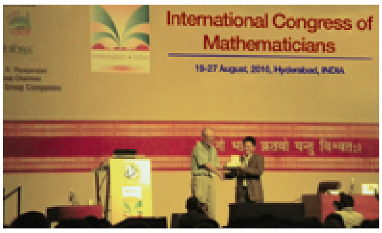
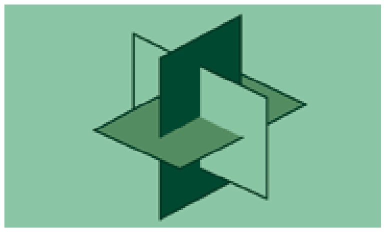
NEWSLETTER

OF THE EUROPEAN MATHEMATICAL SOCIETY





December 2010
Issue 78
ISSN 1027-488X

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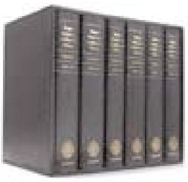

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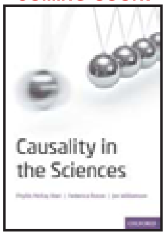

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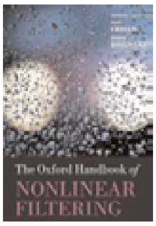

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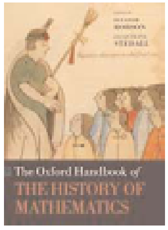

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Oxford Handbooks in Mathematics

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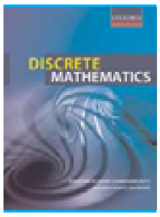

Discrete Mathematics

S. K. Chakraborty and B. K. Sarkar

Designed to serve as a textbook for undergraduate engineering students of computer science and postgraduate students of computer applications, it provides exhaustive coverage of the various concepts of discrete mathematics. It will also be of interest to postgraduate students of mathematics.

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October 2010 | 776 pp | Hardback | 978-0-19-957704-0

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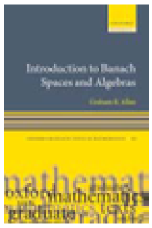

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The late Graham Allan

Edited by H. Garth Dales

A timely graduate level text in an active field covering functional analysis, with an emphasis on Banach algebras.

Oxford Graduate Texts in Mathematics No. 20

November 2010 | 384 pp. | Paperback 978-0-19-920654-4

EMS member price: ~~£34.95~~ £27.96

November 2010 | 384 pp. | Hardback | 978-0-19-920653-7

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European Mathematical Society

Newsletter No. 78, December 2010

The views expressed in this Newsletter are those of the authors and do not necessarily represent those of the EMS or the Editorial Team.

ISSN 1027-488X

Published by the
EMS Publishing House
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CH-8092 Zürich, Switzerland.
homepage: www.ems-ph.org

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EMS Agenda

2010

2 December
Final Conference of the Forward Look "Mathematics and Industry", Bruxelles
http://www.ceremade.dauphine.fr/FLMI/FLMI-frames-index.html

2011

9–13 February
5th World Conference on 21st Century Mathematics 2011
Lahore, Pakistan
http://wc2011.sms.edu.pk

19–20 March
EMS Executive Committee Meeting
Weierstrass Institute, Berlin
Contact: s.huggett@plymouth.ac.uk

30 March–3 April
EUROMATH-European Student Conference in Mathematics
Athens, Greece
www.euromath.org

7–8 May
EMS Meeting of Presidents of Mathematical Societies in Europe, Spain (exact location to be fixed)

18–22 July
ICIAM 2011 Congress, Vancouver, Canada
www.iciam2011.com

3–8 July
Third European Set Theory Conference, ESF-EMS-ERCOM Conference, Edinburgh, United Kingdom
http://www.esf.org/activities/esf-conferences/details/2011/confdetail368.html

3–8 July
Completely Integrable Systems and Applications, ESF-EMS-ERCOM Conference
Vienna, Austria

2012

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

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

Editorial

EMS now and in the future

Ari Laptev, President of the EMS

It has been a great honour and pleasure for me to serve the European mathematical community as President of the European Mathematical Society over the last four years. My thanks to all the members of the EMS Executive Committee and to the active members of national mathematical societies.

Over these last few years, we have seen the EMS move forward in very many ways but especially in uniting our forces to convince policymakers of the importance of mathematical sciences for Europe.

We have always kept in mind that one of the main missions of the EMS is to promote mathematics in Europe and to provide a link between European institutions and national mathematical societies. In order to achieve this, the EMS Executive Committee has encouraged a closer contact with and between the societies. One very successful result of this development has been the establishing of annual meetings of the presidents of the national mathematical societies. These meetings give the participants the opportunity to discuss informally and provide important feedback for the Executive Committee members, as well as encouraging a feeling of unity between European mathematicians.

A recent example of the latter has been the enormous response from numerous European mathematical societies to the devastating news that the Erwin Schrödinger Institute has been threatened with closure, as from 1 January 2011. Many presidents on behalf of their societies, as well as individuals, have written letters of support to the Austrian Ministry.

Now (8 November), we have received information that the ESI funding will be secured during 2011. I would like to express my gratitude to all those whose commitment and belief have undoubtedly contributed to the Austrian Government reconsidering its decision.

We are now actively involved in various European projects. Among them is the ESF-EMS-ERCOM series of conferences. The ESF office in Brussels provides matching funds of up to €20,000 for organising conferences at ERCOM centres. Some of these centres have found it difficult to fulfil the requirements of the ESF. However, as a result of a meeting between the EMS, the ESF and ERCOM on 17 September, many of these requirements have been cancelled. We now hope that this series of conferences will become even more successful and popular.

I would like to congratulate our Electronic Publishing Committee on finally receiving a very substantial EU grant for the European Digital Mathematical Library, after many unsuccessful attempts. This confirms how important it is to be persistent when applying for funds and not to be deterred or discouraged by continuous rejection.

One of the biggest success stories has been the 2 year ESF-EMS Forward Look project "Math&Industry". On 2 December 2010, there will be a final, very highly representative conference for this project in Brussels. One of the recommendations for the final text of the project includes the creation of a European Institute of Mathematical Sciences and Innovation. The project Math&Industry has been led by the EMS Applied Mathematics Committee, chaired by M. Primicerio, and the committee, together with the ESF, are now deeply involved in preparing an application for an EU Design Study e-infrastructure.

Congratulations also go to our "Raising Public Awareness of Mathematics" Committee, chaired by E. Behrends. This committee has succeeded in receiving funds from the insurance company Munich-Re. Such funds will allow the committee to substantially increase its impact on promoting mathematics in Europe through a large number of interesting projects.

The EMS Developing Countries Committee has always been one of our most successful committees. The committee has received substantial donations from individual sponsors, which have enabled the committee to give financial support to mathematicians from developing countries to attend European conferences.

Recently we were able to rebuild our Education Committee. Chaired by G. Törner, we now hope that this committee will be able to provide a fruitful link between the community of mathematicians involved in problems of education in mathematics and university professors.

The committee "Women and Mathematics" is extremely active in promoting mathematics among young female students and mathematicians and has particular support from the EMS Executive Committee.

The EMS Meeting Committee, responsible for organising European conferences, mathematical weekends, etc, is very active now and I am sure that it will contribute substantially to the visibility of EMS.

The EMS has finally built a new Ethics Committee, chaired by A. Jensen. It had its first meeting recently in Oberwolfach. We all feel that it is time for the EMS to have such a committee, which at present will be involved in ethical problems related to plagiarism and the unethical behaviour of some of our colleagues. Regrettably this still exists within our community.

The EMS Publishing House, organised by the former EMS President Rolf Jeltsch, is developing very well. During the last few years it has substantially increased its portfolio of mathematical journals. It now has a large number of different book series and enjoys financial security. For its success we are obliged to the professionalism of its director T. Hintermann and his colleague M. Karbe. We now conclude that they have been able to build up a truly non-commercial publishing house with a very high reputation.

The EMS Newsletter continues to make most interesting reading and we are all very grateful to M. Raussen and lately to V. Muñoz and the members of the Newsletter Editorial Board for their devoted work.

The major task for the EMS is the organisation of the EMS congresses. We are grateful to our Dutch colleagues for organising the 5th European Congress of Mathematics in Amsterdam in 2002. The next congress will be in Kraków and the 6ECM Prize and Programme Committees have already been put together. After some negotiation with Springer it has been agreed that there will be a new EMS-Springer History Prize. Springer has now resolved to support this prize with €5,000 every four years. It is important to mention the collaboration between the EMS and Springer in the future development of Zentralblatt. This is a very important project for the EMS and our dream is now to make Zentralblatt free of charge for all European Countries. So far, we have only succeeded in making it free for individual EMS members and for a large number of developing countries. Every EMS individual member is entitled to obtain their own login password for ZMATH.

Another joint project between the EMS and Springer concerns an encyclopedia of mathematics that was originally published in Russia and later translated and published by Springer. The EMS and Springer have recently signed a contract according to which the text of this encyclopedia will be digitised by Springer and made available on the internet, free of charge, with a comment facility (as a wiki-part). The EMS Electronic Publishing Committee will be responsible for running this webpage.

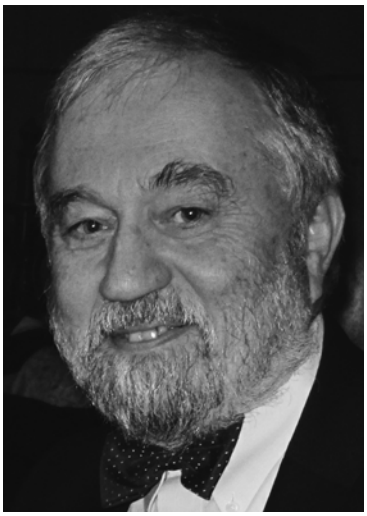
Unfortunately not everything we planned was successful. Most painful for the EMS (and especially for me) was the failure of the EU infrastructure application MATH-EI last year. We had great hopes for this project. Its aim was to build a European mathematics infrastructure that would be able to finance conferences, workshops, schools research visits and programmes. Next year, there will be another EU call of this kind and we feel that it is our duty to try once again.

We also have to admit that we failed in attracting more new EMS individual members, despite all our efforts. However, we did succeed in attracting new society members. Among them are mathematical societies from Serbia, Montenegro and Turkey. Looking at the map of Europe on our new and very attractive EMS webpage we see almost no red spots left. This reflects the fact that mathematical societies from almost all European countries are members of the EMS.

In conclusion I would like to thank all the Executive Committee members for their hard work and for the warm and friendly atmosphere we had during our meetings. It has been a challenging, exhausting but wonderful experience for me.

I am confident that the next EMS President Marta Sanz-Solé and the members of the EMS Executive Committee will continue to strengthen the EMS, making it more influential and more useful for the members of the European mathematical community. I wish the new team every success in generating new ideas that will further promote our beautiful subject in Europe.

Looking back – and forward



Pavel Exner

Our lives and careers have natural periods and it is useful to stop briefly and reflect at the end of each of them. One such moment comes for me now that my eight years of service on the EMS Executive Committee (EC) are coming to a conclusion. I was elected to the EC at the council meeting in Oslo in 2002 and two years later in Uppsala I was promoted to Vice-President and served as such for six years under the presidency of John Kingman and Ari Laptev. I want to thank them, as well as my fellow Vice-Presidents Luc Lemaire and Helge Holden, the other officers, EC members and collaborators for the privilege and pleasure of working with them.

While the EC composition and the style of its meetings kept changing, it was always an efficiently working body, both face to face, when we met 2–3 times a year at various places of Europe, and in electronic exchanges, often with everyday frequency. I am sure that this will remain true in the future. In fact, Marta Sanz-Solé was an EC member when I joined and I am glad to see her back and at the helm of the EMS six years later. I wish her and her fellow EC members success in their work.

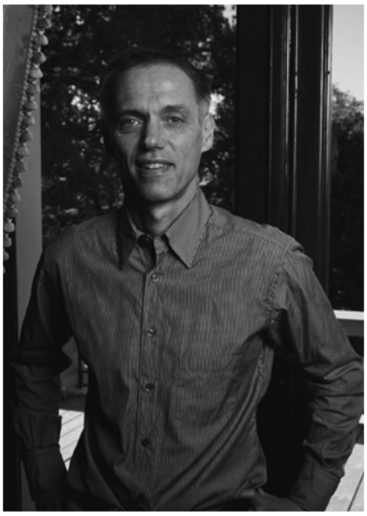
This is not a place for a detailed overview of my work in the EC, some of which I am proud of while other parts in the rear mirror could have definitely been done better. I restrict myself to a single thing, which is my work in the ERC Scientific Council. It originated in the EMS because I was selected as its member based on a nomination of the EMS Executive Committee.

I wrote an account about ERC activities and its impact on mathematics recently in the newsletter and I am not going to repeat myself; rather, I want to reflect on the ERC from a more general perspective. I must confess that I entered the Scientific Council with hesitation. We are used to communicating with our own brand or our close neighbours and it was not clear to me how a dialogue would look like in a company covering the whole science spectrum, from mathematics to medicine, art history, etc. It was remarkable how fast we found a common language; it became immediately clear that there is a notion of "excellent science" common to all of us.

I think in the past few years European science has made great progress and that it has to go on at the set pace if we should not lose out in the ever intensifying global competition. The lesson we have to remember is that such a goal can be achieved only if we act in collaboration with our peers in other disciplines in the interests of high-level science and with the broad support of the scientific community. Let me express the hope that we will be able to do that and that Europe will preserve and strengthen its role as a birthplace of great scientific ideas.

Pavel Exner
EMS Vice-President and an
ERC Scientific Council Member

The EMS – providing a European identity



Helge Holden
Picture from abelprisen.no/en

It is not unusual for executives, when stepping down, to polish the accomplishments during their tenure while at the same time offering advice for their successors. I will try to avoid that. I have been Secretary for 4 years followed by a 4-year term as Vice-President. Preceded by one year as a "trainee", this means that I have been affiliated with the EMS for close to half its lifetime. It has been a very interesting experience.

The main challenge during my tenure, as it will be for the future officers of the EMS, is to increase membership. The EMS has a special membership structure, distinct from most other transnational societies. We have both individual members and society members. This reflects the special European structure with many nations, many more different languages and quite substantial cultural diversity. This construction was the result of a compromise at the time when the EMS was established. There are close to no white spots on the European map when it comes to society members. Except for Albania, we cover an extended Europe. However, when it comes to individual members, the map contains many white areas. It is not good enough to have individual membership around 2500. During my tenure we have tried to increase membership, not too successfully, and the new Executive Committee will have to continue the work and try to do better. I am in favour of the dual membership structure. Many of the national societies are weak, certainly financially but also regarding stability and activity. Other societies are big and financially very solid. To compensate for this substantial diversity, it is important to be able to become an active individual member of the EMS.

We now have better visibility on the web. The web develops very dynamically, unpredictably and very fast. To keep up with this development is not easy, and is made more difficult by our limited financial resources. But it is necessary to have high visibility on the web, in order to increase membership in particular. We have to compensate for our lack of financial resources by appealing to the creativity, voluntary work and inventiveness of our members.

The EMS is more important than ever before. To create a European identity in mathematical sciences is vital. In particular, it is crucial to promote the importance of mathematics to the EU and its funding bodies. To be successful we have to speak with one voice and only the EMS has the legitimacy to act on behalf of European mathematics.

I have always insisted that we should elect our officers with definite terms and with a maximum length of term of office. With approximately 500 million people living in the realm of the European Mathematical Society, it is important to have a regular turnover and let new people serve the EMS. Now my time has come and I am happy to see the new Executive Committee and its incoming officers, and I am convinced that they will be able to develop the EMS further.

Helge Holden
Vice-President of the EMS

EMS Executive Committee and Council meetings in Sofia, 9–11 July 2010

Vasile Berinde, EMS Publicity Officer

According to Article 5 of the EMS Statutes, the Council meets "at least once every two years not earlier than May and not later than October". So, after its previous regular meetings of the current decade, all located in western European countries: Barcelona (2000), Oslo (2002), Uppsala (2004) and Utrecht (2006), the EMS moved towards south-eastern Europe for its 2010 Council Meeting, in Sofia, at the invitation of the Union of Bulgarian Mathematicians. Being, as a rule, preceded by the Executive Meeting (9–10 July), this time the Council Meeting (10–11 July) was also accompanied by two associated events: the Round Table "20$^{\text{th}}$ anniversary of EMS" (10 July) and the conference "Mathematics in Industry" (11–14 July). All these events were hosted by the Metropolitan Hotel in Sofia, an excellent location and perhaps the best Council meeting location amongst the ones mentioned above. We shall briefly report here on the first three EMS events held in Sofia.

EC Meeting

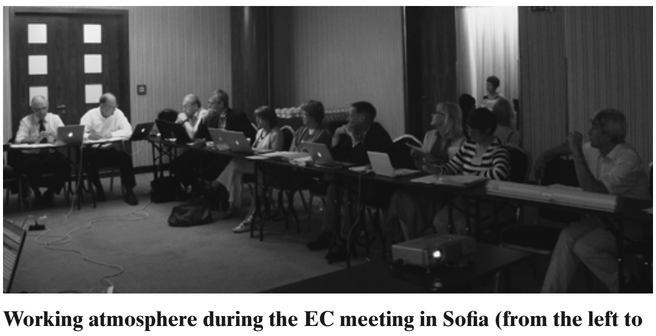
Present were the EC members Ari Laptev (*President* and Chair), Pavel Exner and Helge Holden (*Vice-Presidents*), Stephen Huggett (*Secretary*), Jouko Väänänen (*Treasurer*), Zvi Artstein, Franco Brezzi, Igor Krichever, Mireille Martin-Deschamps and Martin Raussen and, by invitation, Marta Sanz-Solé, Terhi Hautala and Riitta Ulmanen (*Helsinki EMS Secretariat*), Vicente Muñoz (*Editor-in-Chief of the EMS Newsletter*), Mario Primicerio (*Chair of the EMS Committee for Applied Mathematics*) and Vasile Berinde. In the beginning, as the Sofia meeting would be her last EC meeting, the President thanked Riitta Ulmanen very much for her work at the Helsinki EMS Secretariat in the period 2006–2010. Following the agenda, brief reports by the officers (President, Treasurer, Secretary, Vice-President Helge Holden and Publicity Officer) were given. Then, it was agreed that the application of the Mathematical Society of the Republic of Moldova to be a member of EMS should be considered by the current Council meeting, while the one by Kosovar Mathematical Society (to whom Dr Qëndrim Gashi gave a short presentation describing the history and structure in the second part of the meeting) would have to wait until the next Council meeting. An important issue was related to the reduced individual membership fee for people from developing countries with the following resolution after discussions: the EC would set a fee and the EMS Committee for Developing Countries would set eligibility criteria of both a country and an individual but the individual members paying a reduced fee would not receive the printed copy of the newsletter.

In connection with the preparations for the next European Congress of Mathematics, it was reported that all committees for 6ECM have a Chair (Programme Committee: Eduard Feireisl; Prizes Committee: Frances Kirwan; Felix Klein Prize Committee: Wil Schilders; and History of Mathematics Prize Committee: Jeremy Gray) and, except for the Programme Committee, all are complete. Another important item concerning Scientific Meetings has been the question of appointing a new Chair of the Meetings Committee, because the Chair had just offered his resignation. Zvi Artstein has been appointed as an interim Chair for this committee until the end of 2011.

After the Executive Committee had gone through the agenda for the Council item by item, checking that the papers and presentations were in order, the reports from the standing committees were presented. For this part of the meeting, Ehrhard Behrends, Dusanka Perisic and Qendrim Gashi were invited to join the meeting. The present Committee Chairs, i.e. Mario Primicerio (Applied Mathematics), Dusanka Perisic (Women and Mathematics) and Ehrhard Behrends (Raising Public Awareness) gave reports on the activity of their committees, while Mireille Martin-Deschamps (Developing Countries), Igor Krichever (Eastern Europe), Franco Brezzi (Education) and Pavel Exner (Electronic Publishing), reported on behalf of the Chair of the respective committees. Ari Laptev gave a brief report on the Meetings Committee and reported that the Ethics Committee had now been set up and that its first meeting would be in Oberwolfach in September 2010.

In the last part of the meeting, Mario Primicerio reported on the EMS Summer Schools that would be organised this year and Vicente Muñoz gave his healthy report on the newsletter while Susan Oakes, specifically invited to the meeting, gave a brief report on her work so far in improving the take-up of individual membership through the national societies.

The next Executive Committee meeting was held in Lausanne, at the invitation of the Swiss Mathematical Society, on 13–14 November 2010.



Working atmosphere during the EC meeting in Sofia (from the left to the right): A. Laptev, S. Huggett, Z. Artstein, M. Raussen, M. Sanz-Solé, M. Martin-Deschamps, H. Holden, T. Hautala, R. Ulmanen and E. Behrends. Susan Oakes, from the opposite side, can be seen in the mirror.

During the Council meeting. First row to the right: Olga Gil-Medrano and Stefan Dodunekov.

Council Meeting

In the afternoon of 10 July, 58 delegates of EMS members were present and so the quorum of the Council, which requires two-fifths of its total number of delegates, was easily satisfied. Also in attendance were several invitees, alongside Terhi Hautala and Riita Ulmanen from EMS Helsinki Secretariat. The meeting was opened by the President who welcomed the delegates and expressed his thanks and very warm gratitude to the local hosts.

After the detailed reports of the President, Executive Committee and Finance, the Council approved the request from the London Mathematical Society to change from class 3 to class 4. Mitrofan Cioban, President of the Mathematical Society of the Republic of Moldova, then gave a short presentation in support of the application to join the EMS, which was approved by the Council. The next important issue of the first part of the meeting was the Elections to Executive Committee. There were five vacant officer positions, left open by the ending of the terms of the President, the two Vice-Presidents, the Secretary and Treasurer and two of the ordinary members. The Executive Committee had one nomination for each position and no nominations were made on the floor for the vacant officer positions. Marta Sanz-Solé was elected as President and Mireille Martin-Deschamps and Martin Raussen as Vice-Presidents, while Jouko Väänänen and Stephen Huggett were re-elected as Treasurer and Secretary, respectively. There were six nominations for the two vacant Member-at-Large seats. As Vasile Berinde took 20 votes, Rui Loja Fernandes 26, Ignacio Luengo 7, Volker Mehrmann 37 and Jiří Rákosník 20, Rui Loja Fernandes and Volker Mehrmann were elected as Members-at-Large of the EC. The Council also elected Gregory Makrides and Rolf Jeltsch as lay auditors and PricewaterhouseCoopers as professional auditors for the accounts for the years 2011 and 2012.

The agenda of the second day of the Council meeting included several important items that unfortunately cannot be presented here in detail but just enumerated: report of the Newsletter Editor, reports on the Publishing House and Zentralblatt MATH, the report of the Publicity Officer and then the reports from the EMS Committees (these reports were discussed in parallel sessions), 6ECM (Roman Srzednicki, the Chair of the Organizing Committee, gave a presentation of the preparations for 6ECM) and EUROMATH conferences for young people (presented by Gregory Makrides).

To close the Council, the President thanked the local organisers, Stefan Dodunekov and his team, for the excellent organisation of this Council and announced the next meetings. So, the next meeting of Presidents would be in Spain in April or May 2011, at the invitation of the Real Sociedad Matemática Española, while the next Council meeting would be on 30 June–1 July 2012 in Kraków, in conjunction with the 6th European Congress of Mathematics (6ECM).

20th anniversary of EMS

As at the end of this year EMS will celebrate the 20th anniversary of its foundation on 28 October 1990 (in Madralin, Poland), this important event was marked a little in advance during the EMS Council meeting in Sofia, on the afternoon of 10 July, by means of the two hours Round Table "The 20th anniversary of EMS" to which were invited all Council delegates. This consisted of some historical presentations and speeches by Jean-Pierre Bourguignon and Rolf Jeltsch (former EMS Presidents), Ari Laptev (current EMS President) and M. Sanz-Solé (EMS President elect), David Salinger (former Publicity Officer), Tuulikki Makelainen from the EMS Helsinki Secretariat (1990–2006) and Vasile Berinde. David Salinger's presentation "EMS history (2000–2005)" included many personal remembrances around some pictures taken from various EMS events during his term of office, while the presentation "EMS history by pictures" by Tuulikki Makelainen and Vasile Berinde proposed an 88 slide tour with pictures from the main locations of the EMS meetings, extracted from the EMS pictures archive located at http://vberinde.ubm.ro/?m=ems/european-mathematical-society, emotionally commented on by Tuulikki Makelainen. Last but not least, Rolf Jeltsch gave an interesting presentation on EMS history and activities under the title "Presidency (1999–2002)".

A "side" of the Round Table "20th anniversary of EMS": R. Jeltsch, J. P. Bourguignon, T. Makelainnen, A. Laptev, D. Salinger and M. Sanz-Solé.

International Centre for Pure and Applied Mathematics CIMPA-ICPAM: a European outlook

In an article in the Newsletter of September 2010, CIMPA was mentioned. Actually, its status has just changed in 2010; below we provide an up-to-date presentation.

CIMPA-ICPAM is an international organisation whose aim is to promote international cooperation with developing countries in the areas of higher education and research in pure and applied mathematics, as well as in related disciplines. CIMPA-ICPAM was founded in 1978 with support from the French government and is located in Nice.

CIMPA-ICPAM is recognised as a category 2 Centre by UNESCO. Its status is that of a non-profit association (according to French law of 1901) and works with a large number of mathematicians and member institutions throughout the world.

In 2007, CIMPA-ICPAM's Governing Board expressed the wish to expand it at the European level, so that other countries can take part in its activities and provide financial support. This development will make it possible to better meet the numerous requests from developing countries that are not met with current resources.

In March 2010, Spain signed an agreement with CIMPA-ICPAM, becoming its second member state. The support of the Spanish mathematics community was essential for this development.

We are now exploring this collaboration with several European countries. This message is a call for support from national mathematics communities in Europe who are interested. Please contact: cimpa@unice.fr.

CIMPA-ICPAM's main activities concern the organisation of research schools. Their aim is to contribute to training the next generation of mathematicians of both genders through research. Every year, a call for research projects is issued with a view to organising research schools, all in developing countries. These projects are assessed by CIMPA-ICPAM's Scientific Council. Three main balances to be respected as much as possible are geographic, thematic and gender. CIMPA-ICPAM would like to increase the number of research schools to 20 a year (11 in 2009, 14 in 2010). In each school, selected young mathematicians from neighbouring countries receive full support from CIMPA-ICPAM.

EMS is an institutional member of CIMPA-ICPAM.

For more details on the activities of CIMPA-ICPAM, please visit its website:

http://www.cimpa-icpam.org/?lang=en.

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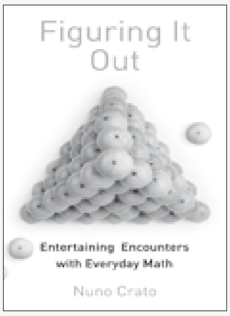

This is a book of mathematical stories — funny and puzzling mathematical stories. They tell of villains who try to steal secrets, heroes who encode their messages, and mathematicians who spend years on end searching for the best way to pile oranges. There are also stories about highway confusions occurring when the rules of Cartesian geometry are ignored, small-change errors due to ignorance of ancient paradoxes, and mistakes in calendars arising from poor numerical approximations.

2010. X, 227 p. 36 illus. Hardcover
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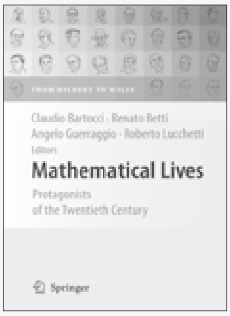

This volume brings to the forefront some of the proponents of the mathematics of the twentieth century who have put at our disposal new and powerful instruments for investigating the reality around us. The portraits present people who have impressive charisma and wide-ranging cultural interests, who are passionate about defending the importance of their own research, are sensitive to beauty, and attentive to the social and political problems of their times.

2011. XIII, 330 p. 50 illus. Hardcover
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Mathematics as a production factor or driving force for innovation? Those, who want to know and understand why mathematics is deeply involved in the design of products, the layout of production processes and supply chains will find this book an indispensable and rich source.
19 articles written by mixed teams of authors of engineering, industry and mathematics offer a fascinating insight of the interaction between mathematics and engineering.

2010. XIV, 402 p. Softcover
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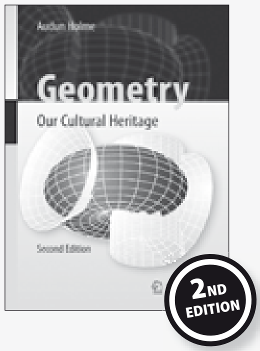

This book contains selected topics from the history of geometry, with "modern" proofs of some of the results, as well as a fully modern treatment of selected basic issues in geometry. It is geared towards the needs of future mathematics teachers. For the 2nd edition, some of the historical material giving historical context has been expanded and numerous illustrations have been added. The main difference is however the inclusion of a large number of exercises with some suggestions for solutions.

2nd Edition. 2011. XIX, 551 p. 295 illus., 85 in color. Hardcover
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J. Falmagne, **J. Doignon**

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H. Gronau, **H. Langmann**, **D. Schleicher**, (Eds.)

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A Man of Conviction

A. Stubhaug

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L. W. Tu

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The ICM 2010 in Hyderabad (India)

Ulf Persson (Göteborg)

Hyderabad (India)

Nothing you say about India is true. Nothing you say about India is false. Those were the words, or words to that effect, with which the ICM 2010 in Hyderabad was opened. India is only the second developing country that has been assigned the hosting of the International Congress of Mathematicians and comparisons with that of Beijing in 2002 are very hard to escape.

India is indeed a large country, especially when one considers its astronomically large population, half of which live in abject poverty. Yet among the sea of Indian humanity there is a middle-class of perhaps close to a hundred million people, the majority of whom enjoy a wealth comparable to the middle-class of a Western country, and of course a financial upper class the likes of which you would never encounter in more egalitarian countries.

Thus India is bound to be a country of spectacular contrasts. The Indian reality that a Western tourist will face is bound to be overwhelming. They will be subjected to a veritable bombardment of sensory impressions of sight, sound and smell. Everything seems to scream out for attention, cluttering your mind with a relentless overload. Sensory deprivation is a serious condition, invariably leading to hallucinations fabricated by a starved brain. In India the brain need not fabricate its own hallucinations; external stimuli create their own psychedelic hallucinatory world. No wonder that Indian expatriates long to go back to the multifarious Indian reality whose riches never seem to run the risk of drying out.

A land of contrasts indeed, from one point of view a hell of unmitigated misery, from another a magical fairytale, in which one can still enjoy nostalgically the remnants of age-old traditions – people working manually in the fields, carts with big wheels drawn by bullocks, herds of goats roaming around. (And not to be too sentimental about it, begging and starvation was also the lot of most of our ancestors in the West too.) Nowadays there is much talk about sustainable living and development; the majority of Indians are very adept at least as to the former, a harsh existence that few Westerners would look forward to. Maybe it is after all the land of the future, giving a glimpse of what will be in store for us all?

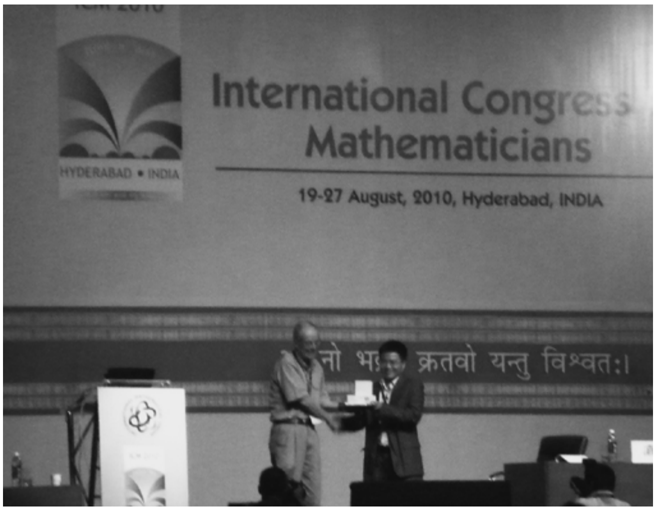
Infrastructure is a real problem in India. Parts of it are very advanced; one need only think of IT technology and the ubiquity of mobile telephones (one would not be surprised to see beggars using them), whereas more traditional, basic features are in very poor condition. But India is rapidly changing, for better and for worse, as the result of the relatively recent liberalisation of its economy. Thus arranging a congress of the size of an ICM still presents a real challenge. Supposedly there is only one venue in the whole of India that would be able to hold at its opening ceremony an audience of the expected size, namely the Hyderabad International Convention Centre (HICC). This is basically a huge hangar, situated in the north-western outskirts of the sprawling city of some six million inhabitants.

Hyderabad is vying with Bangalore (officially nowadays Bengalure) to be the cyber-capital of the Indian subcontinent. It is set in the middle, equidistant from the Arabic Sea and the Bay of Bengal, roughly halfway between Mumbai and Chennai, located at an altitude of 1,000 metres, surrounded by a geologically ancient landscape strewn with huge boulders. It is also a city with a more recent human history, predominantly a Muslim one. The Golconda fort on its western outskirts was reduced to rubble by the great (and ruthless) Moghal conquerer Aurangzeb at the end of the 17th century. (Incidentally Aurangzeb was the son and successor of Shah, who was responsible for the erection of the Taj Mahal.) After the disintegration and subsequent collapse of the Moghal Empire in the south, following the death of Aurangzeb in 1707, Hyderabad was ruled by a succession of so called Nizams and eventually became the capital of a nominally independent, princely state, which was not fully integrated into India until the 1950s. (It refused to join India, attaining independence in 1947, but was included by force the following year.) The old thronged centre, access to which passes through arched gates, sports busy bazaars and is dominated by its Charminar (consisting of four minarets forming the protruding corners of a rectangular structure - 'char' incidentally is Hindi for 'four') and may in the right kind of mood appear as out of One Thousand and One Nights, permeated as it is with numerous women shrouded in black, with only narrow slits through which to peer. It might also strike even the seasoned traveller as a suffocating mess, out of which they desire nothing more than to be delivered.

Thus the most striking drawback of the ICM in India was the isolation of the venue, a bubble that could as well have been situated on the Moon. For those delegates staying on the premises, their encounter with India would be of the most fleeting kind indeed, consisting mostly in glimpses from an air-conditioned cab during the transits from and back to the airport. Those staying at outside hotels would in general have only a marginally more intimate relation while being shuttled back and forth. One may sarcastically remark that the same may hold for many of India's more affluent residents, many of whom reside in gated communities. The almost total reliance on restricted shuttle schedules turned the delegates into a captive audience, as alternative transport was not readily available and, definitely by Indian standards, rather expensive. The contrast to Beijing was striking, where the delegate's badges allowed them free transport on subways and buses, opening up the Imperial City with all its sights (one may also suspect not infrequently to the detriment of mathematical attendance).

The internal organisation was excellent, with the exception of the chaos invariably accompanying the departure of shuttle buses from the HICC. Food at lunch may not have been very exciting, yet it was distributed quickly and conveniently. Talks were spatially concentrated, making it easy to go from one section to another. The layout (on three levels) was compact and easy to survey. There was never any problem getting something to drink or gorging on cookies and there were usually enough wall-sockets to be able to connect laptops, enjoying fairly reliable wireless connection everywhere on the premises. (In addition a cyber-cafe had been set up.) As noted above, people were usually around, although it was not always so easy to find somebody you were really looking for. Remarkably, as many people no doubt observe at meetings like these, some faces you encounter over and over again while others remain forever elusive.

In many ways the high point of an International Congress is the opening ceremony, at which the well concealed identity of the Fields Medallist is finally revealed. The prestige of the Congress in India was amply illustrated by the presence of the first ever woman to serve as the Indian President – Pratiba Patil – whose arrival in the city caused parts of it to be closed down for security reasons (and for some of us to suffer nocturnal security visits to our hotel rooms by armed men). Her entrance was surrounded by a small military escort (one of the attendants carrying a small suitcase, the contents of which was up for speculation) and was heralded by the playing of the National Anthem by a small military band and the standing up of the audience. She shared the stage with local and mathematical dignitaries, all of whom duly gave speeches, after which the president also spoke after having performed her duties, thereby magically being transformed from an idol to a human being. Unlike Madrid and Beijing, the heightened security measures were not confined to the day of the opening ceremony but continued throughout the congress, forcing participants to pass through security checks whenever entering the building. This kind of paranoia may be understandable in view of the terrorist attacks in Mumbai less than two years ago.



Ngô receives the Fields Medal


Main Conference Theatre

Four young mathematicians had been selected for the award by a committee, whose identity, like that of the program committee, was also revealed for the first time. There was Ngô Bao Châu, a Vietnamese mathematician working within the Langlands program and having solved a fundamental lemma. Lindenstrauss, son of the well-known Israeli functional analyst Joram Lindenstrauss, received his distinction due to work involving ergodic theory and number theory. The Russian Smirnov was honoured for his work in percolation and complex analysis, and there was the spectacularly well-dressed Villani from France, who was awarded for his contribution to mathematical physics, in particular Boltzmann theory. In addition to those medals, in later years more and more prizes have been added to the ceremony. The Nevanlinna Prize for work in computer science already has a respectable tradition, this time being awarded to Spielman at Yale. The Gauss Prize was only awarded for the second time, this time received by Yves Meyer. Finally in honour of Chern a new prize has been instituted this year, the recipient being Louis Nirenberg. Still the Fields Medal remains special; it is one of the very few, if not the only mathematical prize that bestows greatness on the recipient and not the other way around. This time around there was no drama connected to the award, as it was in Madrid with Perelman. In a conference immediately following the delivery of the awards, the seven medallists were exposed to the press. Unlike the case in Madrid it was well-attended and concluded with a standing buffet-lunch. The winners were asked the usual inane questions by the bewildered press to which it is not always so easy to provide intelligent and eloquent answers.

The congress offered a variety of social programmes. There was Indian dancing as well as a regular concert of Indian music (to which Western audiences usually need some preliminary tutoring). For two consecutive nights there were performances of the play 'The Disappearing Number' at the Global Peace Theatre, presented by a touring British company and depicting the well-known story of Hardy and Ramanujam (a play that is not restricted to a mathematical audience but has also caught the attention of the media). Then there was a social dinner on the second night, unlike Madrid, that was included in the registration fee. It turned out to be a bit chaotic, with food running out at an early stage, but this is India after all. Apart from the official programme there are always more or less exclusive social functions taking place at a congress. The Norwegians threw a cocktail-party in connection with the Abel lecture on the first night. The lecture, now given for the first time, was delivered by Varadjan, the Abel laureate of 2007. The Germans, the British and the French threw theirs later on but the most spectacular was the dinner reception offered by the Koreans. South Korea emerged as the winner for the bid of ICM 2014 in competition with Brazil and Canada. They have at the outset announced that they are offering support for one thousand participants from the developing world. This might provide enough of a critical mass to make it a fully attended event. The decision on the future location was, as usual, made at the preceding general assembly, which this time took place in Bangalore. Among other momentous decisions made at that assembly one may mark the choice of a permanent home for the IMU secretarial office, which until now has been ambulatory. It will from now on reside in Berlin.

EC executive Piene selling T-shirts in the IMU booth

Chess match. The 14 year old prodigy, the only to draw with the chess master.

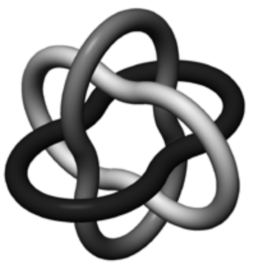
Among the spectacles offered at the congress was a performance of the current world champion Viswanathan Anand who took on 40 intrepid mathematicians in a game of simultaneous chess. All of his opponents were beaten except a 14-year-old Indian boy by the name of Srikar Varadaraj, who was offered a draw. For the promotion of this event it was argued that mathematics and chess are closely related, a statement of dubious merit at best. However, Anand inadvertently made the ICM appear on the news. He had, along with David Mumford (incidentally a past president of the IMU), been offered an honorary degree at Hyderabad University but some bureaucrat disrupted the process, claiming that neither were Indian citizens. It not only caused a delay but carried that delay so far that the ceremony had to be cancelled. Anand subsequently refused to accept the honour, in view of the fact that he was an Indian citizen after all and was unable to understand the issue. It caused a furore and was one of the top stories on the national news one evening. Remarkably, and maybe not entirely inappropriately, Anand was presented as a sports athlete being snubbed, with the moral that India does not cherish their sports heroes sufficiently. Apparently, after profuse apologies from a local minister he relented and the degree will be conferred to him at a later date convenient to him. Mumford left the ICM ahead of plan but that probably had nothing to do with the furore above.

The general lecture structure of the congress was similar to the previous congresses, adhering to an established tradition. Laudatory lectures were given on the work of the medallists, as well as the opportunity for the Fields and Nevanlinna medallists to talk on their own work later in the week. There were the usual plenary lectures in the morning and the invited speakers in the afternoon divided into sections. In addition there were short communications later on, usually delivered after the departure of the shuttle buses. For one day or so there was also a poster session. The quality of the talks varied greatly. To give a good mathematical talk for a general mathematical audience is far from easy. In fact, from a mathematician's point of view it is impossible, as contradictory demands have to be met. Mathematicians are very good at precise statements (in fact their thinking depends on them to a large extent) and they also have a penchant for systematics. These are admirable and commendable qualities as far as their work is concerned but more in the nature of an occupational hazard when it comes to communication, as precision and systematic presentations may not be fully appropriate to talks intended not so much to instruct as to entertain and inform. Unlike in a lecture course, the audience has neither the time to digest nor the obligation to be responsible on the material delivered. Some talks, I am sorry to report, were egregiously disorganised, sinning against the most elementary rules of clear exposition. Yet, for all their shortcomings, mathematicians tend to be very honest in their presentations, unlike other fields of endeavour; they always strive to present something solid and do not stoop gladly to deliver inanities.

At Madrid there were some interesting panel discussions; such events were also scheduled at Hyderabad but I am sorry to not be able to report favourably on any of them.

Much criticism has been levied against the ICM. They are seen as chaotic events to be avoided if possible. But nevertheless they continue, for all their obvious shortcomings, to play an important role in the extended social life of mathematics, a recurring event, which is supposedly the envy of many other scientific fields. To attend the conference may be considered a pain but to be an invited speaker is an honour few if any would be arrogant enough to disdain. The experience may be exhausting and overwhelming but the very people who may complain most loudly nevertheless look forward to the next event. And after all, at what other conference can you expect to meet people from very different fields and thus gain a stronger identity as a mathematician? At the ICM at Madrid Curbera hosted an exhibition on past ICMs, which attracted a lot of interest. Subsequently he published an article on the ICM in the EMS Newsletter (and a sequel was promised to me when I met him in Hyderabad), to be followed by a book. He has now been appointed the curator of the archives of the ICM and was busy during the congress recording interviews with past presidents, thereby further enhancing the archives that have been entrusted to his custody.

International Mathematical Union issues Best Practice document on Journals



At its General Assembly held August 16–17, 2010 in Bangalore, India, the International Mathematical Union (IMU) endorsed a new document giving best practice guidelines for the running of mathematical journals (see http://www.mathunion.org/fileadmin/CEIC/bestpractice/bpfinal.pdf.). The document deals with the rights and responsibilities of authors, referees, editors and publishers, and makes recommendations for the good running of such journals based on principles of transparency, integrity and professionalism.

The document was written by the IMU Committee on Electronic Information and Communication (CEIC) in collaboration with Professor Douglas Arnold (University of Minnesota), President of the Society for Industrial and Applied Mathematics, who has recently made a study[1] of unethical practices such as impact factor manipulation in mathematics. Sir John Ball (University of Oxford), the Chair of CEIC, said "*It is important that everyone involved in the publication process has full information on how papers are handled and on what basis they are accepted or rejected. For example, we are uncomfortable with the routine use of confidential parts of referee reports that are not transmitted to authors.*"

The IMU President Professor László Lovász (Eötvös Loránd University, Budapest) commented "*Well run journals play a vital role in the scientific process. Although the document is concerned with mathematics journals, we hope that those in other fields will find it interesting and useful.*"

Contact details:
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Sir John Ball (ball@maths.ox.ac.uk)
Professor Douglas Arnold (arnold@umn.edu)

[1] Integrity Under Attack: The State of Scholarly Publishing http://ima.umn.edu/~arnold/siam-columns/integrity-under-attack.pdf

Interview with Fields Medallist Elon Lindenstrauss

Ulf Persson (Göteborg)

Elon Lindenstrauss

Were you surprised finding out that you were going to be awarded a Fields Medal?
I knew that I was a candidate, though I did not expect to get the medal. When I got an email from Lovasz about a pending phone call, I more or less suspected what was in store.

Do you think that being a Fields Medallist will change your life significantly?
I certainly hope not. Probably I should try to devote a bit more time to try to increase math awareness, e.g. by giving public talks or coming to schools. I fear that getting the medal may also have the side-effect that I am saddled with a bit more administrative duties, such as writing letters and sitting on committees.

A natural question to ask is how you got interested in mathematics but in view of your father being a well-known mathematician, this might be a superfluous one. But was having a mathematician father an advantage or not?
Of course it was an advantage. At a very early age I got an idea what it meant being a mathematician, and met many mathematicians. And of course it also meant that there were a lot of mathematical books at home, to which I had early access.

The French educational system is very elitist, and in Russia there is a great emphasis on mathematics with all those mathematical competitions. Is the Israeli system similar?
No. In fact the mathematical education in Israel at school is a source of concern to me and many of my colleagues. Several of my colleagues at the Hebrew University (and also some colleagues from other universities in Israel) are quite involved with trying to improve the situation. On the other hand, some of the students we get, especially on the graduate level, are super-strong. If you think about it this is quite remarkable: in Princeton there were hundreds of applications to graduate studies from all over the world, and we chose only the best of the best. In Jerusalem, essentially any qualified applicant gets accepted, and yet the level of the better students in Jerusalem is similar to the level of the strong graduate students in Princeton.

And what was your personal experience?
I think the high school I went to in Jerusalem was not bad. We had excellent teachers in history and literature, and at least one reasonable mathematics teacher, but I don't think the basic math I learned in high school was a very significant influence on my mathematical development. However, there was a mathematician working as an assistant in the school lab, a recent immigrant from the Soviet Union who knew very little Hebrew at the time, who in recess gave me and a friend of mine some rather interesting problems to work on.

Did you ever consider other careers than mathematics?
Certainly not in history and literature, if that is what you are after. But I was seriously interested in physics; in particular I found the *Feynman Lectures on Physics* really fascinating. We did not have them at home but I was able to read them at school.

What made you give up physics?
No good reason. As you know, many of the pivotal decisions you make in your life are made on the basis of irrelevant reasons.

So you were not appalled by its lack of rigour or feeling that you lacked a proper physical intuition?
No. In fact being an analyst and a physicist is in some ways similar. In both disciplines it is a matter of knowing what is large and important, and what is small and negligible and hence can be discounted. The difference being that the physicist just claims it, while the analyst has to make the relevant estimates to prove his hunch. But basically it is about having the same intuition.

So what made you go into analysis?
It just happened, as with my move from physics, out of no real rational reason. Perhaps it was in part the influence of my interest in physics. And besides, there is algebra as well as analysis in my work.

Your work, although involving ostensible algebraic concepts, such as algebraic groups, is it not basically about analysis, at least when it comes to the tools and the way you attack the problems? The algebra is only visible in the formulations and do not go that deep.
I would not agree – many of the problems I work on have a substantial amount of algebra involved.

In your talk you mentioned both some rather abstract results, very elegant but which to an outsider do not reverberate so to speak (because as an outsider you cannot tell whether they are deep or whether they are just trivial and formal), and some very concrete down-to-earth problems, to which any mathematician can relate and become fascinated by. Is it the same when you do your research that you start with something very specific and concrete, and then only later work it into a more abstract scheme?
I would not make such a clear division of labour; it is more like a see-saw, a giving and taking. You attack a concrete question, prove something and then, as you note, you must step back and try to analyse what you really use, what is the conceptual part of the proof. In so doing you get a better understanding and can return to the original concrete problem, or problems associated to it, with renewed force. And so it goes up and down between the specific and the concrete and the more abstract generality.

When you read a paper, do you do it systematically starting from page one?
Writing is really a poor way of communicating in mathematics. It is terribly inefficient. Say you solve a problem, or rather develop an idea, of which you only have a hazy conception at first. You work a lot and gradually you come to an understanding. The idea takes shape and becomes useful. But how do you communicate it? You encode your understanding in some formal way that is acceptable to the community. Somebody reads your paper, the encoding of your idea. If he or she takes your paper seriously, there will be a lot of work and thinking, and eventually the reader will come to my original, maybe disorganised understanding – in short, having made a great effort of just decoding. There should be a more direct way.

It is the same with talks. Maybe even worse.
Maybe, but not necessarily. Of course the best way of learning mathematics is to talk to someone. In this way you get to the heart of the matter immediately. This is the way I have learned most of the mathematics I know – by talking or even better by collaborating with people more knowledgeable than me.

Maybe math papers are written with too many details. Of course they serve many functions, one being of documentation. A proof has to be established showing that there are no gaps. But if you want to learn from a proof, not just act as a referee, it is different. Perhaps a proof should just be sketchy, concentrating on the crucial steps, and letting the rest be an exercise to the reader. There is nothing as boring as going through the steps of somebody's 'calculations'. Of course, that would make more demands on the reader – on the other hand, if you want to digest a paper, that is necessary anyway.
Maybe. I do think that books tend to be better than papers. It is a pity, though, that so many good books are out of print. It should be the responsibility of the publishing industry to make them available to the community, and at reasonable prices too. In this day and age of computers, this is not only unacceptable but incomprehensible as well.

There is a project underway of digitalising the entire mathematical literature. I have seen figures of about forty million pages. That is not very much, considering that there are a thousand million Indians. But as I understand it, the project is running into legal difficulties of copyrights. To turn to another issue, is it important to you that mathematics has applications?
Of course it is always very nice to be able to point at applications.

But that is an a posteriori justification.
Exactly. And you can never tell in advance what mathematics will have applications. Who could ever predict that elliptic curves would have it?

Motivation is very important. If you are told that we need to solve those problems to improve mobile telephone communication, that does not excite you at all?
I would not necessarily say so. I have friends working in the industry, and some of them seem intellectually excited. What worries me though is the demand for immediate applications. This can lead to over emphasis of certain directions that are perhaps more immediately applicable at the expense of other equally meritable and sometimes more fundamental directions. And there is another concern I have: the culture in the high-tech business is very alien to the mathematical culture we work in. It is a culture of secrecy and patents. The essence, at least from the social point of view, of mathematics is the free dissemination of ideas. It is exactly this agreement on the universal ownership of mathematical ideas that makes mathematics so powerful and accounts for its rapid progress.

And how can you patent mathematical ideas in the first place?
Indeed so. You point to a very problematic issue, for which there is not enough awareness, certainly not in the general public. In any case, universities are not corporations. Their purpose is to nurture an intellectual culture of ideas. I think that the commercial pressure on immediately applicable research at universities is not healthy.

To return to motivation. What makes a mathematical problem interesting to you? Is it because you know the tools with which to attack it or because it is genuinely interesting on its own and you are also excited about developing the tools that are needed to solve it?
One thing that attracts me to a problem is the feeling that I can solve it, that I have some idea of where to start, that it intuitively seems amenable to being attacked. To use a climber's metaphor, you cannot climb a smooth wall – you look out for ledges and protuberances which will allow you purchase. I also try to work on problems

that seem to me to be important, that do not stand out in isolation but connect in interesting ways to more general themes. Often, I try to return to problems that I have tried and failed to solve, trying to see if I can find new footholds that I previously missed.

Are you emotionally attracted by the problems and their objects?
Of course. You have to be. Otherwise you would not be able to use your full mental apparatus. If a problem has no emotional hold on you, you can only muster a partial effort.

I also think that the emotional attachment to mathematical concepts is crucial. In a formal sense every mathematical problem can be translated into a problem of graphs. To me this is a profoundly depressing idea. I have no emotional attachments to graphs.
I happen to like graphs.

I have an emotional attachment of sorts to matrices, but of course in a sense, they are not very different from graphs.
I hope you will not be too upset if I say that one trend I think we are seeing is that graph theory in particular and combinatorics in general plays a more prominent role in mathematics than it used to.

I do not question the ubiquity of combinatorial reasoning in mathematics; on the contrary, every kind of hard mathematical reasoning sooner or later reduces to a combinatorial problem. But my point is that those combinatorial problems are in general unpredictable and ad-hoc, and that there is no general body of systematic combinatorial knowledge that you can only ignore at your peril.
I do not think the combinatorics would agree with you. And I am personally quite interested in arithmetic combinatorics – Freimann theorem and its generalisations, sum-product type results, etc. – which definitely belong to the field of combinatorics (and some of the basic statements there are most naturally phrased in terms of graphs).

Of course there are sub-disciplines, such as combinatorial geometry, which make perfect sense. But there is, in my opinion, no overreaching discipline of combinatorics. But I am digressing. Let me conclude by asking you some more human-interest type questions. Do you have any other interests than mathematics?
Well, one very significant interest that takes a significant amount of time is my family – I have three daughters. There is not too much time left after you take out mathematics, family and the various chores we all need to do.

What about physics. As you told me you were seriously interested in it once. Do you still keep up with it?
Only on a very pedestrian level. Very down-to-earth and low-brow. Recent high-powered developments such as string theory are beyond me. It would simply take too much effort.

But string theory is not really physics – most if not all of its applications have been made to mathematics, not the physical world.
There is nothing wrong with that.

True. It shows that it is non-trivial, that something is definitely going on. Even the most sarcastic of its critics, such as Penrose, would concede that. So your physical interest is on the level of the Feynman lecture notes.
Basically yes, but of course occasionally they go beyond that.

What about biology, astronomy?
As I indicated before, there is really no time; there is too much to read. I am interested in information theory, codings and the like – but you can say this is, at least in part, a professional interest as these topics are related to ergodic theory.

What about philosophy. Have you ever pondered the philosophical aspects of mathematics? Are you a Platonist?
What do you mean by that?

That you believe that what mathematics is is independent of us, that it exists independently of us.
I am probably not very original here. When I think about it, it is clear to me that mathematics is very much a human creation. But when I work I feel that I am not creating things but that I am discovering them.

This is the essence of Platonism in mathematics. What about reading? Are you interested in literature?
Of course I like to read but my tastes are not high-brow. I love to immerse myself in a book for a day or two and forget everything else. It is a good way of recharging yourself. Excellent relaxation.

So reading is not part of your intellectual life.
Maybe not – but I enjoy it.

Interview with Fields Medallist Ngô Bao Châu

Ulf Persson (Göteborg)

Ngô Bao Châu

Were you surprised learning that you would get the Fields Medal? What was your emotional reaction upon finding out? Do you think that being a Fields Medallist will significantly change your life?

It was not a complete surprise because I have seen rumours circulating about me getting the medal. But when I was officially announced by Professor Lovasz, I was completely overwhelmed. Of course, I'm proud about the medal but I also know it is going to change my life significantly. A citizen of the third world winning such a prestigious prize would and did generate a considerable enthusiasm. This means a lot for the development of mathematics and fundamental sciences in Vietnam but this also means that I would have to bear a certain amount of responsibility. It took me some time to get prepared for this idea.

I also heard rumours that the Vietnamese government is indeed very proud of you and you have been given a house in Vietnam. Is this true? And would this indicate that scholarship is still very much regarded in Vietnam.

The Vietnamese government had intention to offer me an apartment in Hanoi. What is more important is the foundation of a new institute in the model of the IAS in Princeton. This institute may have a long-lasting effect on the future of fundamental sciences in Vietnam. There may be a renewed interest in scholarship after my Fields Medal. Of course, I would be very happy if this is not only ephemeral.

How did you become interested in mathematics (the influence of a parent, a teacher or somebody else)? When did you realise that you wanted to devote yourself to mathematics?

I became interested in mathematics after having been enrolled in the special class for gifted students in mathematics at middle school. But I realised that I would devote myself to mathematics in the course of preparing my PhD thesis under the supervision of G. Laumon in Orsay.

Did you grow up in Vietnam? If so did you feel that this was a disadvantage to you in pursuing your mathematical interests?

I grew up in Vietnam. My parents are still living in Hanoi. In my childhood, people in Vietnam experienced terrible economical difficulty. It was the aftermath of 30 years long war, the consequence of American embargo, and also the disastrous economical policy. However, people in Vietnam were more genuinely attracted by scholarship than they are now. From this point of view, I was not particularly disadvantaged. I had the privilege of having some professional mathematicians as benevolent private teachers when I was in middle school and high school.

Did you ever participate in any Mathematical Olympiads while in Vietnam? What is your opinion of them?

I did participate in the IMO in 1988 and 1989. The preparation for the IMO attracts gifted children to mathematics and gives them a taste for challenging problems. In Vietnam, most of the mathematicians of my age were IMO participants. At the same time, some of the IMO participants got disgusted by this too intense preparation. I understand that in China very few IMO participants pursue higher study in mathematics. The focus on challenging problems is also somehow wrong because mathematics is also about understanding deeply simple phenomenon.

How did you enter the field you have been working on? Were you directed by an advisor or was it something to which you gravitated naturally?

At that time, many students of the Ecole Normale Superieure were attracted by algebraic geometry and the Langlands programme. I got advice from my tutor at the ENS to do my PhD thesis with G. Laumon. I understand that the director of the math department of the ENS at that time M. Broué convinced G. Laumon to have me as a PhD student.

The Langlands program involves mastering an extensive apparatus. Was that a problem for you, trying to master so much material before you could really start doing research? Or were you able to start at an early stage, much of what you needed to know being picked up on the way.

It is true that working in the Langlands programme requires an extensive apparatus. In my case, I started working on a rather concrete problem in my PhD thesis for which we need to know some apparatus but not the whole machine. At that time, I already got a vague idea of how the fundamental lemma should be proved but I was obviously not prepared technically to implement it. It was by working on other problems in the Langlands programme that I gradually digested the necessary apparatus. A certain amount of luck was also necessary. I read the paper of Hitchin at least three times before understanding that it was exactly what I needed.

Now the lemma for which you became famous was originally thought of as being routine, and Langlands believed it could be proved in a day. Could you expound on the experience? It took several years and 700 pages to write it down eventually. Why was it thought so routine initially and what made it so hard in the end?
With the trace formula, certain deep arithmetic phenomena were reduced to an identity of integrals that looks like an exercise in combinatorics. For groups with small rank, it is indeed an exercise but not an easy one. I don't think that Langlands believed it could be proved in a day. He believed that the fundamental lemma could be proved with a reasonable amount of hard work but within the limit of local harmonic analysis as is its statement. It happens that, in general, these integrals cannot be calculated by elementary means because the result involved expressions like the number of points on abelian varieties over finite fields. The proof we know does not involve any calculations but a deep understanding of the geometry of some integrable system that was discovered by Hitchin in studying certain equations coming from mathematical physics. Of course, when Langlands stated this conjecture, nobody could realise the connection with integrable systems. The field needs maturation through the development of the so-called geometric Langlands program where the Hitchin system also has an important but different role. Also, the formidable machines like l-adic cohomology and perverse sheaves need to be known in a wider public before they can be used as effective tools in as remote an area of mathematics as harmonic analysis. This is why a lot of time and hard work of many people are needed before the fundamental lemma is proved.

What mathematicians in the past do you particular admire? Are any of them an idol of sorts?
It is certainly not original to admire people like Grothendieck, Langlands and Deligne. I should add however that we should not put Deligne and Langlands in the category of mathematicians in the past. They are still very active.

How do you like to work? Do you read a lot of mathematics or do you mostly learn from doing and talking to people? Of course this question has two aspects depending on whether it refers to your student period or your present period.
I enjoy very much reading books. Being quite shy, I do not talk easily to other people but I learn a lot from talking to people I know well.

Your area is very pure. Are you concerned about applications?
NBC: I'm not really concerned by the applications of my own mathematics. But I keep a keen interest in the mathematics that may have applications in real life.

Did you ever consider an alternate career to mathematics (you indicated that only at your PhD level did you realise that you wanted to devote yourself fully to mathematics)? And if so, what?
have a lot of interests outside of mathematics. But they have never figured as an alternate career in my mind.

Interview with Fields Medallist Stanislav Smirnov

Ulf Persson (Göteborg)

Stanislav Smirnov

Were you surprised getting the Fields Medal? I want an honest answer, not a modest one.
Certainly I will give you an honest answer. I was not totally surprised since many colleagues said that it was a possibility. But mathematics is going through very exciting times nowadays, with much progress in several areas, and there are many other worthy mathematicians. So I see it more as recognition of the field I am working in, and it is nice to get attention from the outside.

Has getting the Field Medal in any way changed your life?
I do not think it has, and I hope it won't. In one way it was nice having this six month buffer when no one else knew about it, so life was proceeding as always, but I was surprised by the attention we received here. I hope it will soon subside. As to changing my life, I am not really sure what you mean.

Villani expressed a certain apprehension that he will now be saddled with high expectations.
I do not feel that way. I often try to attack difficult problems with no guarantee of success, so I am always prepared to have some unsuccessful projects. The only thing

that conceivably may worry me is that people may look to me for new directions, that I will be seen as a trendsetter. I do not like to set up fashion in mathematics. I do not think it a good idea to have fashion in science.

But it is inevitable; after all mathematics is also a human activity like others.
True, but it is unfortunate. There are many mathematicians working on very important and interesting problems, but lacking outside interest or recognition because their areas are not fashionable.

But that will change of course. Let us not go astray but get back to you. How did you get interested in mathematics, when did you realise that you were very good at it?
My grandfather was a mathematician by education, working as professor of engineering, and he gave me many popular books about mathematics and physics when I was little. So I wanted to do something scientific when I was quite young, but more in the spirit of designing airplanes or spaceships.

When I was 11 I took part in a mathematical Olympiad, which turned me to mathematics. I started going to mathematical circles and became really interested.

Now you grew up in Russia, in the Soviet Union to be precise. There mathematical competitions play a very important role in the educational system as I understand it.
I would not say that they are part of the educational system; they are outside it.

In fact I did not have any particularly inspiring math teachers for my first eight years in school, so only getting to an Olympiad made me realise that I had a special interest in mathematics. Olympiads (not only in mathematics but in everything from literature to biology) were at the time very widespread in the Soviet Union, with many students participating at the school and district level. So they worked as a sieve to filter out talented and interested students who were not spotted by their teachers. Then there was a very wide reaching system of extracurricular activities for schoolchildren interested in mathematics, from correspondence-based for those in provincial towns to mathematical circles in big cities like St. Petersburg.

So good pupils were inspired and encouraged to participate in them and that certainly must add to the prestige of mathematics. After all those Olympiads are considered almost as athletic events, as the very designation 'Olympiad' indicates.
Correct. It is very hard to be a good teacher of mathematics, much harder than to be a good teacher in history say. It is so much harder to make the subject attractive to the general student. And many potentially interested pupils had the misfortune of never having a good mathematics teacher in their school. So in this way those Olympiads, and also those mathematical circles we had, were important, as I have already said. Students were given the opportunity to see mathematics beyond the school curriculum.

You did very well in the Olympiads. How did it affect you? And do you think competitions are good to foster mathematical talent?
In my case my success certainly gave me self-confidence. It was also very interesting to meet many students from different places, all interested in mathematics. But I think that school and district level Olympiads in many ways are more important than IMOs: they attracted to mathematics many students who otherwise would have never thought of doing science.

Is there not also a risk that doing well in those competitions can saddle you with high expectations and inhibit you in the future?
In my case it was probably beneficial – it gave me the confidence that I could do mathematics. But I can imagine it being harmful as well.

The strange thing is that there is such a high correlation between doing well in a mathematical Olympiad and doing well professionally. One event is a sprint, the other a marathon.
I would not say that the correlation is that high.

It is higher than what you would expect.
True. There are certain advantages that come with being good at those things, like combinatorial thinking and manipulating formulae quickly and correctly.

… without thinking?
Almost. If you now run into some messy calculations you always have the confidence that you can swing it, say after four hours hard work. You just sit down and do it.

So you are talking about mathematical muscular power, of being self-sufficient.
That is one way of putting it. It also has disadvantages. At least I felt during my first years as an undergraduate a difficulty in absorbing new material and thinking up my own problems. But at least the latter difficulty soon went away.

Let me get back to your school days. When I, along with other Westerners, participated in the Olympiads in the late 60s when they became open to students outside the Eastern Bloc, we were struck with how young the Russians were. The age-limit was 19, while the Russians were 15 and 16. Part of this of course reflected the much tougher competitions the Russians had to confront but also I believe that they benefited from a more advanced education, being exposed to real mathematics at an earlier age. Would you like to comment on that?
Well, in the mathematics circles I went to from 5th to 10th grade (11 to 16 years) the emphasis was on the problem solving, rather than on extra theory. And I think it was certainly a good idea, at least up to the last grades. The problem-solving experience at circles and Olympiads helped me a lot in my research life, as I have already noted, though at times I think that I have spent too much time on it.

For the first eight grades I was in an average city school, but during the last two years I went to one of the well-known mathematical high schools #239 in St. Petersburg. That was a very enjoyable experience, for one thing because I encountered exceptional teachers for the first time, but even more important I would say because of the good classmates. But again, I cannot say that we learned much more theory there, rather we did it on a more solid basis. When I was 17 and about to enter the university I only knew a bit of calculus and that only because I had read Walter Rudin's 'Principles of mathematical analysis' shortly before.

The Soviet school system strikes me as being rather conservative in the good sense of being solid and demanding, especially when it came to mathematics. Can one speak about a special Russian tradition in mathematics? In the West the Russians were held almost in awe, and it was the feeling that even secondary Russian mathematicians were first class by Western standards. And definitely possessed a wider culture (mathematical and otherwise) than their Western counterparts. What would you say to that?

We certainly have good traditions, with studying mathematics considered more of a vocation than a mere job. But I do not think the difference between good Western mathematicians and their Russian counterparts is really so pronounced.

What do you remember from your undergraduate days in Leningrad? How come you went into analysis?

Already as undergraduates we had access to research-oriented courses. Some were rather advanced but specially tailored to beginning students. I recall that during my first two years as an undergraduate we had such courses as Viktor Havin about harmonic functions and vector fields, Anatoly Vershik about ergodic theory, Oleg Viro about topology of manifolds, Andrei Suslin about Galois theory, and we attended courses aimed at older students as well.

A great variety of topics were treated and I remember hesitating between doing geometry or analysis or perhaps ergodic theory. In the end I chose analysis because Havin ran a very good research seminar for students and I started working on problems I picked up there. Later I wrote my Master's thesis with Havin, and that was a very nice experience. Of course it helped that Leningrad had a very solid tradition in hard analysis, not unlike that of Scandinavia.

Did you have other interests than mathematics at the time? Did you consider physics seriously?

Yes I did. At least while still in high school. But my problem with physics is that you have to learn so much. You need to know and have in the back of your mind even stuff you are not actively working on. I have such a bad memory and tend to forget things I am not actively working on.

You left Leningrad, which I guess was already back to St. Petersburg by that time, for Caltech.

Yes, Nikolai Marakov invited me to do a PhD with him. I had followed, some years before, his course at St. Petersburg Steklov Institute on geometric function theory, and I liked the subject very much. Caltech years went very nicely, and I have learned a lot of complex analysis. I also started to work in dynamical systems. There, for the first time, I briefly thought about percolation, after reading Robert Langlands' papers. In particular, he published, in the AMS Bulletin, a paper with Philippe Pouliot and Yvan Saint-Aubin, where they made precise a few physical observations originating in Conformal Field Theory.

There were very beautiful conjectures, supported by John Cardy's physical arguments and convincing numerical evidence to support them.

Later I learned that this paper attracted many mathematicians to the area, including Lennart Carleson, Peter Jones and Oded Schramm.

It was actually quite natural for analysts to attack these problems – complex analysis certainly had a role to play.

At that moment I had not made much progress. But later, when I was in Stockholm, Carleson and Jones were very interested in such problems and I restarted the project.

Why did you go to Stockholm?

Carleson. He is my hero. And there were many other top people in dynamical systems, which I was doing at the time: Benedicks, Eliasson, Graczyk, Kurt Johansson…

I was going to ask you about mathematical heroes later?

Carleson is certainly one. Back in Leningrad when I was an undergraduate he was like a demigod. I never hoped to work alongside him one day. And Lennart is also a very good person.

You mentioned earlier that you have a problem with learning mathematics. Knowing a lot – is that a disadvantage in attacking a problem?

Depends. It certainly could be, but of course sometimes it is also absolutely essential, not only advantageous, to know the problem's background. In papers you seldom find negative results, such as this and that approach does not work. That is a pity. It could save a lot of time to know what does not work.

Massive Internet collaborations were discussed at this ICM, and this is one thing they are good at: cutting away unsuccessful approaches and highlighting difficulties.

Mordell once explained his success due to ignorance; he tried approaches experts 'knew' did not work.

True. What does not work for me might work for you. It happened to me once. I tried thrice a certain approach to a problem. It did not work. A co-worker did the same. He succeeded on the first try. It turned out that in a preliminary transformation I repeated thrice a stupid calculation mistake. This was actually contrary to my old Olympiad training, so I was surprised.

You have worked in different fields. How do you effect the transition? Do you read up a lot on the literature or do you take the plunge?

The plunge with a specific problem in mind. It helps of course if you are not alone, but have a co-worker. Better still to have a co-worker who is already familiar with the other field. He should not be too much of an expert; then you are only being taken on a ride and learn nothing. So you try to solve the problem, and in the process you pick up what you need to know. This is quick and it is effective.

Which makes me think of reading math papers. Those are usually written in a logical linear way. Is that the way to read or write them?

That is a good question. I do not really know. Of course when a student I read papers from page one to the last, and I wrote my own accordingly. I was taught so, but now I am not so sure. Sometimes the most important thing in a paper is a remark or an observation hidden in the middle of a proof. If you know what you are looking for you can skip the preliminaries and find it. But many people would never reach this point. So one has to keep in mind that some interested people will only read the introduction.

In that sense the physics papers are better – they also have a conclusion at the end.

The same thing with talks. You expect them to be logical and systematic with a clear narrative. On the other hand the thing you could expect to bring back from a talk is some remark. Still a talk consisting of random remarks would be torture to listen to.

Even a not very understandable talk could be good. It could contain some idea that you could remember for years and then make sense and fit into what you are working on. On the other hand some talks could be quite enjoyable but leave no deep residue. You are entertained for the moment and afterwards you forget everything. Talks and articles are different. A bad article you would tend not to read to the end, but even if you do not understand the beginning of a talk you are stuck there, you have to listen. That could be good – maybe something interesting would follow.

It is customary at such interviews to ask about your stand on philosophical issues. Are you a Platonist?

I think I am. But I do not think, for reasons of mental health, one should think of those matters too much. It is hard to say something new on the topic, and if you delve into them too deeply you get lost. It is enough to be dimly aware, no need to dwell and formulate on the issues. As to philosophy the great mystery is this Eugene Wigner's 'unreasonable effectiveness' of mathematics – why it should be so deeply connected to the physical world. This really intrigues me. I see it as a great mystery.

In addition there is nowadays also an 'unreasonable effectiveness' of physics in mathematics. A physical intuition seems very helpful in solving mathematical problems. Nothing like that is the case with say economics and biology.

Wigner did not discuss this point much. It was already present in his time, but now it is much more impressive, with physics motivating very abstract areas of mathematics – take for example the influence of Edward Witten's work. It is truly amazing.

As to second part of your remark, I do not really agree. There certainly have been striking applications of mathematics. For example, stochastic processes, the Ito calculus as applied to economics. It certainly has been unreasonably effective, those crude approximations giving such precise predictions, barring a major crisis of course.

No, I do not agree with you. I think there are going to be great applications beyond physics, and certainly to economics and biology, in the future. And I hope it will go the other way around as well. The problem is of course one of communications. Mathematicians have a hard time learning biology, not to mention the other way around.

But an important problem in biology is to predict the spatial structure of complicated molecules, as those may give clues as to how they will interact with each other. But this seems to be a rather tedious problem of numerical simulation with no particular principles in the background. It is exactly the beautiful simplicity of the mathematical formulations of physical general laws that attract the mathematician. Biology does not work that way. It is far too complicated, far too adhoc. There are no simple overriding principles. Mathematicians are shunned by biologists.

I think that our current understanding is insufficient, but we will eventually find an elegant mathematical structure behind many biological processes, just like in physics.

Speaking of molecules, though it seems complicated, there are simplifying mechanisms – look, for example, how certain enzymes unknot the DNA molecules.

In some other areas of biology we are already better off. Say, the general principles of evolution could be understood mathematically – with greatly oversimplified models, of course. Completely and accurately describing the real thing is more of a problem.

It might be complicated by necessity, but still it looks eventually doable – many people are working on it, and some aspects are well described.

What looks a more puzzling problem to me is pre-evolution, how those basic complicated molecules of life emerged in the first place.

The theory of natural selection is indeed intellectually a very simple and powerful idea but it has no predictive power at all. As in the tales of Kipling you can come up with all kinds of evolutionary scenarios. It is a mystery to me that all life on earth is DNA based. Why are there not competing forms of life living side by side? The variety of DNA-based life is truly striking. Could the evolution of DNA be the real bottleneck?

I do not think this is so surprising. For one thing, we understand well replication or error-correction for linearly encoded information, so perhaps this is indeed the best way to encode the genome.

Maybe our discussion is being derailed. What about your interests outside mathematics?
I have many but my kids are my priority. I have a girl of eight and a boy of four and I love spending time with them: playing, studying, reading, doing sports, traveling (though we decided that India is too far a trip for them and now they are jealous that I get to see the elephants and they stay home). As to sports I particularly like to ski, which basically means downhill of which there are plenty of opportunities here in Switzerland where I live nowadays. I got enough of cross-country when I lived in Russia. And sure, I try to keep up with what is happening in general, culturally as well as politically, be it music, films, and I read a lot, or at least as much as there is time to.

Interview with Fields Medallist Cédric Villani

Ulf Persson (Göteborg)


Cédric Villani

How did you feel learning that you would be a Fields Medallist? Surprise?
There certainly was some surprise but that was not the major emotion. True, at first I feared it might have been a joke, so I waited for confirmation before I felt I could really rejoice. It certainly was an emotional moment. On one hand I felt relief because the acknowledgment of a Fields medal is something that can never be taken away from you. On the other hand I feel pressure, pressure to live up to the expectations that being a Fields Medallist incurs.

So it has changed your life?
It certainly has.

To start from the beginning, when did you realise that you wanted to become a mathematician?
My taste for mathematics started very early. I was always good at mathematics. But when I was 13 or so I had a couple of teachers that went beyond the standard curriculum and that was very inspiring for me. It might not have been so good to most students but I certainly benefited from it.

What in particular did you encounter?
Learning about what is a group, that sort of thing. The joy of understanding new concepts is crucial. I remember vividly the simple exercise: show that a real-valued function is the sum of an even and an odd function. Of course I solved it easily but was taken aback by the solution. For the first time I understood that a function is not necessarily given by a formula but can be constructed abstractly from other functions. I also remember marvelling about the theorem according to which in a parallelogram the sum of the squares of the diagonals equals the sum of the squares of the sides, being fascinated by the simplicity and beauty of this identity.

Yet, as a young kid I would not have bet on becoming a mathematician. If you had asked me, I would rather have said I wanted to be a palaeontologist – I was crazy about dinosaurs. When you think about it, a good palaeontologist needs above all tenacity, rigour and inventiveness, and somehow these are the same qualities needed by mathematicians.

You had no mathematical parents?
No, both my parents were teachers of French literature. But of course I grew up in an intellectual home.

The French system is very elitist. Is that a good thing?
I have been told that the French high school students do not do better on average than say the Scandinavian. But certainly when it comes to the good students, the French have a very distinguished tradition and they do very well.

In particular you have those high prestige schools grooming the elite.
Yes, after my baccalauréat (i.e. the finish of high school) I spent two years in intensive preparation classes. This was very good and it gave me a very good grinding. Eventually I became a student of Ecole Normale Supérieure.

How was that?
That was very stimulating of course. In my early years I started to redefine myself, getting a new self-image so to speak. I became interested in clothes; before that I was unusually scruffy. I adopted the haircut, of longish hair, which I have kept ever since. Of course this is by itself trivial, although dressing is not entirely trivial as it defines

you socially, and it should be really seen in a larger context of cultural awakening. I became interested in classical music; when I grew up there had been very little music at home. I discovered the cinema and I also became involved in student politics, which took a lot of my time.

You were becoming a young adult.
Yes, as a high school student I had been very shy and retiring. In my early 20s a new world opened up.

Mathematically too?
Of course, but I would probably say that the general cultural awakening at the time was more significant.

You are an analyst. When did you become aware that you were an analyst?
Me becoming an analyst was more by chance than design. In fact initially I was quite interested in algebra and did very well. And algebra is the mainstream of French mathematical culture. In fact when you study you have your ups and downs. It happened that the analysis course was given during the time I was very active academically, while the algebra course was given when I found other things to do.

Do you regret it?
Of course not. I would not go so far to say that there is an analyst's brain, as opposed to say an algebraic one, but clearly my way of thinking of mathematics is that of an analyst at heart. Obviously it is congenial to my mathematical temperament.

But why mathematical physics?
At some early stage I had decided that I would like to do something applied.

But the kind of mathematical physics you do is not very applied is it? Is it not quite pure after all?
I hate this distinction between pure and applied. I do not think that one can make a real distinction; one thing blends into the other. True, there are very diverse sources of inspiration in mathematics, some coming from within mathematics, some coming from the "real" world, whatever this means.

But one can speak of a difference in attitude, if not in subject matter?
That is true. Some so-called applied mathematics is just application. That is not really mathematics.

Maybe we should leave this subject for the moment and proceed. How were your days of being a graduate student? Many people feel quite lost as graduate students, especially initially.
That is true – that I was quite lost initially as most graduate students – and my advisor Pierre-Louis Lions did not give much direction, which was a very good thing. In this way you learn to find your own way and become autonomous. After all, the role of an advisor is to advise, not to direct. In my case, I also think the transition was very much helped by Yann Brenier, my excellent mentor during my ENS days. It is so hard to find your right way in mathematics, especially by yourself, so the assistance of an older, more experienced individual is invaluable. Other researchers who have been very influential were Eric Carlen, at the time at Georgia Tech, and Michel Ledoux from Toulouse.

What are the mistakes many beginning graduate students make?
I think they often fall into the trap of systematic study. I myself read a book, the first really research-oriented book that I read, written by Carlo Cercignani. Incidentally, I am so sorry that the author just died – he certainly would have treasured the increased attention our field has received because of my medal. I read it cover to cover. That is one thing you definitely should not do with a book of mathematics, especially as a beginning student. You do not have the overview – you proceed slowly and incrementally, only having local understanding.

So what changed?
Becoming focused on a problem. You do not need to know very much; in fact knowing a lot and being well-read could be a disadvantage. It certainly inhibits you…

… seeing so much polished and powerful mathematics and invariably comparing it with what you can do yourself.
Exactly. Always try to work out things for yourself. If you eventually get stuck, you are in a far better position to appreciate the literature on the subject.

Is there not the danger of reinventing the wheel? There is so much mathematics around nowadays; there is no way you can reinvent it by yourself.
Nor is there any need for it. But you really need to figure out things for yourself; there is no other road into mathematics. Even the work of others – you have to interiorise and reinterpret it in your own feelings.

So things came into place?
Yes, I was given strong encouragement, such as the definite possibility of getting an assistant professorship should my PhD advance well. This helped a lot, and made me more focused on finishing, and put an end to my social student obligations. Almost overnight, I was back to focused research.

To return to the elitist nature of French education, in Russia especially there is the tradition of Mathematical Olympiads. There is nothing similar in France? True, I guess the French send a team to the International Olympics like everybody else but it is not part of the French educational system.
That is true. We have nothing like those math competitions in France. In Russia it is very different, and Smirnov got perfect scores when he was a participant, like many other successful Russian mathematicians. In fact there is a surprisingly high correlation between doing well in

the Olympiads and being a first class mathematician. I mean, the correlation is not so high after all but given that Olympiads are so different to real research, you might have expected the correlation to be really small. As for me, I never participated in Olympiads, neither by the way in nationwide mathematics competitions. My parents wanted me to compete at a national level but my teacher opposed the idea, thinking that it was irrelevant and potentially discouraging. Looking back to it, she was certainly right – having not received the proper training at the time I would probably not have performed so well. And anyway I never suffered from it. Really Olympiads and mathematical research are very different sports.

It is like the difference between a sprint and a marathon.
Yes. Doing mathematics long-term is so different. You need to find the right problem. This is extremely important. Some problems are just too easy, while many other problems are not only too hard but what is worse, lead nowhere. Veritable cul de sacs. Of course nothing of that is an issue in a problem solving competition.

Doing well on a mathematical short-term competition is evidence of so to speak muscular strength. As a mathematician you constantly come up against technical snitches. You need to be able to handle them by yourself. If not, it does not matter if you have good taste and exciting visions. You need to be able to deal with the mundane real world of mathematics too.
To some extent that is true, and self-sufficiency can be important. However there are exceptions too, and some mathematicians are excellent at putting together a grand vision without being able to get some of the technical details themselves but nevertheless being able to find out who are the right people to ask and getting their help. One great example is Nash's proof of continuity of solutions of parabolic equations with non-smooth coefficients, one of the highest achievements of partial differential equations ever. Nash came up with the great design himself but for some of the technicalities he needed help from others. For instance the so-called Nash inequality was in fact given to him by Stein. And by the way Nash did not perform well in the national US math competition - the Putnam prize.

On the other hand doing well in mathematical competitions has a down-side. It may set up unreasonable expectations at a very young age.
That is true. If you are a Tao, of course this is not an issue – you just take it in stride.

But for people below that level, early success could have an inhibiting effect.
That is true. I was spared the risk of that inhibiting effect.

To return to the issue of applied versus pure. Manin speaks about the difference between mathematical models and theories, referring to the latter as the aristocracy of models. A mathematical model, as I understand it, is something you concoct more or less ad-hoc in order to serve as a vehicle for simulations and predictions. The Ptolemaic model of epicycles is the proverbial example. Given enough tinkering with epicycles the model can give arbitrarily precise predictions. But it is ad-hoc; it explains nothing. As I understand it, much of applied work of mathematics is actually on this level. A theory on the other hand has explanatory power. In fact you are tempted to believe it is 'the real thing' not just a convenient way of ordering facts and data. I would say that the Navier-Stokes is a model, while the Maxwell equations constitute a theory. As to the latter you get out much more of those equations than you put into them. Special relativity was hidden inside them via their invariance under the Lorentz transformations.
I do not agree with you. I would definitely not make such a clear cut distinction between theory and model…

…I guess Manin never meant it that way…
…You speak about models based on principles on one hand and phenomenology on the other, with Navier-Stokes being an example of the latter. In fact I once spoke to a world-expert on fluid mechanics and I was surprised to learn that he adhered to the opinion you have just formulated. To him the Navier-Stokes was essentially ad-hoc, meaning that you added fudge terms to account for certain phenomena. But those fudge terms in the Navier-Stokes come up very naturally out of Boltzmann theory. He did not know that. So as you see, Navier-Stokes is also based on general principles, although that does not seem to be well-known. Thus you cannot really make this distinction – principles and phenomenology blend.

Now the Boltzmann equations are based on Newtonian mechanics, and on the micro level they are completely deterministic and can be run both backwards and forwards, yet you have that basic notion such as entropy that gives time an arrow.
One should distinguish between Vlasov theory, in which entropy as well as energy stays constant, and Boltzmann theory. Both are based on Newtonian mechanics but the interaction ranges are different. In the Boltzmann case, close encounters are dominant; in the Vlasov case the interactions are macroscopic. Boltzmann's equation is fundamental to rarefied gas dynamics such as in high atmosphere, while Vlasov is the cornerstone of classical plasma physics and galactic dynamics. For this equation the entropy does not change.

You explained it by saying that the history of the system is preserved, but invisibly because hidden in the tiny variations of positions and velocities. On the other hand we have the modern paradigm of quantum theory, in which it no longer makes sense to speak about arbitrary positional and momental precision. The Newtonian model is physically irrelevant at that stage.
That is true. But I see the model as having an intrinsic worth. Physicists on the other hand sometimes use several models. I find that somewhat dishonest, unless they are very upfront as to what they are doing.

Is this not the difference between a mathematical physicist and a theoretical physicist? To the mathematical physicist, physics certainly provides inspiration but the objects they create will have interest regardless of physical relevance.
I would not agree. To me physical relevance is very important.

One thing that is striking about the mathematical-physics interrelation is that it is a true one. It is a two-way street. Not only does physics benefit from mathematics but also physical intuition can be very helpful in solving mathematical problems. Lately physicists have not only supplied mathematicians with powerful new ideas, they have even been able to supply proofs of facts which have previously stymied the mathematicians. Nothing like that is the case with say economics or biology. A biological intuition (to say nothing about an economical one, if such exists at all) seems to be of no help whatsoever in solving a mathematical problem.
I do not agree. As for economics, let me just quote one example which I like: the auction algorithm used for the numerical simulation of optimal transport – one of my favourite areas of research. Well, the auction algorithm exactly reproduces the mechanism of auction sales where participants bid one after another. This even includes the principle of minimum on the prize increment and works quite well in many situations. And in computer science they develop programs imitating natural evolution.

But this is not exactly what I have in mind. And besides, is not biology far too complicated to lend itself to beautiful, understandable mathematics? A crucial problem in biology is to understand the spatial structure of complicated molecules, which gives you clues as to how those interact with each other. Those problems are solved by simulations. The results may be very interesting but getting to them not.
It is true. Biology is very complicated. Nevertheless I think that mathematics (and biology) will benefit greatly from more mutual interaction, although I believe that physics and theoretical computer science will be the main sources of inspiration for still many years to come.

Do you have any mathematical heroes?
Yes, one definitely, and that is Nash, whom I have already mentioned. Years ago I fell in love with his paper on continuity of elliptic and parabolic equations with discontinuous coefficients. Later I found out that Gromov also had a lot of admiration for him. Reading Nash is such a revelation. He really had the mind of an analyst. The way he analyses a problem, looking for its crucial features, reducing it. Nash did not bother with reading, he just attacked a problem, not ashamed of using elementary tools – his solutions are so self-contained.

He had a very short career.
Yes, only ten years.

I think that using elementary tools in mathematics is very satisfying. I fear that many professional mathematicians end up combining high-powered theorems, which they do not necessarily understand and thus treat as black-boxes, in order to get new results. It reminds me of modern civilisation in which you all the time are using sophisticated gadgets. Life in more primitive societies must have been far more satisfying, when you made your own tools.
I think one should be wary of getting carried away. Take the computer. I certainly would not like to live without it. It is such an important part of my life; I really cannot imagine being deprived of it. One has to be realistic.

Yet, using elementary methods in mathematics is satisfying. Are there really still areas of mathematics in which you can work without learning a huge apparatus?
I am sure there are. There are bound to be such areas but I am at a loss at the moment to identify any. Take someone like Gromov. He has really done great work in geometry by elementary means. So much can be done by introducing a new point of view. By simple means you can really transform a subject.

Your talk was very nice, especially because it was easy to motivate to a more general mathematical public. In other fields the speaker spends almost all the time setting up the basic definitions and hence goes nowhere.
It is true but even in such fields it is not impossible to give accessible talks. You just have to work at it.

I think that a common trap many speakers fall into is to be too precise and too systematic. After all you are not giving lecture courses; you are out to entertain and inform, not to instruct. The audience is not expected to take a test on it afterwards.
That is true. There is no reason not to be sketchy. Some experts may react but let them fume in private.

Precision is what mathematicians are professionally trained to be very good at. Second nature so to speak. But to give an accessible talk is not so much a matter of simplification. It is a matter of viewing your work from above, to see its significance and its position in the general mathematical landscape. Something many people may refuse to do. Just to present an illuminating example can save you from the need of presenting formal definitions.
At the last ICM in Madrid Ghys gave such a beautiful talk on dynamics illustrated by animation.

Yes I know. People were very excited about it afterwards and it was considered to be one of the high points of that congress. I still regret stupidly having missed it.
But it takes a lot of work.

But work you may expect from plenary speakers at a congress. So let me change tack and speak about more so-called human-interest things. Do you have other interests than mathematics?

Of course. I do not have so much time but I listen to a lot of music, read a lot. I go to the cinema and follow things in physics and biology, especially astrophysics.

Were you interested in astronomy as a child?
Yes, as a child, but then I lost interest when I got older and found biology more fascinating. In later years I have become interested in physics more, including astrophysics of course. But my interest is contingent upon a key; I need some specific motivation.

You are talking of mathematical keys?
Of course. I need to be able to understand how the physics relates to mathematics, or rather how I can apply mathematics to the situation. To refer to a previous question about the physicist's intuition; what I am really interested in is not to confirm the physicist's hunch but to actually discover, through mathematics, new physical facts.

This is of course very close to your professional work. What about philosophy? Do you have any interest in philosophy?
The problem with philosophy is that there are too many contradictory views. People often stick dogmatically to their systems and not all of them can be true.

Or maybe none. I like to think of philosophy as the poetry of science. You proceed by evocation rather than argument, in fact you try to enter the realm beyond the rational and dwell on the metaphysical fringe. So doing is fraught with danger and you cannot really avoid the risk of being pompous and silly. What about the ongoing debate about Platonism in mathematics?
I am definitely a Platonist. Mathematical truths exist independently of us. True, there are other human aspects of mathematics, such as fashions, but that is inevitable. I would like to think of mathematics as being science, art and social activity. It is definitely a science, and the assurance that there is something 'out there' is very crucial in making you pursue research. On the other hand you can sometimes be led astray, believing that there are hidden connections where in fact there are none. Being a Platonist I am not ashamed when finding a theorem of mine beautiful to say so – because the beauty is not due to me, it existed before I discovered it, just as when you dig out a beautiful gem.

Yes we have Newton looking for nice pebbles on the beach. As a working mathematician you definitely have the experience of facts kicking back at you. You are constricted to a concrete reality, which feels almost physically palpable. This I think is the danger of abstraction. If you just make definitions, you encounter no resistance. Resistance is the crucial thing in concrete, honest mathematical work.
But of course abstractions are inevitable and very important. And the French are known for their abstract bent.

But it is one thing to have abstraction forced upon you by grappling with concrete problems and quite another to use it as an evasion from difficulty.
True, abstraction should stem from concrete situations, yet I would be reluctant to be so categorical about it, as you seem to be.

What about literature?
As I already told you both my parents were teachers of French literature and as a child I read a lot.

What do you like?
I have omnivorous tastes. I even enjoy comic books very much. To me action is very important in literature …

… So you have not read Proust?
No, I have not read him. He is no doubt very elegant and caters to those of refined literary tastes but I prefer writers such as Balzac and Zola. Or Dostoievski, Melville, to quote a couple of examples from other cultures. I know that Zola is not considered very elegant but he writes with such force.

… He had a mission.
Yes, he had a mission. And I love Balzac. Also I enjoyed when I was young very much the Sherlock Holmes stories but above all I was a fan of science fiction. There were a lot of science fiction books at home. I can easily rattle off a dozen names, would you care. And while we are on the topic of literature and such things, I also listen to classical music; I prefer to have it on very loud. At one time I myself played the piano on a very regular basis but regrettably I had to give that up because of time. Rock music, text songs, are also on top of my list – there has to be some energy.

Other things?
Steady sustained physical exercise, which allows me to think, such as cycling.

France is really great for cycling. It is my favourite place when it comes to cycling. Drivers respect you – they cycle too.
Yes, we have so many good roads and not so much traffic on them.

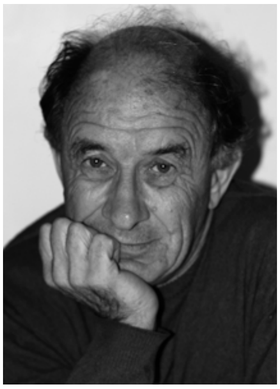
I am afraid that I am keeping you. We were only allowed to sit here until ten. Do you have any idea of the time – my cellphone broke down.
Are you going to remember everything we said?

I hope so. I will find out when I start writing it down, which I will do as soon as I discover an available socket into which to plug my laptop.
Meeting personally is so much better than doing emails. Just as in mathematics, people still need to meet personally.

This is what an ICM is for. The talks are just an excuse…
… I would not be so categorical.

Vladimir Arnold (12 June 1937–3 June 2010)

S. M. Gusein-Zade, A. N. Varchenko



Vladimir I. Arnold

Vladimir Igorevich Arnold, a great mathematician, passed away on 3 June 2010 in Paris. He was one of the few mathematicians who formed the skeleton of modern mathematics in the last half of the 20th century.

This is a bereavement not only for his relatives, friends and students but for the whole mathematical and more generally scientific community.

V. Arnold was born on 12 June 1937 in Odessa, USSR. His father Igor Vladimirovich Arnold (1900–1948) was a well-known mathematician. His mother Nina Alexandrovna Arnold, born Isakovich (1909–1986), was an art historian and worked at the Pushkin Museum of Fine Arts in Moscow. He was a student at Moscow High School no. 59, which produced a number of members of the Russian Academy of Sciences. In 1954–1959 Arnold was an undergraduate student at Moscow State University. He defended his PhD thesis on the solution of Hilbert's 13th problem at Moscow Institute of Applied Mathematics in 1961 and his DrSc thesis on the stability of Hamiltonian systems at the same institute shortly after that in 1963.

Whilst a third year undergraduate student V. Arnold together with his teacher Andrey Nikolaevich Kolmogorov (1903–1987) solved Hilbert's 13th problem by showing that the solution of a general algebraic equation of degree 7 can be represented by superpositions of continuous functions of two variables.

V. Arnold was one of the creators of the celebrated KAM (Kolmogorov-Arnold-Moser) theory. KAM theory was a subject of Arnold's works at the beginning of the 1960s. The theory states that if an integrable Hamiltonian system is subjected to weak perturbations then some of the invariant tori are deformed and survive, while others are destroyed. KAM theory solved a number of 200-year-old problems.

V. Arnold was a creator of singularity theory. Singularity theory is concerned with the geometry and topology of spaces and maps. It draws on many areas of mathematics, and in its turn has contributed to many areas both within and outside mathematics in particular differential and algebraic geometry, knot theory, differential equations, bifurcation theory, Hamiltonian mechanics, optics, robotics and computer vision.

It is impossible even briefly to list here all the mathematical achievements of V. Arnold. His name appears in many notions of dynamical systems theory: Liouville-Arnold theorem, Arnold's tongue, Arnold's diffusion, Arnold's cat, etc. Symplectic topology emerged from Arnold's conjecture on the number of fixed points of a symplectomorphism formulated in the mid 1960s. Modern revitalisation of real algebraic geometry was initiated by Arnold's work on arrangements of ovals of a real plane algebraic curve (1971). The work of V. Arnold on the cohomology of the pure braid group (1968) was one of the starting points of the modern theory of hyperplane arrangements. Arnold's works on classification of critical points of functions led to the Newton polyhedra theory. Arnold's strange duality (1973) for 14 exceptional unimodal singularities was one of the first examples of the mirror symmetry phenomenon.

V. Arnold never separated mathematics from natural sciences. He liked to claim that "Mathematics is a part of physics. Physics is an experimental science, a part of natural sciences. Mathematics is the part of physics where experiments are cheap."

V. Arnold was the author of more than 500 papers and almost 50 books. His *Mathematical Methods of Classical Mechanics* and *Ordinary Differential Equations* are classical textbooks.

V. Arnold held positions at Moscow State University (1961–1986), the Steklov Mathematical Institute in Moscow (since 1986) and University Paris-Dauphine (1993–2004). V. Arnold was one of the creators of the Independent University of Moscow and one of its first professors. He was a member of the Russian Academy of Sciences. V. Arnold was President of the Moscow Mathematical Society from 1996 until his death.

V. Arnold had a big influence on the international mathematical community. He was Editor-in-Chief of the journal Functional Analysis and its Applications. This was a remarkable journal founded by another great mathematician I. M. Gelfand who passed away less than half a year before V. Arnold. Recently V. Arnold started a new journal Functional Analysis and Other Mathematics. V. Arnold was a member of Editorial Boards of numerous mathematical journals: *Inventiones Mathematicae*, J*ournal of Algebraic Geometry*, *Journal of Geometry and Physics*, *Bulletin des Sciences Mathématiques*, *Selecta Mathematica*, *Physica D – Nonlinear Phenomena*, *Topological Methods in Nonlinear Analysis*, *Russian Mathematical Surveys*, *Izvestiya Mathematics*, *Doklady Mathematics*, *Moscow Mathematical Journal*, *Quantum*. V. Arnold was Vice-President of the International Mathematical Union (1995–1998) and a member of the Executive Committee of the International Mathematical Union (1999–2002).

V. Arnold was the recipient of many awards, such as the Award of the Moscow Mathematical Society (1958), the Lenin Prize (1965, with Andrey Kolmogorov), the Crafoord Prize (1982, with Louis Nirenberg), the Wolf Prize in Mathematics (2001), the State Prize of the Russian Federation (2007), the Shaw Prize in Mathematical Sciences (2008, with Ludwig Faddev). He was a Foreign Member of a number of academies and Doctor Honoris Causa of several universities. The minor planet 10031 Vladarnolda was named after him in 1981.

V. Arnold had strong opinions on many subjects. He was an ardent fighter against formal axiomatic Bourbaki style exposition of mathematics. Here is a quotation from Arnold's interview in AMS Notices (1997): "The Bourbakists claimed that all the great mathematicians were, using the words of Dirichlet, replacing blind calculations by clear ideas. The Bourbaki manifesto containing these words was translated into Russian as all clear ideas were replaced by blind calculations. The editor of the translation was Kolmogorov. His French was excellent. I was shocked to find such a mistake in the translation and discussed it with Kolmogorov.

His answer was: I had not realized that something was wrong in the translation since the translator described the Bourbaki style much better than the Bourbakists did."

V. Arnold was critical of modern emasculation of mathematical education in Russia and in the world. His ideas on teaching students were reflected in the papers "Mathematical trivium" and "Mathematical trivium – II" (*Russian Mathematical Surveys*, 1991, 1993) where he offered a list of problems from different parts of mathematics to test student mathematical knowledge. In 2004 he published a book *Problems for children from 5 to 15* (in Russian).

V. Arnold had the strong personality of a leader. He was always surrounded by students and colleagues. His lectures attracted crowds of people. Every semester V. Arnold started his seminar with a new list of problems. Very often these problems were becoming topics of research of seminar members. The problems were published later in a 600 page book Arnold's Problems.

V. Arnold was interested in the history of science. He wrote a remarkable book *Huygens and Barrow, Newton and Hooke*. V. Arnold was an editor of the translation to Russian of the Selected Works of Poincaré.

According to V. Arnold, active mathematical life should be supported by active physical exercises. He liked cross-country skiing in winter (about 100 km a week) and cycling in summer. One of his enjoyments was swimming in iced water in winter. He taught this to some of his students.

V. Arnold was interested in many things outside mathematics. He knew a lot of history and poetry. He had a number of stories on people of different times from Ancient Egypt to our days and liked to tell them. Some of the stories were collected in his book *Yesterday and Long Ago* (2006). It is a pity that a lot of his stories seem to be lost now.

V. Arnold leaves us his heritage in his works, books, problems and students. His influence on mathematics will last for many years to come.

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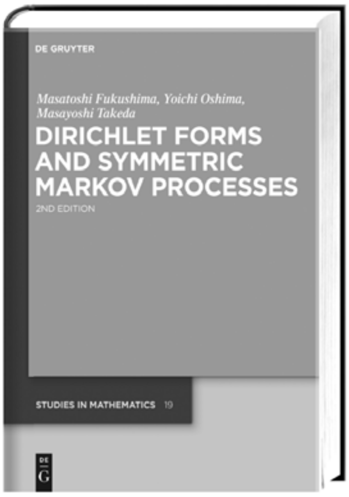

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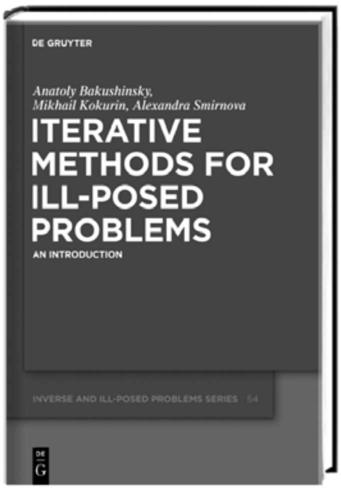

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The evolution of Loewner's differential equations

Marco Abate, Filippo Bracci, Manuel D. Contreras and Santiago Díaz-Madrigal

1 Introduction

Ch. Loewner

Charles Loewner was born as Karel Löwner on 29 May 1893 in Lány, Bohemia. He also used the German spelling Karl of his first name; indeed, although he spoke Czech at home, all of his education was in German.

Loewner received his PhD from the University of Prague in 1917 under the supervision of George Pick; then he spent some years at the Universities of Berlin and Cologne. In 1930, he returned to Charles University of Prague as a professor. When the Nazis occupied Prague, he was put in jail. Luckily, after paying the "emigration tax" he was allowed to leave the country with his family and move, in 1939, to the US, where he changed his name to Charles Loewner. Although J. von Neumann promptly arranged a position for him at Louisville University, he had to start his life from scratch. In the United States, he worked at Brown University, Syracuse University and eventually at Stanford University, where he remained until his death on 8 January 1968.

Loewner's work covers wide areas of complex analysis and differential geometry, and displays his deep understanding of Lie theory and his passion for semigroup theory; his papers are nowadays cornerstones of the theory bearing his name.

He began his research in the theory of conformal mappings. His most prominent contribution to this field was the introduction of infinitesimal methods for univalent functions, leading to the Loewner differential equations that are now a classical tool of complex analysis. Loewner's basic idea to consider semigroups related to conformal mappings led him to the general study of semigroups of transformations. In this context he characterised monotone matrix transformations, sets of projective mappings and similar geometric transformation classes.

Here we are mainly interested in Loewner's early research about the composition semigroups of conformal mappings and in the developments (some quite recent) springing from his work. Loewner's most important work in this area is his 1923 paper [41] where he introduced the nowadays well-known Loewner parametric method and the so-called Loewner differential equations, allowing him to prove the first non-elementary case of the celebrated Bieberbach conjecture: if f is a univalent function defined on the unit disc in the complex plane, with expansion at the origin given by

$$f(z) = z + a_2 z^2 + \cdots + a_n z^n + \cdots ,$$

then $|a_n| \leq n$ for each $n \geq 1$. Loewner was able to prove that $|a_3| \leq 3$. It is well-known that the Bieberbach conjecture was finally proven by L. de Branges [15] in 1985. In his proof, de Branges introduced ideas related to Loewner's but he used only a distant relative of Loewner's equation in connection with his main apparatus, his own rather sophisticated theory of composition operators. However, elaborating on de Branges' ideas, FitzGerald and Pommerenke [18] discovered how to avoid altogether the composition operators and rewrote the proof in classical terms, applying the *bona fide* Loewner equation and providing in this way a direct and powerful application of Loewner's classical method.

The seminal paper [41] has been a source of inspiration for many mathematicians and there have been many further developments and extensions of the results and techniques introduced there. This is especially true for the differential equations he first considered and this note will be a brief tour of the development of Loewner's theory and its several applications and generalisations.

We would like to end this introduction by recalling that one of the last (but definitely not least) contributions to this growing theory was the discovery, by Oded Schramm in 2000 [49], of the stochastic Loewner equation (SLE), also known as the Schramm-Loewner equation. The SLE is a conformally invariant stochastic process; more precisely, it is a family of random planar curves generated by solving Loewner's differential equation with a Brownian motion as the driving term. This equation was studied and developed by Schramm together with Greg Lawler and Wendelin Werner in a series of joint papers that led, among other things, to a proof of Mandelbrot's conjecture about the Hausdorff dimension of the Brownian frontier [36], [37]. This achievement was one of the reasons Werner was awarded the Fields Medal in 2006. Sadly, Oded Schramm, born 10 December 1961 in Jerusalem, died in a tragic hiking accident on 01 September 2008 while climbing Guye Peak, north of Snoqualmie Pass in Washington.

Quite recently, Stanislav Smirnov has also been awarded the Fields Medal (2010) for his outstanding contributions to SLE and the theory of percolation.

2 The slit radial Loewner equation

In his 1923 paper [41], Loewner proved that the class of single-slit mappings (i.e. holomorphic functions mapping univalently the unit disc $\mathbb{D} \subset \mathbb{C}$ onto the complement in $\mathbb{C}$ of a Jordan arc) is a dense subset of the class $\mathcal{S}$ of all univalent mappings f in the unit disc normalised by $f(0) = 0$ and $f'(0) = 1$. He also discovered a method to parametrise single-slit maps. Let g be a single-slit map whose image in $\mathbb{C}$ avoids the Jordan arc $\gamma\colon [0, +\infty) \to \mathbb{C}$. Loewner introduced the family (g_t) of univalent maps in $\mathbb{D}$, indexed by the time $t \in [0, +\infty)$, where $g_0 = g$ and g_t is the Riemann mapping whose image is the complement in $\mathbb{C}$ of the Jordan arc $\gamma|_{[t,+\infty)}$. The family of domains $\{g_t(\mathbb{D})\}$ is increasing, and as time goes to ∞ it fills out the whole complex plane.

Loewner's crucial observation is that the family (g_t) can be described by differential equations. More precisely, with a suitable choice of parametrisation, there exists a continuous function $\kappa\colon [0, +\infty) \to \partial\mathbb{D}$, called the *driving term*, such that (g_t) satisfies

$$\frac{\partial g_t(w)}{\partial t} = w\,\frac{\kappa(t) + w}{\kappa(t) - w}\,\frac{\partial g_t(w)}{\partial w}\,. \tag{2.1}$$

This equation is usually called the *(slit-radial) Loewner PDE* (and it is the first one of several *evolution equations* we shall see originated by Loewner's ideas). Loewner also remarked (and used) that the associated family of holomorphic self-maps of the unit disc $(\varphi_{s,t}) := (g_t^{-1} \circ g_s)$ for $0 \le s \le t$ gives solutions of the characteristic equation

$$\frac{dw}{dt} = -w\,\frac{\kappa(t) + w}{\kappa(t) - w} \tag{2.2}$$

subjected to the initial condition $w(s) = z \in \mathbb{D}$. Equation (2.2) is nowadays known as the *(slit-radial) Loewner ODE*. The adjective "radial" in these names comes from the fact that the image of each $\varphi_{s,t}$ is the unit disc minus a single Jordan arc approaching a sort of radius as t goes to ∞.

The two slit-radial Loewner equations can be studied on their own without any reference to parametrised families of univalent maps. Imposing the initial condition $w(s) = z$, the Loewner ODE (2.2) has a unique solution $w_s^z(t)$ defined for all $t \in [s, +\infty)$. Moreover, $\varphi_{s,t}(z) := w_s^z(t)$ is a holomorphic self-map of the unit disc for all $0 \le s \le t < +\infty$. However, without conditions on the driving term, the solutions of the Loewner ODE are in general not of slit type. For instance, P. P. Kufarev in 1947 gave examples of continuous driving terms such that the solutions to (2.2) have subdomains of the unit disc bounded by hyperbolic geodesics as image, and thus are non-slit maps. The problem of understanding exactly which driving terms produce slit solutions of (2.2) has become, and still is, a basic problem in the theory. Deep and very recent contributions to this question are due to J. R. Lind, D. E. Marshall and S. Rhode [42], [39], [40]. See also [44] and references therein.

3 The general radial Loewner equations

It is not easy to follow the historical development of the parametric method because in the middle of the 20th century a number of papers appeared independently; moreover, some of them were published in the Soviet Union, remaining partially unknown to Western mathematicians. Anyhow, it is widely recognised that the Loewner method was brought to its full power by Pavel Parfen'evich Kufarev (Tomsk, 18 March 1909 – Tomsk, 17 July 1968) and Christian Pommerenke (Copenhagen, 17 December 1933).

P.P. Kufarev

Using slightly different points of view, both Kufarev and Pommerenke merged Loewner's ideas with evolutionary aspects of increasing families of *general* complex domains. Pommerenke's approach was to impose an ordering on the images of univalent mappings of the unit disc, and it seems to have been the first one to use the expression "Loewner chain" for describing the family of increasing univalent mappings in Loewner's theory. Kufarev [31] too studied increasing families of domains and, although they were not exactly Loewner chains in the sense of Pommerenke, his theory bears some resemblance to the one developed by Pommerenke [45].

A *Loewner chain* (in the sense of Pommerenke) is a family (f_t) of univalent mappings of the unit disc whose images form an increasing family of simply connected domains and normalised imposing $f_t(0) = 0$ and $f_t'(0) = e^t$ for all $t \ge 0$ (we notice that as soon as $f_t(0) = 0$ holds, the second normalising condition can always be obtained by means of a reparametrisation in the time variable). The families of single-slit mappings originally considered by Loewner are thus a very particular example of Loewner chains.

Again, to a Loewner chain (f_t) we can associate a family $(\varphi_{s,t}) := (f_t^{-1} \circ f_s)$ for $0 \le s \le t$ of holomorphic self-maps of the unit disc, and again both (f_t) and $(\varphi_{s,t})$ can be recovered as solutions of differential equations. In fact, (f_t) satisfies the (general) *radial Loewner PDE*

$$\frac{\partial f_t(w)}{\partial t} = w\,p(w,t)\frac{\partial f_t(w)}{\partial w}\,, \tag{3.1}$$

where $p\colon \mathbb{D} \times [0, +\infty) \to \mathbb{C}$ is a normalised parametric *Herglotz function*, i.e. it satisfies the following conditions:
- $p(0,\cdot) \equiv 1$,
- $p(\cdot, t)$ is holomorphic for all $t \ge 0$,
- $p(z, \cdot)$ is measurable for all $z \in \mathbb{D}$,
- $\operatorname{Re} p(z,t) \ge 0$ for all $t \ge 0$ and $z \in \mathbb{D}$.

Analogously, $(\varphi_{s,t})$ satisfies the so-called (general) *radial Loewner ODE*

$$\frac{dw}{dt} = -w\,p(w,t), \quad w(s) = z\,, \tag{3.2}$$

where p again is a normalised parametric Herglotz function.

Since

$$\operatorname{Re}\frac{\kappa(t) + w}{\kappa(t) - w} \ge 0$$

for all w and any driving term κ, equations (2.1) and (2.2) are particular cases of (3.1) and (3.2); furthermore, in contrast with the slit case, the general radial Loewner equations yield a one-to-one correspondence between Loewner chains and normalised parametric Herglotz functions.


Ch. Pommerenke

Roughly speaking, differential equations such as (3.1) and (3.2) are important because they allow one to get estimates and growth bounds for f_t and $\varphi_{s,t}$ starting from the well-known estimates and growth bounds for maps, such as $p(z,t)$, having image in the right half-plane. For instance, in this way Pommerenke [45], and also [19], [27], solved the "embedding problem", showing that for any $f \in \mathcal{S}$ it is possible to find a Loewner chain (f_t) such that $f_0 = f$. More general questions of embeddability in Loewner chains satisfying specific properties are still open and this is an active area of research.

Loewner's and Pommerenke's approaches to the parametric method work under the essential assumption that all the elements of the chain fix a given point of the unit disc, usually the origin. This is a natural hypothesis if one deals with increasing sequences of simply connected domains, and it also yields (up to a reparametrisation) good regularity in the time parameter – a basic fact necessary for the derivation of the associated differential equations. However, in certain situations related to some concrete physical and stochastic processes we will discuss later, there is no fixed point in the interior, and a similar role has to be played by a point on the boundary of the unit disc. Because the geometry of the boundary of the unit disc, the "infinity" of hyperbolic geometry, is quite different from the geometry inside the unit disc, new phenomena appear, and it does not seem possible to deal with this case by somehow appealing to the classical case. As a consequence, new extensions of Loewner's theory have been provided.

4 The chordal equation

In 1946, Kufarev [32] proposed an evolution equation in the upper half-plane analogous to the one introduced by Loewner in the unit disc. In 1968, Kufarev, Sobolev and Sporysheva [33] established a parametric method, based on this equation, for the class of univalent functions in the upper half-plane, which is known to be related to physical problems in hydrodynamics. Moreover, during the second half of the past century, the Soviet school intensively studied Kufarev's equation. We ought to cite here at least the contributions of I. A. Aleksandrov [2], S. T. Aleksandrov and V. V. Sobolev [4], V. V. Goryainov and I. Ba [22, 23]. However, this work was mostly unknown to many Western mathematicians, mainly because some of it appeared in journals not easily accessible outside the Soviet Union. In fact, some of Kufarev's papers were not even reviewed by Mathematical Reviews. Anyhow, we refer the reader to [3], which contains a complete bibliography of his papers.

In order to introduce Kufarev's equation properly, let us fix some notation. Let γ be a Jordan arc in the upper half-plane $\mathbb{H}$ with starting point $\gamma(0) = 0$. Then there exists a unique conformal map $g_t : \mathbb{H} \setminus \gamma[0,t] \to \mathbb{H}$ with the normalisation

$$g_t(z) = z + \frac{c(t)}{z} + O\left(\frac{1}{z^2}\right).$$

After a reparametrisation of the curve γ, one can assume that $c(t) = 2t$. Under this normalisation, one can show that g_t satisfies the following differential equation:

$$\frac{\partial g_t(z)}{\partial t} = \frac{2}{g_t(z) - h(t)}, \qquad g_0(z) = z. \tag{4.1}$$

The equation is valid up to a time $T_z \in (0, +\infty]$ that can be characterised as the first time t such that $g_t(z) \in \mathbb{R}$ and where h is a continuous real-valued function. Conversely, given a continuous function $h\colon [0,+\infty) \to \mathbb{R}$, one can consider the following initial value problem for each $z \in \mathbb{H}$:

$$\frac{dw}{dt} = \frac{2}{w - h(t)}, \qquad w(0) = z\,. \tag{4.2}$$

Let $t \mapsto w^z(t)$ denote the unique solution of this Cauchy problem and let $g_t(z) := w^z(t)$. Then g_t maps holomorphically a (not necessarily slit) subdomain of the upper half-plane $\mathbb{H}$ onto $\mathbb{H}$. Equation (4.2) is nowadays known as the *chordal Loewner differential equation* with the function h as the driving term. The name is due to the fact that the curve $\gamma[0,t]$ evolves in time as t tends to infinity into a sort of chord joining two boundary points. This kind of construction can be used to model evolutionary aspects of decreasing families of domains in the complex plane.

For later use, we remark that using the Cayley transform we may assume (working in an increasing context) that the chordal Loewner equation in the unit disc takes the form

$$\frac{dz}{dt} = (1-z)^2 p(z,t), \qquad z(0) = z, \tag{4.3}$$

where $\operatorname{Re} p(z,t) \geq 0$ for all $t \geq 0$ and $z \in \mathbb{D}$.


O. Schramm

In 2000 Schramm [49] had the simple but very effective idea of replacing the function h in (4.2) by a Brownian motion, and of using the resulting chordal Loewner equation, nowadays known as the SLE (*stochastic Loewner equation*) to understand critical processes in two dimensions, relating probability theory to complex analysis in a completely novel way. In fact, the SLE was discovered by Schramm as a conjectured scaling limit of the planar uniform spanning tree and the planar loop-erased random walk probabilistic processes. Moreover, this tool also turned out to be very important for the proofs of conjectured scaling limit relations on some other models from statistical mechanics, such as self-avoiding random walks and percolation.

5 Semigroups of holomorphic mappings

To each Loewner chain (f_t) one can associate a family of holomorphic self-maps of the unit disc $(\varphi_{s,t}) := (f_t^{-1} \circ f_s)$, sometimes called *transition functions* or the *evolution family*. By the very construction, an evolution family satisfies the algebraic property

$$\varphi_{s,t} = \varphi_{u,t} \circ \varphi_{s,u} \tag{5.1}$$

for all $0 \leq s \leq u \leq t < +\infty$.

Special but important cases of evolution families are semigroups of holomorphic self-maps of the unit disc. A family of holomorphic self-maps of the unit disc (ϕ_t) is a *(continuous) semigroup* if $\phi\colon (\mathbb{R}^+, +) \to \mathsf{Hol}(\mathbb{D}, \mathbb{D})$ is a continuous homomorphism between the semigroup of non-negative real numbers and the semigroup of holomorphic self-maps of the disc with respect to composition, endowed with the topology of uniform convergence on compact sets. In other words: $\phi_0 = \mathsf{id}_{\mathbb{D}}$; $\phi_{t+s} = \phi_s \circ \phi_t$ for all $s, t \geq 0$; and ϕ_t converges to ϕ_{t_0} uniformly on compact sets as t goes to t_0.

Setting $\varphi_{s,t} := \phi_{t-s}$ for $0 \leq s \leq t < +\infty$, it can be checked that $\varphi_{s,t}$ satisfies (5.1) and semigroups of holomorphic maps provide examples of evolution families in the sense of Section 6.

Semigroups of holomorphic maps are a classical subject of study, both as (local/global) flows of continuous dynamical systems and from the point of view of "fractional iteration", the problem of embedding the discrete set of iterates generated by a single self-map into a one-parameter family (a problem that is still open even in the disc). It is difficult to exactly date the birth of this notion but it seems that the first paper dealing with semigroups of holomorphic maps and their asymptotic behaviour is due to F. Tricomi in 1917 [53]. Semigroups of holomorphic maps also appear in connection with the theory of Galton-Watson processes (branching processes) started in the 40s by A. Kolmogorov and N. A. Dmitriev [28]. Furthermore, they are an important tool in the theory of strongly continuous semigroups of operators between spaces of analytic functions (see, for example, [51]).

A very important contribution to the theory of semigroups of holomorphic self-maps of the unit disc is due to E. Berkson and H. Porta [9]. They proved that a semigroup of holomorphic self-maps of the unit disc (ϕ_t) is in fact real-analytic in the variable t, and is the solution of the Cauchy problem

$$\frac{\partial \phi_t(z)}{\partial t} = G(\phi_t(z)), \quad \phi_0(z) = z\,, \tag{5.2}$$

where the map G, the *infinitesimal generator* of the semigroup, has the form

$$G(z) = (z - \tau)(\overline{\tau} z - 1)p(z) \tag{5.3}$$

for some $\tau \in \overline{\mathbb{D}}$ and a holomorphic function $p\colon \mathbb{D} \to \mathbb{C}$ with $\mathsf{Re}\, p \geq 0$.

The dynamics of the semigroup (ϕ_t) are governed by the analytical properties of the infinitesimal generator G. For instance, the semigroup has a common fixed point at τ (in the sense of non-tangential limit if τ belongs to the boundary of the unit disc) and asymptotically tends to τ, which can thus be considered a sink point of the dynamical system generated by G.

When $\tau = 0$, it is clear that (5.3) is a particular case of (3.2), because the infinitesimal generator G is of the form $-wp(w)$, where p is a (autonomous, and not necessarily normalised) Herglotz function. As a consequence, when the semigroup has a fixed point in the unit disc (which, up to a conjugation by an automorphism of the disc, amounts to taking $\tau = 0$), *once differentiability in t is proved* Berkson-Porta's theorem can be easily deduced from Loewner's theory. However, when the semigroup has no common fixed points in the interior of the unit disc, Berkson-Porta's result is really a new advance in the theory.

We have already remarked that semigroups give rise to evolution families; they also provide examples of Loewner chains. Indeed, M. H. Heins [29] and A. G. Siskasis [50] have independently proved that if (ϕ_t) is a semigroup of holomorphic self-maps of the unit disc then there exists a (unique, when suitably normalised) holomorphic function $h\colon \mathbb{D} \to \mathbb{C}$, the *Königs function* of the semigroup, such that $h(\phi_t(z)) = m_t(h(z))$ for all $t \geq 0$, where m_t is an affine map (in other words, the semigroup is semiconjugated to a semigroup of affine maps). Then it is easy to see that the maps $f_t(z) := m_t^{-1}(h(z))$, for $t \geq 0$, form a Loewner chain (in the sense explained in the next section).

The theory of semigroups of holomorphic self-maps has been extensively studied and generalised: to Riemann surfaces (in particular, Heins [29] has shown that Riemann surfaces with non-Abelian fundamental group admit no non-trivial semigroup of holomorphic self-maps); to several complex variables; and to infinitely dimensional complex Banach spaces, by I. N. Baker, C. C. Cowen, M. Elin, V. V. Goryainov, P. Poggi-Corradini, Ch. Pommerenke, S. Reich, D. Shoikhet, A. G. Siskakis, E. Vesentini and many others. We refer to [10] and the books [1] and [47] for references and more information on the subject.

6 A general Loewner's theory

Comparing the radial Loewner equation (3.2), the chordal Loewner equation (4.3) and the Berkson-Porta decomposition (5.3) for infinitesimal generators of semigroups, one realises that for all fixed $t \geq 0$ the maps appearing in Loewner's theory are infinitesimal generators of semigroups of holomorphic self-maps of the unit disc. Therefore, one is tempted to consider a general Loewner equation of the following form:

$$\frac{dz}{dt} = G(z, t), \qquad z(0) = z, \tag{6.1}$$

with $G(\cdot, t)$ being an infinitesimal generator for almost all fixed $t \geq 0$, as well as the associated general Loewner PDE:

$$\frac{\partial f_t(z)}{\partial t} = -G(z, t)\frac{\partial f_t(z)}{\partial z}. \tag{6.2}$$

Thanks to (5.3), when assuming $G(0, t) \equiv 0$ or $G(z, t)$ of the special chordal form, these equations coincide with those we have already discussed, and hence they can be viewed as general and unified Loewner equations (see, for example, [11] and [14]).

As we have seen, Loewner introduced his theory to deal with univalent normalised functions. Hence he put more emphasis on the concept of Loewner chains than on evolution families, as did Pommerenke. An intrinsic study of evolution families and of their relationship with other aspects of the theory has not been carried out until recently; let us describe the approach proposed in [11] and [14]. An *evolution family of order* $d \in [1, +\infty]$ is a family $(\varphi_{s,t})_{0 \leq s \leq t < +\infty}$ of holomorphic self-maps of the unit disc such that $\varphi_{s,s} = \mathsf{id}_{\mathbb{D}}$, $\varphi_{s,t} = \varphi_{u,t} \circ \varphi_{s,u}$ for all $0 \leq s \leq u \leq t < +\infty$ and such that for all $z \in \mathbb{D}$ and for all $T > 0$ there exists a non-negative function $k_{z,T} \in L^d([0, T], \mathbb{R})$ satisfying

$$|\varphi_{s,u}(z) - \varphi_{s,t}(z)| \leq \int_u^t k_{z,T}(\xi)\, d\xi \tag{6.3}$$

for all $0 \leq s \leq u \leq t \leq T$.

If (f_t) is a normalised Loewner chain (in the sense of Pommerenke) then the family $(\varphi_{s,t}) := (f_t^{-1} \circ f_s)$ is an evolution family of order $+\infty$: the regularity condition (6.3) (with $d = +\infty$) holds because $\varphi'_{s,t}(0) = e^{s-t}$ [45, Lemma 6.1]. Similarly, one can show that the solutions of the chordal Loewner ordinary differential equations satisfy (6.3). Hence, this concept of evolution families of order d is a natural generalisation of the evolution families appearing in the classical Loewner theory. We also remark that, although it is not assumed in the definition, it turns out that maps belonging to an evolution family are always univalent.

Associated to evolution families of order d there are *Herglotz vector fields of order* $d \in [1, +\infty]$. These are time-dependent vector fields $G(z,t)$ that are measurable in t for all fixed z, are holomorphic infinitesimal generators of semigroups for almost all fixed t and are such that for each compact set $K \subset \mathbb{D}$ and all $T > 0$ there exists a non-negative function $k_{K,T} \in L^d([0,T], \mathbb{R})$ so that

$$|G(z,t)| \leq k_{K,T}(t)$$

for all $z \in K$ and almost all $t \in [0,T]$. Once again, the vector fields introduced in classical Loewner theory satisfy these conditions, with $d = +\infty$.

In [11] it is proved that there is a one-to-one correspondence between evolution families $(\varphi_{s,t})$ of order d and Herglotz vector fields $G(z,t)$ of order d, and the bridge producing such a correspondence is precisely (6.1), namely,

$$\frac{\partial \varphi_{s,t}}{\partial t}(z) = G(\varphi_{s,t}(z), t), \quad \varphi_{s,s}(z) = z. \tag{6.4}$$

Moreover, a Herglotz vector field $G(z,t)$ admits a Berkson-Porta-like decomposition. Namely, there exists a function $p\colon \mathbb{D} \times [0, +\infty) \to \mathbb{C}$ satisfying

- $z \mapsto p(z,t)$ is holomorphic for all $t \in [0, +\infty)$,
- $\mathrm{Re}\, p(z,t) \geq 0$ for all $z \in \mathbb{D}$ and almost all $t \in [0, +\infty]$,
- $t \mapsto p(z,t) \in L^d_{\mathrm{loc}}([0, +\infty], \mathbb{C})$ for all $z \in \mathbb{D}$,

and a measurable function $\tau\colon [0, +\infty] \to \overline{\mathbb{D}}$ such that

$$G(z,t) = (z - \tau(t))(\overline{\tau(t)}z - 1)p(z,t)\,. \tag{6.5}$$

Conversely, any vector field of the form (6.5) is a Herglotz vector field. Notice that when $\tau \equiv 0$ (respectively, $\tau \equiv 1$) equation (6.4) (via (6.5)) reduces to (3.2) (respectively, to (4.3)) and when $G(z,t)$ does not depend on t it reduces to the semigroup equation (5.2).

In the classical theory, every Loewner chain can be obtained from a normalised evolution family $(\varphi_{s,t})$ by taking

$$f_s(z) := \lim_{t\to\infty} e^t \varphi_{s,t}(z). \tag{6.6}$$

In [14], a new definition of Loewner chains was introduced, allowing one to reproduce the relationship between Loewner chains and evolution families in this more general context. A family of univalent maps (f_t) in the unit disc is said to be a *Loewner chain of order d* if the ranges $f_t(\mathbb{D})$ form an increasing family of complex domains and for any compact set $K \subset \mathbb{D}$ and any $T > 0$ there exists a non-negative function $k_{K,T} \in L^d([0,T], \mathbb{R})$ such that

$$|f_s(z) - f_t(z)| \leq \int_s^t k_{K,T}(\xi)d\xi$$

for all $z \in K$ and all $0 \leq s \leq t \leq T$. Exploiting the (classical) parametric representation of univalent maps (6.6), it can be proved that there is a one-to-one (up to composition with biholomorphisms) correspondence between evolution families of order d and Loewner chains of the same order, related by the equation

$$f_s = f_t \circ \varphi_{s,t}. \tag{6.7}$$

An alternative functorial method to create Loewner chains from evolution families, which also works on abstract complex manifolds, has been introduced in [5].

Once the previous correspondences are established, given a Loewner chain (f_t) of order d, the general Loewner PDE (6.2) follows by differentiating the structural equation (6.7). Conversely, given a Herglotz vector field $G(z,t)$ of order d, one can build the associated Loewner chain (of the same order d), solving (6.2) by means of the associated evolution family.

The Berkson-Porta decomposition (6.5) of a Herglotz vector field $G(z,t)$ also gives information on the dynamics of the associated evolution family. For instance, when $\tau(t) \equiv \tau \in \mathbb{D}$, the point τ is a (common) fixed point of $(\varphi_{s,t})$ for all $0 \leq s \leq t < +\infty$. Moreover, it can be proved that, in such a case, there exists a unique locally absolutely continuous function $\lambda\colon [0, +\infty) \to \mathbb{C}$ with $\lambda' \in L^d_{loc}([0, +\infty), \mathbb{C})$, $\lambda(0) = 0$ and $\mathrm{Re}\,\lambda(t) \geq \mathrm{Re}\,\lambda(s) \geq 0$ for all $0 \leq s \leq t < +\infty$ such that for all $s \leq t$

$$\varphi'_{s,t}(\tau) = \exp(\lambda(s) - \lambda(t)).$$

A similar characterisation holds when $\tau(t) \equiv \tau \in \partial\mathbb{D}$ [11].

7 Applications and extension of Loewner's theory

Loewner's theory has been used to prove several deep results in various branches of mathematics, even apparently unrelated to complex analysis. In this last section we briefly highlight some of these applications and extensions, referring to the bibliography for more information and details. Necessarily, the list of topics we have chosen to present is rather incomplete and only reflects our personal tastes and, certainly, we have not tried to give an exhaustive picture. For a more comprehensive view of the theory we strongly recommend the monographs [16] and [46].

Extremal problems

After Loewner and E. Peschl, the first to apply Loewner's method to extremal problems in the theory of univalent functions was G. M. Goluzin, obtaining in an elegant way several new and sharp estimates. The most important of them is the sharp estimate for the so-called rotation theorem (estimate of the argument of the derivative – see [20], [21]).

As already recalled, the main conjecture solved with the help of Loewner's theory is the Bieberbach conjecture. Loewner himself proved the case $n = 3$; P. R. Garabedian and M. Schiffer in 1955 solved the case $n = 4$; M. Ozawa in 1969 and R. N. Pederson in 1968 solved the case $n = 6$; and Pederson and Schiffer in 1972 solved $n = 5$. Finally, in 1985, L. de Branges [15] proved the full conjecture and, as already remarked, FitzGerald and Pommerenke [18] gave an alternative proof explicitly based on Loewner's method. In both cases, the main point was proving the validity of the Milin conjecture. Previously, Milin had shown, using the Lebedev-Milin inequality, that his conjecture implied the Bieberbach conjecture (see, for example, [16] for details). This is just a short and incomplete list of the many mathematicians who have worked on this and related problems; Loewner's method is by now an important analytical device, which generates a number of sharp inequalities not accessible by other means (see, for example, [16]).

Univalence criteria

To obtain practical criteria ensuring univalence of conformal maps is a basic and fundamental problem in complex analysis. Perhaps the most famous criterion of this type is due to Z. Nehari [43]. He showed that an estimate on the Schwarzian derivative $(f''/f')' - \frac{1}{2}(f''/f')^2$ implies the univalence of f in the unit disc. Later, P. L. Duren, H. S. Shapiro and A. L. Shields observed that an estimate on the pre-Schwarzian f''/f' implies Nehari's estimate and therefore implies univalence. Then, J. Becker [8] found a totally different approach based on Loewner's equation to show that a weaker estimate on the pre-Schwarzian implies univalence. In the same paper, Becker also applied Loewner's equation to give an independent derivation of Nehari's criterion. In fact, many univalence criteria have later been reproved using Loewner's method; and this approach sometimes provided further insight. We refer the reader to [25, Chapter 3] and [46] for further information.

Optimisation and Loewner chains

The variational method is a standard way to deal with extremal problems in a given class of functions. Roughly speaking, this means that one can try and get information on an extremal function by comparing it with nearby elements in the given class. The larger the family of perturbations, the more relevant the information one obtains. One example of this kind is the class of normalised univalent functions in the unit disc, which, as we already know, are "reachable" by the class of Loewner chains, and this approach has been applied to optimal control theory.

Variational methods were pioneered in the late 1930s by M. Schiffer and independently by G. M. Goluzin. Schiffer wrote a paper in 1945 that applied a variational method to Loewner's equation, the first introduction of a technique later refined as "optimal control". In particular, coefficient extremal problems for univalent functions as optimal control problems for finite-dimensional control systems have been treated by I. A. Aleksandrov and V. I. Popov, G. S. Goodman, S. Friedland and M. Schiffer, and D. V. Prokhorov, and later developed in an infinite dimensional setting by O. Roth [48].

Stochastic Loewner equation

As mentioned in the introduction, this equation was introduced by Schramm in 2000, replacing the driving term in the radial and chordal Loewner equation with a Brownian motion. In particular, the *(chordal) stochastic Loewner evolution* with parameter $k \geq 0$ (SLE_k) starting at a point $x \in \mathbb{R}$ is the random family of maps (g_t) obtained from the chordal Loewner equation (4.1) by letting $h(t) = \sqrt{k}B_t$, where B_t is a standard one dimensional Brownian motion such that $\sqrt{k}B_0 = x$. Similarly, one can define a radial stochastic Loewner evolution.

The SLE_k depends on the choice of the Brownian motion and it comes in several flavours depending on the type of Brownian motion exploited. For example, it might start at a fixed point or start at a uniformly distributed point, or might have a built in drift and so on. The parameter k controls the rate of diffusion of the Brownian motion and the behaviour of the SLE_k critically depends on the value of k.

The SLE_2 corresponds to the loop-erased random walk and the uniform spanning tree. The $SLE_{8/3}$ is conjectured to be the scaling limit of self-avoiding random walks. The SLE_3 is conjectured to be the limit of interfaces for the Ising model, while the SLE_4 corresponds to the harmonic explorer and the Gaussian free field. The SLE_6 was used by Lawler, Schramm and Werner in 2001 [36], [37] to prove the conjecture of Mandelbrot (1982) that the boundary of planar Brownian motion has fractal dimension $4/3$. Moreover, Smirnov [52] proved the SLE_6 is the scaling limit of critical site percolation on the triangular lattice. This result follows from his celebrated proof of Cardy's formula.

Also worthy of mention is the work of L. Carleson and N. G. Makarov [13] studying growth processes motivated by DLA (diffusion-limited aggregation) via Loewner's equations.

The expository paper [35] is perhaps the best option to start an exploration of this fascinating branch of mathematics.

Hele-Shaw flows

One of the most influential works in fluid dynamics at the end of the 19th century was that of Henry Selby Hele-Shaw. A Hele-Shaw cell is a tool for studying the two-dimensional flow of a viscous fluid in a narrow gap between two parallel plates. Nowadays the Hele-Shaw cell is used as a powerful tool in several fields of natural sciences and engineering, in particular soft condensed matter physics, material sciences, crystal growth and, of course, fluid mechanics.

In 1945, P. I. Polubarinova-Kochina and L. A. Galin introduced an evolution equation for conformal mappings related to Hele-Shaw flows. Kufarev and Vinogradov in 1948 reformulated this equation in the form of a non-linear (even non-quasilinear) integro-differential equation of Loewner type. Despite apparent differences, these two equations have some evident geometric connections and the properties of Loewner's equations play a fundamental role in the study of Polubarinova-Galin's equation. Moreover, this close relationship has suggested the interesting problem of analysing when the solutions of Loewner's equations admit quasiconformal extensions beyond the closed unit disc, a problem studied by J. Becker, V. Ya. Gutlyanskiĭ, A. Vasil'ev and others [8], [26, Chapters 2, 3 and 4].

A nice monograph on Hele-Shaw flows from the point of view of complex analysis, discussing in particular their connection with the Loewner method, is [26] (see also [54]).

Extensions to multiply connected domains

I. Komatu, in 1943 [30], was the first to generalise Loewner's parametric representation to univalent holomorphic functions defined in a circular annulus and with images in the exterior of a disc. Later, G. M. Goluzin [20] gave a much simpler way to establish Komatu's results. With the same techniques, E. P. Li [38] considered a slightly different case, when the image of the annulus is the complex plane with two slits (ending at infinity and at the origin, respectively).

Another way of adapting Loewner's method to multiply connected domains was developed by P. P. Kufarev and M. P. Kuvaev [34]. They obtained a differential equation satisfied by automorphic functions realising conformal covering mappings of the unit disc onto multiply connected domains with a gradually erased slit. Roughly speaking, these results can be considered a version for multiply connected domains of the slit-radial Loewner equation.

Recently, and in a similar way, R. O. Bauer and R. M. Friedrich have developed a slit-chordal theory for multiply connected domains. Moreover, they have even dealt with stochastic versions of both the radial and the chordal cases. In this framework the situation is more subtle than in the simply connected case, because moduli spaces enter the picture [6], [7].

Extension to several complex variables

As far as we know, the first to propose a Loewner theory in several complex variables was J. Pfaltzgraff, who in 1974 extended the basic Loewner theory to $\mathbb{C}^n$ with the aim of obtaining bounds and growth estimates for some classes of univalent mappings defined in the unit ball of $\mathbb{C}^n$. The theory was later developed by T. Poreda, I. Graham, G. Kohr, M. Kohr, H. Hamada and others [25], [24], [17], [12].

In [12], using an equation similar to (6.4), it is proved that there is a one-to-one correspondence between evolution families of order ∞ and Herglotz vector fields of order ∞ on complete hyperbolic complex manifolds whose Kobayashi distance is smooth enough (as it happens, for instance, in bounded strongly convex domains of $\mathbb{C}^n$).

A clear description of Loewner chains in several complex variables is not yet available. Most of the literature in higher dimensions is devoted to the radial Loewner equation (and its consequences) on the unit ball of complex Banach spaces, mainly $\mathbb{C}^n$. The theory is definitely much more complicated than in dimension one; for instance, the class of normalised univalent mappings on the unit ball of $\mathbb{C}^n$ is not compact, and thus one is forced to restrict attention to suitable compact subclasses. Anyway, many natural cases, such as spiral-like maps, can be treated efficiently and many applications and estimates can be obtained [25].

However, in general, there is not yet a satisfactory answer to the question of whether it is possible to associate to an evolution family (or a Herglotz vector field) on the unit ball of $\mathbb{C}^n$ a Loewner chain with image in $\mathbb{C}^n$ solving a Loewner PDE. Keeping in mind the interpretation of Loewner chains as "time-dependent linearisation" for evolution families, it is clear that resonances among eigenvalues of the differentials at the common fixed point(s) have to play a role.

Acknowledgement. It is a pleasure for us to thank Professor P. Gumenyuk for his invaluable help, especially concerning the Soviet school. We also want to thank the referee for his/her careful reading of the paper and his/her very helpful comments and historical remarks.

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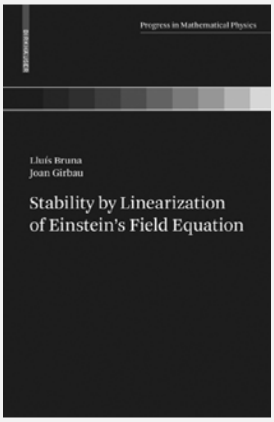

Stability by Linearization of Einstein's Field Equation

L. Bruna; **J. Girbau**
Universitat Autònoma de Barcelona, Spain

The concept of linearization stability arises when one compares the solutions to a linearized equation with solutions to the corresponding true equation. This requires a new definition of linearization stability adapted to Einstein's equation. However, this new definition cannot be applied directly to Einstein's equation because energy conditions tie together deformations of the metric and of the stress-energy tensor. Therefore, a background is necessary where the variables representing the geometry and the energy-matter are independent. This representation is given by a well-posed Cauchy problem for Einstein's field equation. This book establishes a precise mathematical framework in which linearization stability of Einstein's equation with matter makes sense. Using this framework, conditions for this type of stability in Robertson-Walker models of the universe are discussed.

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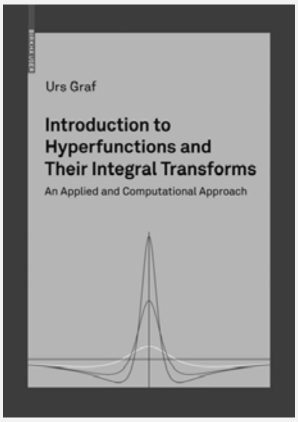

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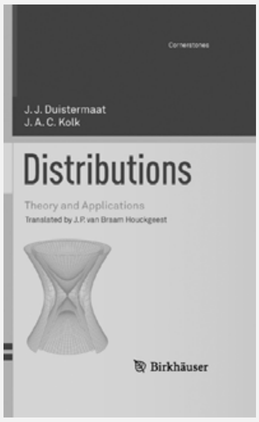

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J.J. Duistermaat; **J.A.C. Kolk**
Mathematical Institute, Utrecht University, The Netherlands

This textbook is an application-oriented introduction to the theory of distributions, a powerful tool used in mathematical analysis. The treatment emphasizes applications that relate distributions to linear partial differential equations and Fourier analysis problems found in mechanics, optics, quantum mechanics, quantum field theory, and signal analysis. Throughout the book, methods are developed to deal with formal calculations involving functions, series, and integrals that cannot be mathematically justified within the classical framework.

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2010, XVI, 445 p. 41 illus., Hardcover
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The Lvov School of Mathematics

Roman Duda (Wrocław University)
Translation from the Polish text, with supplementary footnotes, by Daniel Davies

The article was first published in Jahresberichte der DMV 112 (2010), 3–24, in German. We thank the DMV and Vieweg+Teubner Verlag (Springer Fachmedien Wiesbaden) for ceding the rights for the English publication. Daniel Davies generously provided a new translation from the Polish original.

1. The University

When King Jan Kazimierz established a university in Lvov in 1661, it was already the third to be built on land belonging to the Polish-Lithuanian state, following the ones in Cracow (est. 1364) and Vilnius (est. 1578). In 1772, Austria acquired Lvov as well as all of Galicia (First Partition of Poland) and governed those lands until 1918. Lvov University had long had the reputation of a substandard, provincial university. Its rapid improvement only began when Galicia (with Lvov as its capital) obtained autonomy within Austro-Hungary and when Polish was introduced, in 1871, as the language of curricular instruction. Wawrzyniec Żmurko (1824–1889) was one of its mathematics professors during the years 1872–1889. He was succeeded by Józef Puzyna (1856–1919). The former studied in Vienna and the latter was trained by the former, but completed his studies in Berlin, under K. Weierstrass amongst others. Both of them were already mathematicians who had made original contributions. And in 1908, Sierpiński°[1] came to Lvov, obtained his habilitation degree and became an ordinary professor. He then began assembling a group of young mathematicians, like Zygmunt Janiszewski°, Stefan Mazurkiewicz° and Stanisław Ruziewicz°. All four of them obtained original results in what was then the new theory of sets and set-theoretic topology, publishing their results in Polish journals (with French summaries) and French journals. It was generally a positive period for Lvov University. Among the professors there at the time were the outstanding physicist Marian Smoluchowski (1872–1917), the founder of the Lvov School of Philosophy Kazimierz Twardowski (1866–1938)[2] and the well-known researcher on Siberia Benedykt Dybowski (1833–1930), as well as others. In other words, it was a good university and it had a young, ambitious group of mathematicians.

However, the outbreak of World War I caused the break-up of the group of mathematicians. Sierpiński° happened to be in Russia at the time and was interned there. Janiszewski signed up as a volunteer with the Polish Legions fighting the Russians. Meanwhile, Mazurkiewicz returned to his home town of Warsaw. What is more, right after World War I had ended, there followed the Polish-Ukrainian war, over Lvov and West Galicia. After that had ended, with the Poles emerging victorious, there then followed the Polish-Soviet War, with all of Poland at stake. That too ended with a victory for the Poles and it halted the Red Army's march on Poland and Western Europe.[3] According to the terms of the peace treaty made with Soviet Russia (Riga, 1921), all of Galicia lay within Poland's borders, together with Lvov, where the old university had assumed the name Jan Kazimierz University (henceforth referred to as UJK for brevity) since 1919.

2. The Program

Such was the backdrop upon which was established, during the years 1919–1939, the phenomenon that became the Lvov School of Mathematics. Back then, it was a school of young, as yet unknown, mathematicians. But to understand it better, we must briefly transport ourselves to Warsaw. Let us recall that by the 1815 Treaty of Vienna, most of Poland's ancient lands, including her capital in Warsaw, ended up as part of Tsarist Russia. At first, some of those lands near to Warsaw enjoyed a relative degree of autonomy (under the Tsar's sceptre) but all that was quickly taken away in 1831, after the November Uprising against the Russians, when Polish schools were drastically curtailed and Warsaw University (established in 1816) was closed down. There then followed a long period of ruthless russification. One element of that process was the establishment, in 1869, of the Imperial University of Warsaw, in which use of Russian language was enforced. It did not measure up to the other Russian universities. Moreover, it was easier for Poles to enter one of those other universities than the one in Warsaw. The university was already being openly boycotted by Polish youth from 1906 onwards. After the outbreak of World War I, the university was evacuated to Rostov-on-Don, together with all the staff and all the furnishings. When Warsaw was taken by the Germans in 1915, the Polish university was opened there in the autumn, where Janiszewski and Mazurkiewicz, the mathematicians we know from Lvov, were appointed to mathematics chairs. At the same time, a new journal appeared. Entitled "Nauka Polska" [Polish Scholarship], it made an appeal asking for opinions about what needed to be most urgently done about the state of Polish scholarship. Among the respondents were both the directors of mathematical chairs in Warsaw. The answer provided

[1] A circle next to a name (°) signifies someone who is on the list at the back of this article of some representatives of the Lvov School of Mathematics. Full names are given when they appear for the first time.

[2] J. Woleński, *Filozoficzna szkoła lwowsko-warszawska*, Warszawa: PWN, 1985; *Logic and Philosophy in the Lvov-Warsaw School*, Synthèse Library, Dordrecht: Kluwer, 1988.

[3] See N. Davies, *White Eagle, Red Star, The Polish-Soviet War 1919–1920 and the Miracle on the Vistula*, Random House, 2003.

by Janiszewski° became massively influential and not long afterwards it became the main program for the Polish School of Mathematics.[4]

The sheer scale and originality of that program remains striking even to this day. After conducting a preliminary assessment of the situation at the time, Janiszewskio noticed there was a way for "Polish mathematics to achieve independent status". The idea was to identify a single, preferably new, area of mathematics for Polish mathematicians to focus on (the natural choice was set theory, as well as all those fields where set theory plays an important role, such as topology and the theory of functions – the very fields of interest that had been pursued by what was by then the non-existent Lvov group). A culture of mutual cooperation was to be fostered and the young were to be supported. And it was proposed that a journal be established, dedicated exclusively to the chosen field of interest, in which articles were to be published only in languages spoken at congresses.

The programme must have caused a shock. If most of Poland's creative mathematicians were to focus exclusively on a single mathematical field, there would be a risk of neglecting other areas, including those of fundamental importance for classical fields of mathematics, such as analysis, geometry and algebra. A journal dedicated to a single mathematical field, and a new one at that, seemed doomed to failure from the outset because never before had there been a journal with such narrow scope. These were powerful arguments, supported just as much abroad as at home.[5] One may add that national pride was also wounded at the prospect of eliminating usage of the Polish language.

However, the conditions at the time were favourable. Support came from the rebirth of Warsaw University, where a young generation of scholars (Janiszewski° and S. Mazurkiewicz°) and students (Bronisław Knaster°, Kazimierz Kuratowski° and others) were full of enthusiasm, brimming with confidence in their own abilities and for the future. The vision was shared by W. Sierpiński°, who had just then come back from Russia and who, in 1918, assumed the third chair of mathematics at Warsaw University:

> *When, in 1919, all three of us, Janiszewski, Mazurkiewicz and I found ourselves as professors at the revived university in Warsaw, we decided to realize Janiszewski's idea of publishing, in Warsaw, a journal, appearing in various languages, dedicated to set theory, topology, the theory of real functions, and mathematical logic. That is how "Fundamenta Mathematicae" came into being.*[6]

Thus began the Warsaw School of Mathematics. It focused on "set theory and its applications" (quote [trans.] from the cover of the journal) or, more precisely, on the pure theory of sets, set-theoretic topology, the theory of real functions and mathematical logic. The school quickly became a great success and after Janiszewski's premature death (he died in January 1920), W. Sierpiński°and S. Mazurkiewicz° were the leaders. They were later joined by younger people, such as Alfred Tarski (1901), K. Kuratowski°, Stanisław Saks (1897–1942), Karol Borsuk (1905–1982) and others.

3. Steinhaus and Banach

At the same time as the Warsaw School of Mathematics was starting up, mathematics in Lvov also sprang into life. Of the mathematicians who had been active before the war, only Ruziewicz° and Antoni Łomnicki° remained. The revival of mathematics in Lvov became the work of new people. The first of them was Hugo Steinhaus°, a mathematician educated in Göttingen, where he obtained his doctorate *summa cum laude*, bearing the signatures of David Hilbert, Carl Runge and P. Hartmann. He obtained his habilitation degree in 1917 at the university in Lvov and, when he accepted a mathematical chair there in 1920, he took Stefan Banach°[7] with him. Banach had studied a few years earlier at Lvov Polytechnic and had obtained a so-called half-diploma. He spent the war years in his home town of Cracow and pursued mathematics as a hobby.

While taking a stroll one day in the Planty* in Cracow, Steinhaus° overheard the words "Lebesgue integral" being spoken. It was so unexpected he went over and introduced himself, which is how he got to know some young people. Among them was Banach°, the man whom he later used to refer to jokingly as his "greatest mathematical discovery". Shortly afterwards they wrote a joint paper.[8]

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[4] Z. Janiszewski, *Stan i potrzeby matematyki w Polsce*, Nauka Polska. Jej potrzeby, organizacja i rozwój 1 (1918), p. 11–18; reprint: Wiadom. Mat. 7 (1963), p. 3–8. Among those who wrote about the importance of Janiszewski's program were: Sister M. G. Kuzawa, *Polish Mathematics. The Genesis of a School in Poland*, New Haven 1968; K. Kuratowski, *A Half Century of Polish Mathematics. Remembrances and Recollections*, Warsaw 1980; K. Kuratowski, *The Past and the Present of the Polish School of Mathematics*, in: I. Stasiewicz-Jasiukowa (ed.), *The Founders of Polish Schools and Scientific Models Write about Their Works*, Wrocław-Warszawa 1989.

[5] See H. Lebesgue, *À propos d'une nouvelle revue mathématique "Fundamenta Mathematicae"*, Bull. Soc. Math. France 46 (1922), p. 35–46; P. Dugac, *N. Lusin: Lettres à Arnaud Denjoy avec introduction et notes*, Arch. Intern. de l'Histoire des Sciences 27 (1977), p. 179–206 (partial translation into Polish: M. Łuzin, *List do Arnauda Denjoy z 1926 r.*, Wiadom. Mat. 25.1 (1983), p. 65-68).

[6] W. Sierpiński, O polskiej szkole matematycznej, in: J. Hurwic (ed.), *Wkład Polaków do nauki*. Nauki ścisłe, Bibliotekta Problemów 101, Warszawa 1967, p. 413–434. The role of the journal was also described by: Sister M. G. Kuzawa, "Fundamenta Mathematicae" – an examination of its founding and significance, *Amer. Math. Monthly* 77 (1970), p. 485–492; R. Duda, "Fundamenta Mathematicae" and the Warsaw School of Mathematics, in: C. Goldstein, J. Gray, J. Ritter (ed.), *L'Europe mathématique – Mythes, histoires, identités / Mathematical Europe – Myths, History, Identity*, Paris 1996, p. 479–498.

[7] R. Kałuża, The Life of Stefan Banach¸ Transl. and ed. by A. Kostant and W. Woyczyński, Boston 1996. Also see E. Jakimowicz, A. Miranowicz (ed.), Stefan Banach. Niezwykłe życie i genialna matematyka, Gdańsk-Poznań 2007 and (II ed.) 2009; English version: Stefan Banach. Remarkable Life, Brilliant Mathematics, II ed., Gdańsk-Poznań 2009; R. Duda, Facts and Myths about Stefan Banach, Newsletter of the EMS, Issue 71 (March 2009).

* The 'Planty' is a garden park area, with a tree-lined walkway, around the old city of Cracow.

Meanwhile, in Lvov, Steinhaus° helped Banach° obtain his doctorate in 1920 (which was not easy in view of the fact that he had not completed a course of higher studies). After his doctorate, Banach° pursued his academic career independently: almost immediately after obtaining his habilitation degree in 1922, he was appointed as a professor extraordinary and by 1927 he held an ordinary professorship at UJK.

While still in Cracow, but already acquainted with Banach°, Steinhaus° wrote a paper on functional analysis.[9] He placed great importance on what was then a new and expanding mathematical field, encouraging Banach° to get involved.

Let us remember that during the final decades of the 19th century and at the beginning of the 20th century, in mathematics there appeared sets that had as their elements sequences, series, functions and similar such objects. For example, there was the set l_2 of sequences whose sums of squares form a convergent series, the set C of continuous, real-valued functions defined on the interval [0,1], the set L^2 of square-integrable, real functions defined on the interval [0,1], etc. Through the properties of the constituent sets of those objects, it was possible to distinguish algebraic structures (addition, for example), geometric structures (e.g. line segments), topological structures (e.g. uniform convergence made it possible to define a limit), etc. Such sets, having distinguishable structures, had interesting properties and were named "function spaces". They were studied by Vito Volterra, Hilbert, Frigyes Riesz and others. But they looked at those "spaces" one by one. What was missing was a general definition that could accommodate all those "function spaces" as a single notion, in order to investigate just one single "space" instead of what had hitherto been many. And that was the task that Banach took up, introducing in his doctoral thesis[10] the notion of a *type B* space, which encompassed all the known function spaces. Frechet and Steinhaus° suggested the term "Banach space" and that is what it is commonly called today.

Banach's° definition was inspired by geometry. He sought to define a general function space, a generalisation of Euclidean space, such that one could apply geometric methods and extend classical analysis to this general space of functions. He succeeded by skilfully connecting together algebra, analysis and topology. And geometry indicated the way to do it.

The definition of a type B space (in other words a *Banach space*) was axiomatic. The axioms belonged to three different groups, corresponding to linearity, metricality and completeness. The first group of axioms stipulated that X be a vector space over R, meaning that over elements of X, called *vectors*, there exists a well defined law of addition, thus making X into an abelian group, as well as a law of multiplying vectors by real numbers (called the *scalars*), and such that the distributivity and associativity conditions are satisfied.

The second group of axioms was characterised by the *norm* function, denoted $\|x\|$ on vectors $x \in X$. It is a non-negative, real-valued function which satisfies the following conditions:

a) $\|x\| = 0$ if and only if $x = 0$.
b) $\|a.x\| = |a|.\|x\|$.
c) $\|x + y\| \leq \|x\| + \|y\|$.

The intuitive understanding of length is what underpins the notion of the norm of a vector. The last condition, known as the triangle inequality, states that the sum of the lengths of two edges of a triangle is greater than or equal to the length of the third edge. The existence of a norm makes it possible to transform X into a metric space: the distance ρ between two elements $x, y \in X$ is defined to be the norm of their difference, namely $\rho(x,y) = \|x-y\|$.

The third group consists of just one axiom, about completeness. It states that if $\{x_n\}$ is a Cauchy sequence with respect to the norm, that is to say a sequence satisfying the condition $\lim_{n,m\to\infty} \|x_n - x_m\| = 0$, then there exists an element $x \in X$ such that $\lim_{n\to\infty} \|x_n - x\| = 0$.

In a nutshell, a Banach space is a vector space, equipped with a norm, that is complete with respect to the norm. Even more succinctly, it is a normed and complete vector space.

As an aside, let us note that the definition does not include an axiom about the existence of a scalar product, which would then allow for the important geometric notion of orthogonality and, more generally, of angle. That, however, was a deliberate omission. It did indeed impoverish the geometry of a "type B space" but it guaranteed greater generality.

Banach's° thesis did not just stop with a definition and proof that all hitherto known function spaces were accommodated (in other words they are all Banach spaces). It also showed that a Banach space is an interesting mathematical object in itself. Namely, Banach° proved several theorems about it, including the contraction theorem, known also as Banach's fixed point theorem.

4. Priority issues

During the years 1920–1922, Norbert Wiener and Hans Hahn were having similar ideas to Banach. However, Wiener used a complicated system of logic, without incentive and examples,[11] while Hahn's system was formulated in the language of sequence spaces, with the aim of solv-

[8] S. Banach, H. Steinhaus, Sur la convergence en moyenne de séries de Fourier, *Bull. Intern. Acad. Sci. Cracovie, Année 1918, Série A: Sci. Math.*, p. 87–96; reprints: S. Banach, *Oeuvres I*, Warszawa: PWN, 1967, p. 31–39; H. Steinhaus, *Collected Papers*, Warszawa: PWN, 1985, p. 215–222.

[9] H. Steinhaus, Additive und stetige Funktionaloperationen, *Math. Z.* 5 (1919), p. 186–221; reprint: H. Steinhaus, *Selected Papers*, Warszawa: PWN, 1985, p. 252–288. Recollections about this work are in J. Dieudonné, *History of Functional Analysis*, Amsterdam: North-Holland, 1981, p. 128. The name "functional analysis" appeared in 1922. See the book by P. Lévy, *Leçons d'analyse fonctionnelle*, Paris: Gauthier-Villars, 1922.

[10] S. Banach, Sur les opérations dans les ensembles abstraits et leurs applications aux équations intégrales, *Fund. Math.* 3 (1922), p. 133–181; reprint: S. Banach, Oeuvres II, Warszawa: PWN, 1979, p. 305–343.

[11] N. Wiener, *On the theory of sets of points in terms of continuous transformations*, C. R. du Congrès International des Mathématiciens (Strasbourg, 1920), Toulouse 1921, p. 312–315.

ing infinite systems of linear equations in infinitely many variables.[12] The contexts were therefore entirely different from each other. Banach's concept was the clearest and best justified and it ultimately triumphed.[13] Wiener himself recognised that Banach° had priority,[14] while Banach's work and Hahn's crossed over each other several times – for example the Hahn-Banach theorem about the extension of functionals.[15]

5. The beginnings of the school

Banach° was the kind of scholar who liked to work in a group, particularly in café surroundings. Soon young, ambitious people, demanding results, began to work with him, and to some extent with Steinhaus too. The Lvov School of Mathematics was being formed.

Stanisław Mazur°[16] became one of the school's most outstanding representatives and also one of Banach's closest co-workers. Years later he summed up his Master's doctoral thesis as follows:

> *Functional analysis arose just like every new scientific discipline does, as the final stage of a long historical process. The list of mathematicians whose research led to the founding of functional analysis is huge. It includes famous names like Vito Volterra, David Hilbert, Maurice Fréchet and Frigyes Riesz. But 1922, the year when Stefan Banach announced his doctoral thesis, entitled* Sur les opérations dans les ensembles abstraits et leurs applications aux équations intégrales*, in the journal "Fundamenta Mathematicae", was a breakthrough date in the history of XX century mathematics. Because that thesis, running to a few dozen pages, finally laid down the foundations of functional analysis […]*
> *Functional analysis replaced the basic notion of number in analysis with a more general notion, nowadays referred to as a "point in Banach space" in thousands of mathematical theses. Having in this way achieved a generalisation of mathematical analysis, called functional analysis, one could treat seemingly different problems in mathematical analysis in a straightforward and uniform manner, and solve many problems which mathematicians had previously struggled with unsuccessfully.*[17]

6. "Studia Mathematica"

In 1927, Steinhaus° struck upon the idea of establishing a journal in Lvov, dedicated to the "theory of operators", a topic of interest to the school. He persuaded Banach° to help him. The first volume of *Studia Mathematica* appeared two years later, under their joint editorship. At that time it was the second journal, after *Fundamenta Mathematicae*, to appear with such a narrow scope. Furthermore it developed nicely, becoming one of the most important support platforms for the new school. Nine volumes appeared between 1919 and 1940. Of its 161 papers, 111 came from Lvov. The authors whose names appeared most frequently are listed as follows (in order of the number of papers; every contributing author of a jointly written paper is counted): Władysław Orlicz° (21), Mazur° (17), Banach° (16), Stefan Kaczmarz° (12), Steinhaus° (9), Herman Auerbach° (9), Mark Kac° (9), Józef Marcinkiewicz (8), Meier Eidelheit° (7), Juliusz Schauder° (7), Józef Schreier° (6), Antoni Zygmund (6), Władysław Nikliborc (5) and Zygmunt Wilhelm Birnbaum° (4). Of these 14 authors, only Marcinkiewicz and Zygmund came from outside Lvov. All the rest made up the most active kernel of the school.

7. Banach's monograph

The enormous advances made during the first decade of the school were compiled in Banach's monograph[18] of 1932, which brought him international recognition.

> *The appearance of Banach's treatise on "linear operations" marks the beginning of the mature age of the theory of normed spaces. All the results are interspersed with many good examples, taken from various analysis fields […] The work was hugely successful, and one of its direct consequences was the almost total adoption of the terminology and symbols used by Banach.*[19]
> *It is hard to overestimate the influence Banach's book had on the development of functional analysis. Encompassing a far wider range of questions than that provided by Hilbert space theory, it probably led to more works being written than Stone's and von Neumann's books combined.*[20] *Furthermore, in view of its greater generality, Banach space theory retained much more of the original charm of functional analysis […], than did the theory of linear operators on Hilbert spaces.*[21]

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[12] H. Hahn, Über Folgen linearer Operationen, *Monatsh. Math. Phys*. 32 (1922), p. 3-88.

[13] R. Duda, The discovery of Banach spaces, in: W. Więsław (red.), *European Mathematics in the Last Centuries*, Proc. Conference Będlewo (April 2004), Stefan Banach International Mathematical Center and Institute of Mathematics of Wrocław University, 2005, p. 37–46.

[14] N. Wiener, A note on a paper of M. Banach, Fund. Math. 4 (1923), p. 136–143; see also his personal recollections: N. Wiener, *I am a Mathematician*, New York: Doubleday, 1958.

[15] H. Hahn, Über lineare Gleichungssysteme in linearen Räumen, *J. reine angew. Math*. 157 (1927); S. Banach, Sur les fonctionnelles linéaires, *Studia Math.* 1 (1929), p. 211–216 and 223–239, reprint in: S. Banach, *Oeuvres II*¸ Warszawa: PWN, 1979, p. 375–395. See also H. Hochstadt, E. Helly, Father of the Hahn-Banach Theorem, *Math. Intellig*. 2 (1980).

[16] G. Köthe, Stanisław Mazur's contributions to functional analysis, *Math. Ann*. 277 (1987), p. 489–528; Polish version: G. Köthe, Wkład Stanisława Mazura w analizę funkcjonalną, *Wiadom. Mat*. 30.2 (1994), p. 199–250.

[17] S. Mazur, Przemówienie wygłoszone na uroczystości ku uczczeniu pamięci Stefana Banacha, *Wiadom. Mat.* 4.3 (1961), p. 249–250.

[18] S. Banach, *Théorie des opérations linéaires*, Monografie Matematyczne 1, Warszawa 1932.

[19] N. Bourbaki, *Elements d'histoire des mathématiques*, Paris: Hermann, 1969; Polish version: *Elementy historii matematyki*, trans. S. Dobrzycki, Warszawa: PWN, 1980.

[20] The author doubtless has in mind the books: J. von Neumann, *Mathematische Grundlagen der Quantenmechanik*, Berlin: Springer, 1932; M. Stone, *Linear Transformations in Hilbert Spaces and Their Application to Analysis*, New York 1932 – which started the rapid development of Hilbert Space theory.

[21] G. Birkhoff, E. Kreyszig, The establishment of Functional Analysis, *Hist. Math*. 11 (1984), p. 258–321; quoted from p. 315.

It was without doubt one of the books that had the greatest influence on contemporary mathematics. Although the theory developed in the book […] could have exploited methods already developed previously for more specific goals, its overall effectiveness is attributable, almost in entirety, to Banach and his co-workers. Frigyes Riesz always talked about the book with the greatest respect.[22]

Banach presented his ideas in a mature and concise way in his famous monograph. With exceptional clarity, he emphasized the subtle interdependency between algebraic questions and topological ones while producing highly fruitful abstract and general notions in contemporary functional analysis. What made Banach's works so powerful was the way they unified a wide variety of established results in analysis, but which were still threadbare and incomplete.[23]

In 1936, Banach° was invited to deliver a plenary talk at the International Congress of Mathematicians in Oslo[24] (incidentally, it was the second and last time he ever went abroad).

8. Steinhaus' interests

The Lvov School of Mathematics is not all about Banach° and "the theory of operations". In other words it was not all about functional analysis. One of its co-founders was Steinhaus°, who was a different kind of academic to Banach°. He was likened, by Ostwald, to a "butterfly" type of person, perpetually enticed by different "flowers", someone who introduced new research ideas but who took no part in their subsequent cultivation. Right after his initial fascination with *the theory of trigonometric series* and *functional analysis*,[25] he proved a theorem in *measure theory*, frequently cited later, proving that if one considers the set of all distances between points belonging to a set of positive measure then it must contain an interval $[0, c)$ for some $c > 0$.26. It generated further interest in measure theory at Lvov. Some years later there appeared two works that sought to describe *probability theory* in the language of measure theory.[27] They were works of pioneering status.[28] In his paper, Steinhaus° provided a complete mathematical formulation of the game of Heads and Tails, in terms of a non-classical probability system. Namely, by treating infinitely long sequences of coin tosses as sequences of 0s and 1s, thereby defining numbers in the unit interval [0,1], he could think of measurable subsets (in the Lebesgue sense) of this interval as the outcomes of random variables. He could then think of Lebesgue measure as a probability measure – he characterised his notion in terms of a triple $([0,1], L, \lambda)$, where L is a family of measurable subsets belonging to the interval [0,1] and λ denotes the Lebesgue measure. One can call it a "semi-complete axiomatisation of probability theory"[29] because the later notion of a probability space, due to Kolmogorov, was a triple (Ω, F, p), where Ω is the space of elementary events, F is a σ-field of subsets of Ω and p is a normalised measure.[30]

The pioneering thoughts of Łomnicki° and Steinhaus° and their students regarding probability theory later became more widely known thanks to William Feller. We should add that Steinhaus was not happy with Kolmogorov's notion because he thought it strayed from the idea of randomness. Together with his student Kac, he developed a theory of independent functions, intending to apply it as a basis for a more satisfactory notion of probability.[31] However, Kac soon emigrated and that ambition was never realised. Steinhaus was also an initiator of the *non-commutative theory of probability*.[32]

Another topic that interested Steinhaus was *game theory*. In a minor, one-off academic publication, he produced a short work,[33] the significance of which he himself surely did not fully appreciate.

It is a short work, not having the character of a mathematical paper; in essence it consists of several comments, but they are comments which were revelatory for their time, which lie at the very heart of modern game theory. Firstly – the notion of a strategy was introduced most precisely (it was actually called something else, but that is irrelevant). The second important contribution is the

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[22] B. Szökefalvi-Nagy, Przemówienie na uroczystości ku uczczeniu pamięci Stefana Banacha, *Wiadom. Mat*. 4.3 (1961), p. 265–268.

[23] M. H. Stone, Nasz dług wobec Stefana Banacha, *Wiadom. Mat*. 4.3 (1961), p. 252–-259.

[24] S. Banach, *Die Theorie der Operationen und ihre Bedeutung für die Analysis*, C. R. du Congrès International des Mathématiciens (Oslo, 1936), p. 261–268; reprint: S. Banach, *Oeuvres II*, Warszawa: PWN, 1979, p. 434–441.

[25] He wrote yet one more paper on functional analysis with Banach. It was an important one, in which they formulated and proved a general principle of singularity densification: S. Banach, H. Steinhaus, Sur le principe de la condensation de singularités, *Fund. Math.* 9 (1927), p. 50–61; reprints: S. Banach, *Oeuvres II*, Warszawa: PWN, 1979, p. 365–374; H. Steinhaus, *Collected Papers*, Warszawa: PWN, 1985, p. 363–372. That work also entered the history books of functional analysis, see J. Dieudonné, *History of Functional Analysis*, Amsterdam 1981, p. 141–142.

[26] H. Steinhaus, Sur les distances des points dans les ensembles de mesure positive, *Fund. Math*. 1 (1920), p. 93–103; reprint: H. Steinhaus, *Selected Papers*, Warszawa: PWN, 1985, p. 296–405.

[27] A. Łomnicki, Nouveaux fondements du calcul de probabilités, *Fund. Math*. 4 (1923), p. 34–71; H. Steinhaus, Les probabilités dénombrables et leur rapport à la thèorie de mesure, *Fund. Math*. 4 (1923), p. 286–310. The latter was reprinted in: H. Steinhaus, *Selected Papers*, Warszawa: PWN, 1985, p. 322–331.

[28] See H.-J. Girlich, Łomnicki-Steinhaus-Kolmogorov: steps to a modern probability theory, in: W. Więsław (ed.), *European Mathematics in the Last Centuries*, Proc. Conference Będlewo (April 2004), Stefan Banach International Mathematical Center and Institute of Mathematics of Wrocław University, 2005, p. 47–56.

[29] K. Urbanik, Idee Hugona Steinhausa w teorii prawdopodobieństwa, *Wiadom. Mat.* 17 (1973), p. 39–50.

[30] A. Kolmogorov, *Grundbegriffe der Wahrscheinlichkeitsrechnung*, Berlin: Springer, 1933.

[31] See P. Holgate, Independent functions: probability and analysis in Poland between the Wars, *Biometrika* 84 (1980), p. 161–173; M. Kac, Hugo Steinhaus – a reminiscence and tribute, *Amer. Math. Monthly* 81 (1974), p. 572-581; M. Kac, *Enigmas of Chance. An Autobiography*, New York, 1985.

[32] H. Steinhaus, La théorie et les applications des fonctions indépendantes au sens stochastique, in: *Les fonctions aléatoires, Colloque consacré à la théorie des probabilités,* Paris: Hermann, 1938, p. 57–73; reprint: H. Steinhaus, *Selected Papers*, Warszawa: PWN, 1985, p. 493–507.

[33] H. Steinhaus, Definicje potrzebne do teorii gier i pościgu, *Myśl Akademicka* 1 (1925), p. 13–14; English trans.: *Naval Res. Logist. Quater.* 7 (1960), p. 105–107.

so-called normalization of a game and, at last, the notion of a payoff function, which characterizes any given game, as well as the minimax principle of strategy selection.[34]

Although Steinhaus' paper was rediscovered after the war and translated into English, it turned out to be a revelation but only a revelation from the perspective of historical hindsight.

The monograph of Kaczmarz and Steinhaus[35] was a summary of Steinhaus' many years of interest in the theory of trigonometric series and, more generally, the *theory of orthogonal series* (he wrote 20 papers on the subject, including a joint paper with Kaczmarz). Up until the 1960s it was the standard source of reference on orthogonal series (but let us note that S. Kaczmarz's most frequently cited work was not that big monograph – rather it was a short note describing approximate solutions to systems of linear equations in very many variables[36]).

In the 1930s, Steinhaus became ever more attracted to *applications of mathematics*. A spin-off of his lightning fast mind, which noticed mathematics in everything, was a book that appeared in 1938 in both English and Polish. Since then it has gone through four editions, in each of those languages, and has been translated into several dozen other languages.[37] It is one of the most well-known mathematical books in the world.

9. Banach and measure theory

Modern measure theory started with Camille Jordan and Henri Lebesgue. They constructed the first examples of measure: finitely additive measure (Jordan) and countably additive measure (Lebesgue). F. Hausdorff defined the general problem of measure in his monograph,[38] where he showed, to general astonishment, that no measure exists for all subsets of $\mathbb{R}^n$, where $n>2$, even in the case of finitely additive measure. Banach took up the two remaining, unsolved cases $n = 1,2$. He showed, again to much astonishment, that in those cases the general problem of measure had an affirmative answer.[39] Both Hausdorff and Banach relied on the axiom of choice in their reasoning but neither was troubled by it.

A. Tarski was a frequent guest in Lvov, travelling from Warsaw. Tarski had a good understanding of set theory, while Banach had great geometric intuition, and audaciously applied non-constructive methods. They began working together and their first result was the so-called Banach–Tarski Paradox,[40] typically referred to as the paradoxical decomposition of a ball: a ball of unit radius can be decomposed into finitely many pieces, which can be assembled into two balls of unit radius. It is one of the most well-known, paradoxical consequences of the axiom of choice.

The measure question, suitably reformulated, was developed in Lvov with a somewhat more set-theoretic emphasis, regarding sets of large power (Banach°, Kuratowski°, Tarski, Ulam°). One of its subtopics was non-measurable alephs.

10. Schauder and others

Juliusz Schauder was one of the most talented young mathematicians at Lvov. He obtained interesting results bordering Banach spaces, topology and the theory of differential equations. He observed that there exist bijective, continuous, linear mappings from Hilbert space to itself, such that open sets are mapped to nowhere-dense sets. It meant the topology of infinite dimensional linear spaces was so very different from the topology of Euclidean space that one might wonder whether there was any sense pursuing it further. However, Schauder showed that, subject to some additional assumptions, one could ensure that a mapping from such a space to itself (not necessarily linear) would always send open sets to open sets.[41] In so doing he had "rescued" the topology of Banach spaces.[42] At the same time, it was the first major result in non-linear functional analysis (a subject Banach intended to cover in the second volume of his monograph but it never got written). Schauder found a beautiful application of that result to the theory of differential equations.[43] It marked the start of his interest with that theory and his later collaboration with Jean Leray, who was already familiar with Schauder's work and was greatly impressed by the power of his topological methods. Together they generalised those methods and then showed how powerful they can be by proving the existence of a solution to the Dirichlet problem for a particular elliptic equation.[44] That work

[34] C. Ryll-Nardzewski, Prace Hugona Steinhausa o sytuacjach konfliktowych, *Wiadom. Mat.* 17 (1973), p. 29–38.

[35] S. Kaczmarz, H. Steinhaus, *Theorie der Orthogonalreihen*, Monografie Matematyczne 6, Warszawa 1936; translated into English (1951) and Russian (1959).

[36] S. Kaczmarz, Angenäherte Auflösung von Systemen linearer Gleichungen, *Bull. Intern. Acad. Polon. Sci. Let., cl. sci. math. nat. A* (1937), p. 355–357; English trans.: Approximate solution of systems of linear equations, *Intern. J. Control* 57.6 (1993), p. 1269–1271.

[37] H. Steinhaus, *Kalejdoskop matematyczny*, Lwów: Książnica-Atlas, 1938; English version: *Mathematical Snapshots,* 1938; German version: *Kaleidoskop der Mathematik*, Berlin: VEB Deutscher Verlag der Wissenschaften, 1959, and others.

[38] F. Hausdorff, *Grundzüge der Mengenlehre*, Leipzig 1914. Reprints: Chelsea and F. Hausdorff, *Gesammelte Werke*, Band 2, Springer, 2002.

[39] S. Banach, Sur le problème de mesure, *Fund. Math*. 4 (1923), p. 7–33; reprint: S. Banach, *Oeuvres I*, Warszawa: PWN, 1967, p. 66–89.

[40] S. Banach, A. Tarski, Sur la décomposition des ensembles de points en partie respectivement congruentes, *Fund. Math*. 6 (1924), p. 244–277; reprints: S. Banach, *Oeuvres I*, Warszawa: PWN, 1967, p. 118–148, A. Tarski, *Collected Papers*, Basel-Boston-Stuttgart: Birkhäuser, 1986, vol I, p. 119–154. See also S. Wagon, *The Banach-Tarski Paradox*, Cambridge Univ. Press, 1985.

[41] J. Schauder, Invarianz des Gebietes in Funktionalräumen, *Studia Math*. 1 (1929), p. 123–139; Über die Umkehrung linearer stetiger Funktionaloperationen, *Studia Math*. 2 (1930), p. 1–6; reprints of both articles: J. Schauder, *Oeuvres*, Warszawa: PWN, 1978, p. 147–162 and 128–139.

[42] See C. Bessaga, A. Pełczyński, *Selected Topics in Infinite-Dimensional Topology*, Monografie Matematyczne, Warszawa, 1975.

[43] J. Schauder, Über den Zusammenhang zwischen der Eindeutigkeit und Lösbarkeit partieller Differentialgleichungen zweiter Ordnung vom elliptischen Typus, *Math. Ann.* 106 (1932), p. 661–772; reprint: J. Schauder, *Oeuvres*, Warszawa: PWN, 1978, p. 235–297.

[44] J. Leray, J. Schauder, Topologie et equations fonctionnelles, *Ann. de l'Ecole Norm. Sup*. 51 (1934), p. 45–78; reprint: J. Schauder, *Oeuvres*, Warszawa: PWN, 1978, p. 320–348.

earned them the 1938 Metaxas prize. At the same time, it was the start of the algebraic topology of Banach spaces. Following on from that, Schauder wrote a series of papers about the linearity question. Years later Leray summed it up thus:

> *Then Schauder publishes […] the first version of his method, a year later he writes another […] – an uncommonly elegant and short work. Nine pages […] and the six pages making up chapter IV […] constitute a complete theory for the linear Dirichlet problem. An additional note provides an important variant. That theory is astounding in its simplicity and its penetrative power.*[45]

According to Leray,[46] Schauder's greatest achievement was that he managed to "fonder la topologie algébrique des espaces de Banach, réduire les problèmes classiques de la théorie des équations aux dérivées partielles à la preuve que certaines applications linéaires d'espaces fonctionnels ont une norme finie."

The concept of computable functions also appeared in Lvov at almost exactly the same time as Alan Turing's fundamental work on the subject.

> *But it was in Poland, before WWII, where Banach and Mazur developed this idea in a most systematic manner. The war prevented them from publishing the work they had done at that time, so now only an abstract remains.*[47,48]

Banach algebras also had their start in Lvov, in the works of Eidelheit and Mazur, though it was not until 1941 when they were formally introduced, by I. M. Gelfand.[49]

Kazimierz Bartel's monograph about perspective in art[50] gained wide popularity and was based on the author's many years of study of Italian art.

It is not possible to convey in a short article the full richness of the Lvov School of Mathematics.[51] The material covered so far nonetheless testifies to its great liveliness and thematic variety, as well as the significance of the results that were achieved. In a book by Jean-Paul Pier,[52] several mathematicians were tempted to highlight "guidelines" of mathematics for the period 1900–1950. For the years 1922–1938, they identified 19 achievements of the following Lvov mathematicians: Banach°, Steinhaus°, Schauder°, Kuratowski°, Mazur°, Birnbaum°, Orlicz° and Kaczmarz°. Further evidence are the contacts with other centres, including frequent visits by mathematicians from home and abroad. Among them were Emil Borel, Lebesgue, Leray, Leon Lichtenstein, Paul Montel, John von Neumann, Gordon T. Whyburn and others.

11. The Lvov atmosphere

One of the characteristic features of mathematical life in Lvov were the frequent sessions of the Lvov branch of the Polish Mathematical Society, in which the latest results were presented and discussed. Those sessions played the role of what would later be standard seminars on specialist topics (and which did not exist at the time) and were conducive to collaborative endeavours. In the period 1928–1938, when the reports from those sessions were published in *Annales de la Société Polonaise de Mathématiques*, it turned out there had been 180 of them, in which 360 communications had been relayed. Very often a communication might just have been a trace of a result because some participants were chronically bad at publishing results at a later date. Mazur was one of the worst of these "offenders", as the following anecdote illustrates: one day in 1938 Mazur, while glancing through the abstracts of papers written by German mathematicians about convex functions, was heard to comment: "hmm … those results of mine aren't all that bad. They still haven't got everything." Turowicz also related how, when he came to Lvov, Mazur proposed they do work together on ring theory. Shortly afterwards they had proved some twenty theorems about rings. One of them contained a generalisation of the Weierstrass theorem regarding the approximation of continuous functions by polynomials (in arbitrary many variables). The work was completed in April 1939 but Mazur did not want to publish: "I don't like to [publish] straightaway. Perhaps we'll think of something even better." There was still opportunity to publish in 1940 but Mazur again refused and the work never appeared. In the meantime, that important theorem was proved by Marshall H. Stone and nowadays it is known as the Weierstrass-Stone theorem.

Sessions of the society traditionally took place on a Saturday evening. When they were over, members usually went to a café for further discussions. The most popular of them was the "Scottish" café, where they used to meet on an almost daily basis because Banach liked to discuss things and to work in the buzz of conversations. Those café meetings, lasting many long hours on more than one occasion - where cigarettes were smoked (Banach was an inveterate smoker) and coffee and alcohol consumed – passed into legend.[53] The *Scottish book* too is part of that legend. It started as a notebook bought by Banach's wife Łucja, who wanted to spare the café tables

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[45] J. Leray, O moim przyjacielu Juliuszu Schauderze, *Wiadom. Mat.* 23.1 (1959), p. 11–19.

[46] See J. Leray's introduction to: J. Schauder, *Oeuvres*, Warszawa 1978. "to establish the algebraic topology of Banach spaces, to reduce classical problems in the theory of partial differential equations to a proof that certain linear mappings of functional spaces have a finite norm."

[47] S. Banach, S. Mazur, Sur les fonctions calculables, *Ann. de la Soc. Polon. de Math*. 16 (1937), p. 223.

[48] M. Guillaume, La logique mathématique dans sa jeunesse, in: J.-P. Pier (ed.), *Development of Mathematics 1900–1950*, Basel: Birkhäuser, 1994, p. 185–367, quote (trans.) from p. 288.

[49] See A. Shields, Banach Algebras 1939–1989, *Math. Intellig*. 113 (1989), p. 15–17.

[50] K. Bartel, Perspektywa malarska, vol I, Lwów: Książnica-Atlas, 1928; German version: *Malerische Perspektive. Grundsätze, geschichtlicher Überblick, Ästhetik*, hrsg. von Wolfgang Haack, Band I, Leipzig-Berlin: Teubner, 1933.

[51] This is covered in greater detail in my book: *Lwowska szkoła matematyczna*, Wrocław, 2007.

[52] J.-P. Pier (Ed.), *Development* …, op. cit.

[53] See K. Ciesielski, Lost legends of Lvov, 1. The Scottish Café, *Math. Intelligencer* 9.4 (1987), p. 36–37; S. Ulam, Wspomnienia z Kawiarni Szkockiej, *Wiadom. Mat.* 12.1 (1969), p. 49–58.

from all the scribblings on the surfaces and partly to save some of the results obtained on them. The problems and the later comments were written into the *Scottish Book*. Gian-Carlo Rota wrote about its significance thus: "For those of us who grew up in the golden age of functional analysis, the Scottish Book was, and will remain, the romantic source of our mathematics. […] The amazing problems in the Scottish Book heralded the spirit of contemporary mathematics."[54]

Let Kuratowski, in his own words, testify to the atmosphere in Lvov, where he spent the years 1929–1933.

> *Taking up the chair in Lvov, I retained my readership position in Warsaw (taking an annual leave as a reader), because I wasn't sure if I could bear to live away from my home city of Warsaw.*
> *It turned out differently: after a year I resigned from my readership position in Warsaw and got totally enthralled with Lvov.*
> *How did that happen? The uncommon beauty of that city, which I remember even now with a certain emotion, and the academic way of life, which absorbed me with lightning speed. Of particular appeal to me was the academic environment, specifically the mathematical environment, where I could work more closely with others. Above all there were Banach and Steinhaus. […]*
> *This Lvovian "climate" was equally conducive to my creative output, ensuring the Lvov years were the most active period in my life as a scholar.*[55]

Clouds began to gather in the 1930s over this thriving way of life, the foreboding of an incoming storm of catastrophic dimensions that nobody could have envisaged. The paucity of academic positions and rising anti-Semitism persuaded some to seek a better place abroad. Those who emigrated at that time were Birnbaum° (1937), Kac° (1938) and Ulam° (1935) but the latter used to come to Poland every year for three months in the summer, until he left for good in 1939. That time he took his younger brother Adam* with him: they were the only two members of the large Ulam family to survive.

12. War

The Germans invaded Poland on 1 September 1939 and World War II began. They reached the outskirts of Lvov as early as 12 September but the city defended itself. On 17 September 1939, and in accordance with a secret Soviet-German agreement, the Soviet Union attacked a Poland that was fighting back against Germany. The Soviets took over the siege of Lvov from the Germans and the city surrendered to them on 22 September. By the terms of the Molotov-Ribbentrop pact, the country would be divided into two roughly equal parts. The eastern half, including Lvov, would go to the Soviets. Kaczmarz never returned from the September Campaign (he was an officer in the reserves and he died in circumstances unknown to this day). Countless people turned up in Lvov, escaping from Warsaw, which had been occupied by the Germans. The mathematicians among them included Knaster°, Edward Szpilrajn (Marczewski) and Saks. As Steinhaus° wrote: "in normal circumstances we would have achieved quite a lot with a team like that."[56] But circumstances were not normal. Polish schools and colleges were closed down. Although some Ukrainian colleges were opened after a few months, and employed some Poles, and although lectures in Polish were still tolerated, it was nonetheless insisted that lectures be delivered in Russian or Ukrainian. The ninth volume of "Studia Mathematica", prepared before the war, got published, but had the dual numeration 9 (1) and every article had an abstract in Ukrainian. The number of Polish students dropped from 3500 in 1939 to 400 in 1941. Life was a misery because of endless meetings, reorganisations and the constant threat of sudden arrest or deportation. Stanisław Leja (the nephew of Franciszek Leja**) got sent to Kazachstan and Władyslaw Hetper° got sent to a gulag and undoubtedly died there. Bartel° and Szpilrajn (Marczewski) both spent time in Soviet jails. "I developed an implacable, truly physical revulsion to all of the Soviet clerks, politicians and commissars. I saw them to be clumsy, deceitful, stupid barbarians who had us in their grasp, like the giant monkey which grabbed Gulliver up onto a roof."[57]

Germany attacked the Soviet Union on 22 June 1941. Germans entered Lvov just one week later. The German occupation lasted three years, from 30 June 1941 till 27 July 1944. It was the second stage of eradicating Polishness from Lvov. Based on a list that had been drawn up earlier, 23 professors from the university, the polytechnic and other pre-war colleges were arrested in July 1941. All of them were shot dead at the Wulecki Heights, some of them with their families (the sole exception was Franciszek Groër, who was released due to his German ancestry). Of the Lvov mathematicians killed with them were: Bartel°, Łomnicki°, Ruziewicz°, Stożek° (with 2 sons) and Kasper Weigel. The circumstances of that atrocity remain unexplained to this day. However it seems beyond doubt that Ukrainian nationalists were complicit in drawing up the list with the Germans because they had an equal interest in the de-Polonification of Lvov.

Steinhaus° sensed the danger. Without hesitation he burned all his family photos and personal documents. Then, on 4 July 1941, he left his flat, never to return to it again. He and his wife stayed with friends over the first few days. Then they stayed in secret at Professor Fuliński's place, in the suburbs of Lvov. When things got too dangerous there, they moved to the country near Lvov. There

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[54] R. D. Mauldin (Ed.), *The Scottish Book. Mathematics from the Scottish Café*, Boston: Birkhäuser, 1981. Quote taken from the jacket of the book.

[55] K. Kuratowski, *Notatki do autobiografii*, Warszawa: Czytelnik, 1981; quote from p. 86–89.

* Adam Ulam (1922–2000) became a distinguished professor of history and political science at Harvard University.

[56] H. Steinhaus, *Wspomnienia i zapiski*, second edition, Wrocław: Atut, 2002, quote (trans.) from p. 197.

**Franciszek Leja (1885–1979) was a professor of mathematics at Warsaw Polytechnic and Jagiellonian University in Cracow, contributing to group theory and the theory of analytic functions..

[57] H. Steinhaus, *Wspomnienia …*, op. cit., p. 191.

the Underground provided him with the genuine birth certificate of a deceased forestry worker and in July 1942 Steinhaus travelled with his wife to mountain country. They lived there until July 1945. While there, Steinhaus went by the name of Grzegorz Krochmalny and he taught on a clandestine basis. They both survived the war and when it ended they settled in Wrocław.

The Germans closed down all the colleges in Lvov but in spring of 1942 they started 4-year study courses: polytechnic courses (5 fields of study), a medical course, a veterinary course and a forestry course. The courses were run on the same basis as pre-war Polish programs but attendees had no right to transfer to German colleges. A few Lvov mathematicians found employment there.

The institute of Professor Weigel* was a singular feature of the German occupation. It produced anti-typhus vaccines for soldiers of the Wehrmacht. That institute employed, as lice feeders, many excellent representatives of the Polish intelligentsia, including mathematicians Banach°, Knaster, Orlicz° and several others.

In July 1941, Edmund Bulanda, the predecessor of the last UJK Vice-Chancellor Roman Longchamps de Bérier, who was shot dead on the Wulecki Heights, was trying to activate a clandestine UJK. Orlicz°, Żyliński° and others taught at the underground university. Some students even wrote their doctoral theses (including Andrzej Alexiewicz°, under Orlicz's° supervision).

While all this was happening, a systematic extermination was being carried out of the Jewish population and people of Jewish ancestry. Among its victims were the following Lvov mathematicians: Auerbach° (shot dead when the Rapoport hospital was liquidated, 1942), Eidelheit° (murdered, 1943), Marian Jacob (died in unknown circumstances, 1944), Schreier° (poisoned himself, 1943), Ludwik Sternbach° (went missing, 1942) and Menachem Wojdysławski (went missing sometime after 1942).

In July 1944, the population of Lvov dropped below 150 thousand people (the city had 300 thousand inhabitants before the war, and in 1941 over 400 thousand). The Red Army captured the city on 27 July 1941, with effective support from the Polish Home Army. But a few days later, the Soviets arrested and removed the Polish officers and started imposing the Soviet order. It is now known that on 26 July 1944 an agreement was made (in secret) with the PKWN.[58] The terms stated that the Soviets would keep that half of Poland articulated in the Ribbentrop-Molotov pact (with the exception of Podlasie, which they resigned from). Following that, a subsequent agreement was signed one month later regarding the westwards resettlement of the Polish population. The conference in Yalta (January 1945) confirmed the earlier terms made at Tehran concerning the "westwards shift of Poland" but the final demarcation lines of the new borders would only be determined at the Potsdam conference in August 1945. Preparations for the expulsion of the Polish population began, however, in autumn 1944, and the first transportations moved out in the spring of 1945, even before the war had ended and even before the new borders had been decided. Banach died at the beginning of August 1945. The last remaining Polish mathematicians left Lvov shortly afterwards: Knaster° (went to Wrocław), Mazur° (to Łódź), Orlicz° (to Poznań), Nikliborc (to Warsaw) and Żyliński° (to Gliwice).

The Lvov School of Mathematics had ceased to exist.

Roman Duda. A presentation of R. Duda appeared in EMS Newsletter, issue 71, March 2009, page 34.

* The Weigel Institute was named after its founder, the bacteriologist Rudolf Weigel (1883–1957), who should not be confused with Kasper Weigel (1880–1941), the Vice Chancellor of Lvov Polytechnic during 1929/1930, who was murdered (with his son) at the Wulecki Heights – as referred to above.

[58] The Polski Komitet Wyzwolenia Narodowego [Polish National Liberation Committee] was created by the communists as a surrogate Polish government. For a long time nobody recognised it except the Soviets but after fraudulent elections and the semblance of a union with the Polish government-in-exile, it was recognised by the Western powers.

Some representatives of the Lvov School of Mathematics

Andrzej Alexiewicz (1917–1995). Born in Lvov, he studied mathematics and physics at UJK. He obtained his doctorate in 1944 at the underground UJK. He was based in Poznań from 1945, where he was a professor at the university.

Herman Auerbach (1901–1942). Born in Tarnopol, he studied mathematics at UJK, got his doctorate in 1930 and his habilitation degree in 1930. He was murdered during the German occupation.

Stefan Banach (1892–1945). Born in Cracow, he studied at Lvov Polytechnic but that was interrupted by the outbreak of World War I. He obtained his doctorate at UJK in 1920, where he also obtained his habilitation degree in 1922. Immediately afterwards he was made a professor extraordinary and then an ordinary professor in 1927. During the German occupation he supported himself as a lice feeder. He died immediately after the war.

Kazimierz Bartel (1882–1941). Born in Lvov, he studied mechanics at the polytechnic and mathematics at the university. He obtained a doctorate at the polytechnic, where he became a professor extraordinary in 1912. After obtaining his habilitation degree in 1914, he was appointed as an ordinary professor in 1917, the delay in his appointment caused by his participation in the war. He was one of the most well-known politicians between WWI and WWII (he was a prime minister several times, a minister, a member of parliament and a senator). He was Vice-Chancellor of the polytechnic 1930/31. He was shot dead by the Germans on 26 July 1941. [When Kazimierz Bartel was arrested with the other professors in July 1941, he was told his life would be spared, in exchange for his collaboration. He refused the offer. That explains why he was shot a few weeks later than the other professors (translator's addition).]

Zygmunt Wilhelm Birnbaum (1903–2000). He was born in Lvov. After studying law at UJK he got interested in mathematics, so he studied mathematics at the same time as practising as a solicitor. Obtaining a doctorate at UJK in 1929, he completed his studies between 1929-1931, at Göttingen, where he obtained an actuarial diploma. He was an emigré in the United States from 1937, where he became a professor at Seattle University.

Leon Chwistek (1884–1944). He was born in Cracow, where he studied mathematics, obtaining a doctorate in 1906. He spent World War I serving in the Polish Legions. He obtained his habilitation degree in 1928 at the university in Cracow and was appointed to the chair of logic at UJK in 1930, as a professor extraordinary, then as an ordinary professor from 1938. He left for Georgia when the German-Soviet war broke out. He died in Moscow in 1944 (after having rejected an offer to join the PKWN).

Meier Eidelheit (1910–1943). He was born in Lvov and studied mathematics at UJK, obtaining a doctorate in 1938. He was murdered by the Germans.

Władysław Hetper (1909–1940?). Born in Cracow, he studied mathematics there but he obtained his doctorate at UJK in 1937. He took part in the fighting of September 1939 and was captured by the Germans but managed to escape. While he was on his way to Lvov, he was picked up by the Soviets, who accused him of being a spy (he had with him handwritten papers on logic, which were suspected to be secret codes). He was sent to a labour camp, where he died.

Zygmunt Janiszewski (1888–1920). Born in Warsaw, he studied in Zurich, Göttingen, Munich and Paris. He obtained a doctorate at the Sorbonne in 1911 and his habilitation degree in Lvov in 1913. He spent World War I as a volunteer in the Polish Legions. He joined Warsaw University in 1919 and he died in Lvov in 1920.

Mark Kac (1914–1984) Born in Krzemieniec, he studied mathematics at UJK, obtaining his doctorate there in 1937. He lived in the United States from 1938, where he became a professor at Cornell University, Ithaca, New York.

Bronisław Knaster (1893–1980). Born in Warsaw, he studied medicine in Paris, then mathematics in Warsaw, where he obtained a doctorate in 1923 and his habilitation degree in 1926. He was a frequent visitor to Lvov, where he spent the years 1939–1945. During the Soviet occupation he was a professor at the Ukrainian University. During the German occupation he fed lice. He was a professor at the University of Wrocław from 1945.

Kazimierz Kuratowski (1896–1980). Born in Warsaw, he started his studies in Glasgow but completed them at Warsaw University, obtaining a doctorate in 1921 and an habilitation degree immediately afterwards. He was a professor at Lvov Polytechnic during the years 1927–1933. From 1934, he was a professor at Warsaw University.

Antoni Marian Łomnicki (1881–1941). Born in Lvov, he studied mathematics there but completed his studies in Göttingen. He obtained his habilitation degree in 1919 at Lvov Polytechnic where he worked and where he became an ordinary professor in 1921. He was shot dead by the Germans on 4 July 1941.

Stanisław Mazur (1905–1981). Born in Lvov, he studied mathematics at UJK. Even though he did not complete his studies, he obtained a doctorate there in 1932. He obtained his habilitation degree in 1936 at Lvov Polytechnic, where he worked. He left Lvov in 1946. From 1948 he was a professor at the university in Warsaw.

Stefan Mazurkiewicz (1888–1946). Born in Warsaw, he studied mathematics in Cracow, Lvov, Munich and Göttingen. He obtained his doctorate in 1913 at the university in Lvov and his habilitation degree at the university in Cracow. He was a professor at Warsaw University from 1919 onwards.

Władysław Orlicz (1903–1990). Born in Okocim, he studied mechanics at the polytechnic and mathematics at the university in Lvov. He obtained his doctorate in 1926 at UJK, where he also obtained his habilitation degree in 1934. He was a professor at the university in Poznań from 1937 but he spent all of World War II in Lvov.

Stanisław Ruziewicz (1889–1941). Born near Kołomyja, he studied mathematics at the university in Lvov, obtaining a doctorate in 1912, after which he spent a year in Göttingen. He obtained his habilitation degree in 1918 at Lvov University and became a professor extraordinary there in 1920 and a full professor in 1924. Having been deprived of his chair at UJK in 1934, he moved to the Academy of Foreign Trade in Lvov. He was shot dead by the Germans on 12 July 1941.

Juliusz Paweł Schauder (1899–1943). Born in Lvov, he was taken into the Austrian army and sent to the Italian front. After the war he returned with the Polish army to his homeland and started mathematical studies at UJK, where he also obtained a doctorate in 1924 and an habilitation degree in 1927. When the Germans entered Lvov, he hid in Borysław but he returned to Lvov in 1943. He did not adapt well to a life of hiding and during one of his movements away he was picked up by the Germans and shot dead.

Józef Schreier (1908–1943). Born in Drohobycz, he studied mathematics at UJK but he obtained his doctorate in Göttingen. When the Germans came he had to hide but his hideaway was discovered – so he took poison to end his life.

Wacław Sierpiński (1882–1969). Born in Warsaw, he started mathematical studies there but completed them in Cracow, where he obtained his doctorate in 1906. After his doctorate he went to Göttingen. He obtained his habilitation in Lvov, where he became a professor extraordinary in 1910. He was interned in Russia during World War I. He was an ordinary professor at the university in Warsaw from 1918 onwards.

Hugo Dionizy Steinhaus (1887–1972). Born in Jasło, he studied mathematics at Lvov but after a year he transferred to Göttingen, where he obtained a doctorate summa cum laude in 1911. He served in the Polish Legions and participated in the Wołyń Campaign. He obtained his habilitation degree in 1917 at the university in Lvov, where he became a professor extraordinary in 1920 and an ordinary professor in 1923. During the German occupation he hid under a false name, living in the country near Lvov and later in the Carpathians. He never returned to Lvov. He was a professor at the university in Wrocław from 1945 onwards.

Ludwik Sternbach (1905–1942). Born in Sambor, he studied mathematics and physics at UJK. He worked with Mazur (co-authored papers) but he supported himself through teaching and actuarial work. He had to go into hiding when the Germans came. The circumstances of his death are unknown.

Włodzimierz Stożek (1883–1941). Born near Cracow, he did his mathematics studies at the university in Cracow, after which he spent two years in Göttingen. He obtained his doctorate in Cracow in 1922 and that same year he became a professor extraordinary there. From 1926 he was an ordinary professor at Lvov Polytechnic. He was shot dead by the Germans (together with his two sons) on 4 July 1941.

Stanisław Marcin Ulam (1909–1984). Born in Lvov, he studied mathematics at Lvov Polytechnic, where he obtained a doctorate in 1933. From 1935 he was at Princeton but every year he returned to Lvov for three months in the summer. He worked on the Manhattan atom project during World War II and later became a professor at the university in Boulder, Colorado.

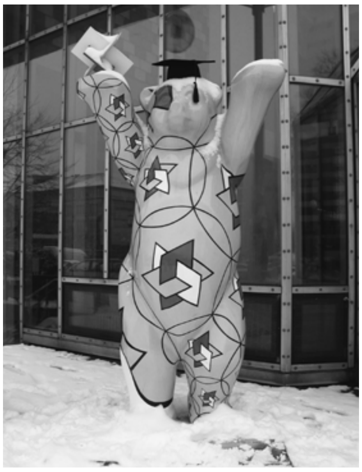
Eustachy Żyliński (1889–1954). Born near Winnica in the Ukraine, he studied mathematics at the university in Kiev and completed those studies in Göttingen, Marburg and Cambridge, obtaining a Masters degree on his return (in the Russian system, this gave him the right to teach at university). He served in the Russian army during World War I and in the Polish army after that. Appointed to a professorship at UJK in 1919, he became an ordinary professor there in 1922. After World War II he left for Łódz.

The DFG Research Centre Matheon

Matheon is a mathematical research centre that has been funded by the German Science Foundation (DFG) since 2002. It is a joint initiative of the five institutions: Freie Universität Berlin (FU), Humboldt-Universität zu Berlin (HU), Technische Universität Berlin (TU), Weierstrass Institute for Applied Analysis and Stochastics (WIAS) and Zuse Institute Berlin (ZIB), with TU Berlin as the leading university.

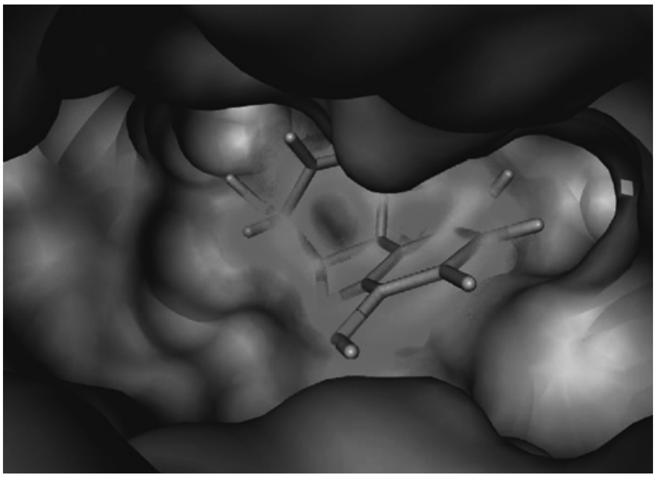
A little bit of history

Matheon is one of seven DFG research centers. The DFG (Deutsche Forschungsgemeinschaft, German Science Foundation) is funding basic research in all fields. This funding concept was created in 2000 after DFG received funding from the federal minister of science and technology BMBF with the goal of starting a new funding initiative that would lead to structural changes in the German research landscape. In the first competitive round (which had no specific topic), three (out of 90) centres were granted in the areas of nano-science, neuro-science and ocean-science. A year later a special call towards modelling and simulation in science and engineering was issued. Three of 14 proposals were preselected; they had to prepare a full proposal (about 1000 pages) and then present their case in front of an international panel. The Berlin initiative *Mathematics for key technologies: Modelling, simulation, and optimisation of real-world processes* was selected as the winner. It became clear very quickly that the long name of the centre was not adequate, so with the help of a student design group from the Berlin University of Arts a new name for the centre was created: Matheon, which has become a real trademark.



In 2002, the DFG funds (about 5.5 million euros per year) were granted for four years, with an option of two four-year extensions. In June 2010, Matheon has just been extended until 2014, when DFG funding will terminate. The five participating institutions and the federal state government of Berlin, however, have already agreed to continue Matheon beyond 2014.

Matheon was founded based on the vision: *Key technologies become more complex, innovation cycles get shorter. Flexible mathematical models open new possibilities to master complexity, to react quickly, and to explore new smart options. Such models can only be obtained via abstraction. Thus: Innovation needs flexibility, flexibility needs abstraction, the language of abstraction is mathematics. But mathematics is not only a language, it adds value: theoretical insight, efficient algorithms, optimal solutions. Thus, key technologies and mathematics interact in a joint innovation process.*



Newly developed drug candidate (pain reliever) in respective binding pocket. (©ZIB 2009)

Research in Matheon

More than 200 scientists do basic research in application driven mathematics in more than 60 research projects. These research projects are chosen on the basis of strong internal competition that includes a written proposal and an oral presentation followed a reviewing process. A typical project has an incubation period where the fundamental mathematical properties are investigated with the goal to then at a later stage transfer the knowledge into other fields of science and engineering as well as industrial practice. On top of this it is expected that every project also carries out some outreach activities, demonstrating to the general public as well as to high school students the achievements and the impact of mathematics.

The internal structure of Matheon

In view of the Matheon vision and the fact that the solution of today's complex problems typically requires expertise in many mathematical fields, the classical organisation of applied mathematics, in fields such as optimisation, discrete mathematics, numerical analysis, scientific computing, applied analysis or stochastics, etc. does not lead to a reasonable organisational structure. Although these subfields are still present in Matheon, the main organisational internal structure is the Application Areas that reflect the Berlin expertise in applied mathematics, built upon successful cooperations. In the eight years of existence, Matheon has created a large amount and a huge variety of mathematical results, transferring these applications as well as mathematical software products. To demonstrate this and to make it more visible to potential users, the Application Areas are further divided into Domains of Expertise.

Matheon Application Areas and Domains of Expertise

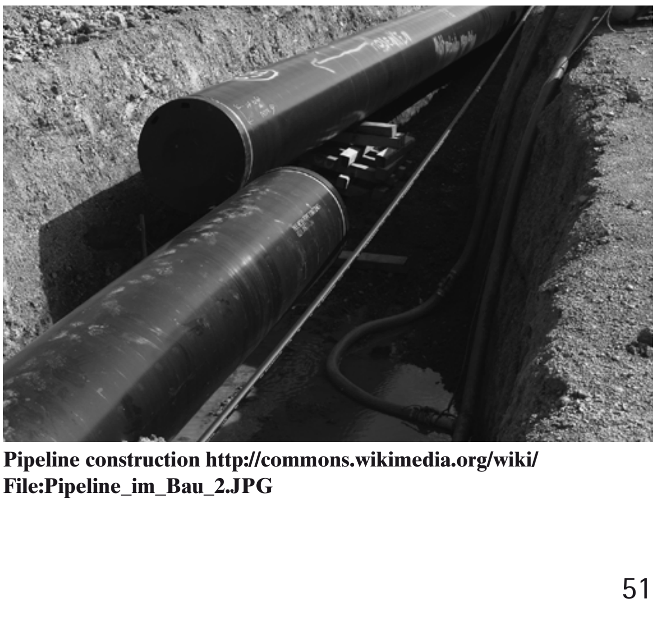
Life sciences

Matheon contributes to *computational aspects* of the life sciences. The long term vision in computational medicine is the development of mathematical models (and their mathematical analysis) that will make quantitative individual medicine possible, i.e. a patient-specific medical treatment on the basis of individual data and physiological models. In computational biology the vision is to understand molecular flexibility and function up to proteomic and systemic networks. This has led to the Domains of Expertise Molecular Processes, Computational Surgery Planning and Mathematical Systems Biology.

A project example: the biomolecular research in project *Towards a mathematics of biomolecular flexibility: derivation and fast simulation of reduced models for conformation dynamics* has had substantial impact on pharmaceutical and clinical research. Potential drug candidates (for three different diseases) were identified by mathematical means and some of these are now on their way through chemical synthesis (NDA). In addition, new techniques for early-stage medical diagnostics enabled the identification of bio-prints for three different kinds of cancer. The latter activity earned an IBM University Research Grant 2008.

Traffic and communication networks

Designing and operating communication, traffic or energy networks are extremely complex tasks that lead directly to mathematical problems. Networks are funda-

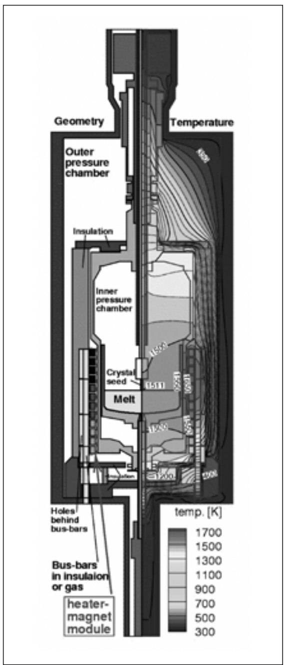


Pipeline construction http://commons.wikimedia.org/wiki/File:Pipeline_im_Bau_2.JPG

mental structures of graph theory and combinatorial optimisation. Their study has become a prosperous subject in recent years, with impressive successes in many applications. The vision is to ultimately develop theory, algorithms and software for a new, advanced level of network analysis and design that addresses network planning problems as a whole. This is reflected by the Domains of Expertise Telecommunications, Logistics, Traffic and Transport, as well as Energy and Utilities. The successful research in the area Networks has led to a large number of industrial research contracts, including one with E.ON Gastransport GmbH on the mathematics for gas transportation networks.

Production

Production is a vast field with many facets. Matheon focuses in particular on selected *multi-functional materials* and various aspects of *power generation* as well as *applications in car production and design*. For multi-functional materials, for instance, we study a variety of phenomena that occur on a whole hierarchy of different space and time scales, ranging from microscopic changes of crystal lattice configurations, over the effects of mesoscopic thermostresses, to macroscopic hysteresis. The mathematical tools required for such studies range from stochastics, asymptotic and multiscale analysis, thermodynamic modelling and phase-field theories, to numerics and optimisation. This is reflected by the Domains of Expertise Automotive, Energy and Phase Transitions.

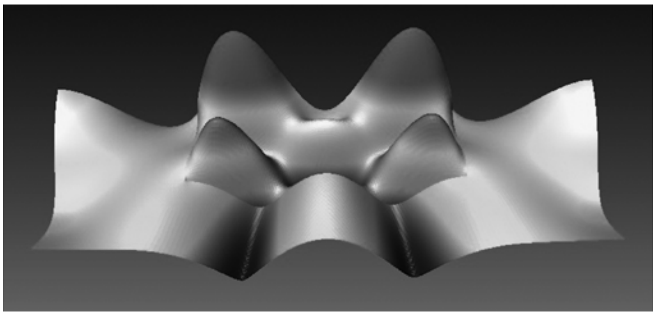


Left: A vapour pressure controlled Czochralski (VCz) growth apparatus, Right: Temperature Field computed with WIAS-HiTNIHS

A project example: in the project KristMAG®, funded by Technologiestiftung Berlin, a group at WIAS cooperated with the Leibniz Institute of Crystal Growth Berlin and two industrial partners to investigate a new technology for the Czochralski process. A technological breakthrough was achieved by demonstrating that travelling magnetic fields can be successfully applied to controlling this process. The consortium obtained numerous patents and received the Innovation Prize Berlin-Brandenburg 2008.

Electronic and photonic devices

Contributing to the development of electronic circuits and opto-electronic devices Matheon gets to the core of most current key technologies. The innovation cycles and the life cycles of products get shorter, implying that new products have to be developed in even shorter time periods. This requires new mathematical models, as well as new simulation, control and optimisation techniques for increased design automation in the Domains of Expertise Electronic Devices and Photonic Devices.



Simulated light field in a lithography mask

A project example: the pole condition – a theoretical concept developed earlier within Matheon for the Maxwell equations – has been extended in project *Design of nanophotonic* devices to a uniform framework for general time-dependent PDEs including, for instance, the Schrödinger equation. As an example of its application, we mention an optical resonator designed by IBM, which IBM itself was unable to solve on their Blue Gene. Our methods produced the solution with controlled accuracy in just three hours of CPU time on a standard multicore workstation using 100 GB of memory.

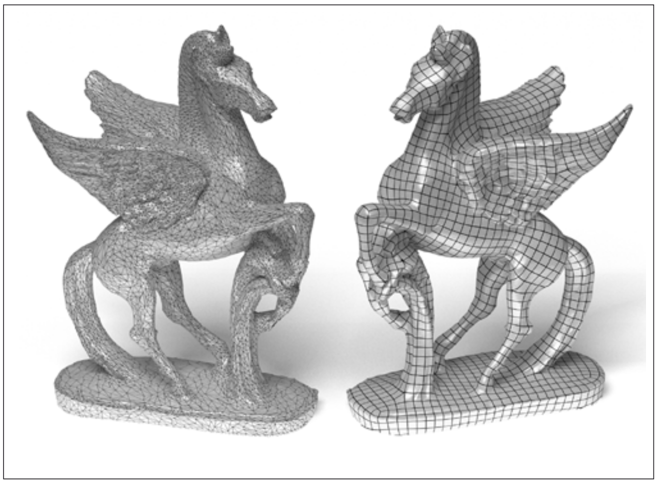
Finance

The central problem that the industry of finance and insurance faces is the management of risk in its various forms. Advanced probabilistic and statistical methods are being applied to qualify and quantify financial risk. Mathematics dominates all levels of the approach: from the conceptual challenge of *modelling* and measuring risk in terms of stochastic processes, to the challenge of statistical-numerical *simulation in the optimisation* of investment by minimising risk. Matheon emphasises the implementation of more sophisticated measures for the financial downside risk. Constructing hedging strategies that are risk-minimal in terms of these risk measures leads, for instance, to new mathematical *optimisation* problems. This is reflected in the Domains of Expertise Risk Management and Hedging as well as Simulation and Calibration.

The successful mathematical research in the Matheon Application Area Finance has led to the founding of a new research lab, the Quantitative Products Lab (QPL) by Deutsche Bank in Berlin in 2006.

Visualisation

Visualisation deals with engineering, biology or computer science aspects. The visualizing techniques open an informative window into mathematical research, scientific simulations and industrial applications. Matheon concentrates on key problems in the fields of geometry processing, medical image processing and virtual reality. Geometric algorithms are key to many industrial technologies, including computer-aided design, image and geometry processing, computer graphics, numerical simulations and animations involving large-scale data sets. The work of recent years has led to the Domains of Expertise



Geometry Processing, Image Processing and Interactive Graphics.

A project example: QuadCover, a new algorithm for quadrilateral mesh generation, was developed in project *Multilevel methods on manifold meshes*. In the international community, this algorithm now serves as a state-of-the-art solution to the hard problem of parametrizing surface meshes for rather general 3D shapes. Applications of the algorithm range from remeshing tools in CAD through architectural design to hierarchical mesh editing techniques in computer animation. The algorithm was developed in cooperation with Mental Images in Berlin, a leading computer graphics company in the world movie business.

Matheon-Math-Show 2007

Education and outreach

The basic motivation for the scientists of **Matheon** to engage themselves in educational activities is the need for more qualified young people in science, in particular in mathematics. To achieve this goal, the basis for a positive attitude towards mathematics has to be built in school. Furthermore, the unbalanced transitions from school to university, and later on into working life, have to be smoothed out by integrating these phases more strongly into each other, specifically in mathematical education.

The continuous dialogue with the general public is another point of focus in the Matheon outreach activities. To attract the attention of the media typically requires publically visible events. One of the success stories of Matheon outreach is the mathematical advent calendar, with more than 16,000 participants in 2009, where every day from 1 to 24 December at 6pm a door opens on the internet with a mathematical question behind it. Participants with the most correct answers win valuable prizes in the form of laptops or cameras. Another success is the development of the new sport MATHEatlON, a combination of running and solving mathematics exercises. The premiere took place with 600 participants during the athletics world championship in 2009 and was repeated with more than 2,500 participants and in cooperation with UNESCO in 2010. For its educational and outreach work Matheon was awarded a prize in the series ‘Land of Ideas’ by the German government, which was celebrated with a Math-Show in front of an audience of 1,200 in November 2007.

Matheon’s impact

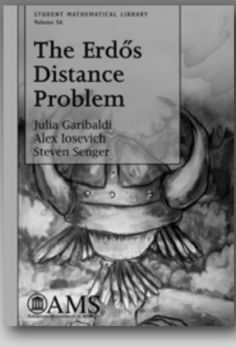
The impact of Matheon’s existence and cooperative actions is very positive and multi-faceted. It has resulted in lasting structural changes at the participating institutions. The success in establishing the Berlin Mathematical School (BMS), started as a Matheon initiative, as a Berlin-wide graduate school of mathematics with a strong cross-disciplinary focus, illustrates this very well. With the BMS-project the three Berlin universities join the competition among schools of excellence: mathematics students from around the world can take up graduate studies in Berlin. They will find a graduate program that uses the full, combined potential of the internationally renowned mathematical institutions of Berlin.

Matheon cooperations

Matheon aims at cooperation with other sciences, engineering, management science and economics, and in particular with partners in commerce and industry that are active in the key technologies the centre is addressing. Matheon has numerous industrial and scientific partners in Berlin and throughout Germany and the world. Its application driven approach is recognised worldwide as an effective model for organising collaborative research in applied mathematics. In the past few years Matheon has built a system of networks with mathematical partner institutions all over the world that work in a similar fashion, for example with MASCOS in Australia, MITACS in Canada, CMM in Chile, ICM in Poland and AMI in the Netherlands.

Matheon will continue its mission to create excellent application driven new mathematics and transfer mathematical research and technology into industry and society. We hope that our concept will find new applications but also that more groups of the European mathematical community will follow this approach and make mathematics an indispensable component of the development of science and technology and the wellbeing of society in Europe.

AMERICAN MATHEMATICAL SOCIETY



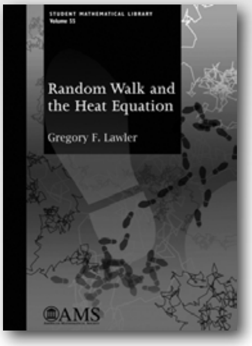
THE ERDÖS DISTANCE PROBLEM

The Life and Science of Cornelius Lanczos

Julia Garibaldi, Alex Iosevich, *University of Rochester* &
Steven Senger, *University of Missouri-Columbia*

The Erdös problem asks, What is the smallest possible number of distinct distances between points of a large finite subset of the Euclidean space in dimensions two and higher? The main goal of this book is to introduce the reader to the techniques, ideas, and consequences related to the Erdös problem. The authors introduce these concepts in a concrete and elementary way that allows a wide audience – from motivated high school students interested in mathematics to graduate students specialising in combinatorics and geometry – to absorb the content and appreciate its far reaching implications.

Student Mathematical Library, Vol. 56
Jan 2011 161pp
978-0-8218-5281-1 Paperback €25.00

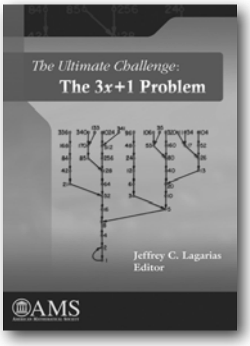


RANDOM WALK AND THE HEAT EQUATION

Gregory F. Lawler, *University of Chicago*

The heat equation can be derived by averaging over a very large number of particles. Traditionally, the resulting PDE is studied as a deterministic equation, an approach that has brought many significant results and a deep understanding of the equation and its solutions. By studying the heat equation by considering the individual random particles, however, one gains further intuition into the problem. While this is now standard for many researchers, this approach is generally not presented at the undergraduate level. In this book, Lawler introduces the heat equation and the closely related notion of harmonic functions from a probabilistic perspective.

Student Mathematical Library, Vol. 55
Dec 2010 156pp
978-0-8218-4829-6 Paperback €25.00



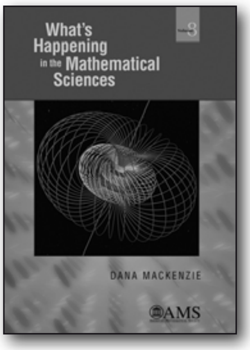
THE ULTIMATE CHALLENGE

The 3*x*+1 Problem

Edited by Jeffrey C. Lagarias, *University of Michigan*

The 3*x*+1 problem, or Collatz problem, concerns the following seemingly innocent arithmetic procedure applied to integers: If an integer *x* is odd then 'multiply by three and add one', while if it is even then 'divide by two'. The 3*x*+1 problem asks whether, starting from any positive integer, repeating this procedure over and over will eventually reach the number 1. Despite its simple appearance, this problem is unsolved. Generalisations of the problem are known to be undecidable, and the problem itself is believed to be extraordinarily difficult. This book reports on what is known on this problem.

Jan 2011 344pp
978-0-8218-4940-8 Hardback €51.00



WHAT'S HAPPENING IN THE MATHEMATICAL SCIENCES

Volume 8

Dana Mackenzie

What's Happening in the Mathematical Sciences showcases the remarkable recent progress in pure and applied mathematics. Once again, there are some surprises, where we discover new properties of familiar things, in this case tightly-packed tetrahedra or curious turtle-like shapes that right themselves. Mathematics has also played significant roles in current events, most notably the financial crisis, but also in screening for breast cancer.

Jan 2011 136pp
978-0-8218-4999-6 Paperback €20.00

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ICMI column

http://www.mathunion.org.icmi
Some activities about mathematics education in Europe from the ICMI perspective

Mariolina Bartolini Bussi

ICMI Affiliate Organisations

In order to foster ICMI efforts about international collaboration and exchanges in mathematics education, the ICMI organisational outreach includes multi-national organisations with interest in mathematics education, each operating in ways consistent with the aims and values of the Commission. The organisations affiliated to the ICMI are independent from the Commission, being neither appointed by the ICMI nor operating on behalf or under the control of the ICMI, and they are self-financed. But they collaborate with the ICMI on specific activities, such as the ICMI Studies or components of the program of the ICMEs. Each of the Affiliate Organisations holds separate meetings on a more or less regular basis.

There are currently three multi-national Mathematical Education Societies and five International Study Groups that have obtained affiliation to the ICMI.

The ICMI Study Groups have already been presented in Issue 71 (March 2009). Below is a short reminder of some conferences to be held in Europe in 2011. The three multinational Mathematical Education Societies are the following, with the year of affiliation.

- CIAEM: Inter-American Committee on Mathematics Education (2009: http://www.ciaem-iacme.org/).
- ERME: European Society for Research in Mathematics Education (2010: http://www.erme.unito.it/).
- CIEAEM: International Commission for the Study and Improvement of Mathematics Teaching (2010: http://www.cieaem.net/index.htm).

The *Inter-American Committee of Mathematics* (CIAEM) was founded in 1961 by a group of mathematicians and mathematics teachers from the three Americas, led by the distinguished mathematician Marshall Stone from the United States, President of the ICMI at the time. The main goal for founding CIAEM was to promote discussion about mathematics education. That discussion was initiated with a group of mathematics teachers coming from some American countries and has continued to grow in the number of participants as well as in the number of editions. CIAEM has organised 12 conferences. The last conference was held in Recife, Brazil, in June 2011, when CIAEM was 50 years old.

The *European Society for Research in Mathematics Education* (ERME) was founded in 1999, with communicative/cooperative/collaborative aims. A column about ERME has been published in this newsletter (Issue 70, December 2008). The society organises congresses with a wide spectrum of themes to profit from the rich diversity in European research, summer schools where experienced researchers work together with beginners and other thematic activities.

The *Commission for the Study and Improvement of Mathematics Teaching* (CIEAEM) was founded in 1950 to investigate the actual conditions and the possibilities for the development of mathematics education in order to improve the quality of mathematics teaching. Annual conferences are the essential means for realising this goal. These conferences are characterized by exchange and discussion of research work and its realisation in practice and by the dialogue between researchers and educators in all domains of practice. Most meetings of the commission have been held in Europe, with some exceptions in the US, Canada and Mexico. At http://www.cieaem.net/index.htm, historical information about CIEAEM is available. In this column, we shall reproduce only some excerpts from the Manifesto, released in 2000, for 50 years of CIEAEM (http://www.cieaem.net/50_years_of_c_i_e_a_e_m.htm).

Since its foundation in 1950, the International Commission for the Study and Improvement of Mathematics Teaching (CIEAEM, Commission Internationale pour l'Etude et l'Amélioration de l'Enseignement des Mathématiques) intended to investigate the actual conditions and future possibilities for changes and developments in mathematics education in order to improve the quality of teaching and learning mathematics. The annual meetings (rencontres) which are the essential means for realising this goal are characterised by exchange and constructive dialogue between researchers and educators in all domains of practice. In its work, the Commission follows the spirit and the humanist tradition of the founders of CIEAEM. The founders intended to integrate the scientific goal to conduct research in mathematics education with the main goal to improve the quality of mathematics education. […] The mathematician, pedagogue and philosopher Caleb Gattegno, University of London, is the spiritus rector of CIEAEM in its foundation. But there were two very distinguished personalities at the beginning who directed and determined the work of CIEAEM in the first ten years: the French Gustave Choquet (President), the Swiss psychologist and cognition theorist Jean Piaget (Vice-president) supported by Caleb Cattegno as secretary. Choquet brought into the discussion the ideas of a reform guided by the restructured new "architecture" of mathematics, Piaget presented his famous *results of research in cognition and conveyed new insights into the relationships between mental-cognitive operative structures and the scientific development of mathematics, Cattegno attempted to connect the new mathematical meta-theory to psychological research by a philosophical and pedagogical synthesis and to create and establish relationships with mathematics education as an important part of general education. […]*

In the *Manifesto 2000* there is an interesting historical reconstruction of what has happened since the 60s, with a focus on some of the features that make CIEAEM in some sense different from other international groups:

The particularity of CIEAEM can be best described by addressing four distinctive characteristics: the themes of the meetings, the specifically designed activities on the meetings, the composition of the group of participants, and the two official languages used in parallel in all activities: English and French. Various forms of working and deliberate and secured support in foreign language provision to all participants by the Commission allow to facilitate and effectively realise the exchange and the debates at the meetings and to connect individual and collective contributions into long-term co-operation. In the friendly and exciting atmosphere of CIEAEM meetings many common projects have started and were encouraged and continued beyond the meetings.

- *Themes of the Meetings/Les Thèmes des Rencontres*
  Each meeting of CIEAEM is organised around a commonly agreed theme addressing generally important or especially relevant problems. Prior to the conferences, themes are outlined and substantiated by related aspects in form of discussion papers or basic texts, together with proposals for sub-themes and questions to be worked on prior to and during the meeting.
- *Working Groups/Les Groupes de Travail*
  The major constituent of the meetings are the working groups which bring together teachers, teacher educators, and researchers from various institutions working in the fields of mathematics, history of mathematics and education, psychology, sociology and philosophy. Working groups focus on a specific sub-theme or on relationships among sub-themes, reflecting the collective and commonly shared input; they allow participants to follow up issues in-depth, to go into details and to create links between experiences and research questions. Discussions, exchange of experiences, problems, and views are prepared in form of individual and collective presentations or workshops. Animators who ensure language provision and mark new questions, research desiderata or proposals for common projects and practical experiments to be presented at the end of the conference direct the working groups. The working groups are the "heart of the conference".
- *Plenary Lectures/Les Plenières*
  The Invited Plenary Lectures serve as a commonly shared input to the meeting as a whole and to the discussions in the working groups. According to the preferences, research areas and experiences of the speakers they offer a special "bouquet" of approaches to the theme. Speakers are chosen from within CIEAEM as well as from outside, reflecting diversity in views and perspectives.
- *Individual and Collective Presentations/ Les Présentations Individuelles et Collectives Individuals or small groups of participants are invited to contribute to the theme of meeting or to a sub-theme by an oral Presentation by presenting their ideas, their research work and their experiences with others. Pertinent and significant research links to the theme of the meeting should be demonstrated. Relevant case studies that offer specific potentialities are particularly welcome. Presenters involve, whenever appropriate, their colleagues in questions or even short activities for the participants.*
- *Workshops/Les Atéliers*
  The Workshops represent a more extended kind of contributions prepared and organised by individuals or small groups: they focus on concrete activities and encourage active participation by working in groups or individually on provided materials, problems, or particular and concrete questions in connection with the sub-themes.
- *Forum of Ideas/La Foire des Idées*
  The Forum of Ideas offers the opportunity to present case studies, systematically documented learning materials, and recent research projects as well as current ideas or debates which are not directly related to the theme or sub-themes. The forum of ideas is often located in an exhibition room.
- *The Constitution and the Newsletter of CIEAEM/La Constitution et le Bulletin de la CIEAEM*
  Since 1992, CIEAEM has established an additional means for the communication among Commission members: the publication of a Newsletter for internal discussion. This opened up a forum for written exchange of problems and of questions to be dealt with, of policy-statements, and various kinds of interesting ideas e.g. themes for future meetings. The language of the Newsletter is English and French. Since 1996 CIEAEM has an officially agreed constitution and since 2000 a legal status as a non-profit organisation for the study and improvement of mathematics education.
- *The composition of the group of CIEAEM-participants*
  CIEAEM-meetings are a working place where teachers and researchers debate and collaborate intensively in an engaged and stimulating climate. Continuous exchange of research work, practical experiences and views around real problems and crucial themes raise critical and constructive discussions on developments in research in mathematics education as well as in educational policy and practice in schools and teacher education institutions. Practitioners and researchers are treated as equal partners in this collaboration. CIEAEM emphasises that links between research and practice have to be re-constructed continuously by mutual efforts, and that changes in mathematics education have to be nourished by both, practice and theory, by critique and transformation of practice as well as critique and application of research into educational development.

The *Manifesto 2000* concludes with the agenda for the future. Ten years have already passed since 2000. The interested reader may continue to read the manifesto and the pre-proceedings of the past few conferences, held in Italy, the Czech Republic, Hungary and Canada, available free on the CIEAEM website. Historically, CIEAEM is a European creation. However, the particular scheme of CIEAEM has more and more attracted participants from less- or non-industrialised parts of the world. The next meeting will be held in Europe (see below).

News – some conferences in Europe in 2011 of ICMI Affiliated Organisations

- 9–13 February 2011; CERME 7: European Society for Research in Mathematics Education, Rzeszow, Poland. Website: http://www.cerme7.univ.rzeszow.pl/index.php.
- 10–15 July 2011; PME 35: International Group on the Psychology of Mathematics Education, Ankara, Turkey. Website: http://www.pme35.metu.edu.tr/.
- CIEAEM meeting is announced in 2011: Barcelona, Spain. Website: http://www.cieaem.net/future_meetings.htm.

News – ongoing ICMI Studies

ICMI Study 20 (joint ICMI/ICIAM Study) on the theme *Educational Interfaces between Mathematics and Industry (EIMI)* was presented in Newsletter 70 (December 2008). The associated conference was expected to take place in April 2010 but was postponed due to volcano Eyjafjllajokull. It was held in Lisbon, PT, on 11–15 October (http://eimi.glocos.org/).

ICMI Study 21 on the theme *Mathematics Education and Language Diversity*, which was presented in Newsletter 76 (June 2010), will be held in São Paulo, Brazil, 16–20 September 2011. For more information, consult the Study website at www.icmi-21.co.za.

Dynamic Reviewing at Zentralblatt MATH

Bernd Wegner, FIZ Karlsruhe

Summary: The words "Dynamic reviewing" describe the option of enhancing the information given in reference databases like Zentralblatt MATH by capturing the impact of a publication on later mathematical research. In contrast to printed documentation services, reference databases provide a lot of opportunities for dynamic reviewing. Two of these opportunities are second reviews and reviews looking back to the past. Starting in 2010, Zentralblatt MATH is offering both of these.

1 Traditional enhancements of reviews

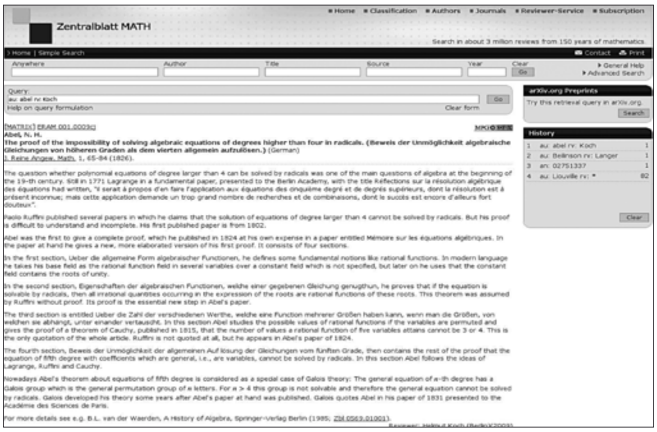
During times when only printed documentation services in mathematics were available, facilities for dynamic reviewing were very limited. There were printed corrections, addenda, enhanced later editions, citations in reviews, etc. These were documented in the printed Zentralblatt MATH but the location of the information was different from that of the review and reference could only be given to the past. Index volumes did not help very much in bringing earlier and later information together.

After the change to reference databases, some better facilities could be quickly implemented. Firstly, there was the link or list called "cited in", which appeared with the original review and gave regularly updated information of where the paper under consideration was cited in later reviews. This enabled users to navigate in the database forward and back but it depended on a citation in a review. In the last decade, links from the database to the electronic version of a paper and links from the references in this paper to the database provided an additional navigation facility, which is currently managed through the DOI, a frequently used identifier for digital publications.

2 Automatic enhancements of reviews

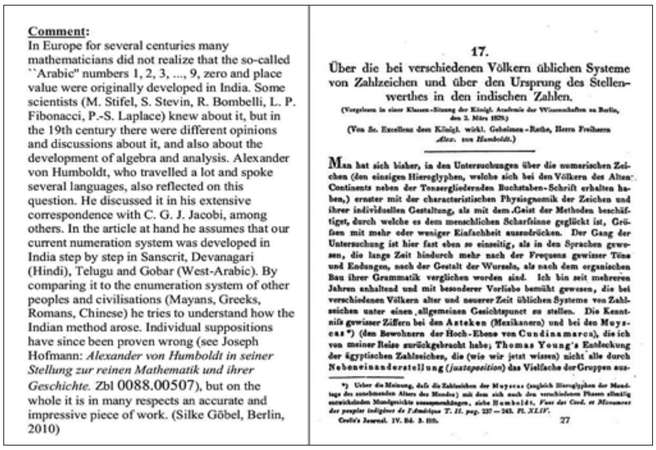
Starting in 2010, Zentralblatt MATH implemented further enhancements by harvesting the references of a paper and adding them to the data provided with the review. This enriches the "cited in" facility, though to be cited in a review has a different quality compared to a citation in a reference. In addition to this, not all journals can be handled in this way. It depends on the availability of an electronic version of an article and the possibility of easily importing the references. Furthermore, to copy these data has to be approved by the publisher or editor.

A side aspect (or for some even the main aspect) for adding the reference data to the database is to develop impact figures, like those provided by the SCI or AMS already. What these figures tell us about the impact of a paper is a permanently discussed question and there are plenty of justified complaints about improper usage of these figures. Referring to the database, additional information where a paper has been cited is a useful enhancement when searching for the literature in a subject of current interest.



Looking back to 1826: Helmut Koch reviews Abel's proof of the impossibility of solving algebraic equations of degrees higher than four in radicals.

30 years later: As Adrian Langer points out in a second review, a large branch of algebraic geometry has grown out of a nutshell 2-page article.



<u>Comment</u>:
In Europe for several centuries many mathematicians did not realize that the so-called ``Arabic" numbers 1, 2, 3, ..., 9, zero and place value were originally developed in India. Some scientists (M. Stifel, S. Stevin, R. Bombelli, L. P. Fibonacci, P.-S. Laplace) knew about it, but in the 19th century there were different opinions and discussions about it, and also about the development of algebra and analysis. Alexander von Humboldt, who travelled a lot and spoke several languages, also reflected on this question. He discussed it in his extensive correspondence with C. G. J. Jacobi, among others. In the article at hand he assumes that our current numeration system was developed in India step by step in Sanscrit, Devanagari (Hindi), Telugu and Gobar (West-Arabic). By comparing it to the enumeration system of other peoples and civilisations (Mayans, Greeks, Romans, Chinese) he tries to understand how the Indian method arose. Individual suppositions have since been proven wrong (see Joseph Hofmann: *Alexander von Humboldt in seiner Stellung zur reinen Mathematik und ihrer Geschichte*. Zbl 0088.00507), but on the whole it is in many respects an accurate and impressive piece of work. (Silke Göbel, Berlin, 2010)

17.

Über die bei verschiedenen Völkern üblichen Systeme von Zahlzeichen und über den Ursprung des Stellenwerthes in den indischen Zahlen.

(Vorgelesen in einer Klassen-Sitzung der Königl. Academie der Wissenschaften zu Berlin, den 2. März 1829.)

(Von Sr. Excellenz dem Königl. wirkl. Geheimen-Rathe, Herrn Freiherrn *Alex. von Humboldt*.)

Man hat sich bisher, in den Untersuchungen über die numerischen Zeichen (den einzigen Hieroglyphen, welche sich bei den Völkern des Alten Continents neben der Tongliedernden Buchstaben-Schrift erhalten haben,) ernster mit der characteristischen Physiognomik der Zeichen und ihrer individuellen Gestaltung, als mit dem Geist der Methoden beschäftiget, durch welche es dem menschlichen Scharfsinne geglückt ist, Größen mit mehr oder weniger Einfachheit auszudrücken. Der Gang der Untersuchung ist hier fast eben so einseitig, als in den Sprachen gewesen, die lange Zeit hindurch mehr nach der Frequenz gewisser Töne und Endungen, nach der Gestalt der Wurzeln, als nach dem organischen Bau ihrer Grammatik verglichen worden sind. Ich bin seit mehreren Jahren anhaltend und mit besonderer Vorliebe bemüht gewesen, die bei verschiedenen Völkern alter und neuerer Zeit üblichen Systeme von Zahlzeichen unter einen allgemeinen Gesichtspunct zu stellen. Die Kenntniß gewisser Ziffern bei den Azteken (Mexikanern) und bei den Muyscas*) (den Bewohnern der Hoch-Ebene von Cundinamarca), die ich von meiner Reise zurückgebracht habe; Thomas Young's Entdeckung der ägyptischen Zahlzeichen, die (wie wir jetzt wissen) nicht alle durch Nebeneinanderstellung (*juxtaposition*) das Vielfache der Gruppen aus-

*) Ueber die Meinung, daß die Zahlzeichen der Muyscas (zugleich Hieroglyphen der Mondtage des zunehmenden Alters des Mondes) mit dem sich nach den verschiedenen Phasen allmälig entwickelnden Mondgesichte zusammenhängen, siehe Humboldt, *Vues des Cord. et Monumens des peuples indigènes de l'Amérique T. II. pag. 237 — 243. Pl. XLIV.*

Crelle's Journal. IV. Bd. 3. Hft. 27

Looking back to the roots of mathematics: Alexander von Humboldt wrote in 1829 about the origin of the place value of Indian numbers, with further review information now available.

3 Second reviews

Looking at a paper again from a later point of view and judging the impact as an expert in the corresponding field is another way to get an idea about the influence of a paper on later mathematical research or on applications to other sciences. This is the idea of the "second reviews" or further later reviews. Technically, such a review should be strongly linked with the first review of a paper or book, making the information on the publication as comprehensive as possible. The technical facilities and the workflow for handling second reviews are available now at Zentralblatt MATH.

But an important question is which publications should deserve a second review. We are obviously not able to revise the literature already reviewed completely after a certain period. It would need a lot of manpower and the expertise to judge on this will not be available on a larger scale. For selecting papers to be reviewed a second time after five or more years, for example, Zentralblatt MATH can only rely on hints given from the mathematical community, may it be single experts for specific papers or expert groups, in charge of special subject areas in mathematics. We are on the way to installing some expert groups.

A good example, though not the most important, may be the publications of Shirshov dealing with the foundations of Groebner bases. When they were published, they were considered as quite theoretical. Being in Russian they have been partially ignored by the mathematical community. But now their importance is visible and this should be acknowledged by a second review. This does not mean that second reviews are appropriate only for almost forgotten papers. Research that has been fully integrated in later publications may also deserve a second review, mentioning this kind of impact of a paper.

4 Looking back

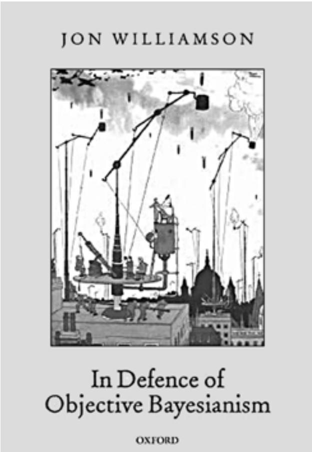
"Looking back" or even better "looking beyond the limits" given by the current scope of Zentralblatt MATH is another part of our dynamic reviewing. There are two kinds of limit: time and subject area. This activity should pick up publications of high interest from the period before the Jahrbuch über die Fortschritte der Mathematik was founded and articles of relevance for mathematics that have been published in journals where mathematical articles would not normally be expected. The latter mostly refers to applications in mathematics, for which it is difficult to draw a precise boundary line where mathematics ends.

Good examples are the early volumes of the *Journal für die Reine und Angewandte Mathematik* going back to the early 19th century. Their data have been covered by Zentralblatt MATH recently and we are starting to ask for reviews for selected articles. As a nice side aspect, an article by Alexander von Humboldt on the development of number systems became visible again through this activity.

Also for this part of dynamic reviewing, Zentralblatt MATH depends on suggestions from the mathematical community. Hence one reason for this note is to invite the readers of the newsletter to send us suggestions. Please use my editor's address or the comment box appearing with the document to be reviewed a second time in the Zentralblatt MATH database for this purpose.

Bernd Wegner [wegner@math.tu-berlin.de]. A presentation of B. Wegner appeared in EMS Newsletter, issue 77, June 2010, page 56.

Book reviews



Jon Williamson

In Defence of Objective Bayesianism

Oxford
2010, 200 p.
ISBN13: 978-0-19-922800-3

Reviewed by Olle Häggström

How objective is objective Bayesianism – and how Bayesian?

The dust jacket of Jon Williamson's *In Defence of Objective Bayesianism* is dominated by an ingenious drawing by early 20th century artist William Heath Robinson that beautifully illustrates the second word of the book's title. From there, the book goes quickly downhill, never to recover.

Objective Bayesianism, in Williamson's view, is an epistemology that prescribes that the degrees to which we believe various propositions should be:

(i) Probabilities.
(ii) Calibrated by evidence.
(iii) Otherwise as equally distributed as possible among basic outcomes.

The task Williamson sets himself is, as the title suggests, to defend the idea that this is the right epistemology to guide how we acquire and accumulate knowledge, especially in science. This makes the book primarily a contribution to the philosophy of science rather than to mathematics, even though mathematical formalism – especially propositional and predicate logic, entropy calculations and probability – pervades it. The author masters such formalism fairly well, apart from the occasional lapse (such as when, on p. 34, he implies that a dense subset of the unit interval must be uncountable).

Among the proposed requirements (i), (ii) and (iii) on the extent to which we should believe various propositions, (ii) strikes me as the least troublesome (it would probably take a theologist to dissent from the idea that evidence should constrain and guide our beliefs), while (i) seems more open to controversy but not obviously wrong. I have more trouble with the final requirement (iii) about equivocation between different outcomes. Given that we accept the premise (i) about expressing degrees of belief in terms of probabilities, surely an unbiased thinker should follow (iii) in spreading his belief uniformly over the possible outcomes, unless constrained otherwise by evidence? This may seem compelling, until we examine some examples. Williamson is aware of the mathematical obstacles to defining uniform distribution on various infinite sets but seems unaware of how poorly assumptions of uniform distribution may perform even in finite situations.

Consider the following image analysis situation. Suppose we have a very fine-grained image with $10^6 \times 10^6$ pixels, each of which can take a value of black or white. The set of possible images then has $2^{10^{12}}$ elements. Suppose that we assign the same probability $1/2^{10^{12}}$ to each element. This is tantamount to assuming that each pixel, independently of all others, is black or white with probability 1/2 each. Standard probability estimates show that with overwhelming probability, the image will, as far as the naked eye can tell, be uniformly grey. In fact, the conviction of uniform greyness is so strong that even if, say, we split the image into four equally sized quadrants and condition on the event that the first three quadrants are pure black, we are still overwhelmingly convinced that the fourth quadrant will turn out grey. In practice, this can hardly be called unbiased or objective.

Intelligent design proponent William Dembski (2002) makes a very similar mistake in his attempt to establish the unfeasibility of Darwinian evolution by appealing to the so-called no free lunch theorems. In doing so, he implicitly assumes that the fitness landscape (a function that describes how fit for reproduction an organism with a given genome is) is randomly chosen from a large but finite set of possible such landscapes with a similar product structure as in the image example. Just as the uniform prior in the image example assigns probability very close to 1 to the event that the image is just grey, the uniform prior in the biology example assigns probability very close to 1 to the (biologically completely unrealistic) event that the fitness landscape is entirely unstructured. See Häggström (2007) for a more detailed discussion.

These examples show that the term "objective" for the habit of preferring uniform distributions whenever possible is about as suitable as the term "objectivist" for someone who favours the night watchman state and who has read and memorised *Atlas Shrugged*.

At this point, a defender of uniform distributions might suggest that the reason why requirement (iii) can lead so badly wrong in these examples is the extremely large state spaces on which the uniform distribution is applied. So let's look at an example with a smaller state space, with just 2 elements. In his first chapter, Williamson describes a situation where a physician needs to judge the probability that a given patient has a given disease S. All the physician knows is that there is scientific evidence that the probability that a patient with the given symptoms actually has disease is somewhere in the interval [0.1,0.4]. Williamson's suggestion is that the physician should settle for $P(\text{ill}) = 0.4$ because this is as close as he can get to a uniform distribution (0.5,0.5) on the space {ill,healthy} under the constraint given by the scientific evidence.

I must admit first thinking that the author was joking in suggesting such an inference, but no – further reading reveals that he is dead serious about it. Rather than giving the whole list of objections that come to my mind, let me restrict to one of them: what Williamson himself calls *language dependence*. Let us suppose that we refine the crude language that only admits the two possible states "ill" and "healthy" to account for the fact that a healthy person can be either susceptible or immune, so that the state space becomes {ill, susceptible, immune} and Williamson's favoured estimate goes down from $P(\text{ill}) = 0.4$ to $P(\text{ill}) = 1/3$. By further linguistic refinement (such as distinguishing between "moderately ill", "somewhat more ill", "very ill" and "terminally ill"), we can make $P(\text{ill})$ land anywhere we wish in [0.1,0.4]. How's that for objectivity?

Williamson is aware of the language dependence problem and devotes Section 9.2 of his book to it. His answer is that one's language has evolved for usefulness in describing the world and may therefore itself constitute evidence for what the world is like. "For example, having dozens of words for snow in one's language says something about the environment in which one lives; if one is going to equivocate about the weather tomorrow, it is better to equivocate between the basic states definable in one's own language than in some arbitrary other language." (Williamson, p. 156–157). This argument is feeble, akin to noting that all sorts of dreams and prejudices we may have are affected by what the world is like, and suggesting that we can therefore happily and unproblematically plug them into the inference machinery.

So much for requirement (iii) about equivocation; let me move on. Concerning requirement (i) that our degrees of beliefs should be probabilities, let me just mention that Williamson attaches much significance to so-called Dutch book arguments. These go as follows. For a proposition θ, define my belief $p(\theta)$ as the number p with the property that I am willing to enter a bet where I receive \$$a(1-p)$ if θ but pay \$$ap$ if $\neg\theta$, regardless of whether a is positive or negative. Leaving aside the issues of existence and uniqueness of such a p, it turns out that I am invulnerable to the possibility of a Dutch book – defined as a collection of bets whose total effect is that I lose money no matter what – if and only if my beliefs satisfy the axioms of probability.

Let me finally discuss requirement (ii) that beliefs should be calibrated by evidence. This, as mentioned above, is in itself pretty much uncontroversial; the real issue is *how* this calibration should go about. Here, when reading the book, I was in for a big surprise. Having spent the last couple of decades in the statistics community, I am used to considering the essence of Bayesianism to be what Williamson calls *Bayesian conditionalization*: given my prior distribution (collection of beliefs), my reaction to evidence is to form my posterior distribution by conditioning the prior distribution on the evidence.

Not so in Williamson's "objective Bayesianism"! His favoured procedure for obtaining the posterior distribution is instead to find the maximum entropy distribution among all those that are consistent with the evidence.

This is especially surprising given the significance that Williamson attaches to Dutch book arguments because it is known that if the way I update my beliefs in the light of evidence deviates from what is consistent with Bayesian conditionalization then I am susceptible to a Dutch book in which some of the bets are made before the evidence is revealed and some after (Teller, 1973). Even more surprisingly, it turns out that Williamson knows this. How, then, does he handle this blatant inconsistency in his arguments?

At this point he opts for an attempt to cast doubt on the use of sequential Dutch book arguments. On p. 85 he claims that:

> "in certain situations one can Dutch book *anyone who changes their degrees of belief at all*, regardless of whether or not they change them by conditionalization. Thus, avoidance of Dutch book is a lousy criterion for deciding on an update rule."

Here, emphasis is from the original but I would have preferred it if Williamson, for clarity, had instead chosen to emphasise the words "*in certain situations*". The force of his argument obviously hinges on what these situations are. The answer: "Suppose it is generally known that you will be presented with evidence that does not count against θ so that your degree of belief in θ will not decrease." (Williamson, p. 85). Here it must be assumed that by "generally known" he means "generally known by everyone but the agent" because as a Bayesian conditionalizer I would never find myself in a situation where I know beforehand in which direction my update will go, because then I would already have adjusted my belief in that direction. So what he's actually referring to is a situation where the Dutch bookmaker has access to evidence that I lack. A typical scenario would be the following. I have certain beliefs about how the football game Arsenal versus Real Madrid will end and set my probabilities accordingly. Now, unbeknownst to me (who was confused about the game's starting time), the first half of the game has already been played and Arsenal are down 0–3. The Dutch bookmaker approaches me for a bet, then reveals what happened in the first half and offers a second bet. Well, of course he can outdo me in such a situation! But if we allow the Dutch bookmaker to peek at evidence that is currently unavailable to me then we might just as well let him see the whole match in advance, in which case he could easily empty my wallet without even the need for a sequential betting procedure.

Hence, what Williamson's intended reductio shows is not that sequential Dutch book arguments should be avoided but rather that we must insist on Dutch bookmakers not having access to evidence that the agent lacks. If we do so, it follows from a straightforward martingale argument that an agent who sticks to Bayesian conditionalization is immune to sequential Dutch books with a bounded number of stages.

Dutch books aside, there is practically no end to the silliness of the author's further arguments for why his maximum entropy method is superior to Bayesian conditionalization. On p. 80, he offers the following example.

> "Suppose A is 'Peterson is a Swede', B is 'Peterson is a Norwegian', C is 'Peterson is a Scandinavian', and ε is '80% of all Scandinavians are Swedes'. Initially, the agent sets $P_\varepsilon(A) = 0.2, P_\varepsilon(B) = 0.8, P_\varepsilon(C) = 1, P_\varepsilon(\varepsilon) = 0.2$ and $P_\varepsilon(A \wedge \varepsilon) = P_\varepsilon(B \wedge \varepsilon) = 0.1$. All these degrees of belief satisfy the norms of subjectivism. Updating by [maximum entropy] on learning ε, the agent believes that Peterson is a Swede to degree 0.8, which seems quite right. On the other hand, updating by conditionalization on ε leads to a degree of belief of 0.5 that Peterson is a Swede, which is quite wrong."

Here Williamson obviously thinks the evidence ε constrains the probability of A to be precisely 0.8. This is plain false – unless we redefine C to say something like: "Peterson was sent to us via some mechanism that picks a Scandinavian at random according to uniform distribution, and we have absolutely no other information about how he speaks, how he dresses, or anything else that may give a clue regarding his nationality." But this is not how the problem was posed.

Suppose however for the sake of the argument that ε does have the consequence that Williamson claims. Then in fact the choice of prior is incoherent because $P_\varepsilon(A \wedge \varepsilon) = P_\varepsilon(B \wedge \varepsilon) = ½\, P_\varepsilon(\varepsilon)$ *means* that given ε, the odds for Peterson being Swedish or Norwegian are fifty-fifty. Hence, this argument of Williamson against Bayesian conditionalization carries about as much force as if I would make the following argument against his objective Bayesianism: "Suppose that, in the course of working out his maximum entropy updating, Williamson assumes that $x < 3$ and that $x = 5$. This obviously leads to a contradiction, so there must be something fishy about objective Bayesianism."

I could go on and on about the weaknesses of Williamson's case for his pet epistemology but this review has already grown too long so I'll just finish by pointing to one more crucial issue. Namely, exactly *how* does evidence lead to constraints on what is reasonable to believe – constraints that serve as boundary conditions in the entropy maximisation procedure that follows next. Williamson tends to treat this step as a black box, which seems to me very much like begging the question. For instance, on p. 83 he discusses what to expect of the 101th raven if we've already seen 100 black ravens – will it be black or non-black? Unconstrained entropy maximisation yields the distribution (0.5,0.5) on {black, non-black} but Williamson rejects this, claiming that the evidence constrains P(black) to be close to 1. And then this: "Exactly how this last constraint is to be made precise is a question of statistical inference – the details need not worry us here." (Williamson, p. 83). An author who wishes to promote some particular philosophy of science but has no more than this to say about the central problem of induction has a long way to go. In his final chapter, Williamson does admit that "there is plenty on the agenda for those wishing to contribute to the objective Bayesian research programme." (p. 163). To this, I would add that they face an uphill struggle.

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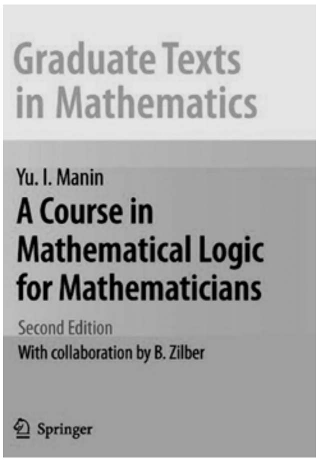

Yu. I. Manin
(in collaboration with
B. Zilber)

A Course in Mathematical Logic for Mathematicians

2nd Edition
Graduate Texts in Mathematics

Springer
2009, 384 p.
ISBN: 978-1-4419-0614-4

Reviewed by Fernando Ferreira

In the early 80s, when I was just starting as a graduate student, I bought the first edition of this book. It was a book on mathematical logic, written in a lively language and discussing unexpected subjects. It was rather different from the textbooks that I was struggling to understand. It included quantum logic, the exposition of Smullyan's elegant SELF language, a brief on Feferman's transfinite recursive progressions of axiomatic theories, Gödel's result on the length of proofs and Kolmogorov complexity, as well as some interesting digressions (e.g. on the linguistics of Icelandic poetry and on Luria's description of a damaged psyche) and mathematical speculations (on recursive geometry – better called geometry of recursion – for instance). Of course, the main thrust was on standard material (see the discussion ahead), including some advanced material not usually covered in textbooks (Chapter VIII is devoted to a full proof of Graham Higman's result on the characterization of the finitely generated subgroups of finitely presented groups as the finitely generated, recursively presented groups). This idiosyncratic view of mathematical logic by a well-known and respected "working mathematician" did not fail to make an impression on a curious, young mind.

It is with pleasure that, more than 25 years later, I have the occasion to review the second edition of Manin's book. This edition includes new material (it has 100 more pages than the old edition). The preface of the first edition says that the book "is above all addressed to mathematicians [and] it is intended to be a textbook of mathematical logic on a sophisticated level". In the second edition, the author clarifies his statement. He now says that he imagined his readers as "working mathematicians like me". The latter statement is, I believe, more to the point (note also the slight change of title from the first to the second edition, where "for Mathematicians" is now inserted). It is certainly a book that can be read and perused profitably by a graduate student in mathematical logic but rather as a complement to a more standard textbook. It also has some interesting discussions for the professional logician but I believe that its natural public is the research mathematician not working in logic. Manin is a very good writer and displays immense culture (mathematical and otherwise). It is a pleasure to read his writing and, for a critical mathematical mind, the book is intellectually very stimulating. Of course, the pereniality and beauty of some of the results expounded also helps in making the book attractive reading for the non-specialist.

The book assumes no previous acquaintance with mathematical logic. The first two chapters are devoted to basic material, including Gödel's completeness theorem, the Löwenheim-Skolem theorem (with a discussion of Skolem's "paradox") and Tarski's theorem on the undefinability of truth. Chapter VII discusses Gödel's first incompleteness theorem in its semantic form: the set of provable sentences of a theory is arithmetically definable whereas the set of truths of arithmetic is not – *ergo*, truth differs from provability. Some reviewers of the first edition observed that the compactness theorem of first-order logic was conspicuously absent. In this edition, however, this is corrected and the compactness theorem is given its proper place in a new chapter (the last, numbered X) by Boris Zilber; indeed, three different proofs of the theorem are now discussed. In less than 50 pages, Zilber gives the reader a masterful tour in model theory taking us to the frontiers of research. The emphasis of this tour is on the connections of model theory with "tame" geometry. This chapter is certainly one of the highlights of the book and should be of much interest to a mathematician working in real or algebraic geometry.

Set theory is well represented by two chapters (III and IV) on the seminal results of Gödel and Cohen concerning, respectively, the consistency and independence of the continuum hypothesis. The latter result is presented in terms of the Scott-Solovay Boolean-valued model approach within the framework of a second-order theory of real numbers. The more standard approach via forcing, within set theory, is also briefly explained. When the first edition of the book came out, set theory had already embarked on the grand project of relating determinacy assumptions with (very) large cardinals. Work of Martin, Woodin, Steel and others culminated in the late 80s with the result that large cardinal assumptions imply nice regularity properties for the projective sets of the real line, among which are counted Lebesgue measurability and the perfect set property. This is mathematical work of the first water by any standards. It also gives a nice closure to the despair of the Moscow mathematician Nikolai Luzin when he wrote in 1925 that "there is a family of effective sets (…) such that we do not know and *will never know* if any uncountable set of this family has the power of the continuum (…) nor even if it is measurable". A by-product of this work is a very interesting axiomatization of second-order arithmetic, which is the launch pad of a recent attempt of Hugh Woodin in tackling the old chestnut of the continuum hypothesis (yes, there are still some people thinking hard on these issues). In a very brief subsection, Manin discusses this attempt of Woodin. In spite of its briefness (slightly more than half a page), the subsection is informative and appropriate, as it follows a discussion on the philosophy of set theory and Gödel's programme for new axioms. However, Manin missed the opportunity to call attention to the above mentioned set-theoretic work of the 80s.

The remaining chapters, numbered V, VI and IX, concern computability and complexity. The first two cover the basic material of recursive function theory and include the solution to Hilbert's tenth problem using Pell's equation to produce an example of a function of exponential growth whose graph is Diophantine. Universal (Manin calls them "versal") families of partial recursive functions are constructed using the fact that every recursively enumerable set is Diophantine. Such families can also be constructed using universal Turing machines but these machines are given short shrift in the first edition. Manin's option was very fine but he somehow felt the need to give an explanation saying that "we again recall that we have not at all concerned ourselves with formalizing computational processes, but only with the results of such processes". Be that as it may, Manin wrote a new chapter (numbered IX) for the second edition where models of computations are discussed, including Turing machines and Boolean circuits. Complexity theory is mentioned and the theory of NP-completeness is developed up to Cook's theorem. There are also three sections devoted to quantum computation, including Shor's factoring and Grover's search algorithms. This is a felicitous choice of topics for the new volume and in tune with the spirit of the first edition. These topics make up the second half of chapter IX. The first half of the chapter is, in Manin's own words, "a tentative introduction to [the categorical] way of thinking, oriented primarily to some reshuffling of classical computability theory". These sections relate to the speculations on the geometry of recursion already present in the first edition. I cannot but feel that Manin is attempting to fit the untame field of computability theory into the Procrustean bed of category theory.

This is a stimulating and audacious book, with something for everyone: for the student, for the professional logician and, specifically, for its intended audience of the "working mathematician". The emphasis on the semantic aspects of logic is a good strategy for not alienating the intended reader. Even though the field of mathematical logic is admired for its results, nowadays it is somewhat isolated from the remainder of mathematics. This is in part due to the specific preoccupations of the field, which result in a perceived entrenchment. However, there are now unmistakable signs of profitable interplay with the wider mathematical community, as can be seen by Zilber's chapter on model theory. I also see Manin's book as fostering an appreciation of mathematical logic within the wider arena of mathematics, not only for the branches of logic that most easily relate to the other parts of mathematics but also for the more foundational aspects of the subject. After all, "foundations" does not seem to be a bad word in Manin's vocabulary. In the preface to the second edition, he advances the view that "mathematics [is] (…) leaving behind old concerns about infinities: a new view of foundations is now emerging" and adds that "much remains to be recognized and said about this [categorical] emerging trend in foundations of mathematics". Let a thousand flowers bloom.

The book has some typos, some of which were already present in the first edition. For instance, it is written twice in the appendix to Chapter II that a chain of the form x,y,z,… where x is an element of y, y an element of z, and so on, must terminate. Of course, it is the other way around. I also noticed a typo in the proof of lemma 11.3 of Chapter II that is absent from the old edition. On page 47, the state of the art concerning Fermat's last theorem is described, for 1977… Curiously, on page 182, a footnote puts the matter aright, citing the work of Wiles. The book would have benefited from a more careful editing.

Fernando Ferreira [ferferr@cii.fc.ul.pt] teaches at the University of Lisbon, Portugal. He received his PhD in mathematics in 1988. His main interests lie in mathematical logic and the foundations of mathematics. In the past few years, he has been working in proof theory, especially in functional interpretations emanating from Kurt Gödel's seminal work of 1958. He also has publications in analytic and ancient philosophy.

Personal column

Please send information on mathematical awards and deaths to Dmitry Feichtner-Kozlov (dfk@math.uni-bremen.de).

Awards

The **2010 Shaw Prize** in Mathematical Sciences has been awarded to **Jean Bourgain** (Institute for Advanced Study).

Laszlo Lovasz (Eötvös Loránd University, Budapest) was awarded the **Kyoto Prize 2010** in Basic Sciences for "Outstanding Contributions to Mathematical Sciences Based on Discrete Optimization Algorithms".

The **2010 Rollo Davisdon Trust Prize** was awarded to **Gady Kozma** (Weizmann Institute), and to **Sourav Chatterjee** (UC Berkeley).

Álvaro Pelayo (University of California at Berkeley, US) has received the **2009 Premio de Investigación José Luis Rubio de Francia.** The prize is awarded by the Real Sociedad Matemática Española to a Spanish mathematician under 32 years of age who has done outstanding research.

Charles M. Elliott (Mathematics Institute, University of Warwick) and **Ragnar-Olaf Buchweitz** (University of Toronto) have been elected as recipients of **Humboldt Research Awards**.

Matthias Kreck, director of the Hausdorff Research Institute for Mathematics in Bonn, has received the **2010 Cantor Medal**, awarded by the German Mathematical Society (DMV).

Carlo Mantegazza (Scuola Normale Superiore di Pisa, Italy) has received the **2010 Ferran Sunyer i Balaguer Prize** for his monograph *Lecture Notes on Mean Curvature Flow*.

Yurii Nesterov (Center for Operations Research and Econometrics, Université Catholique de Louvain) and **Yinyu Ye** (Stanford University) have received the **2009 John von Neumann Theory Prize.**

One of the **Heinz Maier-Leibnitz Prizes 2010** was awarded to **Hannah Markwig** (University of Göttingen) for her work in "tropical geometry".

A **Copley Medal** has been awarded to **David Cox**, formerly of Oxford University, for his seminal contributions to numerous areas of statistics and applied probability.

The **Sylvester Medal** of the Royal Society has been awarded to **Graeme Segal** (University of Oxford), for his work on the development of topology, geometry and quantum field theory.

Radha Charan Gupta of Ganita Bharati Institute and **Ivor Grattan-Guinness**, emeritus professor at Middlesex University, have been named the recipients of the **2009 Kenneth O. May Prize for the History of Mathematics** by the International Commission for the History of Mathematics.

Michael Francis Atiyah was awarded **La Grande Medaille de l'Académie des sciences** for his lifetime achievements in mathematical research.

Professor **Helge Holden** was appointed as chairman of the board of the **Abel foundation** for the period 2010–2014.

The following **IMU Prizes** and **Medals** were awarded at ICM 2010 in Hyderabad. **Elon Lindenstrauss** (Hebrew University and Princeton University), **Ngô Bao Châu** (Université Paris-Sud, Orsay), **Stanislav Smirnov** (Université de Geneve) and **Cédric Villani** (Institut Henri Poincare) received Fields Medals (recognising outstanding mathematical achievement). **Yves Meyer** (Ecole Normal Superieur de Cachan) received the **Carl Friedrich Gauss Prize** (for outstanding mathematical contributions with significant impact outside of mathematics).

María Luisa Rapún Banzo has been awarded the **Prize SeMA 2010** to young researchers, awarded by the Spanish Society for Applied Mathematics (SeMA).

Jacques Vannester (University of Edinburgh) was awarded the **2010 Adams Prize** by the University of Cambridge.

Bolesław Kacewicz of AGH University of Science and Technology, Kraków, Poland, has been awarded the **2010 Prize in Information-Based Complexity.**

Jacob Palis (IMPA) was awarded the **2010 Balzan Prize** for his fundamental contributions to the mathematical theory of dynamical systems.

The ICIAM (International Council for Industrial and Applied Mathematics) has awarded the following prizes: the **ICIAM Maxwell Prize** to **Vladimir Rokhlin** (New Haven, USA), the **ICIAM Collatz Prize** to **Emmanuel Candès** (Stanford & Pasadena, USA) and the **ICIAM Lagrange Prize** to **Alexandre J. Chorin** (Berkeley, USA).

Colin Macdonald (University of Oxford) was awarded the **Richard DiPrima Prize** for outstanding research in applied mathematics.

Martin Grotschel (Zuse-Zentrum) was awarded the **SIAM Prize** for Distinguished Service to the Profession.

Vladimir Manuilov (Moscow State University) and **Klaus Thomsen** (Aarhus University) were awarded the **G. de B. Robinson Prize** by the Canadian Mathematical Society.

Deaths

We regret to announce the deaths of:

Iain Adamson (UK, 9 June 2010)
Vladimir Arnold (Russia, 3 June 2010)
Eliseo Borrás Veses (Spain, 28 May 2010)
Jaume Casanovas (Spain, 14 July 2010)
Graham Everest (UK, 30 July 2010)
Florentino García Santos (Spain, 1 November 2010)
Martin Gardner (USA, 22 May 2010)
Jurgen Herzberger (Germany, 22 November 2009)
Peter Hilton (UK, 6 November 2010)
Anatoly Kilbas (Belarus, 28 June 2010)
Clive Kilmister (UK, 2 May 2010)
Paul Malliavin (France, 3 June 2010)
Benoit Mandelbrot (UK, 14 October 2010)
Jerrold Marsden (USA, 21 September 2010)
Friedrich Roesler (Germany, 22 April 2010)
Walter Rudin (USA, 20 May 2010)
Michelle Schatzmann (France, 20 August 2010)
Jaroslav Stark (UK, 6 June 2010)
Floris Takens (Netherlands, 20 June 2010)
Ursula Viet (Germany, 18 April 2010)

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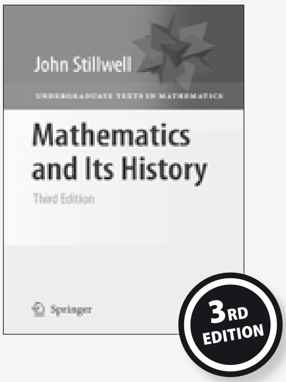

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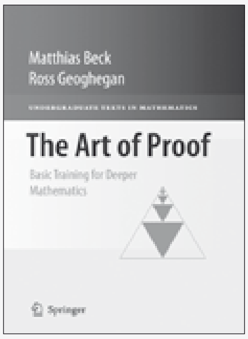

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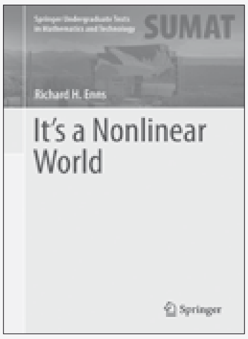

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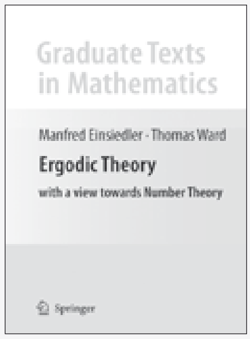

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