on 17 April 1922 Castelnuovo, professor at the University of Rome, wrote to Pincherle that he did not understand "the urgent reasons that required the establishment of a general Union of mathematicians [UMI]", giving the same reasons as Levi-Civita did. And Segre, one of the main protagonists of the school of Italian algebraic geometry, in a letter to Pincherle dated May 9, 1922 threatened to resign from the UMI if it did not distance itself from exclusionary organisations such as the International Mathematical Union.



Pincherle's list of the Italian mathematicians in favour of the UMI or opposed to it: S (*Sì, Yes*) stands for in favour of, and N (*No*) stands for against the UMI (UMI Archives)

A success for Pincherle and the UMI: the Bologna ICM of 1928

Pincherle made huge efforts to defend the UMI project. He wrote numerous letters trying to convince his colleagues that the UMI was necessary and that the Bollettino was different from already existing journals, saying that both the Union and its journal would render a great service to Italian mathematicians. After a few months, in June 1922, there were 152 members, and membership reached 379 in 1924. Little by little, many of the mathematicians who had opposed the UMI joined it – such as Umberto Cisotti and Levi-Civita who became members in autumn/winter 1922-23, Federigo Enriques and Francesco Severi in spring 1923, and Guido Castelnuovo in 1926.

The most significant event of UMI's first years is the International Congress of Mathematicians held in Toronto in 1924. The correspondence of John C. Fields, the president of the organising committee and the founder of the Fields Medal for outstanding achievement in mathematics, offers evidence of the prominent role played by Italian mathematicians in the Toronto congress. On 17 July 1924 Fields wrote to the UMI secretary Ettore Bortolotti: "Yours' is the most brilliant delegation from Europe and it would be too bad if it did not remain intact". As other letters kept in the UMI Archives show, Fields travelled to Turin, Bologna and Rome to meet and invite eminent Italian mathematicians, or wrote to them. The Italian mathematicians who read short notes at the Toronto ICM were: Leonida Tonelli, Guido Fubini, Gregorio Ricci-Curbastro, Giovanni Giorgi, Giuseppe Gianfranceschi, Umberto Puppini, Corrado Gini, Ettore Bortolotti and Giuseppe Peano, while Severi and Pincherle gave two of the six plenary lectures and Puppini was allowed to deliver an hour-long lecture on applied mathematics.

In Toronto, many mathematicians did not agree with the IRC and IMU policy that excluded Germans and their former allies, and opposed the boycott of colleagues coming from the former Central Powers. Therefore, the American delegates presented a motion, endorsed by Italy, Netherlands Sweden, Denmark, Norway and the United Kingdom, asking the IRC to abolish the restrictions on nationality imposed by the post-war Council's rules. The motion was passed by the assembly.

The greatest Italian political success in Toronto was the election of Pincherle as president of the International Mathematical Union, while Koenigs was confirmed as general secretary. Moreover, the choice of Bologna for the following congress prevailed over that of Stockholm proposed by Gösta Mittag-Leffler. As the president of both IMU and UMI, Pincherle then began to work on organising the next ICM in Bologna.

In the meantime, the new international policy supported above all by the League of Nations led the IRC to organise an extraordinary assembly on June 29, 1926 where scientists from Germany and its former allies were invited to join the IRC and its Unions. However, Germany rejected the "invitation" and did not adhere to either the IRC or the IMU, as it demanded an "admission" by the statutes. This request was only satisfied in 1931. (Rasmussen 2007)

Pincherle decided to invite scientists from all nations without restrictions to the Bologna ICM. However, the question of inviting German mathematicians was problematic, as Germany belonged neither to the IRC nor to the IMU. The UMI Archives show how Pincherle was gradually led to the following expedient: although the invitation letters mentioned that the congress was linked to the IMU, they were signed by the rector of the University of Bologna who then "seemed" the real organiser. This deliberate ambiguity was immediately remarked upon by the French Picard and Koenigs. The latter wrote to Pincherle on May 29, 1928:

> *"Your letter of April 26 makes me aware of an event whose gravity you cannot certainly ignore, albeit in a watered-down form. Invited in June 1926 by the International Research Council to join it, the German and Austrian scholars did not respond to this act of high courtesy and openness; they refused to join the work of peace which all desire […]*
> *But leaving aside all questions of peace or courtesy, there is one that particularly complicates things and makes the situation very difficult. It is your benevolent consent to abandon all your rights as President [of the IMU] in favor of the University of Bologna and its Rector […]*
> *This grave shortcoming makes all invitations illegal."*

On the other hand, a group of German mathematicians, led by Ludwig Bieberbach from the University of Berlin, tried to discourage participation in the Bologna Congress. Bieberbach sent a letter to all German universities and secondary schools with a request to boycott the Congress. He reproached Pincherle and Bortolotti for not wanting to officially pull away from the IMU. Many documents of the UMI Archives concern the German boycott. Bieberbach wrote in a letter dated July 14, 1928 that although "the warm words with which you invite me to Bologna go straight to my heart", "there is still no clear separation between the congress and the Union [IMU] itself". He then declined the invitation by adding that "apart from the private difficulties, I also feel the weight of a charge of responsibility in the DMV and that my presence at the congress could be seen as if I were there representing the DMV"; he was indeed the DMV secretary.

Nevertheless, several German mathematicians, especially Hilbert and his colleagues at the University of Göttingen, supported the participation in the ICM of Bologna. Hilbert immediately accepted the invitation to give a general conference and planned to give a short political speech including the famous sentence: *Mathematics knows no race* (Siegmund-Schultze 2016). In a letter to Bortolotti on 23 May 1928, Richard Courant, professor in Göttingen, claimed: "I am very interested in restoring international relations between mathematicians from different countries". He added that German scientific societies and authoritative organisations (academies) were "reluctant to support the Conseil de Recherches [IRC] in its current form", although "according to us [mathematicians of the University of Göttingen] there would be no obstacle for the Germans if the congress were independent of the Conseil des recherches".

For months Pincherle had to mediate between French requests and German criticisms in order to avoid the boycott against the Bologna ICM. In the UMI Archives the correspondence between Pincherle and Ettore Bortolotti with Picard, Mittag-Leffler, Koenigs, Courant, Brouwer, Demoulin, Bieberbach and others testifies to this difficult mediation.

Finally, the international congress of Bologna was a success: 836 mathematicians from 36 countries participated, and around 80 were Germans. Guillermo Curbera denotes the congress as a fascist power "showcase". (Curbera 2009, p. 88) The young Hasso Härlen wrote a letter to Brouwer, in which he recognised Italian organisers' efforts for avoiding contrasts, but he highlighted a general lack of sensitivity towards Germans – for instance there were small Italian flags everywhere, not to mention the dreadful situation of the South Tirol where people were forbidden to speak in German and to teach German at school because Mussolini stifled any opposition by force. (Van Dalen 2011, p. 334–338)

The IMU General Assembly took place in Bologna unofficially because Koenigs refused to convene it. While Pincherle's work was unanimously approved, he was aware that he had not complied with the IMU rules and consequently resigned as its president. (Proceedings ICM 1928, p. 83)

Fascism and mathematics: new elements from the UMI Archives

The UMI Archives not only allow us to clarify administrative issues and difficulties due to international scientific policy, they also shed light on the attitude of mathematicians towards the fascist regime. In fact, the foundation of the UMI took place in a particular period of Italian history. It was founded in 1922, the year of the Rome march that inaugurated the fascist era. During the first years of the UMI, the fascist regime strengthened and showed its true face with the Matteotti assassination (1924). In order to attract intellectuals, the fascist regime developed a cultural policy and created institutions to further it. In 1925 the Istituto Nazionale Fascista di Cultura (National Fascist Institute of Culture) and in 1926 the Accademia d'Italia (Academy of Italy) were founded, followed by the new Istituto Centrale di Statistica (Central Statistical Institute) directed by Corrado Gini. Little by little, Volterra, who had always supported Pincherle as head of the UMI, lost all his institutional influence because of his opposition to fascism: in 1926 and 1927 he was replaced as the president of both the Accademia dei Lincei and the Consiglio Nazionale delle Ricerche (National Research Council, CNR) by Vittorio Scialoja and Guglielmo Marconi respectively.

Letter by S. Pincherle to B. Mussolini, Bologna, September 13,1928 (UMI Archives)

The UMI never reacted officially against fascist laws, not even against those that damaged science and, in particular, mathematics. As an example, in 1923 the neo-idealist philosopher Giovanni Gentile, as the Minister of National Education, completed a reform of the Italian education system at all levels in a single year by blatantly devaluating science. In spite of its statutes claiming the UMI was involved in questions related to mathematics teaching, the UMI did not take any official position against the Gentile Reform, and nothing about the following harsh debates appeared in the Bollettino. In contrast to this, the Accademia dei Lincei took a very strong stance regarding the reform, as did the Italian association of mathematics and physics teachers. No opposition to or debate about the Gentile Reform appears in the documents of the UMI Archives. This silence is probably one of the reasons why the government offered generous aid in 1924 when the UMI participated in the Toronto ICM. The interest of the fascist government was to show the image of the Italian genius ("genialità d'Italia") abroad, as the Minister of National Education, Alessandro Casati, who replaced Gentile from July 1924, wrote to Pincherle. Casati granted 20,000 Lire (about 18,000 Euros) to support participation in the congress, as evidenced in his letter to Pincherle dated July 19, 1924.

In April 1925 Pincherle signed the manifesto of fascist intellectuals – the so-called Gentile manifesto – during the first Congress of Fascist Cultural Institutes held in Bologna. Other and ever more numerous traces of subjugation to the fascist power can be found in the UMI Archives in the following years. In February 1926, Pincherle contacted Mussolini to obtain the necessary funding for the organisation of the Bologna congress. On December 7 Pincherle was received by the Duce, and significantly on December 31 joined the Partito Nazionale Fascista (National Fascist Party, PNF).

We point out that several sections of the Bologna ICM of 1928 dealt with applied mathematics in accordance with Mussolini's ideas on the importance of applied sciences and their relations with society. (Mussolini 1926, p. 30) Significantly, Pincherle offered the prefect of Bologna the right to choose the members of the honorary committee – Mussolini was asked to be the president, and several ministers of his government belonged to this committee. His accommodating attitude towards fascism led him to win the support of the government. Actually, the ICM received a huge contribution from the national government and the Ministry of National Education, and relevant support from political and cultural institutions of Bologna (municipality, university and province), as well as from various public and private entities. In more detail, the national government and the Ministry of Public Education gave 200,000 Lire (about 180,000 Euros today), and the municipality, province, and university of Bologna donated in total 125,000 Lire. (Proceedings ICM 1928, p. 18–19).

In his introductory speech to the congress, Pincherle bestowed lavish praise on the Duce's work. (Proceedings ICM 1928, p. 73) On September 13, 1928 he addressed a letter to Mussolini reporting on the huge success of the congress from different points of view:

> *"In political terms, the most explicit recognition came from all sides, and concerns the order, the well-being, the regular functioning of all the services under the*

Fascist regime, under the Government of the E. V. [Eccellenza Vostra e.g. Mussolini] who is its founder. In the political sphere, too, the result was achieved to bring scientists from countries previously at war with each other back to cordial harmony; so much so that after the truly international Bologna Congress, all future congresses will have to be equally international. From the scientific point of view, there were general lectures of the highest interest, held by scientists of clear and undisputed fame, Italians and foreigners [...] Finally, the exaltation of Italian science. This Congress – in lectures, in communications, in printed works written for this occasion and given as a gift to the participants – shed the clearest light to our results obtained in the last fifty years and to the immense contribution of Italy to the shaping of modern mathematics."

In the UMI Archives there are neither documents nor letters nor assembly reports that show a real opposition to the fascist policy. There was no official opposition and only a few examples of passive resistance even emerged even in the later period, during which the fascist laws further limited freedom of individual and association. In 1931 the government imposed a requirement on professors to swear an oath of allegiance to fascism; Volterra was the only mathematician who refused. Nothing about this event emerges from the UMI Archives. There is no trace even of another crucial law promulgated by the government in 1934: the statutes of the UMI were modified and limited the freedom of the UMI and its members. The new statutes subordinated the appointment of the UMI president and vice president to the assent of the Minister of National Education. No cry for alarm arose even in 1936 when the UMI was not allowed to participate in the ICM in Oslo, despite the fact that Severi had been invited to hold a plenary lecture. (BUMI 15, 1936, pp. 96–97) The reason was that Norway was a country that, following the directives of the League of Nations, sanctioned Italy in response to the attack on Ethiopia.

The attitude of acquiescence of the UMI towards the government continued without interruption. The first UMI Congress held in Florence in 1937 confirms this attitude. There was opportunistic behaviour in the exaggeratedly celebratory tones of the inaugural speeches and in the choice of giving ample space to applied mathematics according to the wishes expressed by the government: 4 out of 8 sections – probability; astronomy, geodesy, optics; aerodynamics; hydraulics. In their introductory speeches, the rector of the University of Florence, Giorgio Abetti, and the president of the UMI, Luigi Berzolari, exalted the work of the regime and emphasised the greatness of the Duce who was the "omnipresent, wonderful architect of the national renaissance". (Proceedings UMI 1937, p. 9, 12) Even Severi, in the plenary conference entitled "Pure science and applications of science", enthusiastically praised Mussolini. In particular, he claimed that mathematicians were ready to collaborate "for getting the maximum of national autarchy". (Proceedings UMI 1937, p. 23) The plenary lectures highlighted the important contributions of Italian mathematics to some crucial sectors: Bompiani spoke about the modern developments of differential projective geometry, Tonelli illustrated the recent Italian contributions to the calculus of variations, and Scorza lectured on the theory of algebras that had recently received an impressive development in Germany and the US.

Racial Laws and the UMI

1938 is a key year for the history of fascism and the UMI. In the summer the newspaper Il Giornale d'Italia published the "Manifesto of Racial Scientists", which established the foundations of fascist racism. After claiming that human races existed and that the concept of race was purely biological, the "racial scientists" declared that the "pure Italian race" had to be preserved. Following this and other actions of racist propaganda the Racial Laws were promulgated by the government from September to December 1938.

In spring 1938 there was an important event for the mathematical community: the elections of the UMI executive board. The result was clear: Berzolari was elected president and Pietro Burgatti vice president. The latter, however, died suddenly on May 20, leaving the position of vice president vacant. The election result was submitted to the Minister of National Education, Giuseppe Bottai, who exercised his power by confirming Berzolari president, and appointing Bompiani vice president, although the latter obtained only 8 votes – Guido Fubini got 74 votes and Annibale Comessatti 61. Moreover, Bottai excluded all Jewish mathematicians from the UMI scientific commission– B. Segre, B. Levi, Fubini and Levi-Civita who got the most votes. (BUMI 17, 1938, pp. 140–141)


Enrico Bompiani (1889–1975)

It was not just by chance that Enrico Bompiani was appointed vice president by ministerial order. A mathematician of strong reputation, he had obtained the prestigious gold medal of the Accademia dei XL in 1926 and the "Premio Reale" of the Accademia dei Lincei in 1935. Bompiani, who had been the secretary of the CNR mathematical committee since 1926, exercised great power in the CNR, as evidenced by the documents contained in the Fondo Bompiani at the Accademia dei XL in Rome. He aspired to obtain a prestigious position in the UMI as well. As the correspondence in the UMI Archives shows, Bompiani actually influenced UMI's policies after 1934, when Luigi Berzolari replaced Pincherle as the UMI president. Also, there is evidence in the archives that Bompiani, directly or indirectly, manipulated his appointment to UMI vice president in the 1938 elections after Burgatti's death. On May 22, 1938, in fact, the UMI secretary and his close friend Ettore Bortolotti advised him to tell the Minister not to take the second or third rankings into account "but to certainly make your [i.e. Bompiani's] appointment", in order to do "good work, for our union and also for Italian culture".

On October 19 Berzolari addressed some critical words to Bompiani:

"I am very happy to have you as a collaborator in the exercise of the "power"; I would not like to know the reasons that led the Minister not to follow the appointment of the Union […] by choosing you who had 8 votes, instead of Comessatti, who had 61."

Even before the Racial Laws, Bompiani exchanged confidential letters with several of his colleagues that were clearly anti-Jewish, especially regarding the UMI administrator Beppo Levi, professor at the University of Bologna and at that time a member of the Bulletin editorial board and of the UMI Scientific Commission. In a letter dated July 27, 1938 Bompiani wrote to Ugo Bordoni, the president of the CNR committee for physics and applied mathematics, that Levi and his colleague Beniamino Segre were "the two real puppeteers" of the UMI. Actually, Berzolari had always tried to defend the work of Beppo Levi from Bompiani's attacks, but only in private. For example, in a letter of January 7, 1938, he wrote to Bompiani:

"It would seem to me a lack of honesty, if no word in his favor [i.e. Beppo Levi] is said […] He is a person of genius and has a very wide mathematical culture […] he has always carefully read all the works sent for printing in the Bollettino, and if they do not contain mistakes, Levi should deserve all the praise: I can assure you that I will never find a person as agile, patient, disinterested as he is."

From autumn 1938 Bompiani, as UMI vice president, immediately set to work to implement the new Racial Laws; he wrote to Berzolari on October 28, 1938:

"It seems appropriate to me if you sent a circular to the UMI members explaining their own responsibility – and making them feel proud – which derives from the recent racial decrees. These decrees commit each one to give the maximum contribution in order that no domain of Italian culture can suffer a decrease. The great founders of Italian mathematics, who created research fields where nothing existed and led them to a leading position, were not Jews (BETTI, BELTRAMI, BRIOSCHI, CASORATI, DINI, CREMONA etc): their names must give young people the confidence of being able to continue this excellent tradition exclusively with Italian forces."

One month later, on November 24, 1938 Berzolari wrote to Bompiani informing him that "the names of the Jews were canceled in accordance with the measures taken by the Government. The list will appear in the issue [of the BUMI] that will be published in a few days."

On December 10, 1938, the UMI Scientific Commission met in Rome. As reported in the proceedings, "after a friendly, exhaustive discussion" and refusing "all solidarity with teachers and colleagues", (Pucci 1986, p. 210) the UMI assembly actually supported the fascist government by claiming that:

- Italian mathematics is the creation of Aryan scientists;
- Italian mathematics, even after its decimation, preserves the conditions for its development and, in any case, is able to cover vacant positions;
- No vacant mathematics professorship due to the Racial Laws must be subtracted from the mathematical disciplines.

Bompiani insisted on rejecting articles by Jewish authors for the Bollettino; an attitude even more intransigent than the fascist government, as Berzolari pointed out in a letter on January 24, 1939:

"The annoying question is the Jewish one. As a first remark, I believe that if the Government – which decided the appointments of the [UMI] President and Vice-President, and the Scientific Commission – did not want Jewish works to be published in Italian periodicals, it would have told us: instead I have never received any orders about that."

A few months later, in another letter to Bompiani on March 9, 1939, Berzolari reiterated his opinion:

"As for the fact that F. [probably Bruno Finzi] belongs to Jewish race, I do not see why we must be more intransigent than the government, which has maintained him as a teacher and as a member of the Ist. Lomb. [Istituto Lombardo]. Can't he publish his works in Italian journals?"

Meanwhile, as a result of the Racial Laws, Beppo Levi and Beniamino Segre, like many others Jewish mathematicians, were forced to leave the UMI, as well as the University of Bologna and were forced into exile abroad, the former in Argentina and the latter in England.

The Italian mathematical community was one of the most affected by the effects of the Racial Laws – the UMI expelled 22 members, 10% of the total – and Italian universities had to face the non-trivial problem of replacing the vacant positions left by 96 full and extraordinary professors, over 141 assistants and several dozens of lecturers, and at least 207 university assignments that were revoked. (Sarfatti 2018, p. 218) Some letters show the awareness that the expulsion of many high-level Jewish mathematicians had weakened Italian mathematics. For example, on July 21, 1939 Bompiani wrote to Sabato Visco, the director of the Institute of General Physiology of the University of Rome:

"Mathematics is one of the areas most affected by Judaism; and our will is not enough to defend it, but we also need the means (which, moreover, are limited)."

It is also worth mentioning that after the agreement between Italy and Germany signed in autumn 1936, the Rome-Berlin Axis, the Italian and German mathematical societies (UMI and DMV) sought a way to cooperate and Bompiani had a very active role – like his German colleagues Harald Geppert and Wilhelm Süss had. (Remmert 1999) (Remmert 2017) Bompiani's engagement continued in organising the Second UMI Congress that took place in Bologna in 1940, giving lectures on mathematics in Germany and in other countries of the Axis and holding courses in the Istituto Nazionale di Alta Matematica (National Institute of High Mathematics, INDAM) founded by Severi in Rome in 1939 with the support of the fascist government.

Label on the box bearing the words "Correspondence relating to the Italian Mathematical Union 1938–1950. Do not open before the year 2000" (UMI Archives)

Conclusion

To conclude, our research based on the documents of the UMI Archives sheds light on both "theoretical" and "practical" aspects of the UMI policies towards the fascist government. (Capristo 2013) Theoretical aspects refer to the ideological support to the regime through, for example, the celebration of the Duce's extraordinary abilities and far-sighted generosity towards sciences. But it is above all the practical aspects that emerge and that were implemented by personal or collective behaviour in the face of bureaucratic procedures; they actually allowed fascist legislation, particularly the Racial Laws, to have an extremely effective application.

We met figures like Bompiani, Ettore Bortolotti and others, who were not true persecutors, but who supported and strictly followed, sometimes with "zeal", the procedures imposed by the government for personal ambition, or for preserving mathematics chairs, or for envy or revenge against Jewish colleagues. Others, like Berzolari, were simply "aligned". (Capristo 2013) They were often aware of the illegitimacy of certain laws and expressed their disappointment in private. Therefore, they were able to be indignant but not actually to rebel publicly, either because they were manipulated or because they could not understand that "the great and irremediable evils depend on the indulgence towards the evils still small and remediable". (Foa 1996, p. 151) Finally, there were those, probably most of Italian mathematicians, who obeyed without even getting angry. All these "bystanders", according to the historian Raul Hilberg, were also responsible for the anti-Jewish persecutions, and deserve to be studied as the persecutors and their victims. (Hilberg 1992)

So the UMI Archives are a useful research tool for reconstructing the attitude of mathematicians towards fascism. However, these archives only concern mathematicians who had a significant role in the UMI and therefore a small part of the Italian mathematical community. In particular, they contain only a few documents concerning two of the most important mathematicians of the fascist era, namely Francesco Severi and Mauro Picone. This is the reason why the documents from the UMI Archives should be integrated with those from other Italian and foreign archives. Further research should be done to give a more faithful image of the relationships between Italian mathematicians and fascist regime, as well as other European scientific communities. As Severi's papers have not been preserved, documents concerning his political and institutional activity are scattered either in personal funds or in institutional archives such as the Archivio Centrale dello Stato (Central State Archives, ACS) in Rome. For instance, Severi asked Gentile to submit to the Grand Council of Fascism a new formula of oath of allegiance to the Fascist Party that suggested a political line that would be successful. He indeed proposed a sort of "regularization of political acts happened a long time ago" – for those in particular who, like him, had signed the anti-fascist manifesto of 1925, but then became supporters of the regime. Severi's request is expressed in a letter dated February 15, 1929 preserved in the Gentile papers (published in (Guerraggio, Nastasi 2005, p. 101–102)).

Mauro Picone was a member of the UMI scientific commission and at the same time directed an important CNR institute, the Istituto Nazionale per le Applicazioni del Calcolo (National Institute for Calculus Applications, INAC). Fortunately, Picone's documents and letters are kept in the Archivio Storico of this institute, and give a lot of detailed information about Picone's activity during fascism. (Nastasi 2007)

Other interesting documents concerning the attitude of mathematicians towards fascism can be found in personal archives, such as Volterra's papers and Levi-Civita's papers both at the Accademia dei Lincei in Rome, Sansone's papers at the University of Florence, or Marcolongo's papers at the Dipartimento di Matematica, University "La Sapienza" of Rome and others that should still be explored.[4]

Here we have focused our attention on a particular aspect of the research, that is how to use the UMI Archives to reconstruct the history of a crucial period of this institution and its interactions with fascism, but the

[4] In particular, we can mention the personal archives of Alessandro Terracini (Department of Mathematics, University of Turin), and Gustavo Colonnetti (Archivio di Stato of Turin).

UMI Archives, complemented by other Italian archives, are also relevant from a different perspective. In fact, further studies could help historians of mathematics to establish the influence of political events on Italian mathematics specifically. One might wonder, for instance, if the fascist regime produced a real isolation of Italian mathematics in the thirties that could have contributed to the decay of the Italian school of algebraic geometry. And this despite the fact that Francesco Severi, one of the prominent figures of this school, succeeded in founding the Istituto Nazionale di Alta Matematica in Rome thanks to the support of the fascist government. Furthermore, did some disciplines benefit from a favourable political climate for their development? For example, the implication in the fascist politics of Mauro Picone, director of the Institute for the Application of Calculus, together with the extraordinary applications of calculus to other sciences, could explain the extraordinary development of numerical analysis already in the Thirties. On the other hand, some disciplines may have suffered as a result of the political climate and the consequent expulsion of many Jewish mathematicians from Italian universities. For example, Levi-Civita's excellent scientific research, especially in the field of mathematical physics, abruptly stopped in 1938, when he was made to retire and replaced by his pupil Antonio Signorini, whose scientific stature was decidedly inferior.

We hope that this research will help institutions to become aware that it is important to recover and digitalise historical archives in order to create a network connecting them to each other for a better understanding of history.

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The Secondary-Tertiary Transition in Mathematics
Successful Students in Crisis

Francesca Gregorio (HEP Vaud, Lausanne, Switzerland and LDAR, Université Paris Diderot, France), Pietro Di Martino (Università di Pisa, Italy) and Paola Iannone (Loughborough University, UK)

Introduction

The transition from secondary school into university mathematics – also referred to as secondary-tertiary transition (STT) – is a sensitive moment for many students, also for those who have achieved high marks at the end of their schooling and are considered excellent in mathematics in the school context.

For this reason, the EMS Education Committee identified STT to be one major issue for mathematics departments across Europe and their students, as explained in the last EMS newsletter (Koichu & Pinto, 2019). In order to shed some light onto this process and to support mathematics departments in implementing appropriate measures to help students overcome the difficulties connected to the move into university mathematics, the EMS has recently designed and distributed a survey for mathematicians across Europe. The EMS Education Committee's interest reveals the concern of the mathematics community for the experiences of students joining degree courses in mathematics and the will to understand this phenomenon in order to alleviate some of the problems connected to this transition. Indeed, the STT has been of interest to mathematics educators for a while now, and many empirical studies have been carried out to better understand all aspects of this transition. Gueudet (2008) highlights three aspects of this transition: a cognitive aspect, which focuses on the increased cognitive difficulties of university mathematics and its increased formalism and rigour (e.g. Tall, 1991); a social aspect, which focuses on the social changes that students face when moving to university, where they receive less support than in school and where they often experience living away from their families for the first time (e.g. Pampaka, Williams and Hutcheson, 2012); and an affective aspect, linked to the feelings and emotions that students experience during the STT. The first aspect – the cognitive one – has interested researchers since the late seventies, but the interest in the other two aspects is growing, as research reveals just how important social and emotional experiences are for students moving into university. Recently we have investigated the emotional aspect further and we will report here on some of our findings (Di Martino & Gregorio, 2018) underlying the necessary developments.

The STT can be seen as a real rite of passage (Clark and Lovric, 2008) characterised by three stages:

- separation stage (from secondary school – when students leave behind the context in which they have been successful mathematics learners),
- liminal stage (from secondary school to university – where students join the new context, but they are still relative novices in the practices and norms of this new context) and
- incorporation stage (when students become full participants of the practices of university mathematics).

Each stage of this passage is often associated with a period of crisis caused by the need to reorganise well-established mathematical routines and negotiate new methods of interaction with the subject, the peers and the instructors in the new context. Difficulties in reaching the incorporation stage of this passage may cause students to drop out from their courses.

In line with a general movement in mathematics education, we have dedicated more attention to a holistic approach to the STT, focusing on the affective factors that shape this crisis and to this extent we developed a research study at the University of Pisa, in Italy (Di Martino & Gregorio, 2018). Students at this university are usually considered excellent in mathematics at the end of their schooling: more than 65% of the students in the first year of the bachelor in mathematics in Pisa achieve a final mark at secondary school between 90/100 and 100/100, but despite the high results these students face many difficulties during their bachelor's degree, and more than 22% of them drop out at the end of the first year (in line with the average Italian dropout rates for the bachelor in mathematics).

Our research was organised into two phases: the first phase consisted of the design and administration of a questionnaire, the second phase involved interviews with volunteering students. We included in the questionnaire and interviews both successful students and those who had dropped out of their course so that we could collect both experiences of success and failure. We chose mainly qualitative methods and open-ended questions in order to stimulate students to compose narratives related to facts and emotions that they themselves recognised as significant, using the words they consider more appropriate for their memories. Therefore, our research has been intentionally student-centred. As part of this study we collected and analysed 137 questionnaires and 37 interviews.

Results

From the qualitative data collected emerges that students joining the bachelor in mathematics are generally high achievers and strongly motivated in their academic choice. Nevertheless, they often experience their first failure in mathematics. For the first time, some successful students have to come to terms with difficulties in establishing effective studying routines, obtaining poor marks for their work and not being the best in mathematics amongst their peers. This is a big change for them, and they have to learn to manage this change from a cognitive as well as an emotional point of view.

We are interested in describing and understanding this change. From our data, this shift is usually unexpected, and this surprise makes students' negative reactions even more powerful: their mathematical identity as a successful student is suddenly questioned. In most cases, students start associating negative emotions to their university experience and to mathematics; shame, anxiety, insecurity, sadness and frustration are the emotions more often mentioned in our data. Moreover, students begin to question their own self-image as mathematicians: switching from feeling highly competent to feeling inadequate for the task can be a hard change. The university experience also elicits an awareness of the difference between secondary school mathematics and university mathematics, questioning the theories of success in mathematics developed during the school experience. Students recognise secondary school mathematics as a procedural subject – that requires performing calculations following standard steps – compared with university mathematics, a formal and abstract subject that consists of definitions and theorems and deals for the most part with abstract objects. Most students like this new version of their subject, but they find it hard to come to terms with it at the start of their studies, where so many other aspects of their lives are changing.

The big differences in teaching methods between secondary school and university do not help students in their STT; relatively small mathematics classes are replaced by large lectures where students may feel lost, and studying strategies which worked at school level suddenly become ineffective at university level. Students then become stuck in this first impression of failure and they do not know how to change the situation. In addition, despite the increasing efforts made by the institution to help students during tertiary transition, some struggling students believe that they have to smoothly adapt to the new context and materials. This attitude exacerbates the difference with the secondary school experience of mathematics. To sum up, students feel a sense of impotence, in the conviction that they cannot gain control of the situation of failure which then becomes unavoidable and unchangeable for them.

Facing failure for the first time and the change of their perceived competence in mathematics makes students feel ashamed. Students become afraid of disappointing people close to them and are afraid of being compared to their peers. This attitude supports the misconception that students often have of being the worst of their year and the only one experiencing difficulties, with important consequences to their self-esteem. The same sense of shame often leads students to close within themselves and to avoid sharing their difficulties with peers and university teachers, an attitude which in turn precludes the possibility of improvement and isolates these students. Our data suggests this point is really important and the main difference in behaviour between dropout students and students who successfully progress in their studies. Successful students manage to overcome the isolation caused by early failures and difficult experiences, while students who drop out of their studies remain isolated until their situation is so unbearable that dropping out seems to them to be the only solution. Indeed, we see in our data that overcoming shame and sharing difficulties reduces the emotional charge experienced by the students, enabling them to perceive these difficulties as surmountable and to focus on the new cognitive demands of learning mathematics at university level. Therefore, we believe that the university experience in mathematics requires a deep reconstruction of the students' perception of mathematics, of the disposition towards the subject, and of their perceived competences. Emotional factors should be taken into account to make the STT easier, for example by promoting reflection on the change in the context, on the acceptance of the difficulties and of a possible new mathematical identity.

Our study is ongoing, and the next objective is to investigate the role of the higher educational context (and therefore also the role of cultural factors) in this transition. We have started to collect the data following the methodology of the Italian study in two other countries with very different higher educational systems: Switzerland and the U.K. We ask in this new part of the study what the institutional features that facilitate and hinder this transition are, whether the experiences of students in very different educational systems can be compared and whether these experiences have common features.

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The Italian Society for Industrial and Applied Mathematics

Luca Formaggia (Politecnico di Milano, Italy) and Nicola Bellomo (Politecnico di Torino, Italy)

The Società Italiana di Matematica Applicata e Industriale (SIMAI), Italian Society for Industrial and Applied Mathematics in English, was founded on December the 20th, 1988 by Vinicio Boffi, Antonio Fasano and Alberto Tesei, and it has been operative since 1989.

It was a historical period in Italy with a strong impulse to research in applied mathematics, and the need for a collective organisation of the Italian community of applied mathematicians, both from the academic and industrial sectors, was strongly felt. It was decided that the managing board of the new Society would be formed by four members from academic and four from non-academic institutions to favour interactions and joint initiatives.

Vinicio Boffi (1927–2010), first President of SIMAI

Alberto Tesei, professor of analysis at the University of Rome "La Sapienza" and member of Accademia dei Lincei (the Italian Academy of Science), managed the Society until 1990, when the late Vinicio Boffi (1927–2010) was officially elected as the first president of SIMAI. Vinicio Boffi was a professor of mathematical physics at the University of Rome "La Sapienza", with primary research activity in kinetic theory and plasma physics, and a keen interest to applications of mathematics. He kept the presidency for two terms, until 2002, and was followed in that role by Mario Primicerio, Nicola Bellomo and Luca Formaggia.

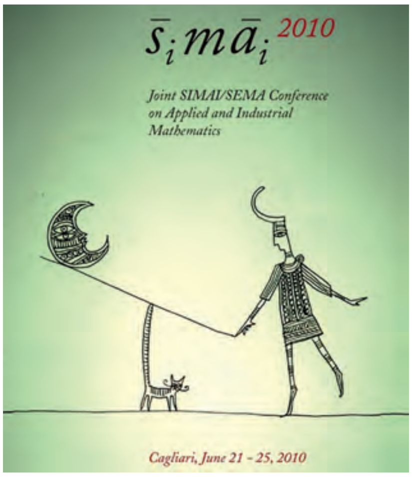
The Society currently consists of around 350 members and collaborates with several other mathematical societies on an international and national level. In particular, it is a member of ECCOMAS, ICIAM and EMS and has a reciprocity agreement with SIAM. It has promoted the foundation of the Italian Federation for Applied Mathematics (FIMA), which combines, besides SIMAI, the other five Italian Associations (AIRO, AMASES, AIMETA, AICA, AILA) covering operations research, mathematical finance, computational mechanics, computer science and mathematical logic.

The primary mission of SIMAI has always been to foster active interaction among universities, research institutions, industries and schools in various fields of applied mathematics across a wide range of applications, including not only technological but also biomedical, economic and social sciences. This objective has been pursued in the last years by operating in several directions:

The former SIMAI President Mario Primicerio together with the Presidents of AIRO and AMASES announcing the foundation of the Italian Federation of Applied Mathematics Societies FIMA.

Biennial SIMAI congress and workshop organisation

Since 1992 SIMAI has organised a congress every two years which is open to international participants, with plenary talks given by key figures from the academic and industrial world and thematic sessions covering a wide range of applied and industrial mathematical topics. Special sessions are dedicated to specific aspects of industrial mathematics. For instance, at the latest congress in Rome, there was a special session on the application of mathematics to sport. The number of participants at the last congresses has reached a number between 400 and 500, and the SIMAI congress is now the most important local event for Italian applied mathematicians. In 2010, it was organised together with SEMA, the Spanish Society for Industrial Mathematics. Beside the biennial congress, SIMAI is active in hosting and sponsoring workshops and events. Notable events in the past



The cover of the booklet of SIMAI10 Biennial Congress, jointly organised with the Spanish Mathematical Society SEMA.

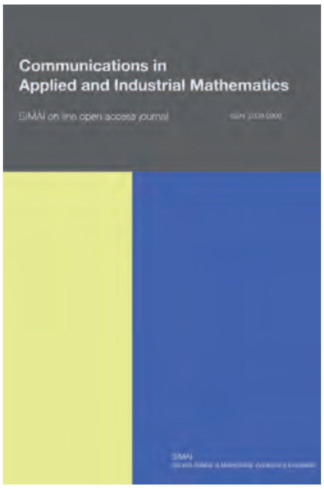
few years were the joint meeting with the Italian Mathematical Union and the Polish Mathematical Society PTM, held in Wroclaw, Poland and the conference "Mathematics for Industry 4.0", with the participation of speakers from industry and academia. Recently, a special workshop was organised in Milan to celebrate thirty years since the foundation of SIMAI, with the involvement of representatives from the leading societies of applied and industrial mathematics in Europe and ICIAM.

Prizes and awards

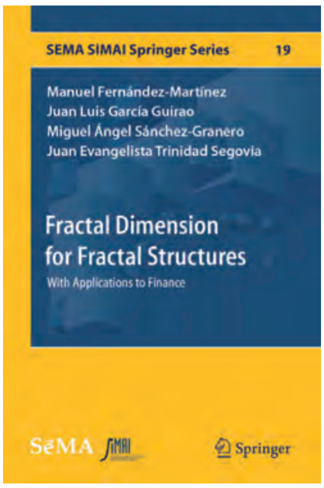
The SIMAI Prize, recently renamed "Fausto Saleri Prize", and awarded every two years, is the most prestigious prize awarded by the association. The winner is chosen among young members who have given outstanding contributions to applied mathematics. Beside this main prize, SIMAI awards prizes to junior applied mathematicians, and participates with the Italian Mathematical Union (UMI) and the Istituto Nazionale di Alta Matematica "Francesco Severi" (INdAM) for a prize awarded for the best PhD thesis in pure and applied mathematics in Italian universities. The collaboration with ICIAM has been strengthened with the co-sponsorship of the prestigious Lagrange Prize.

Editorial activity

Communications in Applied and Industrial Mathematics (CAIM) is the official journal of SIMAI. The journal focuses on the applications of mathematics to the solution of problems in industry, technology, environment, cultural heritage and natural sciences, and is now indexed in Scopus. As a joint initiative with the Spanish Society of Applied Mathematics (SEMA) and Springer, the SEMA-SIMAI Springer Series was established to publish advanced textbooks, research-level monographs and collected works that focus on applications of mathematics to social and industrial problems, including biology, medicine, engineering, environment and finance. This series, which now numbers more than twenty volumes and is indexed in Scopus, is meant to provide useful reference material to students, academic and industrial researchers at an international level.



The cover of CAIM.



The cover of a volume of the SEMA-SIMAI Springer Series.

Dissemination and outreach activities

Since 2008, SIMAI sponsors and hosts the site Madd Maths (http://maddmaths.simai.eu), dedicated to the dissemination of mathematics and broadcasting through social media mathematical news to the general public, with particular attention to secondary school teachers. MaddMaths was an initiative by Roberto Natalini, a former member of the SIMAI Board, and now counts an editorial board of 25 members and many more contributors. In the last 15 years other associations (UMI, AIRO and recently AILA) have also joined SIMAI in sponsoring this endeavour, which is now an invaluable reference that reaches an audience well beyond that of professional mathematicians. The attention to secondary school teachers of mathematics has been strengthened in the last years, in the spirit of contributing to a renewal of mathematical education in Italy. Dedicated sessions were organised at the latest SIMAI biennial congresses, with the broad active participation of teachers.

Since 1980 Nicola Bellomo has been a professor at Politecnico of Torino, Italy. His scientific activity covers nonlinear partial differential equations and modelling complex systems like traffic and crowds. He has coordinated several European research projects and delivered various distinguished lectures. He has been awarded the third level honour for scientific merits by the president of Italy, and since 2014 he has been ranked as a Highly Cited, Influential Mind by Clarivate, WEB of Science.

Luca Formaggia is the current president of SIMAI. After five years in the major Italian aerospace industry, he became head of the CFD unit at CRS4, a research centre in Cagliari, Italy to then move to EPFL as the first assistant to the chair of modelling and scientific computing and, finally, to Politecnico di Milano where he is professor of numerical analysis. His principal research interests are the numerical analysis of PDEs, multiphysics problems, scientific programming and industrial applications of mathematics.

ICMI Column

Merrilyn Goos (University of Limerick, Ireland), Vice President of ICMI

ICMI Statement on Evaluation of Scholarly Work in Mathematics Education. A call for comments

At the ICMI executive committee meeting held in Geneva in March 2017, it was noted that ICMI had been approached to inquire whether our organisation has an official stance regarding use of citation indices as the basis for evaluation and promotion of scholars in academic positions. A suggestion arising from that meeting was that ICMI could refer to the recommendation on the evaluation of individual researchers in the mathematical sciences that had been issued by the International Mathematical Union (IMU) (available at https://www.mathunion.org/fileadmin/IMU/Report/140810_Evaluation_of_Individuals_WEB.pdf)

A similar document based on the same considerations has now been developed by ICMI. We invite all members of the ICMI community to read this document (see below) and send us any comments by 30 September 2019. Please email comments to ICMI vice president Merrilyn Goos at merrilyn.goos@ul.ie. The final version of this document will then be published on the ICMI website.

Evaluation of scholarly work in mathematics education

Evaluating the quality and impact of scholarly work in all academic disciplines has become an increasing concern of universities as well as many national governments. However, generic evaluation processes do not always take into account discipline-specific norms for conducting and publishing research and other forms of scholarly work undertaken to influence practice or policy. Even within the global field of educational research there exist various sub-fields that take different approaches to theory, method and dissemination of findings.

Concerns about the need to improve the evaluation of scholarly work have led to the formulation of various statements and recommendations that are either specific to a discipline[1] or applicable to all research fields.[2] The purpose of the present document is to consider the question of how to evaluate scholarly work in the specialised educational sub-field of mathematics education. It sets out ICMI's position on evaluation of individual researchers in mathematics education.

This document is organised around three questions, with brief responses set out below that are elaborated in subsequent sections:

1. What is being evaluated and for what purpose?

- Individuals or institutions? Research output or other forms of scholarly work?
- For decisions about hiring, promotion and tenure?
- For decisions about institutional resource allocation and continuation or cessation of funding for research centres or institutes?

2. What problems arise in evaluating scholarly work in mathematics education?

- Mathematics education research journals are not adequately represented in citation databases.
- Journal citation metrics are improperly used as an indicator of article quality.
- Predatory publishers exploit inexperienced researchers.
- Evaluation focuses on too narrow a range of scholarly work.

3. What solutions can be proposed?

- Promote alternatives to citation-based evaluation systems.
- Develop ways of evidencing research impact as well as research quality.
- Broaden the scope of evaluation to include scholarly activity that influences educational practice and policy.

1. What is being evaluated and for what purpose?

Academics employed in universities are expected to devote some of their time to evaluating the scholarly work of other *individuals*, for example, by reviewing journal manuscripts, conference papers and grant applications, examining research students' theses, or assessing academic performance to inform decisions about hiring or promotion. Expert peer review is universally recognised as being fundamental to research evaluation, since only experts in a field can judge the significance and originality of a piece of research or the quality and relevance of the publication outlets in which the findings are disseminated.

Research evaluation can also be used to judge the performance of higher education *institutions* with the goal of providing accountability for public spending on research. Some countries (e.g., the UK, Australia, New Zealand) conduct regular national research evaluation exercises that typically place most emphasis on publication quality, with scores or ratings being assigned to either individual academics or discipline-based units of assessment within each institution.[3] Judgments about

[1] See the IMU (2014) statement on evaluation on researchers in the mathematical sciences.

[2] See the San Francisco Declaration on Research Assessment (DORA, n.d.) – a worldwide initiative covering all scholarly disciplines and all key stakeholders including funders, publishers, professional societies, institutions and individual researchers.

[3] For more information, see https://www.ref.ac.uk/about/ (UK), https://www.arc.gov.au/excellence-research-australia (Australia), https://www.tec.govt.nz/funding/funding-and-performance/funding/fund-finder/performance-based-research-fund/ (New Zealand).

research quality may be made on the basis of expert peer review or bibliometric data, or some combination of these.

Evaluation of the scholarly work of individuals or institutions is a high-stakes enterprise with significant implications for career progression and academic reputation, and sometimes for the selective allocation of institutional research funding. It is therefore essential to use valid measures that not only capture the distinguishing features of quality in a specific discipline, but also avoid perverse consequences that might lead to "gaming" of the evaluation system and thus distortion or undermining of research goals.

2. What problems arise in evaluating scholarly work in mathematics education?

Research evaluation depends largely on assessment of the quality of research outputs. In mathematics education, papers in peer-reviewed journals are typically the most highly regarded form of publication. Evaluation of such outputs can be either quantitative, relying on various forms of bibliometric analysis using citation data, or qualitative, relying on expert peer judgment.

A major limitation of citation-based systems for evaluating journal quality is the limited coverage they give to mathematics education journals. Nivens and Otten (2017) compiled a list of 69 journals that have an explicit focus on mathematics education research, but found that only six appeared in the Web of Science database from which journal impact factors are calculated. They concluded that Web of Science is of little value to mathematics education, despite its widespread use to measure scholarly output in other disciplines. A further limitation of all three major journal ranking systems – Web of Science (Impact Factor, IF), Scopus (Scopus Journal Ranking, SJR), and Google Scholar (h5-index) – is that they only trace citations within their own databases, thus excluding the vast majority of mathematics education journals.

Nivens and Otten (2017) warn of a further problem: when journal citation metrics are improperly used to draw conclusions about the impact of articles published in particular journals. They show that there is little correlation between a journal's citation-based measures of impact (such as IF) and the number of citations received by articles published in that journal. Yet journal impact measures and rankings are often used – inappropriately – in making decisions about tenure and promotion of individual academics.

Evaluations based on so-called "objective" quantitative methods are not inherently more reliable than expert human judgments. Williams and Leatham (2017) cautioned against giving too much credence to citation analysis in mathematics education, noting that "at a minimum, the literature raises questions of whether citation-based indices are valid and meaningful in our field and how they compare with other ranking methods" (p. 372).

Despite the significant problems outlined above, citation-based measures are increasingly being used to compare and rank individual academics or even entire academic departments and disciplines. Such ill-advised evaluation practices can have perverse consequences. For example, researchers whose universities evaluate their performance on the basis of journal impact factors or quantitatively derived rankings can be exploited by predatory publishers that promise fast peer-reviewing without the full editorial and publishing services of a legitimate journal. Early career researchers, doctoral students and academics in developing countries are especially vulnerable to these unethical practices.

A different kind of problem that arises from attempts to evaluate scholarly work in mathematics education concerns the practice-engaged nature of our field (Nivens & Otten, 2017). Thus citations in scholarly journals are not the only way of measuring impact; in addition, researchers in mathematics education value dissemination of their scholarship in practitioner journals, through teacher education and professional development work and by influencing education policy development.

3. What solutions can be proposed?

Recommendation 1

ICMI does not support reliance on only quantitative measures of research quality, and in particular citation analyses, to evaluate scholarly work in mathematics education. ICMI supports the IMU's (2014) argument that "nothing (and in particular no semi-automatised pseudo-scientific evaluation that involves numbers or data) can replace evaluation by an individual who actually understands what he/she is evaluating". Education in general and mathematics education in particular are grounded in diverse cultures and social contexts. Yet the richness and effectiveness of the mathematics education communities worldwide depend on this diversity.

Evaluating the contributions of individual researchers to advancing knowledge therefore requires different and complementary approaches in order to do justice to these complexities. At the very least, any quantitatively-based rankings of journals should be supplemented with qualitative judgments informed by the expert survey of journals conducted by Williams and Leatham (2017).

Recommendation 2

Analysis of journal citation data leads to flawed measures of academic impact. Alternative impact measures are being developed in some countries, where impact is defined in terms of "the demonstrable contribution that research makes to the economy, society, culture, national security, public policy or services, health, the environment, or quality of life, beyond contributions to academia" (Australian Research Council, 2012).These broader measures of impact should be included in any evaluation of scholarly work in mathematics education.

Recommendation 3

Following on from the previous recommendation, ICMI supports broadening the scope of evaluation of scholarly work to recognise academic activities that influence practice and policy in mathematics education.

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ERME Column

Paola Iannone (Loughborough University, UK) and Jason Cooper (Weizmann Institute of Science, Israel)

ERME Thematic Working Groups

The European Society for Research in Mathematics Education (ERME), holds a bi-yearly conference (CERME), in which research is presented and discussed in Thematic Working Groups (TWG). We continue the initiative of introducing the working groups, which we began in the September 2017 issue, focusing on ways in which European research in the field of mathematics education may be interesting or relevant for research mathematicians. Our aim is to extend the ERME community with new participants, who may benefit from hearing about research methods and findings and who may contribute to future CERMEs.

Introducing CERME's Thematic Working Group 21 – Assessment in Mathematics Education

Group leaders: Paola Iannone, Michal Ayalon, Johannes Beck, Jeremy Hodgen and Francesca Morselli

TWG21 is concerned with the role of assessment in the teaching and learning of mathematics at all educational levels and has so far met twice, at CERME10 (Dublin, IRL) and CERME11 (Utrecht, NL). Given the importance that mathematicians, researchers in mathematics education, students and teachers ascribe to assessment it is surprising that, prior to CERME10, there had been no TWG dedicated to this theme since 2001. Instead, the assessment of mathematics had been previously discussed in other working groups such as TWG14 for assessment at university level, TWG15–16 for computer aided assessment, and many others. With TWG21 we intend to offer our communities a forum to focus specifically on the assessment of mathematics and to gauge what are the issues that most concern our communities when talking about assessment.

As TWG21 is a new TWG, we have intentionally kept the brief for the paper submissions very broad, including any type of assessment at any educational level. At CERME11 we received 14 research papers and three posters representing a wide variety of methodologies and foci. Papers presented in TWG21 have reported both large quantitative studies on the validity and reliability of standardised tests in school settings as well as small qualitative case studies of the impact of formative assessment on student learning at university level. The importance of focusing on assessment originates from the pervasive impact that assessment has on the learning of mathematics at all levels. For example, what we assess indicates to the students what we value, and the mode in which we assess our students can change the way in which they interact with the mathematics we teach. Indeed, students may engage superficially with mathematics learning if they perceive the assessment to require only memorisation.

When thinking about assessment, the first definitions that come to mind are those of *summative assessment* and *formative assessment*, as posed for example by Wiliam and Black (1996). In this framework, summative assessment is the assessment that has a feed-out function: results of summative assessment are used for certification, to progress through educational stages or to enter the workplace, while formative assessment has a feed-in function in that it informs subsequent teaching and learning and it is characterised by feedback. Indeed, formative assessment is an integral and necessary part of the teaching and learning cycle and supports students and teachers in bridging the gap between actual achievement levels and desired achievement levels (Knight, 2010).

The way in which both formative and summative assessment impact and provide information about student learning has been one of the central issues of discussion in TWG21. More specifically, four topics recurred in both the TWG21 meetings: the design, purpose and use of large-scale standardised tests; the implementation, affordances and drawbacks of computer-aided assessment (CAA), especially at university level, aspects of assessment that are germane to mathematics, e.g. how to best assess procedural and/or conceptual understanding in mathematics, and the impact of assessment on students' engagement and teachers' actions at all educational levels.

There are at least two aspects of the work of this group that are of relevance to university mathematics. The first is the discussion on the issues which are germane to assessing mathematics, which also links to the use of CAA. The papers discussed as part of these themes addressed both the nature of the reasoning that can be assessed by CAA and the way in which CAA systems can provide tailored feedback to students. This is of particular relevance to university mathematics because, in this setting, classes can be very large and assessment very time-consuming. The research in this field so far indicates that there are CAA systems which are suitable for the assessment of mathematics at university level when the assessment is of procedural proficiency. Some of these systems, as the ones presented in the papers of TWG21, can also offer formative feedback tailored to students' responses. The possibility of obtaining formative feedback makes these systems suitable for formative tasks which can also be very time-consuming for large classes. The ready availability of the outcomes of the formative tasks may allow university teachers to review such tasks and take into consideration the outcomes for subsequent teaching.

The second aspect relevant to assessing university mathematics is the investigation of affordances and drawbacks of assessment methods, both formative and summative, which are different from the standard closed book, timed written exam. The closed book exam is ubiquitous in university mathematics across all countries, but increasingly those who teach mathematics are encouraged to introduce small-scale assessment innovations for their students to include some variety. Part of the TWG21 work could be a discussion regarding the effects of these small assessment innovations on the students' experience, both in terms of what reasoning skills are assessed by these new methods and what the impact is of the new assessment on student engagement, both with the mathematics and more generally with their university studies. Papers of this sort could report on evaluation of assessment innovations designed collaboratively between mathematicians teaching the courses and mathematics educators, and could help our communities to understand the role of small evaluations of assessment interventions and how to design them. Studies like the ones outlined above also foster the much needed collaboration between mathematicians and mathematics educators.

TWG21 will meet for the third time in Bolzano, Italy, at CERME12. We are looking forward to consolidating our work, and hope to attract mathematicians, as well as mathematics education researchers, to contribute to the work of the group.

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Paola Iannone's photo and CV can be found on page 47 in this Newsletter issue.

Jason Cooper's photo and CV can be found in previous Newsletter issues..

Mathematical Research Data – An Analysis Through zbMATH References

Klaus Hulek (Gottfried Wilhelm Leibniz Universität Hannover), Fabian Müller (FIZ Karlsruhe, Berlin, Germany), Moritz Schubotz (FIZ Karlsruhe, Berlin, Germany), Olaf Teschke (FIZ Karlsruhe, Berlin, Germany)

Are there mathematical research data?

In fitting with our data-driven age, research data have become an increasingly important aspect of scientific work. In Germany, the Federal Ministry for Education and Research has launched a program to build a national research data infrastructure. Correspondingly, the DFG issued a call to form consortia dealing with the management of such research data. Within the German mathematical community a proposal to establish MaRDI (Mathematical Research Data Initiative) is prepared [1]. One may initially wonder what the mathematical equivalent of the vast amount of LHC measurements or data from clinical trials might be. Indeed, as one of the driving forces of storing research data has been the reproducibility crisis in several fields, one may ask whether storing research data is relevant to our subject at all, since mathematical results usually come with an inherently much higher level of confirmability than those connected with empirical scientific methods.

However, reproducibility is just one aspect connected to research data, and perhaps not even the most important one in the future. Storing and sharing research data according to the FAIR principles (Findability, Accessibility, Interoperability, Reusability) generates several benefits for mathematicians (as for all scientists):

1. Improved citability: work that does not fit the classical format of journal articles or books should still be adequately acknowledged and cited when used as a basis for further work.
2. Better findability: appropriate data repositories (ideally, intrinsically cross-linked with each other, as well as the literature) would enable mathematicians to easily identify prior results on a different level rather than just entangled in the context of an article.
3. Confirmability: For appropriate peer review, computational results must be available to redo the computations, or provide a way to confirm the correctness of the results.
4. Reusability: research data should be available in a form that facilitates building upon these results in a manner that is as efficient as possible. This also prevents the unnecessary repetition of work and uses human resources and available publication space more efficiently.
5. Long-term preservation: storage of research data in a dedicated infrastructure framework ensures that its longevity is independent of individuals or institutions.

How urgent are these aspects for mathematics? Historically, our subject has been the origin of arguably the most frequently used data: from Babylonian multiplication tables to Greek and Indian tables for sine values to the logarithmic tables ubiquitous for calculations until the second half of the 20th century. While computers have made such tables obsolete, they also generate a vast landscape of new resources. Today, mathematical research data may still derive from tables like collections of special functions, algebraic representations or combinatorial data, but likewise exist as libraries of formalised mathematics or be generated by extensive computations involving computer algebra systems or numerical simulations. Based on zbMATH references, we will derive a rough heuristic of the current usage and discuss some examples.

A heuristic analysis of possible research data references

In this section, we report on the current status of our preliminary investigations. A more in-depth analysis is in preparation.

The zbMATH database [2] currently contains more than 30M references. Of those, currently 53.7% link back to other publications that are indexed in zbMATH. Other references are out of the scope: overall, 36.7% have a DOI and 10.9% have a DOI, but not one connected to a publication within zbMATH. One can estimate from this that more than 75% of references are connected to the published literature. Moreover, much of the rest consists of literature available at the arXiv, other repositories, or personal homepages.

We used the following heuristic to detect links to non-literature online resources. There are about 795,000 references containing a (‘http’, ‘www.’, ‘ftp’) link to a website. Excluding the most common patterns to literature repositories leaves us with about 161,000 links. Of these, 20,518 are links to mathematical software as identified in the swMATH database [3]. For the remaining 141,000 references, we identified 3 common link patterns: references to mathematical online compendia such as the Online Encyclopedia of Integer Sequences [4], references to normative data like standards or benchmarks, and references to community-maintained websites such as Wikipedia or MathOverflow. There is a large variety of different links included, and it becomes clear that there is an extremely long tail of specific data used in relatively few publications. Although we did not yet identify a suitable method to classify the links automatically into reasonable categories, the general structure of the sample analysed in [5] could be confirmed. To give an impression, we will present some examples in the following.

Examples

Singular

While it is still debated whether software code should be considered as research data, its output certainly is. Here we will take the example of the computer algebra system SINGULAR [6], which is widely used and has been frequently cited in mathematical papers throughout the last two decades (http://purl.org/zb/1). Here, as for other mathematical software, we can employ the swMATH database to track its usage in mathematical papers, although it is frequently referenced in a rather diverse form, ranging from the direct weblink or the manual to the related book [7] (see [8] for the current status of standardisation for software citations). An analysis of these publications reveals that the involved computational results almost never exist in a fully FAIR form, although the initial additional effort would likely pay off greatly in the long term.

This appears to be a general issue for computational results: The recent article [9] demands (emphasizing the reproducibility aspect) that results should be reproducible in identical, and comparable to runs in varied, settings. For long-running computations, this involves in particular the explicit saving of intermediate states (checkpoints). This involves among other things an exact specification of the computing environment used (software, libraries, versions, etc.) and the possibility for the full publication of all relevant entities (i.e. code/algorithms together with input datasets and results). Overall, while mathematics already enjoys an appropriate service to interlink information on the used software via swMATH, the task of adequately documenting the computational output still needs to be addressed.

DLMF

The NIST Digital Library of Mathematical Functions (DLMF, [10]) is among the most frequently cited collections identified through the above approach. It is the successor of the *Handbook of mathematical functions with formulas, graphs and mathematical tables* [11], which is currently the most cited document in zbMATH (http://purl.org/zb/2) with about 10,000 citations gathered by its five different editions. In comparison, there are still much fewer references (about 1,500) to the electronic version recorded by the DLMF entry (http://swmath.org/software/4968), although referencing to a function or formula can be done much more precisely within the DLMF, as in the handbook. The attitudes to citing such data appear to be changing slowly, but steadily; the ratio of DLMF citations has increased in recent years. This is also confirmed by a recent study by the NIST library [12] based on citation data from the Web of science dataset, which obtained a similar pattern (cf., Fig. 1). According to the NIST data analysis and the assumption of a linear growth model, the DLMF will be cited more often than the printed book as early as 2028. As depicted in Figure 1,

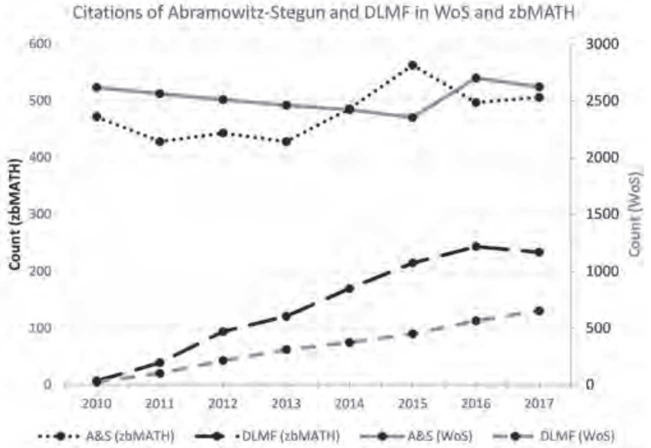

Fig. 1. Citations counts of the Handbook of mathematical functions and DLMF inWoS (according to the NIST library) and zbMATH. The citation counts of the online versions growwith a constant factor in contrast to the citation counts of the printed version

our data from swMATH confirm a linear growth of the citation rates of the DLMF project. Despite the prominent link to citation instructions (http://purl.org/zb/4) only fewer than 1% of all citations that DLMF received in the zbMATH database use a deep link to a chapter, formula or section.

This example illustrates that although the heuristic above may be helpful in identifying interesting datasets, the distinction "literature" vs. "data" may be extremely misleading, since many literature references may in fact be research data in disguise (a pattern that we also already noted in the relation between software and related publications). This can also be seen by the next example.

OEIS

The On-Line Encyclopedia of Integer Sequences (OEIS [4]) is a browsable and searchable online resource launched in 1996 that grew out of N.J.A. Sloane's 1973 book *A Handbook of Integer Sequences* [13]. Starting in 1994, there are 2,752 references to it in zbMATH (http://purl.org/zb/5).

Of these, more than 70% cite OEIS as a whole, while the remaining refer to one or, in about 5% of the cases, several actual entries of the database (with a single reference citing as many as 14 sequences in one case).

However, in contrast to the previous example, the references to the online service have quickly substituted those to the printed handbook (compare to http://purl.org/zb/6). The easy usability of the OEIS and its powerful search features (which benefit from the rather simple data shape of integer sequences) appear to be a crucial factor here, making it a model for highly findable, accessible, and reusable mathematical data. Nevertheless, interoperability remains an issue even for this resource. Currently, one can only dream of seamlessly cross-linking the generating functions of sequences in OEIS with respective entries in DLMF – a service which would open a whole new dimension of opportunities.

Calabi–Yau data

Lists of Calabi–Yau manifolds play a crucial role not just within mathematics, but due to their relation to string theory in physics. The data available at [14] are among the most prominent (although it is once more impossible to determine its real use, since related original publications are still as frequently cited as the data itself, see http://purl.org/zb/7). They also form a model case in the sense that both the software and the computational output were made available in a transparent, reusable form. However, this static page also illustrates an urgent issue manifest for many research data. Due to the untimely death of its creator, it has remained in a frozen state ever since, and its status with respect to sustainability is completely unclear (which is only underscored by several links to further Calabi–Yau sites which have partially ceased to exist). Many examples of such valuable resources in a potentially precarious state exist throughout the references and underscore the need for a more sustainable framework.

Further resources, big data vs. deep data, interdisciplinary issues

The reader is free to explore further examples by analysing our dataset of non-literature references generated by the procedure described above) available at github (http://purl.org/zb/8), e.g., by checking for entries collected in the catalogue of mathematical datasets [15]. As indicated by the discussed examples, mathematical research data are typically no "big data" of many terabytes (although there exists, e.g., the rather large collection of finite lattices [16]) but come along with highly diverse and sophisticated descriptional metadata, necessary to facilitate their FAIR usage. In this sense, mathematical metadata are rather "deep data" [19], which would require extensive semantic enrichment before they could be properly cross-linked with each other and the literature, finally leading to a framework from which a mathematician could benefit in everyday work. The vision of a Global Digital Mathematics Library [17] can be understood as such an infrastructure.

Another important aspect is, of course, interdisciplinarity. Mathematics, as the language of exact sciences, is naturally connected to other disciplines, which have their own collections of research data. These are often of a different nature, and are preserved according to the standards of the discipline. Large genome or medical datasets may also be of interest for mathematical work, but are associated with quite different legal and computational aspects. One may even ask whether a precise definition of mathematical research data is possible; certainly, the distinction is not always as clear as between Calabi–Yau data (mathematical) and LHC measurements (physical) in high-energy physics.

Mathematical modelling and simulation are now omnipresent in many sciences, and the related computations open up a whole new dimension of interdisciplinary research data [18]. Hence, a FAIR framework for mathematical research data would also require interfaces to application areas potentially dealing with them.

Conclusion and future work

Research data are widely used within mathematics, and their sustainable storage and FAIR availability will very likely become an important issue in the future. The requirement of an utmost level of confirmability for mathematical results in connection with the growing importance of computer aided computations and proofs will almost certainly be a driving force in establishing standards which should eventually lead to an interconnected, powerful infrastructure. However, the amount of work required to reach this goal is substantial: mathematical research data exists in very different forms, from small databases through to diverse software and its output to huge amounts of data, some of them created in collaboration with other sciences. Currently, they are not even always referenced in a transparent manner, but are often intrinsically connected to the literature. Building a framework that cross-links the various types of mathematical research data will require substantial metadata and semantic enrichment, enabling them to serve as "deep data" in an infrastructure facilitating new research dimensions, not just within mathematics but also its applications.

To achieve this goal, we at zbMATH are investigating diverse approaches: For one, we analyze citation data and mathematical formulae to identify similar (or even plagiarized) content [25]. Moreover, we connect our datasets to external datasets such as Wikidata or MathOverflow [23, 24]. Additionally, after having switched to a LaTeX the input format for zbMATH reviews [22], we are considering to allow for semantically enriched LaTeX dialects as used in the DLMF and DRMF [21] projects, or optional semantic annotations for mathematical formulae via graphical tools [20].

Acknowledgements

We thank the DLMF editor Ronald Boisvert for sharing the DLMF citation analysis with us.

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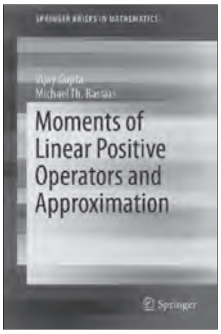
Pictures and CVs of the authors can be found in previous Newsletter issues.

Book Reviews



Vijay Gupta and
Michael Th. Rassias
Moments of Linear Positive Operators and Approximation
Springer, 2019
96 p.
ISBN 978-3-030-19455-0

Reviewer: Dorin Andrica

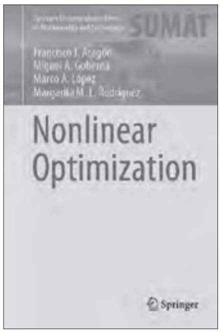
The well-known approximation theorem of Weierstrass on polynomial functions sparked the interest of researchers towards the study of convergence of polynomial functions. Linear positive operators constitute one of the active areas of research in approximation theory, functional analysis, ODEs and PDEs, linear algebra, engineering as well as in Physics. In this domain, Korovkin's theorem plays an important role. For linear positive operators, if the moments are known one can easily study the convergence of these operators or can obtain some sharp estimates of higher order moments.

The present monograph is certainly a valuable source of classical and modern results for graduate/post graduate students and research mathematicians who are working on or are interested in problems relevant to the convergence behaviour of linear positive operators. Actually, moments are essential for the investigation of the convergence of a sequence of linear positive operators.

The first two chapters of the present monograph present a series of moments of several known operators. Various methods consisting of applications of forward differences, Stirling numbers and hypergeometric series are presented within this nice publication, so that the reader can potentially apply these methods and theories to obtain higher order moments as well. The monograph under review deals with discretely defined operators, some of which are of exponential type whose basis function satisfies a certain differential equation. They include Bernstein polynomials, Baskakov operators, Szasz operators, Post–Widder operators, Ismail–May operators, to mention just a few. As such, not all operators discussed in the present monograph are of exponential type. Furthermore, more emphasis is given on certain integral operators of Durrmeyer type. Hybrid operators are also nicely discussed and their moments are presented. These operators were introduced and studied during the last five decades.

In the last chapter, a variety of results which include preservation of exponential functions of Baskakov–Szasz–Mirakyan operators, preservation of general higher order moment of Post–Widder operators, combinations, a modified form of certain operators and differences of two operators, are very well surveyed with fundamental theories and recent developments. Examples with a detailed discussion are included to assist readers in their effort to familiarize themselves with the subject. This monograph is written in a very clear and accessible style. It serves as well as an excellent reference source. By reading this monograph, one can be introduced in this vibrant area of research and gradually start working in this direction.

Dorin Andrica's photo and CV can be found on page 59 in Newsletter 108, June 2018.



Francisco J. Aragón,
Miguel A. Goberna,
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Margarita M. L. Rodríguez
Nonlinear Optimization
Springer, 2019
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Reviewer: Michel Théra

The present text has grown out of notes from courses taught by the authors at the university of Alicante for upper-level undergraduate students. The contents are organized into two parts, essentially independent, and the book is positioned as a comprehensive introduction to nonlinear optimization for undergraduate students of mathematics and statistics and graduate students of industrial engineering whose basic knowledge is mainly differential and matrix calculus. The material presented is very well written and structured and richly illustrated by numerous examples, nice illustrations and well-chosen exercises with detailed solutions, making the presentation easy to follow. This textbook will be an excellent choice for a comprehensive introduction to nonlinear optimization, since it gives a good picture of the fundamentals and the numerical methods used in the field of optimization.

The book consists of 6 chapters and starts with a preface containing historical background about optimization theory. Chapters 1 and 2 contain the basic ingredients for the calculus of local minima (optimality conditions for differentiable functions) and global minima (coercivity and convexity) for unconstrained optimization problems. Chapter 3 provides formulas for unconstrained optimization problems arising in various areas, while Chapter 4 deals with unconstrained and constrained convex optimization problems for which local and global minima coincide.

Part II is focused on the numerical calculation of local optima in problems whose solutions cannot be analytically obtained, and it consists of two chapters. Chapter 5 deals with standard algorithms for unconstrained optimization problems, such as the steepest descent method, Newton's method and variants (trust regions, Gauss–Newton, Levenberg–Marquardt) and other gradient-based methods such as those using conjugate directions (conjugate gradient and quasi-Newton). There are some competitive textbooks at this level, especially for the first part related to differential optimization. Chapter 6 presents, in the first part, an introduction to the so-called penalty and barrier methods. The second part of this final chapter is devoted to the optimality conditions for constrained optimization problems, with equality and/or inequality restrictions.

In summary, this textbook certainly provides a sound approach to non-smooth optimization and also really deserves to be on every teacher's bookshelf. The authors have earned our appreciation for their wonderful exposition that will be a great companion for an undergraduate optimization course. Despite the large number of competing books, I recommend this one enthusiastically.

Michel Théra is a professor emeritus of mathematics in the laboratory XLIM from the University of Limoges and Adjunct professor from Federation University Australia. He is presently Scientic co-Director of the Stampacchia School at the Ettore Majorana Foundation and Centre for Scientic Culture in Erice and has been president of the Society for Applied and Industrial Mathematics (SMAI). His research focuses on variational analysis, convex analysis, continuous optimization, monotone operator theory and the interaction among these fields of research, and their applications. He has published more than 110 articles in international journals and he serves as editor for several international journals on continuous optimization.

Solved and Unsolved Problems

Michael Th. Rassias (University of Zürich, Switzerland)

Newton has shown us that a law is only a necessary relation between the present state of the world and its immediately subsequent state. All the other laws since discovered are nothing else; they are in sum, differential equations..

Henri Poincaré (1854–1912)

The present column is devoted to Partial Differential Equations (PDEs). The study of PDEs has proved to have a tremendously wide spectrum of applications to various domains, from the study of black holes to mathematical finance. Such equations can be used to describe and quantitatively investigate various and diverse phenomena such as heat, sound, elasticity, fluid dynamics, quantum mechanics, etc.

I Six new problems – solutions solicited

Solutions will appear in a subsequent issue.

211. Recall that a smooth function $u : \mathbf{R}^2 \to \mathbf{R}$ is called harmonic if

$$\Delta u(x,y) := \frac{\partial^2 u}{\partial x^2}(x,y) + \frac{\partial^2 u}{\partial y^2}(x,y) = 0\,, \quad \text{for any } (x,y) \in \mathbf{R}^2.$$

Determine all harmonic polynomials in two real variables.

(Giovanni Bellettini, Dipartimento di Ingegneria dell'Informazione e Scienze Matematiche, Siena, Italia, and ICTP International Centre for Theoretical Physics, Mathematics Section, Trieste, Italy)

212 Reaction-diffusion systems of the form

$$u_t = Du_{xx} + g(u) + \mu M u, \qquad (x,t) \in \mathbb{R} \times (0,\infty),$$

where

$$u(x,t) \in \mathbb{R}^n,\ g_i(u) = r_i u_i \left(1 - \sum_{j=1}^{n} \alpha_j u_j\right),\ r_i, \alpha_i > 0,$$
$$i = 1,\ldots,n,\ \mu > 0,$$

and D and M are constant $n \times n$ matrices such that D is positive-definite diagonal and M has strictly positive off-diagonal elements and zero column sums, arise in the modelling of the population densities of n phenotypes of a species that diffuse, compete both within a phenotype and with other phenotypes, and may mutate from one phenotype to another. Denoting the Perron-Frobenius eigenvalue of a matrix Q by $\eta_{PF}[Q]$ and assuming that the n phenotypes spread together into an unoccupied spatial region at the μ-dependent speed

$$c(\mu) := \inf_{\beta>0} \eta_{PF}\left[\beta D + \beta^{-1}(\mathrm{diag}(r_1,\ldots,r_n) + \mu M)\right],$$

which is determined by the linearisation of the reaction-diffusion system about the extinction steady state $u = (0,\ldots,0) \in \mathbb{R}^n$, prove that spreading speed $c(\mu)$ is a non-increasing function of μ.

(Elaine Crooks, Department of Mathematics, College of Science, Swansea University, Swansea, UK)

213. Consider the second-order PDE with non-constant coefficients,

$$u_{xx} - x^2 u_{yy} = 0.$$

Find at least one family of solutions.

(Jonathan Fraser, School of Mathematics and Statistics, The University of St Andrews, Scotland)

214. Let u solve

$$(\Delta + 200^2 xy^2)u = 1$$

on the triangle $T = \{(x,y) : 0 < x < 1, 0 < y < 1 - x\}$ with zero Dirichlet conditions:

$$u(x,0) = u(0,y) = u(x,1-x) = 0.$$

What are the first 10 significant digits of $u(0.1, 0.2)$?

(Sheehan Olver, Department of Mathematics, Imperial College, London, UK)

215. Let u be an entire harmonic function in $\mathbf{R}^n$, satisfying $u(x) \geq -c(1+|x|^m)$ for some constants $c > 0$ and $m \in \mathbb{N}$. Show that u is a polynomial of degree less or equal to m.

(Gantumur Tsogtgerel, McGill University, Department of Mathematics and Statistics, Montreal, Canada)

216. Let $f : [0,\infty) \to (0,\infty)$ be a continuous function satisfying $f(x) \to 0$ as $x \to \infty$, and let

$$\Omega = \{(x,y) \in \mathbf{R}^2 : x > 0,\ 0 < y < f(x)\}.$$

Exhibit an unbounded function u in Ω, such that $u \in H^k(\Omega)$ for all $k \geq 0$. Here $H^k(\Omega)$ is the standard Sobolev space of functions whose partial derivatives of all orders up to k are square integrable.

(Gantumur Tsogtgerel, McGill University, Department of Mathematics and Statistics, Montreal, Canada)

II Open Problem. New rigorous developments regarding the Fokas method and an open problem

by A. S. Fokas (DAMTP, University of Cambridge, UK) and T. Özsarı[1] (Department of Mathematics, Izmir Institute of Technology, Turkey)

Initial-boundary value problems for nonlinear Schrödinger type equations

Consider the following nonlinear Schrödinger equation (NLS) on a domain $\Omega \subset \mathbb{R}^n$, $n \geq 1$ with $p > 0$, $\kappa \in \mathbb{R} - \{0\}$, $\sigma \in \{2, 4\}$, disregarding for the moment initial and boundary conditions (b.c.):

$$i\partial_t u + (-\Delta)^{\frac{\sigma}{2}} u + \kappa |u|^p u = 0, x \in \Omega, t \in (0, T). \tag{1}$$

This equation is the *classical* NLS when $\sigma = 2$ and the *biharmonic* NLS when $\sigma = 4$.

It is easy to see that if $\Omega = \mathbb{R}^n$, then

$$u_\epsilon(x, t) \doteq \epsilon^{-\frac{\sigma}{p}} u(\epsilon^{-1}x, \epsilon^{-\sigma}t)$$

defines an invariant scaling of the above equation. Namely, u solves (1) on $(0, T)$ iff u_ϵ solves (1) on $(0, \epsilon^\sigma T)$. Moreover,

$$|u_\epsilon(0)|_{\dot{H}^s_x} = \epsilon^{\frac{n}{2} - \frac{\sigma}{p} - s} |u(0)|_{\dot{H}^s_x}.$$

Therefore, if $s < s^* \doteq \frac{n}{2} - \frac{\sigma}{p}$, then both $|u_\epsilon(0)|_{\dot{H}^s_x}$ and the life span of u_ϵ vanish as $\epsilon \to 0^+$. This suggests that the problem is locally illposed for $s < s^*$ and locally wellposed otherwise, where local wellposedness is in the Hadamard's sense (existence, uniqueness, and uniform continuity with respect to data for some $T > 0$). It is generally easier to establish such results for $0 \leq s < s^*$ whenever $s^* > 0$ (L^2-*supercritical*) or for $s < s^* = 0$ (L^2-*critical*). For instance, at least for the focusing problems for NLS ($\sigma = 2, \kappa < 0$), one can simply reduce the problem to one of blow-up in arbitrarily small time in the case $s^* \geq 0$ by constructing a blow-up solution and rescaling it. However, if $p < \frac{2\sigma}{n}$, then $s^* < 0$, in which case an explicit blow-up solution cannot be constructed. Therefore, one wonders what is the range of s for which local wellposedness fails when $s^* < 0$ (L^2-*subcritical*). The answer to this question for the Cauchy problem in $\mathbb{R}^n$ is that wellposedness fails in H^s_x indeed for any $s < \max(0, s^*)$ [2]. This is proven by showing that the solution operator is no longer uniformly continuous. These observations (see [2] for further details) motivate us to consider the local wellposedness problem for (1) with respect to the above ranges also in the case of domains with a boundary.

If $\partial\Omega \neq \emptyset$, then (1) also requires appropriate boundary conditions (and compatibility conditions if s is sufficiently large that traces exist) for wellposedness to hold. Recent papers treating the half-space case $\Omega = \mathbb{R}^n_+$ ($n = 1, 2$) for the problem (1) obtained wellposedness for nonnegative s. For instance, in the one dimensional case with $\Omega = \mathbb{R}_+$, the natural space for the data of NLS subject to Dirichlet b.c. $u|_{x=0} = g$ turns out to be $(u(0), g) \in H^s_x(\mathbb{R}_+) \times H_t^{\frac{2s+1}{4}}(0, T)$, see for instance [3], [6], and [8]. On the other hand, this space for the biharmonic NLS subject to Dirichlet–Neumann b.c. $u|_{x=0} = g$, $u_x|_{x=0} = h$ becomes $(u(0), g, h) \in H^s_x(\mathbb{R}_+) \times H_t^{\frac{2s+3}{8}}(0, T) \times H_t^{\frac{2s+1}{8}}(0, T)$ [7]. In the two dimensional case, the spaces for boundary data turn out to be of Bourgain type [1], [5].

One of the effective methods for the treatment of the above half-space problems is the so-called *Uniform Transform Method* (a.k.a. Fokas method) [4]. It has been shown by many researchers that the Fokas method is a powerful tool for solving initial – (inhomogeneous) boundary-value problems. Although this method was initially introduced for obtaining formal representation formulas for solutions, it has been shown recently that it can also be used to obtain rigorous wellposedness results in the fractional Sobolev and Bourgain spaces. Initially, nonlinear dispersive partial differential equations (PDEs) with power type nonlinearities such as NLS were treated at the high regularity level with this method by obtaining estimates in the $L^\infty_t H^s_x$ norm with $s > 1/2$, see, e.g., [3] and [5]. In this setting, H^s_x becomes a Banach algebra (i.e., $|uv|_{H^s_x} \lesssim |u|_{H^s_x}|v|_{H^s_x}$) and therefore handling the nonlinearities via contraction is relatively easier. Unfortunately, in the low regularity setting $s \leq \frac{1}{2}$, H^s_x looses its algebra structure and estimates in the $L^\infty_t H^s_x$ norm are not good enough for performing the associated nonlinear analysis. The classical method in the theory of nonlinear dispersive PDEs for dealing with this difficulty is to prove Strichartz type estimates which measure the size and decay of solutions in mixed norm function spaces $L^q_t W^{s,r}_x$, where (q, r) satisfies a special *admissibility* condition intrinsic to the underlying evolution operator. However, proving these inequalities for inhomogeneous initial boundary value problems is generally more difficult than proving them for the corresponding Cauchy problems on the whole space $\mathbb{R}^n$. It is well known that Strichartz estimates holding on $\mathbb{R}^n$ may fail on a general domain $\Omega \subset \mathbb{R}^n$ or on a manifold M with or without boundary and some loss in regularity is indispensable even in nice and smooth geometries. Researchers have used quite technical tools in order to prove these estimates for inhomogeneous initial boundary value problems even in low dimensional settings. The second author has recently shown, in connection with the biharmonic NLS [7], that the kernel of the integral formula obtained by the Fokas method representing the solution has a nice space-time structure for applying the elementary tools of harmonic analysis such as *Van der Corput lemma* to prove decay properties in the time variable, which eventually yields necessary Strichartz estimates. The time decay of the kernel in Fokas's integral formula for the solution of the boundary value problem can also be used to prove Strichartz estimates for a wide range of dispersive PDEs, at least in the half-space case.

The literature mentioned above on the local wellposedness for the inhomogeneous boundary value problems for the classical NLS in fractional spaces covers the half-space case in dimensions $n = 1, 2$ and the finite interval case $\Omega = (0, L)$ in dimension $n = 1$. In the latter case, it was found that the boundary data must be taken from $H_t^{\frac{s+1}{2}}(0, T)$ in order to establish the local wellposedness at the level of $H^s_x(0, L)$ [8]. One observes that boundary input was associated with a smoother space compared to the half-line problem in order to get well-posedness in $H^s_x(0, L)$. To the best of our knowledge, there is no work which establishes the local wellposedness for NLS on bounded rectangular domains in 2 + 1 and higher dimensional settings. Therefore, we would like to end this short note with the following open problem which might be of interest to researchers in analysis of PDEs.

Note

1. T. Özsarı's research is supported by TÜBİTAK 1001 Grant #117F449.

217*. Open Problem. Let $\Omega = (a, b) \times (c, d)$ be a rectangle in $\mathbb{R}^2$, and consider the NLS in (1) ($\sigma = 2$) with Dirichlet b.c. on all sides of $\partial\Omega$ and initial datum $u_0 \in H^s(\Omega)$. Determine the maximal range of s and the (optimal) function spaces for boundary data for which the local wellposedness for (1) holds true in $H^s_x(\Omega)$.

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III Solutions

204. Note that in any topological space with an isolated point, any two dense sets must intersect. Show that there is a 0-dimensional, Hausdorff topological space X with no isolated points so that still, there are no disjoint dense sets in X.

(Daniel Soukup, Kurt Gödel Research Center, University of Vienna, Austria)

Solution by the proposer.

First proof. Take the set of rational numbers $\mathbb{Q}$ and consider the set $\mathcal{T}$ of all possible 0-dimensional topologies τ on $\mathbb{Q}$ that have no isolated point. For example, the usual Euclidean topology is in $\mathcal{T}$. Now, note that any chain in $\mathcal{T}$ has an upper bound; indeed, the union of an increasing chain of such topologies forms a basis for an element in $\mathcal{T}$. Hence, by Zorn's lemma, there must be a maximal element τ in $\mathcal{T}$.

We claim that any two τ-dense subsets D, E of $\mathbb{Q}$ must meet. First, note that neither D nor E can have isolated points; indeed, if U is open and $U \cap D$ is a singleton x then $U \setminus \{x\}$ is a non-empty open set that avoids D. But now, if D and E are disjoint, then the topology generated by $\tau \cup \{D, E\}$ is still in $\mathcal{T}$ and a proper extension of τ. □

Such spaces, with no disjoint dense sets, are called *irresolvable* and the above result was first proved by Hewitt in 1943.[2] Studying the degrees of resolvability, i.e., the maximal number of pairwise disjoint dense sets in spaces, is still an active area of research.[3] In fact, any dense-in-itself compact or metrizable space is *maximally resolvable*, i.e., contains as many pairwise disjoint dense sets as the minimal size of a non-empty open set.[4]

Let us present another, more constructive argument for the existence of irresolvable spaces.

Second proof. We construct a countable, dense subset

$$X = \{x_n : n \in \omega\}$$

of the product $2^{2^{\aleph_0}}$ so that X is also irresolvable (in the subspace topology). We proceed by an induction of length $2^{\aleph_0}$ and at step α, we will specify the coordinates $x_n(\alpha)$. Moreover, we will make sure that

$$X \restriction \alpha = \{x_n \restriction \alpha : n < \omega\}$$

is always dense in 2^α.

Define

$$\{x_n \restriction \omega : n < \omega\}$$

to be an arbitrary dense subset of 2^ω. Now, list all infinite, co-infinite subsets of ω as

$$\{I_\alpha : \omega \le \alpha < 2^{\aleph_0}\}.$$

These correspond to partitions of X and we will make sure at step α that

$$X_{I_\alpha} = \{x_n : n \in I_\alpha\} \text{ and } X \setminus X_{I_\alpha}$$

cannot both be dense in the final space X. Suppose we defined

$$X \restriction \alpha = \{x_n \restriction \alpha : n < \omega\}$$

already. Now, consider the set $X_{I_\alpha} \restriction \alpha$ and its complement in $X \restriction \alpha$. If both these sets are dense in $X \restriction \alpha$, or equivalently in 2^α, then we simply put $x_n(\alpha) = 0$ if and only if $n \in I_\alpha$. Note that our set $X \restriction \alpha + 1$ remained dense in $2^{\alpha+1}$ and $X_{I_\alpha} \restriction \alpha + 1$ is now clopen in $X \restriction \alpha + 1$. In limit steps of the induction, we simply take unions of the functions $x_n \restriction \alpha$ that we constructed already. This finishes the construction.

It should be clear that X is irresolvable. Indeed, if $A \subset X$ is dense and co-dense then $A \restriction \alpha$ is dense and co-dense in $X \restriction \alpha$ for any $\alpha < 2^{2^{\aleph_0}}$. Hence, if $X_{I_\alpha} = A$ then at step α, we must have made $A \restriction \alpha + 1$ clopen. In turn, A is clopen as well, a contradiction. □

Notes

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Also solved by John N. Daras (Greece), Socratis Varelogiannis (France), Alexander Vauth (Germany)

205. For $X = \{\{x, y\} : x, y \in \mathbb{Q}\}$, find a function $b : X \to \mathbb{N}$ such that

$$\{b(\{x, y\}) : x, y \in B\} = \mathbb{N},$$

whenever $B \subseteq \mathbb{Q}$ is homeomorphic to $\mathbb{Q}$.

(Boriša Kuzeljević, University of Novi Sad, Department of Mathematics and Informatics, Serbia)

Solution by the proposer. This solution is by James Baumgartner. First fix an enumeration of $\mathbb{Q} = \{q_n : n \in \mathbb{N}\}$. For each $n \in \mathbb{N}$, fix a set

$$N(q_n, q_0), N(q_n, q_1), \ldots, N(q_n, q_n)$$

of pairwise disjoint neighbourhoods of $q_0, \ldots, q_n$, respectively. Now define a function $f : X \to X$. For $\{q_m, q_n\}$ in X: if $m < n$ and there is $i < m$ so that $q_n \in N(q_m, q_i)$, then let

$$f(\{q_m, q_n\}) = \{q_i, q_m\}.$$

Otherwise let $f(\{q_m, q_n\})$ undefined. Denote

$$f^1(\{x,y\}) = f(\{x,y\}), \text{ and } f^{n+1}(\{x,y\}) = f(f^n(\{x,y\}))$$

for each $n \geq 1$ and $\{x,y\} \in X$. By definition of f, for a fixed $x, y \in \mathbb{Q}$, there is $n \geq 1$ for which $f^n(\{x,y\})$ is undefined. Define $b(\{x,y\})$ to be the least n such that $f^{n+1}(\{x,y\})$ is undefined. Suppose that $B \subseteq \mathbb{Q}$ is homeomorphic to $\mathbb{Q}$. We prove by induction on $l \in \mathbb{N}$ that

$$\{0, \ldots, 2l-1\} \subseteq \{b(\{x,y\}) : x, y \in B\}.$$

This will finish the proof.

Let $l = 1$. There is $q_n \in B \cap N(q_0, q_0)$ for $n > 0$. Note that $f(\{q_0, q_n\})$ is undefined. Also, $N(q_n, q_0) \cap B$ is infinite since q_0 is a limit point of B. So if $x_k \in N(q_n, q_0) \cap B$ and $k > n$, then

$$f(\{q_n, q_k\}) = \{q_0, q_n\}.$$

Hence $b(\{q_0, q_n\}) = 0$, while $b(\{q_n, q_k\}) = 1$.

Now suppose that $l \geq 1$ and that

$$\{0, \ldots, 2l-1\} \subseteq \{b(\{x,y\}) : x, y \in B\}.$$

By inductive hypothesis, there are q_m and q_n in B such that $b(\{q_m, q_n\}) = 2l-1$. Suppose that $m < n$. Since q_m, q_n are limit points of B, there are

$$q_i \in N(q_n, q_m) \cap B \text{ and } q_j \in N(q_i, q_n) \cap B,$$

where $j > i > n$. Now

$$f(\{q_i, q_j\}) = \{q_n, q_i\} \text{ and } f(\{q_n, q_i\}) = \{q_m, q_n\},$$

so

$$b(\{q_n, q_i\}) = 2l \text{ and } b(\{q_i, q_j\}) = 2l+1.$$

□

Also solved by Mihaly Bencze (Romania), Socratis Varelogiannis (France).

206. Suppose that $(G, \cdot)$ is a group, with identity element e and (G, τ) is a compact metrisable topological space. Suppose also that $L_g : (G, \tau) \to (G, \tau)$ and $R_g : (G, \tau) \to (G, \tau)$ defined by, $L_g(x) := g \cdot x$ and $R_g(x) := x \cdot g$ for all $x \in G$, are continuous functions. Show that $(G, \cdot, \tau)$ is in fact a topological group.

(Warren B. Moors, Department of Mathematics, The University of Auckland, New Zealand)

Solution by the proposer. Let $\pi : G \times G \to G$ be defined by $\pi(h, g) := h \cdot g$ for all $(h, g) \in G \times G$. We will first show that there exists an element $h_0 \in G$ such that π is continuous at (h_0, e). Let $(V_n : n \in \mathbf{N})$ be a countable base for the topology on (G, τ). For each $(m, n) \in \mathbf{N} \times \mathbf{N}$, let

$$F_{(m,n)} := \{g \in G : L_g(V_m) \subseteq \overline{V_n}\}.$$

Then, since each R_g is continuous, each set $F_{(m,n)}$ is closed. For each $(m,n) \in \mathbf{N} \times \mathbf{N}$, let $D_{(m,n)} := \mathrm{Bd}(F_{(m,n)}) = F_{(m,n)} \setminus \mathrm{int}(F_{(m,n)})$. Then each $D_{(m,n)}$ is closed and has no interior.

We claim that π is continuous at each point of

$$\left(G \setminus \bigcup_{(m,n)\in\mathbf{N}\times\mathbf{N}} D_{(m,n)}\right) \times G\ ;$$

which is nonempty, by the Baire category theorem. Let

$$(h_0, g) \in \left(G \setminus \bigcup_{(m,n)\in\mathbf{N}\times\mathbf{N}} D_{(m,n)}\right) \times G$$

and let W be an open neighbourhood of $\pi(h_0, g)$. By appealing to the regularity of (G, τ) there exists an $n \in \mathbf{N}$ such that

$$\pi(h_0, g) \in V_n \subseteq \overline{V_n} \subseteq W.$$

Since L_{h_0} is continuous at g there exists an $m \in \mathbf{N}$ such that $g \in V_m$ and $L_{h_0}(V_m) \subseteq V_n$. Hence, $h_0 \in F_{(m,n)}$ and so

$$h_0 \in F_{(m,n)} \setminus \textstyle\bigcup_{(m',n')\in\mathbf{N}\times\mathbf{N}} D_{(m',n')} \subseteq F_{(m,n)} \setminus D_{(m,n)} \subseteq \mathrm{int}(F_{(m,n)}).$$

Let $U := \mathrm{int}(F_{(m,n)})$. Then $h_0 \in U$ and

$$\pi(U \times V_m) \subseteq \overline{V_n} \subseteq W.$$

This shows that π is continuous at each point of $\{h_0\} \times G$. In particular, at (h_0, e). We now show that π is continuous at any point of $G \times G$. Let (x, y) be any point of $G \times G$ and let $(x_n : n \in \mathbf{N})$ be a sequence in G converging to x and let $(y_n : n \in \mathbf{N})$ be a sequence in G converging to y. Then, $(h_0 \cdot x^{-1} \cdot x_n : n \in \mathbf{N})$ converges to h_0 and $(y_n \cdot y^{-1} : n \in \mathbf{N})$ converges to e. Therefore,

$$\begin{aligned}\lim_{n\to\infty} (h_0 \cdot x^{-1} \cdot x_n) \cdot (y_n \cdot y^{-1}) &= \lim_{n\to\infty} \pi((h_0 \cdot x^{-1} \cdot x_n), y_n \cdot y^{-1}) \\ &= \pi(h_0, e) \\ &= h_0.\end{aligned}$$

and so

$$\begin{aligned} x \cdot y &= (x \cdot h_0^{-1}) \cdot (h_0) \cdot y \\ &= (x \cdot h_0^{-1}) \cdot \left(\lim_{n\to\infty} (h_0 \cdot x^{-1} \cdot x_n) \cdot (y_n \cdot y^{-1})\right) \cdot y \quad \text{by above} \\ &= \lim_{n\to\infty} \left((x \cdot h_0^{-1}) \cdot (h_0 \cdot x^{-1} \cdot x_n) \cdot (y_n \cdot y^{-1}) \cdot y\right) \quad (**) \\ &= \lim_{n\to\infty} x_n \cdot y_n. \end{aligned}$$

Note that $(**)$ follows from the continuity of the function, $g \mapsto (x \cdot h_0^{-1}) \cdot g \cdot y$. Hence, we have that $\lim_{n\to\infty} x_n \cdot y_n = x \cdot y$. It now only remains to show that inversion $I : (G, \tau) \to (G, \tau)$ defined by, $I(x) := x^{-1}$ for all $x \in G$, is continuous on G. In fact, since (G, τ) is compact it is sufficient to show that the graph of I is closed. However,

$$\begin{aligned}\mathrm{Graph}(I) &= \{(x, y) \in G \times G : y = x^{-1}\} \\ &= \{(x, y) \in G \times G : x \cdot y = e\} \\ &= \pi^{-1}(\{e\});\end{aligned}$$

which is closed, since $\{e\}$ is closed and π is continuous. □

Also solved by Sotirios Louridas (Greece), Alexander Vauth (Germany)

207. We will say that a nonempty subset A of a normed linear space $(X, \|\cdot\|)$ is a *uniquely remotal set* if for each $x \in X$,

$$\{y \in A : \|y - x\| = \sup\{\|a - x\| : a \in A\}\}$$

is a singleton. Clearly, nonempty uniquely remotal sets are bounded. Show that if $(X, \|\cdot\|)$ is a finite-dimensional normed linear space and A is a nonempty closed and convex uniquely remotal subset of X, then A is a singleton set.

(Warren B. Moors, Department of Mathematics, The University of Auckland, New Zealand)

Solution by the proposer. Let A be a nonempty uniquely remotal subset of a finite dimensional normed linear space $(X, \|\cdot\|)$. For each $a \in A$, let $r_a : X \to [0, \infty)$ be defined by, $r_a(x) := \|x - a\|$ for all $x \in X$. Let $r : X \to [0, \infty)$ be defined by, $r(x) := \sup_{a\in A} r_a(x)$ for all $x \in X$. Then r is 1-Lipschitz and convex, as it is the pointwise supremum of a family of 1-Lipschitz convex functions. Since A is a nonempty uniquely remotal we can define a function $f_A : X \to A$ (called the *farthest point mapping*) by,

$$\{f_A(x)\} := \{y \in A : \|y - x\| = r(x)\} \quad \text{for all } x \in X.$$

Since A is closed and bounded, A is compact (in the norm topology). Thus, to show that f_A is continuous, it is sufficient to show that f_A has a closed graph. To this end, suppose that $x = \lim_{n\to\infty} x_n$ and $y := \lim_{n\to\infty} f_A(x_n)$. Then, $y \in A$, since A is closed and

$$\begin{aligned} r(x) &= \lim_{n\to\infty} r(x_n) \\ &= \lim_{n\to\infty} \|f_A(x_n) - x_n\| \\ &= \left\| \lim_{n\to\infty} (f_A(x_n) - x_n) \right\| \\ &= \|y - x\|. \end{aligned}$$

Therefore, $y = f_A(x)$. We now apply Brouwer's fixed-point theorem to the continuous function $(f_A)|_A : A \to A$ to obtain a fixed-point $x_0 \in A$. That is, $f_A(x_0) = x_0$. Since x_0 is the "farthest point in A" from x_0, we must have that $A = \{x_0\}$. □

Also solved by Mihaly Bencze (Romania), Socratis Varelogiannis (France).

208. Let X be any set. A family $\mathcal{F}$ of functions from X to $\{0, 1\}$ is said to separate *countable sets and points* if for every countable set $B \subseteq X$ and every $x \in X \setminus B$, there is a function $f \in \mathcal{F}$ so that $f(x) = 1$ and $f[B] = \{0\}$.
Let κ and λ be infinite cardinals with $\lambda \leq 2^\kappa$. Give $\{0, 1\}$ the discrete topology and $\{0, 1\}^\lambda$ the usual product topology. Show that the following are equivalent:

1. there is a family $\mathcal{F}$ of λ many functions from κ to $\{0, 1\}$ such that $\mathcal{F}$ separates countable sets and points;
2. there is a subspace $X \subseteq \{0, 1\}^\lambda$ of size κ such that every countable subset of X is closed in X.

(Dilip Raghavan, Department of Mathematics, National University of Singapore, Singapore)

Solution by the proposer. The proof of (1) $\Longrightarrow$ (2) just requires reinterpreting the functions, but the proof of (2) $\Longrightarrow$ (1) uses the coding that is used in the proof that large independent families exist.

(1) $\Longrightarrow$ (2): Let $\{f_\xi : \xi < \lambda\}$ be a 1-1 enumeration of the family $\mathcal{F}$. Thus for each $\xi < \lambda$, $f_\xi : \kappa \to \{0, 1\}$. Now for each $\alpha < \kappa$, we define a function $g_\alpha : \lambda \to \{0, 1\}$ by stipulating that $g_\alpha(\xi) = f_\xi(\alpha)$, for each $\xi < \lambda$. Suppose $B \subseteq \kappa$ is countable and $\beta \in \kappa \setminus B$. By hypothesis there exists $\xi < \lambda$ such that $f_\xi(\beta) = 1$ and $f_\xi(\alpha) = 0$, for all $\alpha \in B$. Thus $g_\beta(\xi) = 1$ and $g_\alpha(\xi) = 0$, for all $\alpha \in B$. So

$$U = \left\{ g \in \{0, 1\}^\lambda : g(\xi) = 1 \right\}$$

is an open neighbourhood of g_β which has empty intersection with $\{g_\alpha : \alpha \in B\}$. This shows that $\{g_\alpha : \alpha < \kappa\}$ is a collection of κ many distinct points of $\{0, 1\}^\lambda$ with the property that every countable subset of it is relatively closed. This proves (2).

(2) $\Longrightarrow$ (1): Let $\{g_\alpha : \alpha < \kappa\}$ be a 1-1 enumeration of X. Thus for each $\alpha < \kappa$, $g_\alpha : \lambda \to \{0, 1\}$. Let

$$L = \{\langle s, H\rangle : s \subseteq \lambda \text{ is a finite set and } H \subseteq \{0, 1\}^s\}.$$

The cardinality of L is λ. We will now produce a family

$$\{f_{\langle s,H\rangle} : \langle s, H\rangle \in L\}$$

of functions from κ to $\{0, 1\}$ which separates countable sets from points. For a fixed $\langle s, H\rangle \in L$, define $f_{\langle s,H\rangle} : \kappa \to \{0, 1\}$ by stipulating that for each $\alpha < \kappa$, $f_{\langle s,H\rangle}(\alpha) = 1$ if and only if $g_\alpha \restriction s \in H$. Suppose $B \subseteq \kappa$ is countable and $\beta \in \kappa \setminus B$. By hypothesis $\{g_\alpha : \alpha \in B\}$ is closed in X, and so g_β is not in the closure of $\{g_\alpha : \alpha \in B\}$. Therefore we can find a finite set $s \subseteq \lambda$ such that the open neighbourhood

$$U = \left\{ g \in \{0, 1\}^\lambda : g \restriction s = g_\beta \restriction s \right\}$$

of g_β misses $\{g_\alpha : \alpha \in B\}$. Let

$$H = \{g_\beta \restriction s\} \subseteq \{0, 1\}^s.$$

So $\langle s, H\rangle \in L$. Now since $g_\beta \restriction s \in H$, we have $f_{\langle s,H\rangle}(\beta) = 1$. On the other hand, for each $\alpha \in B$, $g_\alpha \notin U$, and so $g_\alpha \restriction s \neq g_\beta \restriction s$. Hence for all $\alpha \in B$, $g_\alpha \restriction s \notin H$, whence $f_{\langle s,H\rangle}(\alpha) = 0$. So the function $f_{\langle s,H\rangle}$ separates B from β. Now

$$\{f_{\langle s,H\rangle} : \langle s, H\rangle \in L\}$$

is a family of *at most* λ many functions from κ to $\{0, 1\}$ which separates countable sets from points. Since the hypothesis is that $\lambda \leq 2^\kappa$, we may enlarge this family, if necessary, by adding λ many distinct functions from κ to $\{0, 1\}$ to produce a family of exactly λ many functions from κ to $\{0, 1\}$ which separates countable sets from points. □

Also solved by Mihaly Bencze (Romania), John N. Daras (Greece), Sotirios Louridas (Greece).

209. A subset X of a partial order $(P, \leq)$ is *cofinal* in P if for each $p \in P$ there is an $x \in X$ satisfying $p \leq x$. Let $\beta\omega$ denote the Stone–Čech compactification of the natural numbers, and let ω^* denote the Stone–Čech remainder, $\beta\omega \setminus \omega$. A neighbourhood base $\mathcal{N}_x$ at a point x forms a directed partial order under reverse inclusion. A neighbourhood base $(\mathcal{N}_x, \supseteq)$ is said to be *cofinal* in another neighborhood base $(\mathcal{N}_y, \supseteq)$ if there is a map $f : \mathcal{N}_x \to \mathcal{N}_y$ such that f maps each neighbourhood base at x to a neighborhood base at y. Assume the Continuum Hypothesis. Show that there are at least two points x, y in ω^* with neighbourhood bases $(\mathcal{N}_x, \supseteq)$ and $(\mathcal{N}_y, \supseteq)$ which are cofinally incomparable; that is, neither is cofinal in the other.

(Natasha Dobrinen, Department of Mathematics, University of Denver, USA)

Solution by the proposer. Recall that the points in ω^* are nonprincipal ultrafilters. Let Fin denote the set of all finite nonempty subsets of $\mathbb{N}$. An ultrafilter $\mathcal{U}$ is *selective* if for any collection $\{U_s : s \in \text{Fin}\}$ of members of $\mathcal{U}$, there is a selector $X \in \mathcal{U}$ such that for each $s \in \text{Fin}$, $X/s := X \setminus (\max(s)+1) \subseteq U_s$. Assuming the Continuum Hypothesis, we can build selective ultrafilters by transfinite recursion. As any ultrafilter partially ordered by reverse inclusion is Dedekind complete, one need only consider cofinal maps which are monotone: $Y \supseteq X$ implies $f(Y) \supseteq f(X)$. Identify the collection of all subsets of the natural numbers with the Cantor space $\mathcal{C}$ via their indicator functions. Continuity of cofinal maps is with respect to the topology on $\mathcal{C}$.

Claim 1 If $\mathcal{U}$ is selective, then for any monotone cofinal map $f : \mathcal{U} \to \mathcal{V}$, there is an $X \in \mathcal{U}$ such that f is continuous when restricted to $\{U \in \mathcal{U} : U \subseteq X\}$.

Proof. Given $\mathcal{U}$, $\mathcal{V}$ and f, for each finite set $s \subseteq \mathbb{N}$, take a set $X_s \in \mathcal{U}$ satisfying the following: $X_s = X_s/s$, and for all $k \leq \max(s)$, $k \in f(s \cup X_s)$ if and only if $k \in f(Y)$ for each $Y \in \mathcal{U}$ with s an initial segment of Y. By monotonicity of f, such an X_s in $\mathcal{U}$ exists. Since $\mathcal{U}$ is selective, there is a member $U \in \mathcal{U}$ such that for each finite set s, $U/s \subseteq X_s$. Then f is continuous when restricted to $\{U \in \mathcal{U} : U \subseteq X\}$: Given $U \subseteq X$ in $\mathcal{U}$, for any k, let s be any nitial segment of U for which $k \leq \max(s)$. Then $k \in f(U) \longleftrightarrow k \in f(s \cup X) \longleftrightarrow k \in f(s \cup X_s)$. □

Claim 2 There are two selective ultrafilters which are cofinally incomparable.

Proof. Fix an enumeration $\langle f_\alpha : \alpha < \omega_1 \rangle$ of all monotone continuous maps from the Cantor space into itself. An equivalent form of selective ultrafilter is that for each partition of $\mathbb{N}$ into infinitely many pieces, either one piece is in the ultrafilter, or else there is a member of the ultrafilter which intersects each piece exactly once. Fix an enumeration $\langle P_\alpha : \alpha < \omega_1 \rangle$ of all partitions $\{P_\alpha^n : n < \omega\}$ of ω into infinitely many pieces. We construct a sequence of countable filter bases (closed under finite intersection) via transfinite recursion on ω_1.

Let $\mathcal{U}_0 = \mathcal{V}_0 = \mathcal{F}r$, the Frechét filter of cofinite sets of natural numbers. For $\alpha < \omega_1$, given countable filter bases $\mathcal{U}_\alpha$ and $\mathcal{V}_\alpha$, extend them to filter bases $\mathcal{U}_{\alpha+1}$ and $\mathcal{V}_{\alpha+1}$ as follows: If there is an n such that $P_\alpha^n \in \mathcal{U}_\alpha$, let $\mathcal{U}'_\alpha = \mathcal{U}_\alpha$. Otherwise, there is an infinite set X such that for each n, $|X \cap P_\alpha^n| = 1$ and the set $\mathcal{U}_\alpha \cup \{X\}$ generates a proper filter; let $\mathcal{U}'_\alpha$ be the filter base consisting of all intersections of finitely many members of $\mathcal{U}_\alpha \cup \{X\}$. In a similar manner, construct $\mathcal{V}'_\alpha$.

Since $\mathcal{U}'_\alpha$ is countable, it has a pseudointersection; that is, an infinite set U such that $U \setminus Y$ is finite for each $Y \in \mathcal{U}'_\alpha$. Likewise, there is a pseudointersection V for $\mathcal{V}'_\alpha$. If $V \setminus f_\alpha(U)$ is infinite, let $\mathcal{U}_{\alpha+1} = \mathcal{U}'_\alpha$, and let $\mathcal{V}_{\alpha+1}$ be the filter base generated by $\mathcal{V}'_\alpha \cup \{V \setminus f_\alpha(U)\}$. Otherwise, $V \setminus f_\alpha(U)$ is finite. If there is an infinite subset $V' \subseteq V$ such that $V' \setminus f_\alpha(X)$ is finite for each infinite $X \subseteq U$, then f_α cannot be a cofinal map into any ultrafilter containing V'. In this case, let $\mathcal{U}_{\alpha+1} = \mathcal{U}'_\alpha$ and let $\mathcal{V}_{\alpha+1}$ be the filter base generated by $\mathcal{V}'_\alpha \cup \{V'\}$. The final case is that for each infinite $V' \subseteq V$, there is an infinite $U' \subseteq U$ such that $V' \setminus f_\alpha(U')$ is infinite. In particular, there is an infinite $U' \subseteq U$ such that $V \setminus f_\alpha(U)$ is infinite. In this case, let $\mathcal{U}_{\alpha+1}$ be the filter base generated by $\mathcal{U}'_\alpha \cup \{U'\}$ and $\mathcal{V}_{\alpha+1}$ to be the filter base generated by $\mathcal{V}'_\alpha \cup \{V \setminus f_\alpha(U)\}$.

If $\alpha < \omega_1$ is a limit ordinal, take $\mathcal{U}_\alpha$ to be the the union of the $\mathcal{U}_\beta$, for $\beta < \alpha$; likewise for $\mathcal{V}_\alpha$. Once the sequences of filter bases $\langle \mathcal{U}_\alpha : \alpha < \omega_1 \rangle$ and $\langle \mathcal{V}_\alpha : \alpha < \omega_1 \rangle$ are constructed, let $\mathcal{U}$ be an ultrafilter extending $\bigcup_{\alpha<\omega_1} \mathcal{U}_\alpha$ and let $\mathcal{V}$ be an ultrafilter extending $\bigcup_{\alpha<\omega_1} \mathcal{V}_\alpha$. By the construction, $\mathcal{U}$ and $\mathcal{V}$ are selective ultrafilters, and there is no monotone continuous function mapping one cofinally into the other. □

Also solved by Alexander Vauth (Germany).

Note. A much lengthier construction of $2^{\mathfrak{c}}$ many cofinally incomparable selective ultrafilters appeared in a paper of Dobrinen and Todorcevic, in 2011. However, the short and straightforward construction of two cofinally incomparable selective ultrafilters provided here did not previously appear in the literature.

We encourage you to submit solutions to the proposed problems and ideas on the open problems. Send your solutions by email to Michael Th. Rassias, Institute of Mathematics, University of Zürich, Switzerland, michail.rassias@math.uzh.ch.
We also solicit your new problems with their solutions for the next "Solved and Unsolved Problems" column, which will be devoted to *Analytic Number Theory*.

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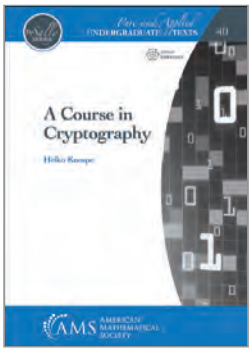

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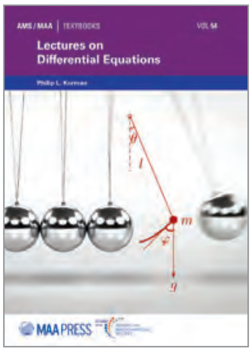

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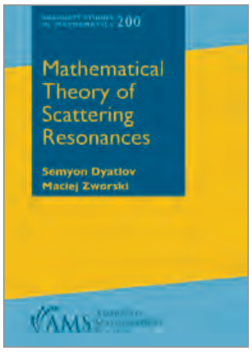

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