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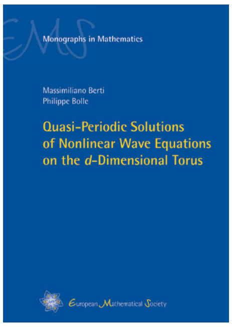
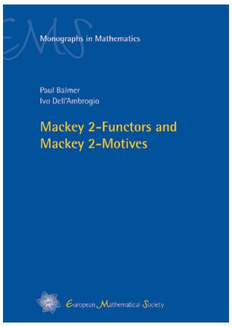


Massimiliano Berti (SISSA, Trieste, Italy) and Philippe Bolle (Avignon Université, France)
Quasi-Periodic Solutions of Nonlinear Wave Equations on the *d*-Dimensional Torus

978-3-03719-211-5. 2020. 374 pages. Hardcover. 16.5 x 23.5 cm. 69.00 Euro

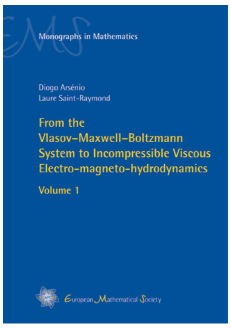
Many partial differential equations (PDEs) arising in physics, such as the nonlinear wave equation and the Schrödinger equation, can be viewed as infinite-dimensional Hamiltonian systems. In the last thirty years, several existence results of time quasi-periodic solutions have been proved adopting a "dynamical systems" point of view. Most of them deal with equations in one space dimension, whereas for multidimensional PDEs a satisfactory picture is still under construction.
An updated introduction to the now rich subject of KAM theory for PDEs is provided in the first part of this research monograph. We then focus on the nonlinear wave equation, endowed with periodic boundary conditions. The main result of the monograph proves the bifurcation of small amplitude finite-dimensional invariant tori for this equation, in any space dimension. This is a difficult small divisor problem due to complex resonance phenomena between the normal mode frequencies of oscillations. The proof requires various mathematical methods, ranging from Nash–Moser and KAM theory to reduction techniques in Hamiltonian dynamics and multiscale analysis for quasi-periodic linear operators, which are presented in a systematic and self-contained way. Some of the techniques introduced in this monograph have deep connections with those used in Anderson localization theory.
This book will be useful to researchers who are interested in small divisor problems, particularly in the setting of Hamiltonian PDEs, and who wish to get acquainted with recent developments in the field.



Paul Balmer (University of California, Los Angeles, USA) and Ivo Dell'Ambrogio (Université de Lille, France)
Mackey 2-Functors and Mackey 2-Motives (EMS Monographs in Mathematics)

ISBN 978-3-03719-209-2. 2020. 235 pages. Hardcover. 16.5 x 23.5 cm. 59.00 Euro

This book is dedicated to equivariant mathematics, specifically the study of additive categories of objects with actions of finite groups. The framework of Mackey 2-functors axiomatizes the variance of such categories as a function of the group. In other words, it provides a categorification of the widely used notion of Mackey functor, familiar to representation theorists and topologists.
The book contains an extended catalogue of examples of such Mackey 2-functors that are already in use in many mathematical fields from algebra to topology, from geometry to KK-theory. Among the first results of the theory, the ambidexterity theorem gives a way to construct further examples and the separable monadicity theorem explains how the value of a Mackey 2-functor at a subgroup can be carved out of the value at a larger group, by a construction that generalizes ordinary localization in the same way that the étale topology generalizes the Zariski topology. The second part of the book provides a motivic approach to Mackey 2-functors, 2-categorifying the well-known span construction of Dress and Lindner. This motivic theory culminates with the following application: The idempotents of Yoshida's crossed Burnside ring are the universal source of block decompositions.
The book is self-contained, with appendices providing extensive background and terminology. It is written for graduate students and more advanced researchers interested in category theory, representation theory and topology.



Diogo Arsénio (Université Paris Diderot, France) and Laure Saint-Raymond (École Normale Supérieure, Lyon, France)
From the Vlasov–Maxwell–Boltzmann System to Incompressible Viscous Electro-magneto-hydrodynamics. Volume 1

ISBN 978-3-03719-193-4. 2019. 418 pages. Hardcover. 16.5 x 23.5 cm. 78.00 Euro

The Vlasov–Maxwell–Boltzmann system is a microscopic model to describe the dynamics of charged particles subject to self-induced electromagnetic forces. At the macroscopic scale, in the incompressible viscous fluid limit the evolution of the plasma is governed by equations of Navier–Stokes–Fourier type, with some electromagnetic forcing that may take on various forms depending on the number of species and on the strength of the interactions.
From the mathematical point of view, these models have very different behaviors. Their analysis therefore requires various mathematical methods which this book aims to present in a systematic, painstaking, and exhaustive way.
The first part of this work is devoted to the systematic formal analysis of viscous hydrodynamic limits of the Vlasov–Maxwell–Boltzmann system, leading to a precise classification of physically relevant models for viscous incompressible plasmas, some of which have not previously been described in the literature.
In the second part, the convergence results are made precise and rigorous, assuming the existence of renormalized solutions for the Vlasov–Maxwell–Boltzmann system. The analysis is based essentially on the scaled entropy inequality. Important mathematical tools are introduced, with new developments used to prove these convergence results (Chapman–Enskog-type decomposition and regularity in the v variable, hypoelliptic transfer of compactness, analysis of high frequency time oscillations, and more).
The third and fourth parts (which will be published in a second volume) show how to adapt the arguments presented in the conditional case to deal with a weaker notion of solutions to the Vlasov–Maxwell–Boltzmann system, the existence of which is known.

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EMS Agenda

2021

16 January
EMS Executive Committee Meeting
(online)

EMS Scientific Events

2021

Due to the covid-19 pandemic and its consequences many conferences are rescheduled and the dates as yet uncertain. We refrain therefore from a detailed listing.

20–26 June
8th European Congress of Mathematics
Portorož, Slovenia

21–23 September
The Unity of Mathematics: A Conference in Honour of
Sir Michael Atiyah
Isaac Newton Institute for Mathematical Sciences

A Message from the President

Volker Mehrmann, President of the EMS

Dear members of the EMS,

It looks like it is going to take a lot longer to deal with the Covid 19 crisis than anticipated, and that the predictions of virologists (thanks to a lot of mathematics) about the second wave seem to have been very accurate. For this reason, we had to postpone the EMS 30th anniversary and also the presidents meeting in Edinburgh. The meeting of the combined past and new executive meetings took place virtually. The good news is that the EMS finances are in really good shape. The bad news is that our goal to spend more money on scientific projects could not be realised, since the in-person scientific activities have ground to a halt and moved to online platforms. On the one hand this is good, as it reduces the CO_2 emissions since we are travelling less, and for established scientists this can probably be compensated easily, however for the young generation of mathematicians this is becoming a real challenge, hampering their career perspectives. To compensate for this, groups of young researchers have started online research seminars, and EMS is endorsing such activities.

Another group that is suffering badly are the parents of young children (and here in particular women, who still carry out most of the work), who face severe difficulties in pursuing their academic careers. The mathematical community must find ways of addressing this in future evaluation or prize committees.

The improvement of the gender balance is also on the agenda of our discussions when it comes to editorial boards or prize committees, see the article in this issue. We all have to make major efforts in the coming years.

Another crisis (which many of you have noticed) has severely affected our website, which was down for several weeks due to virus attacks followed by complicated administrative and security issues. This has led to increased activities to move to the new website in the new design as soon as possible.

But there is also some positive news for the European mathematical community. From the 1st of January onwards, Zentralblatt will become open access thanks to a grant by the German government, and EMS Press will move the EMS owned journals to a subscribe-to-open model next year, and has also taken over the open Encyclopaedia of Mathematics from Springer.

Furthermore, the Klaus Tschira Foundation is supporting a large number of young scientists for participation in the 2022 ICM.

This is the last Newsletter in the old format. From January onwards it will be succeeded by the new EMS Magazine. Articles will be published in an "online first" fashion and compiled into quarterly issues that can be received in printed form by EMS members on request. We hope that this new format will be very attractive to old and young EMS members alike.

I sincerely hope that all of you remain healthy through the second wave of Covid 19, and that next year we can meet at the European Congress in Portoroz and celebrate the 30th anniversary.

Brief Words from the Editor-in-Chief

Fernando Pestana da Costa, Editor-in-Chief of the EMS Newsletter

As you have read in the Message from the President, this will be the final issue of the Newsletter. From 2021 onwards it will be renamed the EMS Magazine, and, among other changes, it will boast a completely new design and layout, moving the presently existing news section to the website and switching to an "online first" publishing format.

Thus, the present issue will be the last in a series. It will also be the final issue in which Dierk Schleicher and Jean-Paul Allouche participate as editors, both of whom will have completed their second four-year term as editors on the last day of 2020. I am grateful to them both for their highly valued collaboration and active involvement with the Newsletter over the past eight years.

Another collaborator who will cease her work with the Newsletter is EMS Press Head of Production Sylvia Fellmann Lotrovsky, who will be retiring from EMS Press in the spring and whose involvement with the Newsletter will cease with this last issue in its present form. Sylvia is a very knowledgeable and friendly person, and has always been available to help the editors in the myriad of things involved in producing the Newsletter. She will be missed.

This issue will, as usual, contain many interesting articles on a variety of topics. Without diminishing other contributions, let me just highlight the following ones: a timely study about women on editorial boards of scientific and EMS journals and the first four articles of what is intended to be a series presenting the work of the ten winners of the 2020 EMS Prize, as well as the 2020 Felix Klein and the 2020 Otto Neugebauer prizes. The award ceremony was planned to have taken place at the ECM in Portoroz this summer, but had to be postponed for a year due to the pandemic. This unfortunate but unavoidable decision had the fortunate consequence of allowing us time to invite each of the prize winners to write an article about their work. All have kindly agreed and the first four, by Jack Thorne, Kaisa Matomäki, Karim Adiprasito and Phan Thành Nam, appear in the present issue. The next two issues of the EMS Magazine will publish the remaining ones. Hopefully, when we all meet in Portoroz in the Summer of 2021 for the 8ECM, we will already have had the opportunity of reading the Magazine and learning about their work, and will be able to follow the prize winners' talks with enhanced pleasure.

Revival of the Encyclopedia of Mathematics

Ulf Rehmann (University of Bielefeld, Germany), Editor in Chief of EoM, and Maximilian Janisch (University of Zürich, Switzerland)

The Encyclopedia of Mathematics Wiki[1] (EoM) is, as most readers of this text probably already know, an open access resource designed specifically for the mathematics community. With more than 8000 entries, illuminating nearly 50,000 notions in mathematics, the Encyclopedia of Mathematics was the most up-to-date graduate-level reference work in the field of mathematics.[2,3]

From its start in 2011, the EoM had to cope with the problem that the mathematics formula code was only available through png images, based on a former CD edition from 2002, because the TeX code was lost by the former publishers of the EoM.

This problem concerned about 270,000 formulas, which, due to the missing TeX code, needed to be completely retyped whenever they were edited. Therefore, over the course of two decades, the EoM has become more and more out of date, as the loss of the TeX codes has made it difficult to update the 8000 articles of the EoM.

[1] https://encyclopediaofmath.org/wiki/Main_Page

[2] The EoM is based on a book version "Encyclopaedia of Mathematics", edited by Michiel Hazewinkel. Its last print edition, consisting of 13 volumes, was published in 2002.

[3] A statistical EoM example by Boris Tsirelson (https://encyclopediaofmath.org/wiki/User:Boris_Tsirelson#Some_statistics): Measurable space (50,000+ views); Standard Borel space (12,000+ views); Analytic Borel space (5,000+ views); Universally measurable (5,000+ views); Measure space (20,000+ views); Standard probability space (6,000+ views); Measure algebra (measure theory) (7,000+ views).

This problem was recently solved: There were three categories of formulas with missing TeX code:

1) During the last years, about 60% of all formulas had already been manually translated into TeX by worldwide volunteers cooperating with EoM.
2) For the majority of formulas, old markup typesets in an nroff-like style became available, however with no interpreter. Recently, an interpreter for these markup pages has been devised allowing to automatically translate, mostly error-free, the image-based code into TeX.
3) Finally, there were the remaining around 60,000 formulas, for which there were no markup and no manual translations.

Ulf Rehmann, professor at Bielefeld University and editor in chief of EoM, has organized the automatic translation for most pages as described in 2). Maximilian Janisch, student at the University of Zürich, has translated the formulas of type 3) into TeX semi-automatically (i.e. the formulas were translated with machine learning, but the translations were checked twice manually). Now, an almost completely TeXified-version of the EoM is available online.[1]

The Revival of the EoM: Long story short, the renewal of the EoM articles is now possible without tedious manual retyping of the formulas. It would be great if many mathematicians started using this chance in order to bring the EoM back up to date.

Some New Parallels Between Groups and Lie Algebras, or What Can Be Simpler than the Multiplication Table?

Boris Kunyavskiĭ (Bar-Ilan University, Ramat Gan, Israel)

Perhaps in the times of Ahmes
the multiplication table was exciting.
Bertrand Russell[1]

The Greek system of numerals was very bad, so that the multiplication table was quite difficult, and complicated calculations could only be made by very clever people.
Bertrand Russell[2]

We give a survey of recent developments in the study of equations in groups and Lie algebras and related local-global invariants, focusing on parallels between the two algebraic structures.

1 Foreword

Imagine the following situation. Your kid, during the first year in elementary school, asks you to explain the notion of prime numbers (having heard about them from super-nerd classmates). Division is not yet known, only the multiplication table has been taught. What can be done? Here is a possible solution.

- Show multiplication table (see Figure 3).
- Delete the first row and the first column (corresponding to multiplication by 1).
- Say that prime numbers are exactly those that do not appear in such a table (add "infinitely extended" if you feel that your Wunderkind is able to understand this).

One of the goals of the present paper is to consider, in some detail, a similar situation for algebraic systems other than natural numbers, with a focus on groups and Lie algebras, with an eye towards observing some new phenomena and parallels, and in the hope of making these multiplication tables as exciting as the usual one was in the time of Ahmes and as mind-challenging as it was for ancient Greeks (see the epigraphs).

We will also consider some related problems and arising parallels, most of which are still vague and/or hypothetical.

2 Prime elements in general algebras

Let us write down the childlike definition of prime numbers given in the foreword in a formal fashion:

Definition 2.1. Let $A = \mathbb{N} \setminus \{1\} = \{2, 3, 4, 5, \ldots\}$. Equip A with usual multiplication. Then $a \in A$ is prime if the equation $xy = a$ has no solutions $(x, y) \in A \times A$.

Figure 1. "Rhind Mathematical Papyrus", around 1550 BC (British Museum reference: EA10058 © The Trustees of the British Museum)

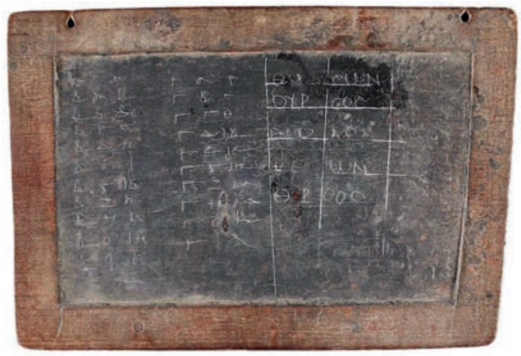

Figure 2. Greek Multiplication table on a wax tablet (British Museum AddMS34186. About 100AD. © The Trustees of the British Museum)

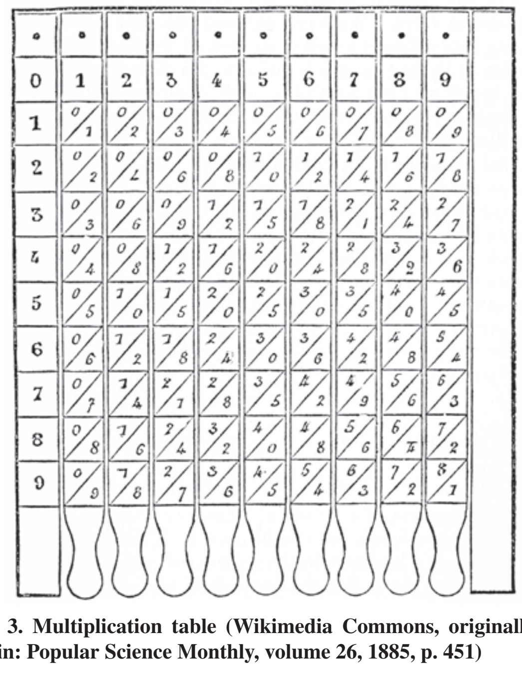

Figure 3. Multiplication table (Wikimedia Commons, originally published in: Popular Science Monthly, volume 26, 1885, p. 451)

1 *What I Believe*, Kegan Paul, Trench, Trübner & Co., London, 1925.
2 *An Outline of Intellectual Rubbish: A Hilarious Catalogue of Organized and Individual Stupidity*, Haldeman-Julius Publications, Girard, Kansas, 1943.

Let us now mimic it and extend it to arbitrary algebras.

Definition 2.2. Let A be an algebra equipped with a binary operation (the sign of which will be omitted). Then we say that $a \in A$ is prime if the equation $xy = a$ has no solutions $(x, y) \in A \times A$.

Example 2.3. (Group commutators) Let G be a group, and let A_G be the underlying set of G with operation $[x, y] := xyx^{-1}y^{-1}$. For brevity, let us call prime elements of the algebra A_G prime elements of G. These are elements of G not representable as a single commutator.

Denote by $[G, G]$ the subgroup of G generated by all commutators $[x, y]$, $x, y \in G$, and recall that G is said to be *perfect* if $[G, G] = G$.

If G is not perfect, it obviously contains prime elements: any $a \notin [G, G]$ is prime.

3 Wide groups

Definition 3.1. We say that a group G is *wide* if $[G, G]$ contains prime elements.

Historically, the first example of a wide group (of order 1024) was attributed to George Abram Miller (see [31]). In 1977, in his PhD thesis, Robert Guralnick proved that the smallest wide group is of order 96 (see [39]). Actually, there are two non-isomorphic wide groups of order 96. Nowadays, such statements can easily be verified by computer. The smallest wide *perfect* group is of order 960. Further examples and results can be found in a survey paper by Luise-Charlotte Kappe and Robert Morse [50] and in the Bourbaki 2013 talk delivered by Gunter Malle [62].

The next step, from perfect to *simple* groups, is far more tricky. The cases of finite and infinite groups should be considered separately.

In the case where G is finite, each element is a single commutator. This was conjectured by Øystein Ore in the 1950s [70]. The proof required lots of various techniques. Most groups of Lie type were treated by Erich Ellers and Nikolai Gordeev in the 1990s [26]. The proof was finished by Martin Liebeck, Eamonn O'Brien, Aner Shalev and Pham Huu Tiep in 2010 [60]. See Malle's Bourbaki talk [62] for details.

If G is infinite, the situation is entirely different.

There are several cases where each element of G is a single commutator: $G = A_\infty$ is the infinite alternating group (Ore [70]); $G = \mathcal{G}(k)$ is the group of k-points of a semisimple adjoint linear algebraic group $\mathcal{G}$ over an algebraically closed field k (Rimhak Ree [74]); and G is the automorphism group of some "nice" topological or combinatorial object (e.g., the Cantor set). Precise references and additional examples and generalisations can be found in our survey [48] (jointly with Alexey Kanel-Belov and Eugene Plotkin).

The first example of the opposite kind was discovered by Jean Barge and Étienne Ghys in 1992 [5]. Looking at the title of their paper, it is hard to suspect that it is about the multiplication table in groups. Indeed, the group they constructed is of differential-geometric origin. It is simple and wide (it contains elements not representable as a single commutator). Both statements are proved using highly nontrivial geometrical arguments. Later on, more examples of such a kind were constructed (Alexey Muranov [67], Pierre-Emmanuel Caprace and Koji Fujiwara [21], Elisabeth Fink and Andreas Thom [30]).

These groups are indeed very different from the "nice" groups discussed above. For any group G, one can introduce the following notions.

For any $a \in [G, G]$ define its length $\ell(a)$ as the smallest number k of commutators needed to represent it as a product $a = [x_1, y_1] \dots [x_k, y_k]$. Define the commutator width of G as $\mathrm{wd}(G) := \sup_{a \in [G,G]} \ell(a)$. It turns out that for a simple group G, the commutator width $\mathrm{wd}(G)$ may be as large as we wish, or even infinite (such examples appear in the papers by Barge–Ghys and Muranov).

4 First parallels: Wide Lie algebras

Now let L be a Lie algebra defined over a field k. As above, we say that L is *wide* if the derived algebra $[L, L]$ contains elements which are not representable as a single Lie bracket. As in the case of groups, wide Lie algebras naturally appear among finite-dimensional *nilpotent* Lie algebras (see, e.g., the MathOverflow discussion [63]). It is worth noting that the smallest dimension where such examples arise is 10, which is parallel to Miller's example of a wide group of order $1024 = 2^{10}$ mentioned above. This phenomenon is not surprising, in light of well-known relations between nilpotent groups and Lie algebras: they go back to the classical Baker–Campbell–Hausdorff formula and culminate in the correspondence of categorical flavour, due to Anatoly Mal'cev and Michel Lazard (see, e.g., the monograph of Evgenii Khukhro [51] for some details concerning this correspondence). With some effort one can also construct wide *perfect* Lie algebras (see [7]). We shall focus on the case of *simple* Lie algebras. Here are our main questions.

Question 4.1. Does there exist a wide *simple* Lie algebra?

More generally, as in the case of groups, one can define for every $a \in [L, L]$ its bracket length $\ell(a)$ as the smallest k such that a is representable as a sum $a = [x_1, y_1] + \dots + [x_k, y_k]$, and then define the bracket width of L as $\mathrm{wd}(L) := \sup_{a \in [L,L]} \ell(a)$.

If Question 4.1 is answered in the affirmative, one can ask the next question:

Question 4.2. Does there exist a simple Lie algebra L of *infinite* bracket width?

Where does one look for counter-examples? Below L is a *simple* Lie algebra over a field k.

First suppose that L is *finite-dimensional*. In the following cases it is known that every element is a single bracket (i.e., $\mathrm{wd}(L) = 1$): L is split and k is sufficiently large (Gordon Brown [14]; Ralf Hirschbühl [41] improved estimates on the size of k); $k = \mathbb{R}$, L is compact (there are many different proofs, attributed to Dragan Đoković and Tin-Yau Tam [25], Karl-Hermann Neeb (see [42]), Dmitri Akhiezer [1], Alessandro D'Andrea and Andrea Maffei [22], Joseph Malkoun and Nazih Nahlus [61]); some non-compact algebras L over $\mathbb{R}$ (Akhiezer, op. cit.).

The most interesting unexplored class in finite-dimensional case is the family of algebras of Cartan type over a field of positive characteristic. As a working hypothesis, one can suspect that none of these algebras are wide.

Remark 4.3. If L is finite-dimensional over any infinite field of characteristic different from 2 and 3, its bracket width is at most two (George Bergman and Nazil Nahlus [7]).

Suppose now that L is *infinite-dimensional*. There are several natural families of simple infinite-dimensional Lie algebras. Here are some of them: four families W_n, H_n, S_n, K_n of algebras of Cartan type; (subquotients of) Kac–Moody algebras; algebras of vector fields on smooth affine varieties.

As to the first case, a theorem by Alexei Rudakov [77] shows that none of the algebras L of Cartan type are wide (we owe this observation to Zhihua Chang). I am unaware of any approach to the second family, though there are simple Kac–Moody groups of infinite width (see the paper by Caprace and Fujiwara [21] mentioned above).

Question 4.4. Are there wide simple Kac–Moody algebras?

Even the case of affine Kac–Moody algebras seems open.

However, the third family, which moves us back to the origin of the area, turned out to be more promising. Actually, algebras of vector fields appeared in the work of the founders of the theory, Sophus Lie and Élie Cartan.

In our work in progress [57] (joint with Andriy Regeta) we established the following fact.

Theorem 4.5. *Among Lie algebras of vector fields on smooth affine varieties there are wide algebras.*

Some details are in order.

Let k be an algebraically closed field of characteristic zero. Let $X \subset \mathbb{A}^n_k$ be an irreducible affine k-variety. Let $\mathrm{Vect}(X)$ denote the collection of (polynomial) vector fields on X, i.e., $\mathrm{Vect}(X) = \mathrm{Der}(\mathcal{O}(X))$, the set of derivations of the algebra of regular functions on X. It carries a natural structure of Lie algebra, as a Lie subalgebra of the algebra of endomorphisms $\mathrm{End}_k(\mathcal{O}(X))$:

$$[\xi, \eta] := \xi \circ \eta - \eta \circ \xi.$$

There are strong relations between properties of X and $\mathrm{Vect}(X)$. We only mention a couple of the most important facts: two normal affine varieties are isomorphic if and only if $\mathrm{Vect}(X)$ and $\mathrm{Vect}(Y)$ are isomorphic as Lie algebras (Janusz Grabowski [37] for smooth varieties, Thomas Siebert [87] in general); X is smooth if and only if $\mathrm{Vect}(X)$ is simple (David Alan Jordan [46], Siebert, op. cit.; see also the lecture notes by Hanspeter Kraft [53] and a new proof by Yuli Billig and Vyacheslav Futorny [8]).

Example 4.6. Let $X = \mathbb{A}^n$. Then $L = \mathrm{Vect}(\mathbb{A}^n)$ is a free $\mathcal{O}(\mathbb{A}^n) = k[x_1, \ldots, x_n]$-module of rank n generated by $\partial_{x_i} = \frac{\partial}{\partial x_i}$, $i = 1, \ldots, n$. It is an easy exercise to show that every element of L can be represented as a single Lie bracket. We leave the proof to the reader.

The situation is not as simple for more general affine varieties, even for curves. The following example, by Billig and Futorny [8], shows the essence of the problem.

Example 4.7 ([8]). Let $H = \{y^2 = 2h(x)\}$ where $h(x)$ is a separable monic polynomial of odd degree $2m + 1 \geq 3$, $A = \mathcal{O}(H) = k[x, y]/\langle y^2 - 2h(x)\rangle$, $D = \mathrm{Vect}(H) = \mathrm{Der}_k(A)$. Then D is a free A-module of rank 1 generated by $\tau = y\partial_x + h'(x)\partial_y$. The algebra D contains neither semisimple nor nilpotent elements. (We say that $\eta \in D$ is semisimple if $\mathrm{ad}(\eta)$ has an eigenvector.)

Theorem 4.8 ([57]). *The Lie algebra D is wide.*

Idea of proof. One can introduce a filtration on D so that the smallest nonzero degree is $2m - 1$. Then any $\eta \in D$ with $\deg \eta = 2m-1$ is not representable as a single Lie bracket. □

Here is another example for the case of surfaces.

Example 4.9. Let $S = \{xy = p(z)\} \subset \mathbb{A}^3_k$ where $p(z)$ is a separable polynomial, $\deg p \geq 3$ (Danielewski surface). Let $L = LND(S)$ be the subalgebra of $\mathrm{Vect}(S)$ generated by all locally nilpotent vector fields.

Lemma 4.10 (Matthias Leuenberger and Andriy Regeta [59]). *L is a simple Lie algebra.*

Assuming that the surface satisfies a certain additional condition on the Jacobian of regular functions, we can prove the following fact.

Theorem 4.11 ([57]). *L is a wide Lie algebra.*

The proof is based on the same paper by Leuenberger and Regeta [59] and uses degree arguments.

Question 4.12. What is the bracket width of the algebras $\mathrm{Vect}(H)$ and $LND(S)$?

Here are some further questions.

- What geometric properties of X are responsible for the fact that the Lie algebra $\mathrm{Vect}(X)$ is wide?
- Does there exist a Lie-algebraic counterpart of the Barge–Ghys example? This requires going over to the category of smooth vector fields on smooth manifolds.
- Where should one look for further examples of wide simple Lie algebras?

There are two candidates, both suggested by Yuli Billig.

Let K_2 denote the Lie algebra obtained from the matrix

$$\begin{pmatrix} 2 & 2 \\ 2 & 2 \end{pmatrix}$$

in the same way as Kac–Moody Lie algebras are obtained from generalised Cartan matrices, see the seminal paper of Victor Kac [47]. Is K_2 wide?

Further eventual examples could be found among the most natural generalisation of examples of Theorem 4.8, in the class of algebras of Krichever–Novikov type (see, e.g., the monograph by Martin Schlichenmaier [80]). One can ask whether there are wide simple algebras of Krichever–Novikov type. If yes, can the width be arbitrarily large? Can it be infinite?

One can ask a 'metamathematical' question.

Question 4.13. Let L be a 'generic' ('random', 'typical') simple Lie algebra. Is L wide?

Of course, any eventual answer will heavily depend on what is meant by 'random', 'typical', etc. However, the absence of semisimple and nilpotent elements in the Lie algebra $\mathrm{Vect}(H)$ mentioned above is a witness of the absence of any analogue of the triangular decomposition. This is in sharp contrast with the situation for Kac–Moody algebras and gives some evidence for the following ('metamathematical') working hypothesis.

Less structured ('amorphous') Lie algebras tend to be wide.

Informally, these opposite hypotheses can be illustrated by the difference between the skeletons of fish and jellyfish (yes, jellyfish do have skeletons).

5 Word equations in groups and polynomial equations in Lie algebras

Let us now present a wider perspective on the notions discussed in the previous section. Namely, suppose we are given a group G with operation $[g, h] = ghg^{-1}h^{-1}$ (resp. a Lie algebra L with bracket $[,]$). Trying to find a given element a of G (resp. of L) in its multiplication table, we search for a solution of the equation

$$[x, y] = a \tag{1}$$

in $G \times G$ (resp. in $L \times L$), i.e., the right-hand side is fixed and x, y are unknowns.

One can generalise equation (1) as follows.

Let $w(x, y)$ denote a group word in x, y (more formally, an element of the free group $\mathcal{F}_2 = \mathcal{F}(x, y)$). One may think of something like $x^2y^{2020}x^{-1}y^{-3}$. Even more generally, for any integer $d \geq 1$ one can consider $w(x_1, \ldots, x_d) \in \mathcal{F}_d = \mathcal{F}(x_1, \ldots, x_d)$ and for every group G and $a \in G$ look for solutions of the word equation

$$w(x_1, \ldots, x_d) = a. \tag{2}$$

In a similar way, one can consider a Lie polynomial $P(X_1, \ldots, X_d)$ (an element of the free Lie algebra $\mathcal{L}_d = \mathcal{L}(X_1, \ldots, X_d)$) and for every Lie algebra L and $A \in L$ look for solutions of the equation

$$P(X_1, \ldots, X_d) = A. \tag{3}$$

Remark 5.1. Note that our set-up only includes equations with *constant-free* left-hand side. This means that if, say, A, B are fixed elements of a Lie algebra L, we consider equations $[X, Y] = A$ but *not* $[B, X] = A$. As to equations with constants, see our joint papers with Nikolai Gordeev and Eugene Plotkin [32]–[34], the paper by Anton Klyachko and Andreas Thom [52], and the references therein. Also, to avoid any confusion, we want to emphasise that in our set-up, solutions of (2) are sought *in* G, and *not in an overgroup* of G.

Here are some natural questions one can ask about equation (2) (of course, similar questions arise for equation (3)).

Question 5.2. Let a group G be given. Is equation (2) solvable

(a) for all $a \in G$, or, at least,
(b) for a 'typical' $a \in G$?

Part (a) leads to a natural generalisation of the notion of commutator width of G discussed in Section 3 (the so-called w-width). Various situations where one can guarantee that part (a) is answered in the affirmative are described in some detail in the monograph by Dan Segal [81] and in several survey papers; apart from [48] mentioned above, see also the papers by Aner Shalev [82, 83], and our more recent papers [2] (jointly with Tatiana Bandman and Shelly Garion) and [35] (jointly with Nikolai Gordeev and Eugene Plotkin).

As to part (b), any change in a precise definition of 'typical' may be critically important for an eventual answer. Say if G can be equipped with different topologies and 'typical' translates as 'belonging to a dense set in the chosen topology', the answer to (b) heavily depends on this choice. Here is an archetypical example where this dependence is the most striking: G is (the group of rational points of) a linear algebraic group defined over a field k. For this class of groups, Armand Borel established a general result. To formulate it, it is convenient to introduce the following notion.

Definition 5.3. Let d be a positive integer, and let $w = w(x_1, \ldots, x_d) \in \mathcal{F}_d$ be a word. For a group G define a map

$$w \colon G^d \to G \tag{4}$$

by evaluation: $(g_1, \ldots, g_d) \mapsto w(g_1, \ldots, g_d)$.

Such maps will be called *word maps*. If G is non-abelian, w is not a group homomorphism. In some special cases, one can say more about these maps. Let $\mathcal{G}$ be a linear algebraic group defined over a field k. Then, given a word $w \in \mathcal{F}_d$, one can define a *morphism* of the underlying algebraic k-varieties $w \colon \mathcal{G}^d \to \mathcal{G}$ which induces the word map (4) on the group $G = \mathcal{G}(k)$ of k-points of $\mathcal{G}$ (and, more generally, on K-points $\mathcal{G}(K)$ for any field extension K/k). We denote all these maps by the same letter w with the hope that this does not cause any confusion.

Theorem 5.4 (Borel [11])**.** *Let $\mathcal{G}$ be a connected semisimple linear algebraic group defined over a field k. Then for any non-identity word $w \in \mathcal{F}_d$, the morphism $w \colon \mathcal{G}^d \to \mathcal{G}$ is dominant.*

Recall that this means that the image of w contains a Zariski-dense open set (or, informally, that equation (2) with a 'typical' right-hand side is solvable).

Remark 5.5. It is worth emphasising the role of Zariski topology in this statement. One should not think that the assertion remains true in any topology. Andreas Thom [89] noticed that for the special unitary group $\mathcal{G} = SU_n$, word maps on the compact group $G = \mathcal{G}(\mathbb{R})$, equipped with the Euclidean topology (some people, especially those who are far removed from algebraic geometry, call it 'natural'), may behave quite differently. Namely, given $\epsilon > 0$, one can find a word $w \in \mathcal{F}_2$ such that the image of the word map (4) is contained in the open disk of radius ϵ centred at the identity matrix. (There is no contradiction with Borel's theorem because such a disk is Zariski-dense.)

Remark 5.6. One cannot expect to extend Borel's theorem too far beyond the class of semisimple groups, see [35] for some argumentation. Perhaps the only general hope is to treat *perfect* algebraic groups. Under certain additional assumptions, the dominance statement has been established in [34], and one has no examples of perfect algebraic groups G and words w for which the word morphism is not dominant.

6 Towards infinitesimal analogues

One can consider infinitesimal analogues of the problems discussed in the previous section. Namely, for a Lie polynomial

$P(X_1, \dots, X_d)$ one can ask the following question, similar to Question 5.2:

Question 6.1. Let a Lie algebra L be given. Is equation (3) solvable
(a) for all $A \in L$, or, at least,
(b) for a 'typical' $A \in L$?

As in the group case, it is convenient to introduce the corresponding evaluation map:

$$P\colon L^d \to L, \quad (a_1, \dots, a_d) \mapsto P(a_1, \dots, a_d). \qquad (5)$$

In these terms, Question 6.1 can be rephrased as the question about the surjectivity or dominance of the map (5).

In view of Remark 5.6, it is reasonable to focus on *simple* Lie algebras.

Let us take an informal look at the known ways to go over from groups to Lie algebras (or in the opposite direction).

Let us briefly recall several classical approaches to such a transition, without pretending to give a comprehensive overview.

First bridges between groups and Lie algebras had been built even before these notions were defined in a formal way. One can mention Poisson and Jacobi, whose pioneering works on Hamiltonian mechanics paved a road towards what is nowadays called Poisson geometry, Poisson–Lie groups, etc. (see a nice survey by Alan Weinstein [91] for details).

One can also mention Arthur Cayley, whose ingenious formula, allowing one to pass from special orthogonal to skew-symmetric matrices, was the first instance of what is now called an equivariant birational isomorphism between an algebraic group and its Lie algebra; see the books by Hermann Weyl [92] and Mikhail Postnikov [72] for a detailed discussion and further development of this idea. Note that like the work of Poisson–Jacobi, Cayley's invention served as a tool in theoretical mechanics (this time the Lagrangian variant). Note also that the limits up to which the Cayley transform can be generalised have recently been established; see the paper by Nicole Lemire, Vladimir Popov and Zinovy Reichstein [58] where this problem was posed and settled in the case of algebraically closed ground field and the subsequent papers [12, 13] for the treatment of the general case.

However, neither these 'prehistoric' methods, nor the exponential map introduced by Lie and Cartan, nor the more recent approach by Mal'cev and Lazard mentioned above can help with our problem of finding infinitesimal analogues of Borel's theorem. The main point is that there is no obvious way to arrange the transfer to move group commutator to Lie bracket.

Moreover, it turns out that a straightforward transfer of the dominance statement to the case of a semisimple Lie algebra $\mathfrak{g}$ cannot hold because there are Lie polynomials identically zero on $\mathfrak{g}$. Here is a counter-optimistic example: for $\mathfrak{g} = \mathfrak{sl}(2, k)$ (k is a field of characteristic zero) and $P(X, Y, Z) = [[[[[Z, Y], Y], X], Y], [[[[Z, Y], X], Y], Y]]$, Yuri Razmyslov [73] showed that $P(X, Y, Z) \equiv 0$ on $\mathfrak{g}$.

Of course, it was known well before Borel's theorem that such a phenomenon cannot occur for word maps on semisimple algebraic groups: in characteristic zero this follows from the famous alternative established by Jacques Tits [90], in general from an even earlier paper by Vladimir Platonov [71].

This observation might lead the reader to the conclusion that a road from Borel's theorem towards its eventual infinitesimal analogue cannot look like a freeway paved with classical works in Lie theory, but is rather similar to a rocky mountain road. However, the following theorem proved in our joint paper [3] with Bandman, Gordeev and Plotkin may suggest a more sober viewpoint, where the phenomena similar to Razmyslov's example are considered as sort of potholes to be circumvented.

Theorem 6.2. *Let $\mathfrak{g}(\mathsf{R}, k)$ be a Chevalley algebra. If $\mathrm{char}(k) = 2$, assume that R does not contain irreducible components of type C_r, $r \geq 1$ (here $\mathsf{C}_1 = \mathsf{A}_1, \mathsf{C}_2 = \mathsf{B}_2$).*

Suppose $P(X_1, \dots, X_d)$ is not an identity of the Lie algebra $\mathfrak{sl}(2, k)$. Then the induced map $P\colon \mathfrak{g}(\mathsf{R}, k)^d \to \mathfrak{g}(\mathsf{R}, k)$ is dominant.

Here R stands for a root system, and $\mathfrak{g}(\mathsf{R}, k)$ denotes the Lie algebra over k obtained from the corresponding complex semisimple Lie algebra $\mathfrak{g}(\mathsf{R}, \mathbb{C})$ using its Chevalley basis. In fact, it is the notion of Chevalley basis that allows one to streamline the road from algebraic groups to Lie algebras (in the case they are semisimple). More specifically, one of the crucial tools here is the so-called adjoint quotient, which was also invented by Claude Chevalley and further developed by Tonny Springer and Robert Steinberg [88].

Remark 6.3. We do not know whether the assumption on the polynomial P in Theorem 6.2 can be removed.

Remark 6.4. Theorem 6.2 can be used to reduce, for any given Lie polynomial, the problem of the dominance of the corresponding evaluation map on simple Lie algebras to the case of algebras of types A_2 and B_2, see [3].

One can try to pursue the obtained parallel regarding Zariski dominance to the case of Euclidean topology, with an aim to check whether some counterpart of Thom's phenomenon (see Remark 5.5) can occur.

Question 6.5. Do there exist a Lie polynomial P and a compact simple real Lie algebra $\mathfrak{g}$ such that the image of map (5) is not dense in Euclidean topology?

Remark 6.6. There is much less hope of establishing any relationship between the affirmative answers to Questions 5.2(a) and 6.1(a), in other words, between the surjectivity of the word map w on a semisimple group G (defined with the help of group commutators) and the corresponding polynomial map on the Lie algebras $\mathfrak{g} = \mathrm{Lie}(G)$ induced by the Lie polynomial obtained from w by replacing group commutators with Lie brackets. An explicit example can be found in the paper by Tatiana Bandman and Yuri Zarhin [4].

In light of these remarks, I regret to say that as of now, here an eventual route towards infinitesimal analogues rather looks like a mountain road.

7 Local-global invariants of groups and Lie algebras

In the last part of this survey, we consider two well-known objects of cohomological nature related to a given group G.

They proved useful in many important problems, some of which are beyond group theory (see my survey [56] and our joint paper with Kang [49] for details). It turned out recently that they naturally appear as parts of certain cohomology of Hopf algebras (called lazy cohomology, see the paper by Pierre Guillot and Christian Kassel [38], or invariant cohomology, see the paper by Pavel Etingof and Shlomo Gelaki [27]). An even more surprising relation, where the two invariants fit together nicely within another invariant with the origin in mathematical physics (the so-called group of braided tensor autoequivalences of the Drinfeld centre of G), was discovered by Alexei Davydov [23, 24].

We are not going to discuss these spectacular achievements here. Our interest is rather in very recent Lie-algebraic analogues of these group-theoretic objects and eventual parallels. (This is an ongoing joint project with Vadim Ostapenko.)

Bogomolov multiplier

Recall that the Schur multiplier of a group G is defined as the second cohomology group $H^2(G, \mathbb{Q}/\mathbb{Z})$ (where the action of G on $\mathbb{Q}/\mathbb{Z}$ is trivial).

The Bogomolov multiplier $B(G)$ of a finite group G is defined by

$$B(G) := \ker[H^2(G, \mathbb{Q}/\mathbb{Z}) \to \prod_{A<G \text{ abelian}} H^2(A, \mathbb{Q}/\mathbb{Z})];$$

Fedor Bogomolov showed [9] that in this formula one can replace ‘abelian’ by ‘bicyclic’.

The group $B(G)$ appeared in some algebraic-geometric context: it coincides with the unramified Brauer group of the quotient V/G of a faithful action of G on a complex vector space V, and thus is a birational invariant of this variety; this allowed David Saltman [79] to produce first counter-examples to Emmy Noether’s problem on the rationality of fields of invariants of permutation groups.

One can also note a recent unexpected application of the Bogomolov multiplier outside group theory: after extending the definition to profinite groups, $B(G)$ can be interpreted in the context of the noncommutative Iwasawa theory (as the kernel of the map $K_1(\mathbb{Z}_p[[G]]) \to K_1(\mathbb{Q}_p[[G]])$, see the paper by Urban Jezernik and Jonatan Sánchez [45]).

One should also mention some recently discovered relations of the Bogomolov multiplier to more conventional group-theoretic problems, in the spirit of Section 3 of the present paper. Namely, in the same paper [45] Jezernik and Sánchez showed that if one fixes an odd prime p and considers the asymptotic behaviour of all finite p-groups, the (logarithmically scaled) proportion of wide groups tends to 1; the same is true of the proportion of groups with nonzero Bogomolov multiplier. In another paper by Jezernik [44] (joint with Primož Moravec), one can find an interesting link between $B(G)$ and the classical notion of commuting probabilty of G, going back to Paul Erdős and Paul Turán.

Let L now be a (finite-dimensional) Lie algebra over a field k. Assume for simplicity that k is of characteristic zero. To define an analogue $B(L)$ of the Bogomolov multiplier, one can use another interpretation of $B(G)$ due to Moravec [65]. It is based on the notion of nonabelian exterior square $G \wedge G$. The Schur multiplier is dual to the kernel $M(G)$ of the natural map $G \wedge G \to [G, G]$, $g \wedge h \mapsto [g, h]$, and $B(G)$ is dual to the quotient $M(G)/M_0(G)$, where $M_0(G)$ is generated by $g \wedge h$ with commuting g, h.

It turns out that this construction can be transferred to Lie algebras; details can be found in recent papers by Zeinab Araghi Rostami, Mohsen Parvizi and Peyman Niroomand [75, 76].

A primary goal is to transfer to $B(L)$ as many known properties of $B(G)$ as possible. Whereas the Bogomolov multiplier vanishes on finite simple groups [55] and simple Lie algebras (follows from the results of Peggy Batten’s PhD thesis [6] where the vanishing of the Schur multiplier of simple Lie algebras is proven), on the other extreme edge (p-groups/nilpotent Lie algebras) there are nontrivial examples. For instance, in the paper [29] Gustavo Fernández-Alcober and Urban Jezernik showed that the Bogomolov multiplier of a p-group can be as large as we wish. In the papers [75, 76] mentioned above, one can find examples of finite-dimensional nilpotent Lie algebras with nontrivial Bogomolov multiplier. However, the following question is still open.

Question 7.1. Can the dimension of $B(L)$ be as large as possible?

Further, recall the notion of isoclinism introduced by Philip Hall in the group-theoretic context [40]. Groups G and H are called isoclinic if there are isomorphisms from $G/Z(G)$ to $H/Z(H)$ and from $G/[G, G]$ to $H/[H, H]$ commuting with the commutator map. This notion proved useful in lots of problems, particularly in the classification of finite p-groups, and was later on extended to associative rings (see the monograph [54] by Robert Kruse and David Price), to Lie algebras by Kay Moneyhun [64], and more recently, with certain modifications, to more general algebraic structures by Stephen Buckley [15].

Isoclinic objects often share many important properties. In this connection, in our joint paper with Akinari Hoshi and Ming-chang Kang [43] it was conjectured that isoclinic finite groups have the same Bogomolov multiplier. This was proved by Moravec [65] and later strengthened by Fedor Bogomolov and Christian Böhning [10] by showing that if G and H are isoclinic, the orbit varieties V/G and W/H with respect to their faithful actions are stably birationally equivalent.

This leads to the following question.

Question 7.2. Is $B(L)$ invariant under isoclinism of Lie algebras?

In a more conceptual mood, one could try to revisit the results of [45] where it is shown that ‘generically’ (in some appropriate sense a hint to which was given above), $B(G)$ is nontrivial. Can one translate this statement into the language of Lie algebras?

Shafarevich–Tate groups and algebras

Let a group G act on itself by conjugation. The Shafarevich–Tate set is defined with the help of (nonabelian) group cohomology corresponding to this action, by the formula

$$Ш(G) := \ker[H^1(G, G) \to \prod_{C<G \text{ cyclic}} H^1(C, G)]. \qquad (6)$$

The definition and the name were introduced by Takashi Ono [68, 69]. The local-global flavour justifies the allusion to the object bearing the same name which appeared in the arithmetic-geometric context (related to the action of the absolute Galois group of a number field K on the group $A(\overline{K})$ of $\overline{K}$-points of an abelian K-variety A). The usage of the Cyrillic letter Ш ("Sha") in this notation was initiated by John Cassels because of its appearance as the first letter in the surname of Igor Shafarevich.

Formula (6) admits a more down-to-earth interpretation, attributed in [69] to Marcin Mazur: $Ш(G)$ can be identified with the collection of all locally inner (=pointwise inner=class preserving) endomorphisms, i.e., $f \in \mathrm{End}(G)$ with the property $f(g) = a^{-1}ga$ (where a depends on g). Note that any class preserving endomorphism is injective. Hence, if G is finite, it is surjective, and we arrive at the object introduced by William Burnside more than 100 years ago:

$$Ш(G) \cong \mathrm{Aut}_c(G)/\mathrm{Inn}(G),$$

where $\mathrm{Aut}_c(G)$ stands for the group of class-preserving automorphisms of G. In particular, this means that if G is finite, $Ш(G)$ is a group, not just a pointed set. (Ono [69] extended this to the case where G is profinite.)

In my survey [56] (see also the survey by Manoi Yadav [94]), one can find many classes of groups G with trivial $Ш(G)$ (they are called Ш-rigid there), as well as some interesting examples with nontrivial $Ш(G)$ (they often give rise to counter-examples to some difficult problems, such as Higman's problem on isomorphism of integral group rings).

Let us now go towards a Lie-algebraic analogue of the Shafarevich–Tate group, taking formula (6) as a starting point. Given a Lie algebra L over a field k (for simplicity assumed to be of characteristic zero), consider the Chevalley–Eilenberg first (adjoint) cohomology $H^1(L, L)$. It is well known that it can be identified with the outer derivations $\mathrm{Out}(L) := \mathrm{Der}(L)/\,\mathrm{ad}(L)$. Recall that $\mathrm{ad}(L)$ is the collection of all inner derivations of L defined by the formula $\mathrm{ad}_Z(X) = [Z, X]$. Viewing $\mathrm{Der}(L)$ as a Lie algebra and noticing that $\mathrm{ad}(L)$ is its Lie ideal, we obtain a Lie algebra structure on $\mathrm{Out}(L)$.

Further, define 'locally inner' derivations by

$$\mathrm{Der}_c(L) := \{D \in \mathrm{Der}(L) \,|\, (\forall X \in L)\, (\exists Z \in L)\, D(X) = [Z, X]\} \tag{7}$$

(here Z depends on X).

This notion was introduced by Carolyn Gordon and Edward Wilson [36] (under the name of 'almost inner' derivations) in the differential-geometric context, allowing them to produce a continuous family of isospectral non-isometric compact Riemann manifolds. Recently, the interest in these Lie-algebraic structures was revived in the series of papers by Farshid Saeedi and his collaborators [84–86], and also in the series of papers by Dietrich Burde, Karel Dekimple and Bert Verbecke [16–18].

First, one can note that $\mathrm{Der}_c(L)$ is a Lie subalgebra of $\mathrm{Der}(L)$ [16, Proof of Proposition 2.3], and $\mathrm{ad}(L)$ is a Lie ideal of $\mathrm{Der}_c(L)$.

Definition 7.3. Set $Ш(L) := \mathrm{Der}_c(L)/\,\mathrm{ad}(L)$ and call it the Shafarevich–Tate algebra of L.

By analogy with the group case, we introduce the following notion (cf. [56]).

Definition 7.4. If $Ш(L) = 0$, we say that L is Ш-rigid.

Here are some parallels between the Ш-rigidity of groups and Lie algebras. If not stated otherwise, the group-theoretic facts mentioned below are taken from the survey [56], where the reader can find the references to the original works, and the Lie-algebraic statements are borrowed from [17].

1. Any simple finite-dimensional Lie algebra L over a field k of characteristic zero is Ш-rigid because L has no outer derivations at all (Whitehead's first lemma, the proof does not use the classification). There are many classes of algebras with no outer derivations, in particular, many complete algebras (with trivial centre and no outer derivations) are known. In the parallel universe of finite groups, it is known that all finite simple groups are Ш-rigid (Walter Feit and Gary Seitz [28], the proof heavily relies on the classification). Note that the situation is different for infinite groups (and may be different for finite-dimensional Lie algebras over fields of positive characteristic or infinite-dimensional Lie algebras).
2. The following groups (Lie algebras over a field of characteristic zero) are Ш-rigid: free nilpotent; abelian-by-cyclic groups (resp. finite-dimensional Lie algebras with codimension one abelian ideal); extraspecial groups of order p^{2n+1} (resp. Heisenberg Lie algebras H_{2n+1} ([16, Example 2.6] for $n = 1$); p-groups of order at most p^4 (resp. nilpotent Lie algebras of dimension at most 4). Note that the list of known classes of Ш-rigid Lie algebras is far shorter than the list of known classes of Ш-rigid groups, and one can continue producing more Ш-rigid Lie algebras, taking Ш-rigid groups as a source for inspiration.
3. On the other hand, many examples of algebras with nonzero $Ш(L)$ can be found among nilpotent and, more generally, solvable algebras [16,36,86]. The dimension of $Ш(L)$ can be arbitrarily large [36]. In the nilpotent case, a more direct relation between $Ш(G)$ and $Ш(L)$ could apparently be formulated, in the spirit of [36].

Here are some open problems.

Question 7.5. Is the Lie algebra $Ш(L)$ solvable?

Note that for any finite group G it is conjectured that the group $Ш(G)$ is solvable. The proof of this statement in the paper by Chih-han Sah [78] contains a gap. This was noticed by Masafumi Murai [66] who showed that its validity depends on the Alperin–McKay conjecture.

Question 7.6. Does there exist L such that $Ш(L)$ is nonabelian?

Note that Sah [78] disproved Burnside's statement [20] and exhibited examples of p-groups G with nonabelian $Ш(G)$, the smallest among them is a group of order 2^{15}.

Further, since $Ш(G)$ is known to be an isoclinic invariant according to Yadav [93], it is natural to pose a question in the spirit of Question 7.2.

Question 7.7. Is $Ш(L)$ invariant under isoclinism of Lie algebras?

Remark 7.8. To conclude, I would like to mention eventual analogues of $Ш(G)$ and $Ш(L)$ that one can introduce for other classes of algebras. First of all, this is the class of associative algebras where, given such an algebra A, one can define the multiplicative Ш consisting of outer 'locally inner' automorphisms of A as well as the additive Ш consisting of outer 'locally inner' derivations of A. Second, one can consider various generalisations of Lie algebras (Leibniz algebras, Mal'cev algebras, etc.) as well as their counterparts serving as analogues of Lie groups. In the opposite direction, one can enrich a Lie algebra with some additional structure and consider the arising versions of Ш. The first interesting case on this route is the class of Poisson algebras. But this is another story. To obtain insight into eventual parallels among these new objects, one will have to use modern bridges rather than the older ones.

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Mathematics for Industry in Europe

Wil Schilders (TU Eindhoven, The Netherlands)

In this paper[1], we give an overview of the development of mathematics for industry in Europe. The advent of such activities was in the 1970s, when, especially in Oxford, the potential of applications of mathematics was realised by Alan Tayler and his colleagues, and the very successful study groups with industry were set up. It led to discussions about European organisations such as ECMI, founded in 1987, as well as to a number of reports on mathematics in industry, to commercial institutes exploiting mathematics for industrial applications and, finally, to a new organisation that has recently been founded, EU-MATHS-IN. It is important to share these experiences and activities with colleagues, anticipating that mathematics in industry will play a key role in enabling technology, leading, in many respects, to a better world, to innovations and solutions for the many challenges humanity is faced with..

Introduction

The mathematical sciences play a vital part in all aspects of modern society. Without research and training in mathematics, there would be no engineering, economics or computer science; no smart phones, MRI scanners, bank accounts or PIN numbers. Mathematics plays a key role in tackling the modern-day challenge of cyber security and in predicting the consequences of climate change, as well as in the manufacturing sectors of the automotive and aerospace industries through the utilisation of superior virtual design processes. Likewise, the life sciences sector, with significant potential for economic growth, would not be in such a strong position without mathematics research and training providing the expertise integral to the development of areas such as personalised healthcare and pharmaceuticals, as well as related medical technologies. The emergence of truly massive datasets across most fields of science and engineering increases the need for new tools from the mathematical sciences, combining traditional methods with artificial intelligence, machine learning and preparing for a future where high-performance computing will play a major role. Modelling, simulation and optimisation will need to be adapted to the data rich environments available nowadays, leading, for example, to major efforts in the area of digital twinning.

One of the classic ways in which mathematical science research plays a role in the economy is through the collection of data to help understand it and the use of tools and techniques to enable the discovery of new relationships or models. Modelling of physical phenomena already dates back several centuries, and well-known systems of equations with the names of Maxwell, Navier–Stokes, Korteweg–de Vries, and more recently the Schrödinger equation, plus many others, are now well established. But it was not until the advent of computers in the middle of the previous century and the development of sophisticated computational methods (like iterative solution methods for large sparse linear systems) that this could be taken to a higher level, by performing computations using these models. Software tools with advanced computational mathematical techniques for the solution of the aforementioned systems of equations have become commonplace and are heavily used by engineers and scientists.

Mirroring this activity is the increased awareness of society and industry that mathematical simulation is ubiquitous to addressing the challenging problems of our times. Industrial processes, economic models and critical events like floods, power failures or epidemics have become so complicated that their realistic description does not require the simulation of a single model, but rather the co-simulation of various models. Better scientific understanding of the factors governing these will provide routes to greater innovation power and economic well-being across an increasingly complex networked world with its competitive and strongly interacting agents. Industry, but also science, is highly dependent on the development of virtual environments that can handle the complex problems that we face today and in the future.

For example, if the origins of life are to be explained, biologists and mathematicians need to work together, and most of the time spent will be on evaluating and simulating the mathematical models[2]. Using the mathematics of evolutionary dynamics, the change from no life to life (referring to the self-replicating molecules dominating early Earth) can be explained. Another example is the electronics industry, which all of us rely on for new developments in virtually every aspect of our everyday life. Innovations in this branch of industry are impossible without the use of virtual design environments that enable engineers to develop and test their complex designs in front of a computer screen, without ever having to go into the time-consuming (several month long) process of prototyping.

Principles of computational science and engineering rooted in modern applied mathematics are at the core of these developments and represent subjects that are set to undergo a renaissance in the 21st century. Indeed,

[1] This article is an adapted version of a keynote presentation given at the Forum "Math-for-Industry" 2013 – The Impact of Applications on Mathematics, November 4–8, 2013, Fukuoka (Japan).

[2] The statement "Biology is the new physics" is heard frequently nowadays; see the EMBO report by Philip Hunter (2010).

no less a figure than Stephen Hawking is on record as having said that the 21st century will be the century of complexity. Another great figure, still young, is Fields medallist Terence Tao, who was a major contributor to the document entitled "The mathematical sciences in 2025" [1], stating: "Mathematical sciences work is becoming an increasingly integral and essential component of a growing array of areas of investigation in biology, medicine, social sciences, business, advanced design, climate, finance, advanced materials, and many more – crucial to economic growth and societal well-being". The recent report by Philip Bond entitled "The era of mathematics" [2] is also a source of inspiration for mathematicians and has led to much additional funding for mathematics in the UK.

Growing computing power, nowadays including multicore architectures and GPUs, does not provide the solution to the ever-growing demand for more complex and more realistic simulations. In fact, it has been demonstrated that Moore's Law, describing the advances in computing power over the last 40 years, holds equally for mathematical algorithms. Hence, it is important to develop both faster computers and faster algorithms at the same time. This is essential if we wish to keep up with the growing demands by science and technology for more complex simulations. For this reason, we have recently introduced the terminology "mathware"[3] to distinguish mathematical method development from software and hardware activities. It is essential that all 3 disciplines cooperate closely, as mathware methodologies may have consequences for hardware, just as hardware has consequences for the development of mathematical methods (Figure 1).

Fig. 1. The development of mathematical methods ("mathware") is an activity distinct from software and hardware. Close cooperation between the three disciplines is of the utmost importance.

Given the above developments, Europe has launched many initiatives to convince industry, society and policymakers that the time is ripe for change. After the OECD report (2008), initiated and chaired by Willi Jaeger from Heidelberg, the European Mathematical Society and the European Science Foundation funded a so-called Forward Look project on "Mathematics in Industry". The result of this project was a report with recommendations to policymakers, industry and the mathematics community, and a very nice book *European success stories in industrial mathematics*, containing more than 100 industrial cases in which mathematics played a decisive role. In 2012, this was followed by a report by Deloitte (accountants and advisers) on "The value of the mathematical sciences for industry and society in the UK", revealing that 38 percent of GVA of the UK can be attributed to results of mathematical sciences research, in a direct, indirect or induced way. Similar studies with comparable conclusions have been undertaken in The Netherlands (2014), France (2017) and Spain (2019)[4]. In Germany, a book was published entitled *Mathematics, engine of the economy* (2008) with more than 20 accounts by captains of industry, emphasising the importance of mathematics. This shows that Europe is putting a lot of effort into demonstrating the necessity and indispensability of mathematics for industry and society.

In this paper, these initiatives will be discussed, as well as the strategy adopted in Europe. All of these efforts have culminated in the formation of a new foundation called EU-MATHS-IN, that aims at collecting all national and European initiatives in the area of industrial mathematics, so as to learn from each other, to share best practice, and to benefit from a unified approach.

The European Consortium for Mathematics in Industry

From a historical point of view, ECMI, the European Consortium for Mathematics in Industry [3], was one of the first organisations that was founded to foster the potential of applications of mathematics in industry. It celebrated its 25th anniversary in 2012. Back when it started, in the middle of the 1980s, mathematics was dominated by mathematicians mainly interested in pure mathematics, in algebra, topology, geometry, analysis and so on. Only a small group of people focussed their attention on cooperation with industry. In 1985, this led to the first conference, called the European Symposium for Mathematics in Industry (ESMI). After this successful symposium, it was felt that it would be good to start a European organisation, and hence, in 1987, ECMI was founded. The goal was to promote and further the effective use of mathematics as well as closely related knowledge and expertise in industrial or management settings. More specifically, concerning research, to see what is needed by industry and commerce, to assess what is available, and to discuss what can be done to fill the gaps. Also, it was important to encourage the participating organisations to have joint research ventures. From an educational point of view, the focus was on the creation, organisation and quality control of a 2-year postgraduate course on industrial and management mathematics. It was also decided to have an annual conference. Quite quickly this became a biennial conference focussing

[3] The terminology "mathware" was first introduced by the Laboratory for Industrial Mathematics Eindhoven (LIME – https://www.mathware.nl/), but has been adopted by EU-MATHS-IN.

[4] All of these reports can be found on the website of EU-MATHS-IN: https://www.eu-maths-in.eu.

on applications of mathematics in industry. The ECMI Newsletter of October 2012 includes a very nice account by one of the founding fathers of ECMI, Helmut Neunzert, about the start and the first 10 years of ECMI. Below is a copy of the official list of signatures on the founding document.


Prof. Dr. A. Bensoussan, INRIA, Le Chesnay, France
date: 15.5.1986 city: Paris

Prof. Dr. A. Fasano, Institute of Mathematics, University of Florence, Italy
date: 14.4.1986 city: Neustadt-Mußbach

Prof. Dr. M. Hazewinkel, CWI, Amsterdam, The Netherlands
date: 14.4.1986 city: Neustadt-Mußbach

Dr. M. Heiliö, Lappeenranta University of Technology and Further Education Centre of Helsinki University, Finland
date: 14.4.1986 city: Neustadt-Mußbach

Dr. P.F. Hodnett, Dept. of Mathematics, National Institute for Higher Education, Limerick, Ireland
date: 14.4.1986 city: Neustadt-Mußbach

Prof. Dr. H.H. Martens, Norwegian Institute of Technology, Trondheim, Norway
date: 14.4.1986 city: Neustadt-Mußbach

Prof. Dr. S. McKee, Dept. of Mathematics, University of Strathclyde, Glasgow, Scotland
date: 14.4.1986 city: Neustadt-Mußbach

Prof. Dr. H. Neunzert, Arbeitsgruppe Technomathematik, Dept. of Mathematics, University of Kaiserslautern, Germany
date: 14.4.1986 city: Neustadt-Mußbach

Dr. D. Sundstrom, The Swedish Institute for Applied Mathematics, Stockholm, Sweden
date: 5.5.1986 city: STOCKHOLM

Dr. A. Tayler, Mathematical Institute, University of Oxford, England
date: 14.4.1986 city: Neustadt-Mußbach

Prof. Dr. HJ. Wacker, Arbeitsgruppe Industrie-und Wirtschaftsmathematik, Mathematical Institute, University of Linz, Austria
date: 14.4.1986 city: Neustadt-Mußbach

Fig. 2. The founding fathers of ECMI.

ECMI is now a mature organisation with over 30 years of experience in the area of mathematics for industry. Its mission can be summarised as follows:

Mathematics, as the universal language of the sciences, plays a key role in technology, economics and life sciences. European industry is increasingly dependent on mathematical expertise in both research and development to keep its world-leading role for high technology innovations and to comply with the EU 2020 agenda for smart, sustainable and inclusive growth. The major objectives to respond to these needs of European industry may be summarized as follows:

- *ECMI advocates the use of mathematical models in industry*
- *ECMI stimulates the education of young scientists to meet the growing demands of industry*
- *ECMI promotes European collaboration, interaction and exchange within academia and industry*

One of the most successful enterprises of ECMI is in the educational field. Its Educational Committee consists of many experts that meet regularly and discuss curricula for master's degrees in industrial mathematics, as well as keeping a close eye on the quality of such curricula in member universities. This quality control is carried out on a regular basis, new members can apply for the status of *qualified node* and are then visited by a team of experts evaluating the curriculum and the means used. Nowadays, the ECMI Educational Committee oversees more than 20 high standard master's programs in industrial and econo-mathematics. Students that have graduated from an ECMI centre are awarded an ECMI certificate.

Another one of ECMIs success stories are the annual European Modelling weeks that started in 1988 in Bari (Italy) with 30 students working on six projects. Each project in a modelling week originates from a real-life problem and an international student group, supervised by an ECMI instructor, works collaboratively for one week towards a solution. In 2019, the 34th modelling week took place in Grenoble (France), where some 40 students from all over Europe worked on 7 projects. Alongside the modelling week, ECMI also organises its summer school, where lecturers both from ECMI and from its industrial partners give courses in various topics of applied and industrial mathematics.

An important aspect of the ECMI education network is the organisation and broadening of the exchange of students among the ECMI centres. The strong coherence within the network and the synchronised local master programs taught in English allow for a smooth relocation from one centre to another and for an easy transfer of credits gained at a foreign centre.

ECMI also undertakes many activities in the research area. The Research and Innovation Committee focuses on strategies to increase the interaction between industry and academia, to foster both academic research and industrial innovation. The committee is multidisciplinary. It marshals the power of mathematics, scientific computing and engineering for industrial modelling and simulation. It also fosters special interest groups (SIGs) that focus on a special theme which is either application oriented or methodology based. A SIG identifies a group of experts and has a strong industry participation or interest. The SIGs organise regular meetings and workshops. Examples of active SIGs are "Scientific Computing in the Electronics Industry" and "Shape and Size in Medicine, Biotechnology and Material Sciences". The SIGs provide a unique opportunity for cooperation on the European level, participating jointly in, for example, an Innovative Training Network within the Marie-Sklodowska-Curie program. Two very successful examples of past projects are MACSI-net (Mathematics, Computing and Simulation for Industry) and COMSON (Coupled Multiscale Simulation and Optimization in Nanoelectronics). The former is considered to have been extremely important for the further development of strategies to bridge the gap with industry, and is therefore discussed below in more detail.

Last, but certainly not least, are the European Study Groups with Industry (ESGI). Study Groups with Industry [4] are an internationally recognised method of technology transfer between academic mathematicians and industry. These weeklong workshops provide a forum for industrial scientists to work alongside academic mathematicians on problems of direct industrial relevance. The success of the Study Groups' unique format, which uses problems presented by industry as a basis for mathematical research, is demonstrated by the extent to which it has been copied around the world and is now extending into other areas where mathematics may be applied. The European Study Groups with Industry started with the first Study Group in Oxford in 1968, and now there are 5–7 meetings held annually in different European countries.

MACSI-net

ECMI provided the cradle for a very successful European network, initiated by Prof. Bob Mattheij at TU Eindhoven, one of the founding fathers of ECMI. MACSI-net [5], short for Mathematics, Computing and Simulation for Industry, was in fact a cooperative venture between ECMI and ECCOMAS [6]. ECCOMAS is a scientific organisation grouping together European associations with interests in the development and application of computational methods in science and technology. The Mission of ECCOMAS is to promote joint efforts of European universities, research institutes and industries which are active in the broader field of numerical methods and computer simulation in Engineering and Applied Sciences, and to address critical societal and technological problems with particular emphasis on multidisciplinary applications.

When MACSI-net was started around the turn of the century, it was apparent that industry was grappling with ever more challenging problems which should be solved by using state of the art mathematical and computational tools. Academic institutions often had the knowledge and expertise to be of great help with this. However, enterprises often did not know how to find the proper academic partners, in particular in mathematical areas. Equally, academic institutes were still not sufficiently aware of the importance of taking up their role in joint endeavours with both smaller and larger problems that could help Europe's industry to maintain or achieve a competitive edge in a variety of areas. MACSI-net was therefore set up as a network where both enterprises and university institutions could cooperate on the solution of such problems, to their mutual benefit. In particular, the network focused on strategies to increase the interaction between industry and academia in order to help industry (in particular SME) with advanced mathematical and computational tools, and to increase awareness of academia concerning industrial needs. The network was multidisciplinary, combining the power of mathematics, scientific computing and engineering, for modelling and simulation activities. The network aimed at achieving its goals through

- Strategic meetings with industries about well-specified topics
- Summer courses
- Workshops
- Visits of experts
- Foundation of special (interest) groups
- Funding and appointment of post docs
- Activity committees who actively look for funded proposals from EU or other bodies

The various nodes in the MACSI-net network each fostered general and specific expertise in areas of mathematics and computing. The role of industrial nodes was somewhat complementary to the academic ones. The network was aiming at the dissemination of ideas, models and algorithms to their mutual benefit, leading to joint research efforts and the forging of (often thinly spread) local initiatives. In particular, joint research proposals were expected to make this network attractive for all involved.

MACSI-net was very successful during its 4 years of existence. In the end, there were 17 working groups concentrating on a large variety of topics. Some of these working groups are still active, in a different form, but the researchers have remained in contact. An example of this is working group 2 on Coupled problems and Model Order Reduction. It actually split into two different communities, one of these active within ECCOMAS and organising biennial conferences on coupled problems (see, for example, [7]). The other group remained concentrated on model order reduction, and ran the European COST Action EU-MORNET from 2014-2018 which was used to coordinate all research in the area of model order reduction taking place in Europe [8].

At the end of its lifetime, in 2004, MACSI-net issued an important document which was one of the first reports on industrial mathematics with guidelines and recommendations. Some quotes from this document:

- *Mathematics should be regarded as a technology in its own right. Its crucial role in many industrial problems requires the active participation of mathematicians. Truly multidisciplinary projects will benefit significantly from the involvement of mathematical modellers and this should be encouraged by future funding programmes. Consideration should be given to making the participation of mathematicians in appropriate multidisciplinary projects a condition of project funding.*
- *There is a need for positive action to promote the increased use of mathematics by European industry. The success of local initiatives where mathematicians are working on industrially relevant problems is clear evidence that they are already making a significant contribution to the development of the knowledge-based economy. However, more needs to be done to encourage companies, especially Small and Medium-sized Enterprises (SMEs), to make use of mathematics and mathematicians. Consideration should be given to creating a programme funding projects that will enable companies, especially SMEs, to explore areas where mathematics can make a contribution to their improved competitiveness.*
- *There is an urgent need for more training in the area of industrial mathematics. It is essential to attract bright students to this area and to convey the challenge and the excitement of solving practical problems. Consideration should be given to specific funding for training programmes in industrial mathematics across Europe.*

The full report can be found here [9]. MACSI-net ended in 2004, but the acronym is in fact still in use. At Limerick University, Prof. Stephen O'Brien attracted funding from the Science Foundation Ireland and is running a project termed MACSI [10], which is a network of mathematical modellers and scientific computational analysts based in Ireland. Its aim is to foster new collaborative research, in particular on problems that arise in industry, in order to produce world-class publications on mathematical modelling. It has been very successful to date, attracting a lot of Irish industry.

A renowned institute for mathematics and industry

One of the founding fathers of ECMI, already mentioned earlier, was Prof. Helmut Neunzert, who was also the key driving force behind the creation and subsequent success of the Fraunhofer Institute for Industrial Mathematics (ITWM) that started in Kaiserslautern in the middle of the 1990s. On their website [11], one can find the following remark which encapsulates the essence of the role and importance of industrial mathematics:

> *"The core competence of ITWM is mathematics: the language used by scientists and engineers to formulate models for technical systems. In our time it is particularly important, as it provides efficient algorithms to compute and analyse such models. The ITWM 's mission is to develop this technology to give innovative impulses and put them into practice together with industry partners. Since its foundation in 1995 the ITWM has shown great success in building mathematical bridges between applied sciences and concrete application. Clients are large international companies as well as small and medium regional enterprises. Fraunhofer ITWM focuses on the development of mathematical applications for industry, technology and economy. Mathematical approaches to practical challenges are the specific competences of the institute and complement knowledge in engineering and economics in an optimal way. In 2001 ITWM became the first mathematical oriented institute of the Fraunhofer Gesellschaft. The main emphases are surface quality inspection, financial mathematics, visualization of large data sets, and optimization of production processes, virtual material design and analysis of 3D models of microstructures."*

ITWM is an example of how mathematics can successfully be turned into a business. Since ITWM's foundation, its budget has increased substantially: beginning with 1,64 million € in 1995, it reached 31.4 million € in 2019. Nearly 75 percent of the operating budget stems from the institute's own profits. At present, ITWM's personnel consists of almost 500 employees, of which 160 are PhD students.

The success of ITWM has also been observed by others, and by now there are various smaller and larger companies that obtain their business from the application of mathematics to industrial problems. An example is the Laboratory for Industrial Mathematics Eindhoven (LIME) [10] in the Netherlands that originally started at the Eindhoven University of Technology, but soon after became an independent company.

Captains of industry reporting on mathematics

The book *Mathematik – Motor der Wirtschaft* [12] came about in close cooperation between the Oberwolfach Foundation and the Mathematisches Forschungsinstitut Oberwolfach, and features articles by renowned business figures. It was launched by the German Federal Minister of Education and Research, Annette Schavan, at a gala event. Oberwolfach is well known for its workshops on mathematics, but this event was a very special one, involving many captains of industry outlining their opinions about mathematics and its utilisation in their companies.

In their articles, various heads of major German companies – Allianz, Daimler, Lufthansa, Linde, and TUI, to name but a few – sum it up in a nutshell: mathematics is everywhere, and our economy would not work without it. SAP's CEO, Henning Kagermann, puts it like this: "Corporate management without mathematics is like space travel without physics. Numbers aren't the be all and end all in business life. But without mathematics, we would be nothing."

The OECD report on mathematics and industry

While ECMI continued to attract new members and spread its activities further across Europe, also including countries in the eastern part, the idea arose in Heidelberg to use the experience gained in several countries to start a series of discussions and produce a report on mathematics for industry. To this end, the initiator, Prof. Willi Jäger, who heads the institute IWR (Interdisciplinary Center for Scientific Computing) [13], suggested the idea to the OECD.

Recognising the importance of mathematics in an industrial context, the delegates to the Global Science Forum (GSF) of the Organization for Economic Cooperation and Development (OECD) agreed to sponsor an international consultation to assess the present state of this interface in the participating countries and to identify mechanisms for strengthening the connection between mathematics and industry. (The interaction between mathematics and other sciences was left for future consideration.)

A workshop on "Mathematics in Industry" was then held in Heidelberg in early 2007. The objectives of the workshop were to

- analyse the relationship between the mathematical sciences and industry in the participating countries;
- identify significant trends in research in the mathematical sciences in academia and the mathematical challenges faced by industry in the globalised economic environment, and to analyse the implications of the trends for the relationships between mathematical scientists in academia and industry;
- identify and analyse major challenges and opportunities for a mutually beneficial partnership between industry and academia; and
- formulate action-oriented practical recommendations for the main stakeholders: the community of mathematical scientists, participating industries and governments.

The report [14] summarised the deliberations and presented the findings and recommendations of the workshop, which will involve further consultations among the participants. The recommendations involved the participation of the academic community, governmental and other funding agencies as well as industry. They were designed to stimulate the interaction between mathematics and industry; to enhance the curriculum for students of mathematics; to improve the infrastructure for increased interaction, both in academia and in industry; and to strengthen coordination and cooperation at national and international levels.

As a follow-up, the OECD also supported an activity that was intended as a corollary to the report "Mathematics-in-Industry". It primarily comprises a factual compendium of the ways in which the various mechanisms cited in that report have been implemented around the world. The compendium, which is not comprehensive, has been compiled with the aim of helping governments, industries and academia to see how they may best exploit mathematics as an industrial resource for both research and training.

The forward look initiative of ESF and EMS

Although the reports commissioned by the OECD were a very valuable asset to the mathematics community, it was felt that an even more in-depth understanding of the problems was necessary. Indeed, the impact of mathematics on industry and society had been the subject of numerous studies, but it was decided at the end of 2009 to start a Forward Look project on mathematics and industry, evolving from the belief that European Mathematics as a whole has the potential to boost European knowledge-based innovation, which is essential for a globally competitive economy. The project was fostered by the European Science Foundation and the European Mathematical Society, and involved many members of ECMI, as it was felt they had the experience and knowledge to be able to implement such an activity. The main driving force for this endeavor was Prof. Mario Primicerio from Firenze University.

The Forward Look at mathematics and industry sprung from the strong belief that European Mathematics has the potential to be an important economic resource for European industry, helping its innovation and hence its capacity for competing on the global market. To fulfil its potential, special attention has to be paid to the reduction of the geographical and scientific fragmentation in the European Research Area. Overcoming this fragmentation will require the involvement of the entire scientific community. Europe needs to combine all experience and synergies at the interface between mathematics and industry and create strong areas of interaction to turn challenges into new opportunities.

The project began in 2009, and working groups were set up to discuss the main issues identified. An extensive survey was carried out to identify whether the topics worked on in academic circles reflected the needs of industry. In the figure below, this is illustrated. It confirms that, apart from a very small number of exceptions, mathematicians are indeed doing valuable work in areas of importance in industry.

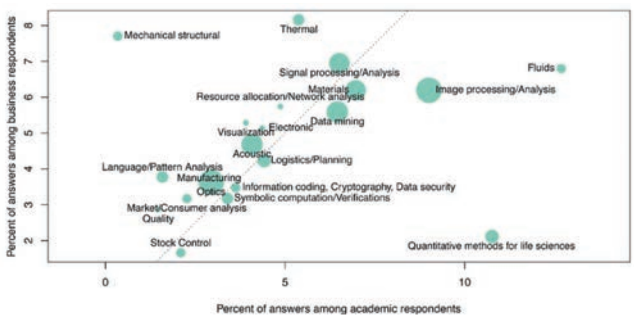

Fig. 3. Main areas of competence available in academia versus major business challenges perceived by industry (size of the bubbles indicates total number of respondents).

The project also organised alignment and consensus conferences, involving many researchers and industrialists from all over Europe, so that the conclusions in the final report [15] were broadly supported and adopted. The final recommendations were:

- Recommendation 1: Policymakers and funding organisations should join their efforts to fund mathematics activities through a European Institute of Mathematics for Innovation.
- Recommendation 2: In order to overcome geographical and scientific fragmentation, academic institutions and industry must share and disseminate best practices across Europe and disciplines via networks and digital means.
- Recommendation 3: Mathematical Societies and academic institutions should create common curricula and educational programmes in mathematics at the European level taking into account local expertise and specificity.

Besides these recommendations, the report also gives roadmaps for their implementation.

The network of networks EU-MATHS-IN

Even though the recommendations from the aforementioned Forward Look report were widely accepted, it turned out to be quite hard to obtain sufficient support to implement them in practice. Therefore, in 2013, it was decided to take the initiative into our own (mathematical) hands and start a new organisation in Europe that would enable cross-fertilisation and exchange of best practice. Collaboration provides a much better basis for funding of European organisations. Consequently, at the end of 2013, EU-MATHS-IN was launched in Amsterdam [16]. It is the European Service Network of Mathematics for Industry and Innovation. As stated on the website: "A new initiative to boost mathematics for industry in Europe. Make the most of our expertise for a more efficient route to innovation!"

EU-MATHS-IN aims to leverage the impact of mathematics on innovations in key technologies through enhanced communication and information exchange between and among the involved stakeholders at a European level. It aims to create a *dedicated one-stop shop*, together with other stakeholders, to coordinate and facilitate the required exchanges in the field of application-driven mathematical research and its exploitation for innovations in industry, science and society. For this, it aims to build an *e-infrastructure* that provides tailored access to information and facilitates communication and exchange by player-specific sets of *services*. It will *act as facilitator, translator, educator and link* between and among the various players and their communities in Europe. In the figure below, a graphical illustration is given of the intended structure.

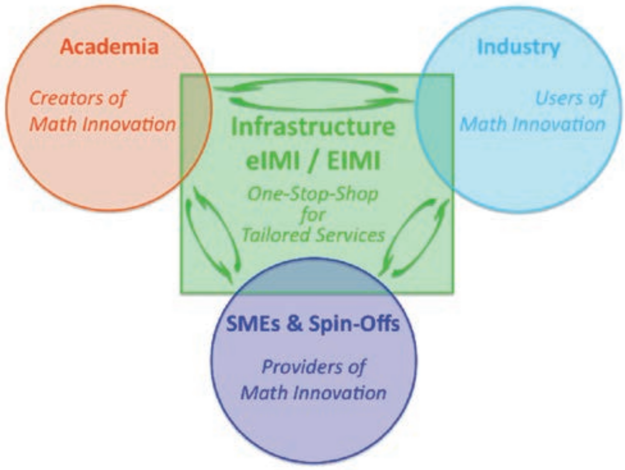

Fig. 4. Graphical illustration of the strategy of EU-MATHS-IN.

The important features of the long-term goals of the organisation are:

- Establish strategic connections among the national networks and centres working in the field of industrial mathematics and mathematics for innovation;
- Create a European service unit that can foster the competitive advantage of the European industry through international cooperation;
- Promote the technological aspects of mathematics raising public awareness;
- Stimulate the cooperation at European level of mathematical research with companies and administrations;
- Establish a one-stop-shop at European level for industrial users of mathematical scientific research results;
- Provide European industry, in particular SMEs, with a competitive advantage taking profit of the scientific excellence of the continent (give Europe the possibility to cash a "scientific dividend");
- Acquire funding for the performance of activities that serve the realisation of the Association's aims.

It is felt that, with EU-MATHS-IN, Europe has a powerful organisation that will be able to bring together all national initiatives, such as those that have arisen in many European countries, to learn from each other, to share experiences and together form a community that is recognised for its capability to bridge the gap between mathematics and industry.

One of the first initiatives of the organisation was to strongly call for the establishment of Mathematical Modelling, Simulation and Optimization (MSO) as a transversal (universal?) Key Enabling Technology (KET). The arguments are as follows. There is no doubt that continuous multidisciplinary research and novel mathematical and computational methods are needed to provide the necessary tools for industrial innovation and European competitiveness. It has become widely recognised that the approach of MSO is the third, and indispensable, pillar for scientific progress and technological innovation, alongside experiments and theory building. Experience shows that future challenges for innovation in industry and society will involve increasing complexity and, at the same time, are subject to ever-shorter innovation cycles. The real-world challenges to be dealt with on our way towards innovation exhibit opportunities that make MSO indispensable and simultaneously a far from trivial task.

In 2017, EU-MATHS-IN invited a number of European companies to actively join in the discussions to promote mathematics for industry in Europe. The Industrial Core Committee (ICC) was formed, currently with Siemens, Michelin, Shell, Bosch, ATOS, Nors, Repsol and ING on board. The formation of the ICC was instrumental for EU-MATHS-IN: two workshops were held in Amsterdam, and a workshop on "Future and emerging mathematical technologies in Europe" was held in the Lorentz Center in Leiden at the end of 2017 [17]. These events led to a vision document entitled "Modelling, Simulation & Optimization in a Data rich Environment – A window of opportunity to boost innovations in Europe" [18] that was presented in the French embassy in Berlin on 18 April 2018. The document contains the joint vision on MSO in a data rich environment. A delegation of EU-MATHS-IN and its ICC visited the unofficial opening event of the new joint undertaking EuroHPC in Sofia [19], and used this opportunity to speak to the (then) European Commissioner for Digital Economy and Society, Marya Gabriel. Mathematics is an important ingredient for EuroHPC, and to make this apparent, a separate document was produced by the ICC (see the web page of EU-MATHS-IN).

Realising that it is important to gather arguments in order to convince policymakers, EU-MATHS-IN set out to write its first Strategic Research Agenda (SRA) in 2019. Vice-president of EU-MATHS-IN, Zoltan Horvath, is leading this effort. There were 9 working groups started, on basic MSODE technologies, 4 working groups on missions (one of them being digital twins) and 1 working group on transfer. Due to the COVID-19 crisis, the work on the SRA has been delayed significantly, but it is anticipated that the final SRA will be published in early 2021. We feel that it will be an extremely important document in discussions with policymakers, firmly backed by the ICC and other European industry.

As a corollary of the SRA efforts, EU-MATHS-IN recently got involved in the TransContinuum Initiative (TCI), initiated by the organisations ETP4HPC and BDVA, set up in the advent of the new Horizon Europe program. The term TransContinuum describes the defining characteristic of the infrastructure required for the convergence of data and compute capabilities in many leading edge industrial and scientific use scenarios. The initiative outlines a vision for a horizontal collaboration between European associations and projects involved in IT technology, application and services provisioning for the Digital Continuum. Mathematical Models and Algorithms are in the kernel of the activities: TCI describes a continuous dynamic workflow between smart sensors and IOT devices at the edge and HPC/cloud centres over smart networks and services executing simulation & modelling, big data analytics and machine learning, based on mathematical methods and algorithms including MSODE, pervasively augmented by artificial intelligence, protected and secured by cybersecurity and back to cyber-physical systems. We feel that such joint undertakings are extremely important for mathematics, being part of the game that is being played.

In order to convince policymakers that mathematics is indispensable in today's world, we are also working on a pamphlet entitled "Horizon Europe needs Mathematics". This pamphlet will contain a few one-liners stressing the importance of mathematics. The statement by Lex Schrijver ("mathematics is like oxygen") will be one of these, as will "Mathematics: invisible contribution to visible success" and "Mathematics: real intelligence is needed to make artificial intelligence work. In addition, we will argue that mathematical algorithm development has outperformed machine improvement ("Moore's Law") in the past 4 decades. Finally, the following statement made in the report "The era of mathematics" [2] provides a very strong argument: "The rate of return on investment as benefit-to-cost ratio may be estimated as follows: Engineering 88, Physics 31, Chemistry 246, and Mathematical Sciences 588." The pamphlet will be made available on the website of EU-MATHS-IN [16].

Industrial mathematics on the world scale

Europe has always been very active in building bridges between mathematicians and industry. But also in other continents, and on the world scale, similar initiatives and organisations have emerged. The largest association is the Society for Industrial and Applied Mathematics (SIAM), based in the USA, but also operating more widely. SIAM was already founded back in 1951, and many researchers worldwide are members of this organisation.

In 1986 the four societies GAMM, IMA, SIAM and SMAI decided to organise large International Conferences on Industrial and Applied Mathematics (ICIAM) every four years. The first of such conferences was held in Paris in 1987, and since then it has been organised on different continents every 4 years, the latest edition in Valencia with over 4000 participants. A great account of the history was written by Iain Duff in 2007 [20]. Currently, ICIAM has over 50 members, and growing. We feel that it is very important to share the experience we have in Europe with the rest of the world, so as to make sure that mathematics is recognised everywhere as a key enabling technology that should be firmly adopted when working on mathematics for industry.

Conclusion

Europe has always been very active in trying to bridge the gap between mathematics and industry. Already since the 1970s, when the Oxford study groups with mathematics started to be held, mathematicians have realised the potential for breakthroughs and innovations in industry and for societal problems. In this paper, we have given a chronological picture of what has happened in Europe since the 1970s. We are observing a very natural and continuous growth of activities, stepping up in intensity over the years. In recent years, a strong linking of mathematicians and mathematics with industry personnel and problems has been occurring, with the importance of industrial mathematics being realised. This is confirmed by recent reports issued on the economic value of mathematics, in various European countries, concluding that about 30 percent of GVA can be attributed to the results of research in the mathematical sciences. This is an enormously large percentage, and it will hopefully convince politicians and policymakers to invest more in mathematics.

The foregoing leads to the natural question: what will our world look like in 2030? And what will the role of the mathematical sciences be in shaping that world? Since the start of the 21st century, it has become clear that the mathematical sciences are gaining a new stature. They are increasingly providing the knowledge to enable innovative breakthroughs and insights in many other disciplines such as biology, healthcare, social sciences and climatology, alongside their traditional role in physics, chemistry and computer science. The importance of the mathematical sciences is also rapidly increasing in the business world, for example in design processes, electronics and finance. All these developments are vital for economic growth and competitive strength, and demand an in-depth review of the overall way we look at the mathematical sciences. This involves the integration of mathematics with statistics, operations research and computational science, and it carries implications for the nature and scale of research funding.

Joining forces is very important in this endeavour, as is using the right kind of PR. To this end, EU-MATHS-IN has developed the "Mathematics Inside" pictogram that can be used to convey the message that mathematics is indispensable and present everywhere. Dutch mathematician Lex Schrijver formulated this in a very succinct way: "Mathematics is like oxygen. You take no notice of it when it's there – if it wasn't, you'd realize you cannot do without it." The pictogram below allows us to spread this message!

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The Ionization Problem

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The question: "How many electrons can a nucleus bind?" is as old as quantum mechanics, but its rigorous answer based on the many-body Schrödinger equation remains a difficult challenge to mathematicians. Nevertheless, there has been remarkable progress in this problem in the past four decades. We will review the current understanding of the Schrödinger equation and then turn to simplified models where the problem has been solved satisfactorily. We will also discuss the connection to the liquid drop model, which is somewhat more classical, but no less interesting.

1 Atomic Schrödinger equation

For us, an atom is a system of N quantum electrons of charge -1 moving around a heavy classical nucleus of charge $Z \in \mathbb{N}$ and interacting via Coulomb force (we use atomic units). The wave functions of N electrons are normalised functions in $L^2(\mathbb{R}^{3N})$ satisfying the anti-symmetry

$$\Psi(x_1, \dots, x_i, \dots, x_j, \dots, x_N) = -\Psi(x_1, \dots, x_j, \dots, x_i, \dots, x_N), \quad \forall i \neq j,$$

where $x_i \in \mathbb{R}^3$ stands for the position of the i-th electron (we will ignore the spin for simplicity). The Hamiltonian of the system is

$$H_N = \sum_{i=1}^{N} \left(-\frac{1}{2}\Delta_{x_i} - \frac{Z}{|x_i|} \right) + \sum_{1 \leqslant i < j \leqslant N} \frac{1}{|x_i - x_j|}.$$

The self-adjointness of H_N follows a famous theorem of Kato.

We are interested in the ground state problem

$$E_N = \inf_{\|\Psi\|_{L^2}=1} \langle \Psi, H_N \Psi \rangle.$$

By a standard variational method, we know that the minimizers, if they exist, are solutions to the Schrödinger equation

$$H_N \Psi = E_N \Psi.$$

The existence/nonexistence issue is related to the stability of the system, namely whether all electrons will be bound, or some of them may escape to infinity. Obviously, H_N and E_N also depend on Z, but let us not include this dependence in the notation.

It is natural to guess that there is a sharp transition when N crosses the value $Z + 1$. Heuristically, if $N < Z + 1$, then the outermost electron sees the rest of the system as a large nucleus of the effective charge $Z - (N - 1) > 0$. Hence, this electron will "prefer to stay" by the Coulomb attraction. On the other hand, if $N > Z + 1$, then the outermost electron will "prefer to go away" by the Coulomb repulsion.

Part of the above heuristic guess was justified by Zhislin in 1960.

Theorem 1. *If $N < Z + 1$, then E_N has a minimizer.*

More precisely, he proved that if $N < Z + 1$, then $E_N < E_{N-1}$. This strict binding inequality prevents any electron from escaping to infinity, thus ensuring the compactness of minimising sequences for E_N. On a more abstract level, if $N < Z + 1$, then E_N is strictly below the essential spectrum of H_N. In fact, the essential spectrum of H_N is $[E_{N-1}, \infty)$ due to the celebrated Hunziker–van Winter–Zhislin (HVZ) theorem.

Thus, Zhislin's theorem ensures the existence of all positive ions and neutral atoms. On the other hand, the nonexistence of highly negative ions is much more difficult, and often referred to as the "ionization conjecture"; see, e.g., [23, Problem 9] and [15, Chapter 12].

Conjecture 2. *There exists a universal constant $C > 0$ (possibly $C = 1$) such that if $N > Z+C$, then E_N has no minimizer.*

Note that the above heuristical argument is purely classical and it is too rough to understand the delicate quantum problem at hand. In 1983, Benguria and Lieb [1] proved that if the anti-symmetry condition of the wave functions is ignored, then the atoms with "bosonic electrons" always exist as soon as $N \leqslant t_c Z$ with a universal constant $t_c > 1$ (numerically $t_c \approx 1.21$, computed by Baumgartner). Thus the ionization problem requires a deep insight, as the particle statistics, more precisely Pauli's exclusion principle, play an essential role.

2 Known results

A rigorous upper bound to the question "How many electrons can a nucleus bind?" was first derived by Ruskai [19] and Sigal [21] independently in 1982. They proved that there exists a critical value $N_c(Z) < \infty$ such that if $N > N_c(Z)$, then E_N has no minimizer. In these works, they applied certain inequalities on classical point particles to the quantum problem via the geometric localization method. In particular, Sigal realized that for every collection $\{x_i\}_{i=1}^N \subset \mathbb{R}^3$ with $N > 2Z + 1$, the energy contributed by the farthest electron, x_N says, is always positive because of the triangle inequality

$$-\frac{Z}{|x_N|} + \sum_{i=1}^{N-1} \frac{1}{|x_i - x_N|} \geqslant -\frac{Z}{|x_N|} + \frac{N-1}{2|x_N|} > 0.$$

This leads to the upper bound $\limsup_{Z\to\infty} N_c(Z)/Z \leqslant 2$ in [22].

Later, Lieb, Sigal, Simon and Thirring [13] found the following improvement: for every $\{x_i\}_{i=1}^N \subset \mathbb{R}^3$ with N large, one has

$$\max_{1\leqslant j\leqslant N} \left\{ \sum_{1\leqslant i\leqslant N, i\neq j} \frac{1}{|x_i - x_j|} - \frac{N + o(N)}{|x_j|} \right\} \geqslant 0. \qquad (1)$$

Consequently, they obtained the asymptotic neutrality

$$\lim_{Z\to\infty} \frac{N_c(Z)}{Z} = 1.$$

It is unclear whether one can improve the quantity $N + o(N)$ in (1) to $N + O(N^\alpha)$ with some constant $0 \leqslant \alpha < 1$.

In 1990, Fefferman and Seco [4], and Seco, Sigal and Solovej [20], proved

Theorem 3. *When $Z \to \infty$, we have $N_c(Z) \leqslant Z + O(Z^{5/7})$.*

This bound was obtained by comparing it with the Thomas–Fermi theory (that we will revisit below) and taking into account quantitative estimates for Scott's correction (studied by Huges, and by Siedentop and Weikard). There has been no further improvement in the past three decades!

Instead of the asymptotics as $Z \to \infty$, one may also be interested in explicit bounds for all Z (in fact, $1 \leqslant Z \leqslant 118$ for realistic atoms in the current periodic table). The best known result in this direction is

Theorem 4 ([12, 17])**.** *For all $Z \geqslant 1$, $N_c(Z) < \min(2Z + 1, 1.22\,Z + 3Z^{1/3})$.*

Let us quickly explain Lieb's proof of the bound $2Z + 1$ in [12], since it is short and important. The starting point is the following identity, which follows from the Schrödinger equation

$$\langle |x_N|\Psi_N, (H_N - E_N)\Psi_N \rangle = 0.$$

The idea of "multiplying the equation by $|x|$" was also used by Benguria on a simplified model. Then we decompose

$$H_N = H_{N-1} - \Delta_N + \sum_{i=1}^{N-1} \frac{1}{|x_i - x_N|}.$$

For the first $(N - 1)$ electrons, we use the obvious inequality

$$H_{N-1} \geqslant E_{N-1} \geqslant E_N.$$

For the N-th electron, we use the operator inequality

$$(-\Delta)|x| + |x|(-\Delta) \geqslant 0 \text{ on } L^2(\mathbb{R}^3)$$

(which is equivalent to Hardy's inequality). Consequently,

$$Z > \sum_{i=1}^{N} \left\langle \Psi, \frac{|x_N|}{|x_i - x_N|}\Psi \right\rangle = \frac{1}{2}\sum_{i=1}^{N} \left\langle \Psi, \frac{|x_N| + |x_i|}{|x_i - x_N|}\Psi \right\rangle > \frac{N-1}{2}$$

Thus $N < 2Z + 1$. Here, we have used the symmetry of $|\Psi|^2$ and the triangle inequality.

To get the bound in [17], we multiply Schrödinger's equation with $|x_N|^2$ instead of $|x_N|$ and proceed similarly. In this case, the operator $(-\Delta)|x|^2 + |x|^2(-\Delta)$ on $L^2(\mathbb{R}^3)$ is not positive, but its negative part can be controlled using a special property of the ground state. The key point is, instead of using the triangle inequality, we now have

$$Z \geqslant \inf_{\{x_i\}_{i=1}^N \subset \mathbb{R}^3} \frac{\sum_{1\leqslant i<j\leqslant N} \frac{|x_i|^2 + |x_j|^2}{|x_i - x_j|}}{(N-1)\sum_{i=1}^N |x_i|} + O(N^{2/3}) = \beta N + O(N^{2/3})$$

with the statistical value

$$\beta := \inf_{\substack{\rho \text{ probability} \\ \text{measure in } \mathbb{R}^3}} \left\{ \frac{\iint_{\mathbb{R}^3\times\mathbb{R}^3} \frac{x^2+y^2}{2|x-y|} \mathrm{d}\rho(x)\mathrm{d}\rho(y)}{\int_{\mathbb{R}^3} |x|\mathrm{d}\rho(x)} \right\}.$$

It is nontrivial to compute β, but we can estimate it using the inequality

$$\iint_{\mathbb{R}^3\times\mathbb{R}^3} \frac{x^2 + y^2}{|x - y|} \mathrm{d}\rho(x)\,\mathrm{d}\rho(y) \geqslant \iint_{\mathbb{R}^3\times\mathbb{R}^3} \left(\max(|x|, |y|) + \frac{\min(|x|, |y|)^2}{|x - y|} \right) \mathrm{d}\rho(x)\mathrm{d}\rho(y).$$

which is a consequence of (1). This gives $\beta \geqslant 0.82$, leading to the bound $1.22\,Z + 3Z^{1/3}$ (as $\beta^{-1} \leqslant 1.22$).

3 Thomas–Fermi theory

Since the Schrödinger equation is too complicated, for practical computations one often relies on approximate models which are nonlinear but dependent on less variables. In density functional theory, a popular method in computational physics and chemistry, one replaces the N-body wave function Ψ with its one-body density

$$\rho_\Psi(x) = N\int_{\mathbb{R}^{3(N-1)}} |\Psi(x,x_2,\dots,x_N)|^2 dx_2\dots dx_N.$$

Clearly, $\rho_\Psi : \mathbb{R}^3 \to [0,\infty)$ and $\int_{\mathbb{R}^3}\rho_\Psi = N$.

The oldest density functional theory was proposed by Thomas and Fermi in 1927. In the Thomas–Fermi (TF) theory, the ground state energy E_N is replaced by its semiclassical approximation

$$E^{\rm TF}(N) = \inf_{\int\rho=N}\Big\{C^{\rm TF}\int_{\mathbb{R}^3}\Big(\rho^{5/3}(x)-\frac{Z}{|x|}\rho(x)\Big)dx + \frac12\int_{\mathbb{R}^3}\int_{\mathbb{R}^3}\frac{\rho(x)\rho(y)}{|x-y|}dxdy\Big\}$$

with a constant $C^{\rm TF}>0$. The existence and properties of the TF minimizers was studied by Lieb and Simon in [14]. In particular, they proved

Theorem 5. *$E^{\rm TF}(N)$ has a minimizer if and only if $N \leqslant Z$.*

By standard techniques, we find that the TF functional is convex and rotation invariant. Therefore, if a minimizer exists, it is unique and radial. Moreover, it satisfies the TF equation

$$\frac53 C^{\rm TF}\rho(x)^{2/3} = \Big[Z|x|^{-1}-\rho*|x|^{-1}-\mu\Big]_+$$

for some chemical potential $\mu \geqslant 0$.

The existence of the TF minimizer is rather similar to Zhislin's theorem for the Schrödinger equation. The nonexistence is more challenging. The original proof of Lieb and Simon is based on a clever use of the maximum principle. Here we offer another proof, using a variant of the Benguria–Lieb argument.

Proof of $N\leqslant Z$ [18]. Assume that the TF equation has a radial solution ρ. Multiplying the equation with $|x|^k\rho(x)$, $k\geqslant 1$, we have the pointwise inequality

$$\Big(Z|x|^{-1}-\rho*|x|^{-1}-\mu\Big)\rho(x)|x|^k = \frac53 C^{\rm TF}\rho(x)^{5/3}|x|^k \geqslant 0.$$

Then we integrate over $\{|x|\leqslant R\}$. Note that $\mu\geqslant 0$. Moreover, since ρ is radial, by Newton's theorem we have

$$\rho*|x|^{-1} = \int_{\mathbb{R}^3}\frac{\rho(y)}{\max(|x|,|y|)}dy.$$

Consequently,

$$Z\int_{|x|\leqslant R}|x|^{k-1}\rho(x) \geqslant \int_{|x|\leqslant R}|x|^k\rho(x)(\rho*|x|^{-1})dx \geqslant \frac12\iint_{|x|,|y|\leqslant R}\frac{(|x|^k+|y|^k)\rho(x)\rho(y)}{\max(|x|,|y|)}dxdy.$$

On the other hand, by the AM-GM inequality,

$$\frac{|x|^k+|y|^k}{\max(|x|,|y|)} \geqslant \Big(1-\frac1k\Big)\Big(|x|^{k-1}+|y|^{k-1}\Big).$$

Thus

$$Z\int_{|x|\leqslant R}|x|^{k-1}\rho(x)dx \geqslant \Big(1-\frac1k\Big)\Big(\int_{|x|\leqslant R}|x|^{k-1}\rho(x)dx\Big)\Big(\int_{|y|\leqslant R}\rho(y){\rm d}y\Big).$$

Taking $R\to\infty$ and $k\to\infty$, we conclude that $\int_{\mathbb{R}^3}\rho\leqslant Z$. □

When $N=Z$, the TF minimizer has the perfect scaling property:

$$\rho_Z^{\rm TF}(x) = Z^2\rho_1^{\rm TF}(Z^{1/3}x), \quad \forall x\in\mathbb{R}^3$$

where the function $\rho_1^{\rm TF}$ is independent of Z. Moreover, it satisfies the TF equation with chemical potential 0. Thus if we denote the TF potential

$$\varphi_Z^{\rm TF}(x) = Z|x|^{-1}-\rho_Z^{\rm TF}*|x|^{-1},$$

then the TF equation can be written as the nonlinear Schrödinger equation

$$\Delta\varphi_Z^{\rm TF}(x) = 4\pi\Big(\frac53 C^{\rm TF}\Big)^{-3/2}\varphi_Z^{\rm TF}(x)^{3/2}.$$

This leads to the following Sommerfeld estimate [24, Theorem 4.6].

Theorem 6. *Denote $A^{\rm TF}=(5C^{\rm TF})^3(3\pi^2)^{-1}$ and $\zeta=(\sqrt{73}-7)/2$. Then*

$$A^{\rm TF}\geqslant\varphi_Z^{\rm TF}(x)|x|^4\geqslant A^{\rm TF}-C(Z^{1/3}|x|)^{-\zeta}, \quad \forall x\neq 0 \qquad (2)$$

In particular, when $|x|\gg Z^{-1/3}$, then the TF potential $\varphi_Z^{\rm TF}$ is more or less independent of Z. This universality makes the TF approximation much more useful than what can normally be explained by its semiclassical nature. More precisely, the standard semiclassical analysis ensures that the TF theory gives a good approximation for the electron density in the distance $|x|\sim Z^{-1/3}$. However, we may expect that the TF theory gives a good approximation for larger distances, possibly up to $|x|\sim 1$. We refer to [25] for a detailed discussion.

4 Hartree–Fock theory

Invented shortly after the discovery of the Schrödinger equation, the Hartree–Fock (HF) theory has been a very useful computational method to describe electronic orbitals. In this theory, one restricts N-body wave functions to Slater determinants, or equivalently to their one-body density matrices which are trace class operators on $L^2(\mathbb{R}^3)$ satisfying

$$0\leqslant\gamma\leqslant 1, \quad \gamma=\gamma^2, \quad {\rm Tr}\gamma = N.$$

The HF ground state energy is

$$E^{\rm HF}(N) = \inf_{{\rm Tr}\gamma=N}\Big({\rm Tr}((-\Delta-Z|x|^{-1})\gamma) + \frac12\iint_{\mathbb{R}^3\times\mathbb{R}^3}\frac{\rho_\gamma(x)\rho_\gamma(y)-|\gamma(x;y)|^2}{|x-y|}dxdy\Big)$$

where $\rho_\gamma(x)=\gamma(x;x)$ (the kernel of γ is defined properly via the spectral decomposition).

The existence of Hartree–Fock minimizers when $N<Z+1$ was proved by Lieb and Simon in 1977. The nonexistence was proved later by Solovej in 2003 [24].

Theorem 7. *There exists a universal constant $C > 0$ such that if $N > Z + C$, then $E^{\mathrm{HF}}(N)$ has no minimizer.*

To explain the proof, let us go back to the heuristic argument discussed before. Assume that we have an efficient method to separate m outermost electrons. Then these particles see the rest of the system as a big nucleus with the effective nuclear charge $Z' = Z - (N - m)$. Thus by the Benguria–Lieb method, we may hope to get a bound like $m < 2Z' + 1$. Since Z' is smaller than Z, the loss of the factor 2 becomes less serious. If the procedure can be iterated to bring Z' down to order 1, then we can conclude that $N - Z$ is of order 1.

In [24], this approach is carried out rigorously by studying the screened nuclear potential

$$\Phi_Z^{\mathrm{HF}}(x) = \frac{Z}{|x|} - \int_{|y|\leqslant|x|} \frac{\rho^{\mathrm{HF}}(y)}{|x-y|}dy.$$

This function will be compared with the corresponding TF version

$$\Phi_Z^{\mathrm{TF}}(x) = \frac{Z}{|x|} - \int_{|y|\leqslant|x|} \frac{\rho^{\mathrm{TF}}(y)}{|x-y|}dy.$$

Similar to the TF potential $\varphi_Z^{\mathrm{TF}}(x)$, $\Phi_Z^{\mathrm{TF}}(x)$ behaves as $|x|^{-4}$ for $|x| \gg Z^{-1/3}$. It turns out that this property holds true for the HF screened potential as well. The key ingredient of the analysis in [24] is

Theorem 8. *There exist constants $C > 0$, $\epsilon > 0$ such that for all $x \neq 0$,*

$$|\Phi_Z^{\mathrm{HF}}(x) - \Phi_Z^{\mathrm{TF}}(x)| \leqslant C(1 + |x|^{-4+\epsilon}).$$

This estimate can be proved by induction in $|x|$. First, for $|x| \leqslant Z^{-1/3+\epsilon}$, it follows by the semiclassical approximation. For longer distances, one repeatedly uses the Sommerfeld estimate (2) to get refined information for "inner electrons", and then controls the "outer electrons" in terms of the screened potential. At the end of the day, the universality of the TF potential makes a miracle happen!

Let us explain why Theorem 8 implies the ionization bound. First, Theorem 8 implies that for $|x| = r \sim 1$,

$$\int_{|y|\leqslant r} \frac{\rho^{\mathrm{HF}}(y) - \rho^{\mathrm{TF}}(y)}{|x-y|}dy \leqslant C_r.$$

We can replace x by νx with $\nu \in S^2$, then average over ν and use Newton's theorem. This gives

$$Z' := \int_{|y|\leqslant r} \Big(\rho^{\mathrm{HF}}(y) - \rho^{\mathrm{TF}}(y)\Big)dy \leqslant C_r.$$

The number of outermost electrons, namely $\int_{|y|\geqslant r} \rho^{\mathrm{HF}}$, can be controlled by a constant time Z', leading to the final bound $N - Z \leqslant C$.

Clearly, this proof strategy requires an efficient way of splitting the problem from the inside and the problem from the outside. This can be done for the Hartree–Fock theory, because the energy functional has been greatly simplified to a one-body functional. For the N-body Schrödinger equation, such a splitting would require a novel many-body localisation technique which is not available at the moment.

5 Liquid drop model

Now let us turn to a related problem in the liquid drop model which is somewhat more classical than the ionization conjecture. This model was proposed by Gamow in 1928 and further developed by Heisenberg, von Weizsäcker and Bohr in the 1930s. Recently, it has gained renewed interest from many mathematicians [3].

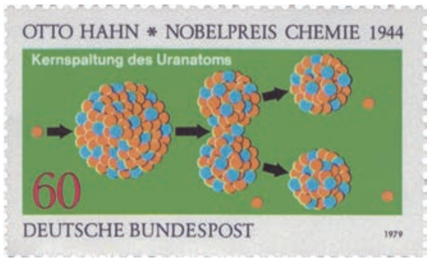

Figure 1. German stamp in 1979 honouring Otto Hahn (Wikipedia 2020)

In modern language, a nucleus is described in this theory by an open set $\Omega \subset \mathbb{R}^3$ which solves the minimisation problem

$$E^{\mathrm{G}}(m) = \inf_{|\Omega|=m} \left\{ \mathrm{Per}(\Omega) + \frac{1}{2}\int_\Omega\int_\Omega \frac{1}{|x-y|}dxdy \right\}.$$

Here m stands for the number of nucleons (protons and neutrons) and $\mathrm{Per}(\Omega)$ is the perimeter in the sense of De Giorgi (which is the surface area of Ω when the boundary is smooth). The Coulomb term captures the electrostatic energy of protons.

It is generally assumed in physics literature that if a minimizer exists, then it is a ball. Consequently, by comparing the energy of a ball of volume m with the energy of a union of two balls of volume $m/2$, one expects the nonexistence of minimizers if $m > m_*$ with

$$m_* = 5\frac{2 - 2^{2/3}}{2^{2/3} - 1} \approx 3.518.$$

Conjecture 9 ([2])**.** *If $m \leqslant m_*$, then $E^{\mathrm{G}}(m)$ is minimised by a ball. If $m > m_*$, then $E^{\mathrm{G}}(m)$ has no minimizer.*

In particular, the nonexistence of minimizers for large m is consistent with nuclear fission of heavy nuclei, which was discovered experimentally by Hahn and Strassmann in 1938.

Mathematically, it is nontrivial to analyse $E^{\mathrm{G}}(m)$ due to the energy competition: among all measurable sets of a given volume, a ball minimises the perimeter (by the isoperimetric inequality) but maximises the Coulomb self-interaction energy (by the Riesz rearrangement inequality).

In 2014, Knüpfer and Muratov [9] proved the following

Theorem 10. *There exist constants $0 < m_1 < m_2$ such that:*
(i) *If $m < m_1$, then $E^{\mathrm{G}}(m)$ has a unique minimizer which is a ball;*
(ii) *If $m > m_2$, then $E^{\mathrm{G}}(m)$ has no minimizer.*

The proof in [9] uses deep techniques in geometric measure theory, including a quantitative isoperimetric inequality proved by Fusco, Maggi and Pratelli in 2008. Independently, the existence of small m was proved by Julin [10] and the nonexistence of large m was proved by Lu and Otto [16]. In 2016, with Rupert Frank and Rowan Killip, we offered a new proof of the nonexistence which also provides the quantitative bound $m_2 \leqslant 8$. Let us explain the short proofs in [10] and [5].

Proof. Existence for m small [10]. Consider

$$D(\Omega) := \mathrm{Per}(\Omega) + \frac{1}{2}\int_\Omega\int_\Omega \frac{1}{|x-y|}dxdy - \mathrm{Per}(\Omega^*) - \frac{1}{2}\int_{\Omega^*}\int_{\Omega^*}\frac{1}{|x-y|}dxdy$$

where Ω^* is the ball centered at 0 with volume $|\Omega^*| = |\Omega| = m$. We need to prove that if m is small, then $D(\Omega) > 0$ unless Ω is a ball. Denote

$$f = \chi_{\Omega^*} - \chi_\Omega, \quad V = f * |x|^{-1}.$$

By a quantitative isoperimetric inequality in [8], there exists a universal constant $\epsilon_0 > 0$ such that after an appropriate translation of Ω, we have

$$\mathrm{Per}(\Omega) - \mathrm{Per}(\Omega^*) \geqslant \epsilon_0 \int_{\mathbb{R}^3}\frac{f(x)}{|x|}dx = \epsilon_0 V(0).$$

Note that by Hardy–Littlewood rearrangement inequality, $V(0) > 0$ unless Ω is a ball. For the Coulomb terms, we can write

$$\begin{aligned}\frac{1}{2}\int_\Omega\int_\Omega \frac{1}{|x-y|}dxdy &- \frac{1}{2}\int_{\Omega^*}\int_{\Omega^*}\frac{1}{|x-y|}dxdy \\ &= \frac{1}{2}\int_{\mathbb{R}^3}\int_{\mathbb{R}^3}\frac{f(x)f(y)}{|x-y|}dxdy + \int_{\mathbb{R}^3}\int_{\mathbb{R}^3}\frac{\chi_{\Omega^*}(x)f(y)}{|x-y|}dxdy \\ &= \frac{1}{8\pi}\int_{\mathbb{R}^3}|\nabla V(x)|^2dx + \int_{\Omega^*}V(x)dx.\end{aligned}$$

In the last equality we used $-\Delta V = 4\pi f$. This Poison equation also shows that V is superharmonic in Ω^* (as $f \geqslant 0$ in Ω^*), and hence

$$\int_{\mathbb{R}^3}\int_{\mathbb{R}^3}\frac{\chi_{\Omega^*}(x)f(y)}{|x-y|}dxdy = \int_{\Omega^*}V(x)dx \leqslant |\Omega^*|V(0) = mV(0).$$

Thus in summary, if $m < \epsilon_0$ and Ω is not a ball, then

$$D(\Omega) \geqslant (\epsilon_0 - m)V(0) > 0.$$

Nonexistence if $m > 8$ [5].
Assume that $E^{\mathrm{G}}(m)$ has a minimizer Ω. We split Ω into two parts, $\Omega = \Omega^+ \cup \Omega^-$, by a hyperplane H and then move Ω^- to infinity by translations. Since Ω is a minimizer, we obtain

$$\begin{aligned}\mathrm{Per}(\Omega) + \int_\Omega\int_\Omega\frac{1}{|x-y|}dxdy &\leqslant \mathrm{Per}(\Omega^+) + \int_{\Omega^+}\int_{\Omega^+}\frac{1}{|x-y|}dxdy \\ &\quad + \mathrm{Per}(\Omega^-) + \int_{\Omega^-}\int_{\Omega^-}\frac{1}{|x-y|}dxdy\end{aligned}$$

which is equivalent to

$$2\mathcal{H}^2(\Omega \cap H) \geqslant \int_{\Omega^+}\int_{\Omega^-}\frac{1}{|x-y|}dxdy.$$

Here $\mathcal{H}^2$ is the two-dimensional Hausdorff measure. Next, we parameterise:

$$H = H_{\nu,\ell} = \{x \in \mathbb{R}^3 : x\cdot\nu = \ell\}$$

with $\nu \in S^2$, $\ell \in \mathbb{R}$. The above inequality becomes

$$2\mathcal{H}^2(\Omega \cap H_{\nu,\ell}) \geqslant \int_\Omega\int_\Omega \frac{\chi(\nu\cdot x > \ell > \nu\cdot y)}{|x-y|}dxdy.$$

Integrating over $\ell \in \mathbb{R}$ and using Fubini's theorem we get

$$2|\Omega| \geqslant \int_\Omega\int_\Omega\frac{[\nu\cdot(x-y)]_+}{|x-y|}dxdy.$$

Finally, averaging over $\nu \in S^2$ and using

$$\int [\nu\cdot z]_+ \frac{d\nu}{4\pi} = \frac{|z|}{2}\int_0^{\pi/2}\cos\theta\sin\theta d\theta = \frac{|z|}{4}$$

with $z = (x-y)$, we conclude that $2|\Omega| \geqslant \frac{1}{4}|\Omega|^2$, namely $|\Omega| \leqslant 8$. □

With Rupert Frank and Hanne Van Den Bosch, we used the cutting argument in the liquid drop model to study the ionization problem in the Thomas–Fermi–Dirac-von Weisäcker theory in [6], and in the Müller density matrix functional theory in [7]. In these theories, the standard Benguria–Lieb method does not apply, but we can replace it by an appropriate modification of the minimizers, leading to an efficient control of the number of particles "outside" in terms of particles "inside". This enables us to employ Solovej's bootstrap argument to establish the uniform bound $N - Z \leqslant C$.

6 Related problems

The ionization problem is an example for a question that is easy to find in physics textbooks, but difficult to answer mathematically. Below we list some related open problems for the Schrödinger operator H_N.

The main concept in the ionization problem is that in a large atom, although most of electrons stay in the domain $|x| \sim Z^{-1/3}$, the binding property only depends on a few outermost electrons in the region $|x| \sim 1$. In fact, only this outer region is relevant to chemical reactions in everyday life. Therefore, an important quantity of an atom is its radius. To fix the notation, we define the radius R_Ψ of a wave function Ψ by requiring

$$\int_{|x|\geqslant R_\Psi}\rho_\Psi(x)dx = 1.$$

Conjecture 11 ([24])**.** *There exist two universal constants $0 < R_1 < R_2$ such that if $N \geqslant Z$ and E_N has a minimizer Ψ, then $R_1 \leqslant R_\Psi \leqslant R_2$.*

Another important quantity is the ionization energy $I_N = E_{N-1} - E_N$.

Conjecture 12 ([15, 23])**.** *There exists a universal constant $C > 0$ such that if $N \geqslant Z$, then $I_N \leqslant C$.*

Conjecture 13 ([15])**.** *The function $N \mapsto I_N$ is non-increasing (equivalently $N \mapsto E_N$ is convex).*

See [20] for partial results on Conjectures 11 and 12. A consequence of Conjecture 13 is that if $E_{N-1} > E_N$ (namely the nucleus can bind N electrons), then $E_{N-2} > E_{N-1}$ (the nucleus can bind $N-1$ electrons). This "obvious fact" is still not proved mathematically!

So far we have only focused on the ground state problem for H_N. Recall from the HVZ theorem that the essential spectrum of H_N is $[E_{N-1}, \infty)$. Conjecture 2 mainly concerns the existence of eigenvalues below E_{N-1}. Since the existence of embedded eigenvalues is generally not expected, we have the following stronger version of Conjecture 2.

Conjecture 14. *There exists a universal constant $C > 0$ such that if $N > Z + C$, then H_N has no eigenvalue.*

The last issue has been studied by Lenzmann and Lewin in [11], who proved that H_N has no eigenvalue if $N > 4Z + 1$.

This question is related to the scattering theory of dispersive PDEs with long-range interaction potentials, which is interesting in its own right.

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Geometry and the Simplex: Results, Questions and Ideas

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When we try to understand an object, first we have to describe it. And for many objects, the best description, in the sense of what we can do with it, is a combinatorial one.

What has always fascinated me is how little we know about such combinatorial descriptions, up close. One case that often arouses interest is a triangulation; a simplicial complex that models a topological or algebraic object.

Let me explain what I mean: we know pretty well what to do with a triangulated manifold if we do not care about the triangulation in question, that is, if we are allowed to retriangulate. Then we are really interested in the global, topological properties rather than the combinatorial ones. Consider instead, for example, the question of counting the number of vertices in a triangulation. The issue arises: this is an invariant of the underlying simplicial chain complex that is not recognised by the homology.

This is often in contrast to geometry, where we understand the local-to-global correspondence in a much better way (though it is far from perfect). This goes so far that even some of the interesting results on triangulated manifolds are proven using a geometric detour, such as Cheeger's trick of seeing a triangulated manifold as a smooth one with singular points [Che86].

That is not to say that smooth questions are easy, but the technology we have, in the form of differential geometry, is developed much further. So we pay a heavy price for the fact that we can actually work with these spaces, encode them, and are still trying to make many of the smooth calculations work in the discrete setting, such as computing characteristic classes [GM92].

I will attempt in this note to discuss some of the techniques that are used to understand such combinatorial-geometric problems, and will start with an algebrao-geometric technique.

1 Complexity of embeddings

Consider the following combinatorial question: We are given a simplicial complex Δ that we know embeds into the euclidean space $\mathbb{R}^d$. How complicated can Δ be? That is, for instance, how many faces of dimension k can it have, given the number of faces of dimension $i < k$?

If we, for example, embed into dimension $\mathbb{R}^2$, then the answer is classical, and Descartes or Euler have already answered it: a planar graph without double edges or loops can have at most three times as many edges E (1-dimensional simplices) as it has vertices V, i.e. we have the famous inequality

$$E \leq 3V$$

What about, say, complexes of dimension two and higher? This is an important question by Grünbaum from 1967, later refined by Kalai and Sarkaria [Grü03, Kal91].

The interesting question then is to embed into dimension 4. Well, we know in principle what to do: we could attempt to use the van Kampen obstruction and topological techniques. However, these techniques often see mostly the topology of the complex, not its combinatorics. That makes it supremely difficult to extract a good bound, and it means that even if Wagner's theorem extends in some sense, it is difficult to tickle it enough to get a practical bound from those versions, such as [Nev07].

A second fact that helps in the planar setting: we can always extend a planar graph that is large enough (at least 3 vertices) to a triangulation of the plane without adding any edges. The same is not possible in higher dimensions. So, an entirely new trick is needed. A key result of [Adi18] is as follows:

Theorem 1.1. *Given a simplicial complex Δ that piecewise-linearly embeds in $\mathbb{R}^4$, then*

$$T \leq 4E,$$

that is, the number of triangles T (2-dimensional simplices) exceeds the number of edges E (1-dimensional faces) by a factor of at most 4.

This result is asymptotically tight, though it can be improved to a tight result by appending an additive error term [Adi18, Remark 4.9]. The result extends similarly to higher dimensions (with a similar bound depending linearly on the embedding dimension), but several questions, including the main one, remain open:

Problem 1.2. *Does the result extend to topological embeddings? Does it extend to cell complexes that are strongly regular, that is cell complexes whose partially ordered set of faces is an atomistic lattice?*

The best result towards topological embeddings is due to Parsa [Par18], who proved that

$$T \leq cEV^{\frac{1}{3}},$$

where V is the number of vertices. It seems that we have to develop some new techniques for the topological case.

To prove the bound of Theorem 1.1, the idea is to move away from simplicial homology, and instead consider a topological model where points itself generate meaningful topology. This is, topologically, encoded in the notion of moment-angle complexes, a space built out of replacing the simplices of the complex with tori of different dimensions [BP15]. Rather than work with these torus complexes, it is more convenient to think about it in algebraic terms. So we follow Melvin Hochster's idea, see [Sta87], modelled after Chow rings of toric varieties.

2 Face rings

If Δ is an abstract simplicial complex defined on the groundset $[n] := \{1, \cdots, n\}$, let $I_\Delta := \langle \mathbf{x}^{\mathbf{a}} : \text{supp}(\mathbf{a}) \notin \Delta \rangle$ denote the nonface ideal in $\mathbb{R}[\mathbf{x}]$, where $\mathbb{R}[\mathbf{x}] = \mathbb{R}[x_1, \cdots, x_n]$. Let $\mathbb{R}^*[\Delta] := \mathbb{R}[\mathbf{x}]/I_\Delta$ denote the **face ring** of Δ. Now, we pick a sufficient number of linear forms to make sure the quotient is finite dimensional:

The reduced face ring with respect to such a system Θ is

$$\mathcal{A}^*(\Delta) := \mathbb{R}^*[\Delta]/\Theta\mathbb{R}^*[\Delta].$$

The following theorem summarises observations by Hochster, Reisner and Stanley:

Theorem 2.1 (Face numbers and Poincaré duality). *For a triangulated sphere Σ of dimension $(d-1)$,*

$$h_i(\Sigma) = \dim \mathcal{A}^i(\Sigma).$$

Moreover, the pairing

$$\mathcal{A}^i(\Sigma) \times \mathcal{A}^{d-i}(\Sigma) \to \mathcal{A}^d(\Sigma) \cong \mathbb{R}$$

is perfect.

Here,

$$h_k := \sum_{i=0}^{k} (-1)^{k-i} \binom{d-i}{k-i} f_{i-1},$$

where f_j denotes the number of faces of dimension j in a simplicial complex.

Now, to return to our original question: If the embedding of Δ is piecewise-linear, then it is not hard to see (and proved for instance in [Bin83]) that Δ extends to a triangulation of a piecewise-linear sphere Σ of dimension $2k$.

It is now not hard to notice that the quotient $\mathcal{A}^*(\Delta) = \mathcal{A}^*(\Sigma)/I_\Delta$ satisfies

$$\dim \mathcal{A}^k(\Delta) \geq f_{k-1}(\Delta)$$

and

$$\dim \mathcal{A}^{k+1}(\Delta) \leq f_k(\Delta) - (k+1) f_{k-1}(\Delta)$$

Notice further that

$$\mathcal{A}^k(\Sigma) \cong \mathcal{A}^{k+1}(\Sigma)$$

by the Poincaré duality property above.

It then remains to establish the inequality

$$\dim \mathcal{A}^k(\Delta) \geq \dim \mathcal{A}^{k+1}(\Delta) \qquad (1)$$

3 Biased pairing properties and Lefschetz

Here are two critical observations concerning the above inequality.

- It follows from the *Lefschetz property*, i.e. the isomorphism

$$\mathcal{A}^k(\Sigma) \xrightarrow{\cdot\ell} \mathcal{A}^{k+1}(\Sigma)$$

induced by multiplication of some element ℓ in $\mathcal{A}^1(\Sigma)$. This is an important, and difficult to prove property from algebraic geometry [Laz04], and is known in the case that Σ is the boundary of a polytope. The desired inequality (1) follows from

$$\begin{array}{ccc} \mathcal{A}^k(\Sigma) & \xrightarrow{\cdot\ell} & \mathcal{A}^{k+1}(\Sigma) \\ \downarrow & & \downarrow \\ \mathcal{A}^k(\Delta) & \xrightarrow{\cdot\ell} & \mathcal{A}^{k+1}(\Delta) \end{array}$$

- It follows from the *biased pairing property*. Consider the kernel I of the map $\mathcal{A}(\Sigma) \to \mathcal{A}(\Delta)$. Then the desired inequality (1) follows from saying that

$$I^k \times I^{k+1} \to \mathcal{A}^d(\Sigma) \cong \mathbb{R}$$

is nondegenerate in the first factor. It is a little tricky to give context for this property, it does seem to have been used before.

Let us put these properties into a tiny bit of context.

4 Interlude: The classical and non-classical Lefschetz theorems

If $\Sigma = \partial P$, where P is a d-dimensional polytope, and one takes Θ to be the linear system induced by the coordinates of P, and ℓ is the sum of variables, then $\mathcal{A}(\Sigma)$ satisfies the Lefschetz property with respect to ℓ. Moreover, if we consider the *Hodge–Riemann bilinear form*

$$\begin{array}{cccccc} \mathrm{Q}_{\ell,k}: & \mathcal{A}^k(\Sigma) & \times & \mathcal{A}^k(\Sigma) & \longrightarrow & \mathcal{A}^d(\Sigma) \cong \mathbb{R} \\ & a & & b & \longmapsto & \deg(ab\ell^{d-2k}) \end{array}$$

then it is definite of sign $(-1)^k$ on the kernel

$$\ker[\mathcal{A}(\Sigma)^k \xrightarrow{\cdot\ell^{d-2k+1}} \mathcal{A}(P)^{d-k+1}].$$

These are the so-called Hodge–Riemann relations.

Unfortunately, most spheres Σ do not arise as boundaries of convex polytopes [Alo86, GP86]. And convexity is crucial: the proof here follows an idea by McMullen [McM96], and the current wave of combinatorial Lefschetz theorems in Coxeter groups [EW14] or matroids [AHK18] all use his basic but amazing idea.

The idea in [Adi18] is different: I discuss what happens for triangulations of general spheres, where Hodge–Riemann relations fail, and instead turn into the so-called Hall–Laman relations, which signify the non-degeneracy of the Hodge–Riemann form on subspaces cut out by squarefree monomial ideals, that is, exactly the ideals arising as kernels of maps

$$I(\Sigma, \Delta) := \mathcal{A}(\Sigma) \longrightarrow \mathcal{A}(\Delta).$$

Let me try to give an overview of the ideas:

5 Back to biased pairings

Now, there are several critical observations that relate the biased pairing property (for all squarefree monomial ideals) and the Lefschetz property, setting up a way to prove the Lefschetz property inductively. Some of the central observations are that *Lower dimensional Lefschetz implies biased pairing*, and that *biased pairing proves Lefschetz*. For the first, the following proposition provides a glimpse

Proposition 5.1. *Assume Δ is a rational hypersurface sphere in a sphere Σ of dimension $2k-1 = d$. Then $\mathcal{A}(\Sigma)$ satisfies biased Poincaré duality in degree k and with respect to $I(\Sigma, \Delta)$ if and only if*

$$\mathcal{A}^k(\Delta) = 0.$$

Note that $\mathcal{A}_k(\Delta) = 0$ is a Lefschetz property: forget one of the elements of the linear system of parameters Θ of $\mathcal{A}(\Sigma)$, arrive at a new and shorter system Θ' and an additional element ϑ. Then the second property is equivalent to the Lefschetz isomorphism for

$$\mathbb{R}^{k-1}[\Delta]/\langle\Theta'\rangle \xrightarrow{\cdot\vartheta} \mathbb{R}^{k}[\Delta]/\langle\Theta'\rangle.$$

Second, we have the following rather beautiful lemma, essentially due to Kronecker:

Lemma 5.2. *Given two linear maps*

$$A, B : \mathcal{X} \longrightarrow \mathcal{Y}$$

of two vector spaces $\mathcal{X}$ and $\mathcal{Y}$ over $\mathbb{R}$ (or any infinite field). Assume that

$$B(\ker A) \cap \operatorname{im} A = 0 \subset \mathcal{Y}.$$

Then a generic linear combination A"+"B of A and B has kernel

$$\ker(A \text{ "+" } B) = \ker A \cap \ker B.$$

The connection to the classical Hall matching theorem, which constructs stable matchings in a discrete setting [Hal35]. This lemma is designed to do the same in the setting of linear maps. The idea is now to prove the following *transversal prime property*: for W a set of vertices in Σ if

$$\ker \text{"}\sum_{v\in W}\text{"}\, x_v = \bigcap_{v\in W} \ker x_v$$

Note: proving the transversal prime property for all vertices together is equivalent to the Lefschetz isomorphism

$$\mathcal{X} = \mathcal{A}^k(\Sigma) \xrightarrow{\cdot\ell} \mathcal{Y} = \mathcal{A}^{k+1}(\Sigma)$$

for ℓ the generic linear combination over all variables. This is because

$$\bigcap_{v \text{ vertex of } \Sigma} \ker x_v = 0$$

because of Poincaré duality.

Note further that, to see how the biased pairing property implies the transversal property by induction on the size of the set W, when we try to apply the criterion by multiplying with a new variable x_v, adding a vertex v to the set W, then we are really pulling back to a principal ideal $\langle x_v\rangle$ in $\mathcal{A}(\Sigma)$, and being asked to prove that $x_v\ker$ "$\sum_{v\in W}$" and im "$\sum_{v\in W}$" $\cap\langle x_v\rangle$ intersect only in 0.

Note finally that both spaces are orthogonal complements. This is the case if and only if the Poincaré pairing is perfect when restricted to either (or equivalently both) of them.

That closes the circle, and gives us a glimpse of the ideas in [Adi18], though the proof takes a detour we do not go over here. We refer the reader to the more friendly introduction [Adi19] to get a better idea.

6 Some relations

Let me mention two interesting applications of the individual results above:

Spaces of low rank tensors and the potential for a trivial lemma

First, let me note that Lemma 5.2, while it seems trivial, has some quite interesting consequences. It tells us that spaces of low-rank maps are restricted. For instance: Consider a space L of tensors in $V_1 \otimes V_2 \otimes \cdots \otimes V_n$, where V_i are vector spaces over some infinite field.

Define $\mathrm{r}(L)$ to be minimum

$$\dim V_1' + \dim V_2' + \ldots$$

so that L lies in subspace

$$V_1' \otimes V_2 \otimes \cdots \otimes V_n + V_1 \otimes V_2' \otimes \cdots \otimes V_n +$$

With David Kazhdan and Tamar Ziegler we proved recently that

$$\mathrm{r}(L) \le C_n \max_{\ell \in L} \mathrm{r}(\ell)$$

This in particular implies that Schmidt rank is linearly bounded from above by Gowers–Wolf analytic rank for cubics, notions important in analytic combinatorics, see for instance [Lov19]. Indeed, it seems to be a powerful trick to construct high-rank linear maps, and I think Kronecker's lemma might see some interesting further use down the line, in particular to construct Lefschetz isomorphisms in non-algebraic settings (for example [Ven17]). See also [Gur02] for a connection to quantum matchings.

A relation to the Dodziuk–Singer conjecture

Let us also take a moment to discuss an interesting relation of Proposition 5.1 to the Dodziuk–Singer conjecture [Dod79], alleging that the ℓ^2-cohomology of the universal cover $\widetilde{M}$ of an aspherical d-manifold M has vanishing ℓ^2-cohomology, except possibly in the middle dimension.

Assume now that M is triangulated. A central result of [Adi18] is that

$$\ker\left[\mathcal{A}^i(M) \longrightarrow \bigoplus_{v \text{ vertex of } M} \mathcal{A}^i(\mathrm{st}_v M)\right] \cong (H^{i-1}(M))^{\binom{d}{i}}$$

where st denotes the closed neighbourhood of a vertex. This extends to ℓ^2-cohomology, and with Proposition 5.1 it follows that if D_i is a family of compact disks exhausting $\widetilde{M}$, then the Dodziuk–Singer conjecture is true for $\widetilde{M}$ if

$$\mathbb{R}^{k-1}[\partial D_i]/\langle\Theta'\rangle \xrightarrow{\cdot\vartheta} \mathbb{R}^{k}[\partial D_i]/\langle\Theta'\rangle. \qquad (2)$$

are isomorphisms and uniformly bounded as operators.

In some situations, such as the case of right-angled Coxeter groups, it is possible to then define a limit of the rings $\mathbb{R}^{k-1}[\partial D_i]$, with individual elements of the sequence connected by pullback maps. This then leads us to a Hilbert space with a graded algebra structure inherited from the $\mathbb{R}^{k-1}[\partial D_i]$, which we need to establish a Lefschetz property on. It seems promising to understand in this context the relation between the work by McMullen [McM96] and Alesker [Ale03] on Lefschetz theorems for valuations, which also stand in a slightly indirect limit relation to each other.

7 How small can you make a combinatorial space, then?

Now you have had a taste of what I am interested in: understanding combinatorial questions in a new algebraic and geometric light, reformulating them, and then proving interesting algebraic and geometric theorems using combinatorial means. We have seen some relations to the Lefschetz property, but I want to change directions and discuss questions where a closer understanding of a (discrete) differential geometry seems to be interesting, though we come full circle in the end.

A central problem is often how large a given manifold has to be. This requires specification of what I want to be large or small. One way that low-dimensional topologists like to go, for instance, is to ask for volume and specify a geometric structure (e.g., hyperbolic). Volume, of course, makes sense in the context of bounded geometry, see for instance [CDMW18] for some recent deep results.

Another can be the number of faces a minimal triangulation must have. These measures are tangentially related, but should not be confused with Gromov's notion of simplicial volume [Gro82], which is asking about cell decompositions rather than triangulations.

About the latter, we know embarassingly little. Let us just ask for the minimal number of vertices, that is, how many vertices you need to triangulate a given topological space (assuming you can).

We do know what, for example, the smallest triangulation of a ball is, or what the smallest triangulation of the sphere is (hint: it is the simplex and its boundary.)

We also know that the number of vertices cannot be lower than the ball-category (*the number of disks needed to cover a manifold*), or the more studied Lusternik–Schnirelmann category (*the number of contractible sets needed to cover a manifold*) [CLOT03]. In particular it is bounded from below in terms of the cup length of the space in question. In fact, it is easy to show, and observed by Arnoux and Marin, that for a space of cup length n, one needs $\binom{n+2}{2}$ vertices [AM91].

Satoshi Murai also gave a lower bound in terms of the Betti numbers of (closed and orientable) manifolds [Mur15], which was simplified and generalised to general manifolds by Adiprasito and Yashfe [Adi18, AY20]. Essentially, we have that if M is a triangulated $(d-1)$-manifold on n vertices (allowing for non-orientability and boundary), then

$$\binom{d}{j}\mathrm{b}_{j-1}(M) + \binom{d}{j-1}\mathrm{b}_{d-j}(M) \leq \binom{n-d+j-1}{j} \qquad \text{for } 1 \leq j \leq \frac{d}{2}.$$

This bound in general is not so good for interesting manifolds, as it seems insensitive to any interesting multiplicative structure in the cohomology ring, let alone homotopy.

Sounds like we know a lot, right? Unfortunately, these bounds are far from tight. Just consider some of the manifolds we learn about first in a topology course:

- Triangulate $\mathbb{RP}^n$. The observation of Arnoux and Marin is best, really, and gives a lower bound of quadratic size in n. But triangulations that small are hard to come by. For a long time, the best construction was exponential in n, though we at least broke through that barrier recently [AAK20].
- Triangulate the torus $(S^1)^n$. The smallest known triangulation, due to Kühnel and Lassman [KL88], needs $2^{n+1}-1$ vertices, and it seems challenging to construct smaller examples. Indeed, as far as we know, this number may even be tight. One is tempted to compare this to a systolic inequality, though I do not know how to make this connection sufficiently precise.

A problem here is that we just do not have good geometric invariants that tell us meaningful answers about the combinatorial size of a complex. The other issue is that constructions in a combinatorial setting are hard to come by, especially if they are to be the small(est), in some sense.

One of the hardest constructions I have ever managed to do was for polytopes with "small" moduli space, that is, where the moduli space of a polytope is the space of polytopes with the same combinatorics, modulo projective transformations. The smallest possible here is a point, and with Ziegler I managed to construct infinitely many polytopes whose moduli space is a point in dimension 69 [AZ15]. Many difficult ideas usually go into these constructions, such as partial differential relations in the former, and for instance probablistic techniques in my possibly favourite example, the counterexample to the extension space conjecture by Gaku Liu [Liu20]. But several related problems remain open, for instance:

Problem 7.1. *Are there infinitely many combinatorially distinct types of polytopes of dimension* 4 *whose moduli space is a point?*

You cannot hope to do the same for three-dimensional polytopes, where the number is finite due to Steinitz' theorem [Ste22]. It implies that the space of realisations of a polytope is of dimension of the number of edges of the polytope plus 6 (if we ignore projective transformations), so polytopes we look for can have at most nine edges. The number of such polytopes is finite (hint: it is four.)

Let me give an example coming from topology that has puzzled me recently.

Consider the following problem: I task you to give me many simplicial complexes. To make it simple, let them only be of dimension 2. I give you n triangles to build them with, and you are asked to make them combinatorially distinct.

Can you make superexponentially many?

Ok, that is actually easy. Let us make it more interesting. Every vertex should only be incident to a bounded, say 1000, number of triangles.

Still, you can construct examples: simply construct a long strip of triangles, and attach some handles.

Ok, final restriction. Please make it contractible.

Problem 7.2. *Is the number of contractible complexes with a given number of triangles n and a uniform bound on the vertex degree exponential, or superexponential? What happens if I only restrict to complexes with vanishing reduced homology?*

Note that any family of complexes one constructs cannot be too simple. For example, it follows from [BZ11] that collapsible complexes are not enough, and neither are complexes whose Andrews–Curtis complexity, that is, the number of Nielsen operations to reshape it into the trivial presentation, is bounded. It seems that "simply connected" is the real obstruction here, and not contractible. So one could equally ask the question and demand that only the first homotopy group or homology group should vanish.

8 Geometry of polyhedra

On the other hand, constructions are not everything. We have to understand the other direction, the restrictions geometry imposes, better as well. Let me begin by asking a question which is rather famous in combinatorics:

Problem 8.1. *Given a triangulation of a connected d-manifold on n vertices, how many steps does one need from any facet to any other facet?*

Here, steps that are allowed are to go from one d-simplex to the next via a $(d-1)$-simplex they share. The main question here: Is this diameter a polynomial in n and d? The answer, so far, is not known. This of course is a version of the polynomial Hirsch conjecture, and related to the question of how long the simplex algorithm could take in the worst case.

What is known is that under geometric restrictions, this diameter is well behaved. For instance, assume that the triangulation satisfies Gromov's no-Δ condition [Gro87]. Then the natural spherical metric is CAT(1), that is, its sectional curvature is bounded above by 1 in the Alexandrov sense. This means neighbourhoods of vertices are convex by a classical result in geometric group theory. Following a shortest path between any two facets gives a diameter of $n - d + 1$ [AB14].

This suggests a stronger relationship between geometric notions of polyhedra and differential geometry that needs to be explored, but for now, we have only very special cases where this is achieved, usually in the form of rather strong comparison theorems.

9 p-curvatures and small intervals

It is tempting to think that every metric sphere has to have a point of positive curvature. This is indeed true if one considers sectional curvature (this is the classical Cartan-Hadamard theorem). On the other hand, Lohkamp proved that spheres and other manifolds of dimension at least 3 admit metrics of negative Ricci curvature [Loh94]. Here is a question that sits in between.

Problem 9.1. *Is it true that for every ℓ, there exists a $k = k(\ell)$ such that every d-polytope (or polyhedral sphere), $d \geq k$, has a Boolean interval of length ℓ in its face lattice?*

The closest geometric analogue is a question asking for obstructions on Gromov's *p-curvature*, rather than the obstructions to curvature bounds studied classically, leading to a tempting Ramsey-like geometric problem.

Problem 9.2 (Adiprasito-Kalai 2015)**.** *Is it true that for every $p \geq 2$ there exists a $k = k(p)$, such that every Riemannian metric R on S^d, $d \geq k$ has a point x and a p-dimensional subspace M of T_xR, such that the average over sectional curvatures at 2-sections in M is non-negative?*

Temptingly, the answer to the second problem is for conjectures to be "no way", especially with the experience taught by differential geometry. The first problem (which seems to be due to Perles originally), on the other hand, is conjectured to have a positive answer at least, and is known to have such an answer for small ℓ, see [KKM00].

10 Stoker conjecture and comparison theorems for mean curvature

Let us close (and close the circle), address a question by Mikhail Gromov, and highlight it. In a recent series of lectures on scalar curvature and comparison theorems, [Gro19] put forward the following question concerning polytopes (though I heard it independently from Arseniy Akopyan):

Conjecture 10.1. *Assume P and Q are combinatorially equivalent polytopes, such that the dihedral angles of P are bigger or equal than the corresponding dihedral angles of Q. Then the polytopes are normally equivalent, that is, their normal fans are related by rotation and reflection.*

It should be noted that the problem is easy for polygons, and that it is a simple exercise for simple polytopes (i.e. d-dimensional polytopes in which every facet is incident to at most d edges).

It is also true infinitesimally, that is, any infinitesimal deformation of a polytope that does not increase any dihedral angle that does not increase any dihedral angle must leave them constant, and can be continued to a normal equivalence. This follows in various ways: by the Schläfli formula [SS03], the angles must stay constant. According to Weiss' work [Wei05], the desired normal equivalence follows from here.

The alternative is to dualise, to take us back and return to the Lefschetz properties we started with: Consider the normal fan of P. The problem then turns to asking whether the geodesic arcs cut by 2-dimensional cones inside the sphere can be infinitesimally deformed without elongating any one of them. It follows from the Hodge–Riemann relations in Section 4 that this cannot happen unless the deformation gives an isometry of the sphere. Indeed, applying them for $k = 1$ shows that their length is given as a local minimum of an optimisation problem.

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The Power of 2: Small Primes in Number Theory

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From Euclid to Gauss

The first proposition in Euclid's *Elements* gives the construction, with ruler and compass, of the equilateral triangle. Later, Euclid shows how to construct a regular n-gon for $n = 5$ and $n = 15$, and how to pass from a construction of the regular n-gon to a construction of the regular $2n$-gon. For which other values of n does a construction of the regular n-gon using ruler and compass exist?

The answer to this ancient question was given roughly 2000 years later, by Gauss. In 1796, Gauss showed that the regular 17-gon could be constructed, and eventually showed in *Disquisitiones Arithmeticae* that the n-gon is constructible when n is of the form $n = 2^k p_1 \dots p_r$, where $p_1, \dots, p_r$ are distinct *Fermat primes*, i.e., prime numbers of the form $F_m = 2^{2^m} + 1$.

Gauss' success in making progress on this question rested on the fact that the Fermat number $F_2 = 17$ is a prime. Legend has it that it was Gauss' pleasure in proving this that made his mind up to pursue mathematics as a career. The next Fermat number, $F_3 = 65537$, is also prime, but no other Fermat primes are known. Today's readers of the *Disquisitiones* can be thankful that the list of Fermat primes does not end at $F_1 = 5$!

My research is in algebraic number theory, and in particular the Langlands program, which aims to give the ultimate non-abelian generalisation of class field theory, a topic which has its roots in topics treated in *Disquisitiones* (consider quadratic reciprocity, the ideal class group and the reduction theory of binary quadratic forms, to name but a few).

Much research in this part of number theory pulls in ideas from many other parts of mathematics (geometry, representation theory, analysis, …) – anything that will help reach the final goal. And in many cases, a lucky numerical coincidence helps to push us over the finishing line. My aim in this article is to introduce the reader to some of the remarkable recent research on the subject (as well as some of my own work), with a particular eye for the supporting roles played by small primes.

Cyclotomic fields

Let us first explain the modern point of view on the ideas behind Gauss' construction. First, identifying the plane with $\mathbb{C}$, one sees that it is enough to construct the n^{th} roots of unity. Second, one sees that the complex numbers which are constructible are precisely those that can be seen inside a tower of quadratic field extensions of the field $\mathbb{Q}$ of rational numbers. The challenge, therefore, is to explain when $e^{2\pi i/n}$ is contained in such a field extension.

This is precisely the kind of question that Galois theory is equipped to answer. Indeed, the number $e^{2\pi i/n}$ generates the n^{th} cyclotomic field $K_n = \mathbb{Q}(e^{2\pi i/n})$. The *Galois correspondence* states that there are as many subfields of K_n as there are subgroups of its Galois group $\mathrm{Gal}(K_n/\mathbb{Q})$, the group of all automorphisms of K_n. Galois theory shows that K_n can be obtained by iterated quadratic extensions precisely when its Galois group has order a power of 2.

One can show that there is an isomorphism $\mathrm{Gal}(K_n/\mathbb{Q}) \cong (\mathbb{Z}/n\mathbb{Z})^\times$, so we see that the n-gon is constructible precisely when the value $\phi(n)$ of Euler's totient function is a power of 2. Elementary number theory shows the equivalence of this condition with the criterion given by Gauss.

Class field theory

Cyclotomic fields are the most basic examples of abelian extensions of number fields. A number field is a field extension $L/\mathbb{Q}$ which can be obtained by adjoining finitely many algebraic numbers. We say that $L/\mathbb{Q}$ is Galois if L can be obtained by adjoining all (not just some) of the roots of a fixed polynomial with rational coefficients. If this condition is satisfied, then the Galois group $\mathrm{Gal}(L/\mathbb{Q})$ of automorphisms of L acts on L (and permutes these roots). We say that $L/\mathbb{Q}$ is abelian when it is Galois and its Galois group $\mathrm{Gal}(L/\mathbb{Q})$ is abelian. As we have seen, this class of number fields includes the cyclotomic fields K_n.

A fundamental additional structure carried by the Galois group of a number field is the presence, for each prime p, of a conjugacy class of subgroups $D_p \subset \mathrm{Gal}(L/\mathbb{Q})$. We call D_p the decomposition group at the prime p; it may be defined as the subgroup of automorphisms of L which are continuous with respect to the topology given by an absolute value on L extending the p-adic absolute value $|\cdot|_p$ on $\mathbb{Q}$ (the completion of which gives the field $\mathbb{Q}_p$ of p-adic rational numbers). Much of the charm of algebraic number theory comes from the interaction between global phenomena (e.g., the arithmetic of the field L) and local phenomena (e.g., the structure of the group D_p and the arithmetic of the completions of L with respect to its p-adic absolute values).

The decomposition group comes with a normal subgroup I_p, the inertia group, and a canonical generator for the cyclic quotient D_p/I_p, called the Frobenius element Frob_p. For all but finitely many primes p (which are said to be unramified in L) the inertia group is trivial, and we obtain a well-defined conjugacy class of elements of $\mathrm{Gal}(L/\mathbb{Q})$. There is a similar story when $\mathbb{Q}$ is replaced by any base number field K.

We can now explain the importance of abelian extensions L/K of number fields. When the Galois group is abelian, the Frobenius elements are well-defined (not just up to conjugacy). This is the mechanism by which class field theory, one of the great achievements of mathematics in the first half of the 20th century, describes all abelian extensions of a given number field: it gives a canonical surjection from a generalised ideal class group of K to the group $\mathrm{Gal}(L/K)$, uniquely characterised by the requirement that the class of a prime ideal of the ring of integers of K is sent to the corresponding Frobenius element.

Serre's conjecture

The Langlands program should include, as a special case, a non-abelian generalisation of class field theory. By duality, we can think of class field theory as giving a correspondence between the 1-dimensional representations of Galois groups of number fields and the irreducible representations of generalised ideal class groups. The Langlands conjectures would describe n-dimensional representations of Galois groups in terms of automorphic representations, which play the role of characters of ideal class groups.

As a window into this circle of ideas, we are now going to describe Serre's conjecture, which aims to give an "automorphic" parameterisation of 2-dimensional representations of Galois groups over $\mathbb{Q}$ in characteristic p. Serre published his conjecture in 1987 [12]. It is closely related to, but not equivalent to, the Langlands conjectures, and has had a tremendous influence on their study.

We introduce some necessary notation. Let $\overline{\mathbb{Q}}$ be an algebraic closure of $\mathbb{Q}$, and let $G_{\mathbb{Q}} = \mathrm{Gal}(\overline{\mathbb{Q}}/\mathbb{Q})$ be the absolute Galois group, equipped with its Krull topology. If $L/\mathbb{Q}$ is any Galois number field (contained in $\overline{\mathbb{Q}}$) then there is a continuous surjection $G_{\mathbb{Q}} \to \mathrm{Gal}(L/\mathbb{Q})$.

Let p be a prime. The first class of objects we consider in Serre's conjecture consists of continuous representations $\overline{\rho} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{F}}_p)$, with coefficients in the algebraic closure of the finite field $\mathbb{F}_p$ of p elements. We say that $\overline{\rho}$ is of S-type if it is irreducible and if $\det \overline{\rho}(c) = -1$, where $c \in G_{\mathbb{Q}}$ is complex conjugation. A typical source of such representations is in the p-torsion subgroups of elliptic curves. We will discuss this example in more detail below.

The second class of objects appears inside the cohomology groups of arithmetic groups, which play a role analogous to that of the generalised ideal class groups in class field theory. If $N \geq 1$ is an integer, then we define $\Gamma_1(N)$ to be the subgroup of matrices

$$\begin{pmatrix} a & b \\ c & d \end{pmatrix} \in \mathrm{SL}_2(\mathbb{Z})$$

satisfying the congruence conditions $c \equiv 0 \bmod N$, $a \equiv d \equiv 1 \bmod N$. The (group) cohomology groups $H^*(\Gamma_1(N), \overline{\mathbb{F}}_p)$ are finite-dimensional vector spaces. More generally, if we are given a pair of integers k, t with $2 \leq k \leq p+1$ and $0 \leq t \leq p-2$, then we can consider $V_{k,t} = \mathrm{Sym}^{k-2}\, \overline{\mathbb{F}}_\ell^2 \otimes \det^t$, a finite-dimensional, irreducible representation of $\mathrm{GL}_2(\mathbb{F}_p)$, hence the cohomology groups $H^*(\Gamma_1(N), V_{k,t})$.

The group $\Gamma_1(N)$ is a *congruence subgroup* of the group $\mathrm{GL}_2(\mathbb{Q})$. As a consequence, its cohomology groups receive additional symmetries, in the form of an action of the *Hecke algebra* of $\mathrm{GL}_2(\mathbb{Q})$. For each prime $\ell \nmid Np$, there is a distinguished Hecke operator T_ℓ which acts on the vector space $H^*(\Gamma_1(N), V_{k,t})$, and can be thought of as playing the role of the ideal class of a prime ideal in class field theory. If ℓ_1, ℓ_2 are two such primes then the operators T_{ℓ_1}, T_{ℓ_2} commute. Consequently, there exist elements $v \in H^*(\Gamma_1(N), V_{k,t})$ which are simultaneous eigenvectors for all of the Hecke operators T_ℓ ($\ell \nmid Np$). We call the collection $(a_\ell)_{\ell \nmid Np}$ of eigenvalues in $\overline{\mathbb{F}}_p$ a system of Hecke eigenvalues of level N and weight (k, t).

We are now ready to state Serre's conjecture.[1] The first approximation is that for any S-type representation, there exists N, (k, t) and a system of Hecke eigenvalues of level N and weight (k, t) such that for all $\ell \nmid Np$,

$$\mathrm{tr}\, \overline{\rho}(\mathrm{Frob}_\ell) = a_\ell.$$

The full conjecture asserts that the smallest possible N with this property should be equal to the conductor $N(\overline{\rho})$, an integer which measures the ramification of the representation $\overline{\rho}$, and that the possible (k, t)'s for which this system of Hecke eigenvalues appears can be described explicitly using a recipe which depends only on the restriction of $\overline{\rho}$ to the inertia group $I_p \subset G_{\mathbb{Q}}$.

This conjecture is remarkable on many levels. It implies that the set of isomorphism classes of S-type representations $\overline{\rho} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{F}}_p)$ of bounded conductor is finite (since the cohomology groups on the automorphic side are finite-dimensional). This elementary statement has no known proof that does not rely on automorphic forms. More generally, the cohomology groups can be computed algorithmically, and this information used to give precise information on the existence (or otherwise) of specific Galois representations. An amazing application of this is the Cremona database, which lists all elliptic curves E over $\mathbb{Q}$ of conductor $N(E) \leq 5 \times 10^5$ [13].

Also significant for the development of the subject has been the paradigm suggested by the conjecture: the most optimistic form of a local-global principle relating the relative position of decomposition groups in the Galois groups of number fields to the cohomology of arithmetic groups. From this point of view one can explain the existence of Ribet's level-raising and level-lowering congruences [10,11] between modular forms using a simple computation with Galois representations. The recipe for the set of weights (k, t) which should give rise to $\overline{\rho}$ suggests the existence of a close relationship between the representation theory of the decomposition group $D_p \subset G_{\mathbb{Q}}$ and that of the group $\mathrm{GL}_2(\mathbb{Z}_p)$. This theme that has been made precise in the Breuil–Mézard conjecture [3] and has seen its ultimate expression in the formulation of the p-adic Langlands correspondence for $\mathrm{GL}_2(\mathbb{Q}_p)$ [2].

Fermat's Last Theorem

Another reason for the great interest of Serre's conjecture is that it implies Fermat's Last Theorem, following a famous gambit using the Frey curve associated to a putative non-trivial solution to the Fermat equation. Indeed, suppose given a solution

$$a^p + b^p = c^p$$

[1] In fact, Serre's formulation uses the reduction modulo p of certain spaces of automorphic forms in the place of the cohomology of arithmetic groups. We have followed Buzzard–Diamond–Jarvis [4] in using cohomology, since it is both easier to describe and more amenable to generalisation, for example to base fields other than $\mathbb{Q}$. The existence of the Eichler–Shimura isomorphism implies that the systems of Hecke eigenvalues are the same in either case.

to the Fermat equation, where $p \geq 5$ is a prime and a, b, c are coprime integers such that $abc \neq 0$. (It is enough to consider this case, since the non-existence of solutions in exponents 3 and 4 was already proved by Euler and Fermat, respectively.) To this solution one associates the elliptic curve E given by the equation

$$E : y^2 = x(x - a^p)(x + b^p).$$

Thus E is an algebraic curve of genus one, defined over $\mathbb{Q}$, which therefore admits a structure of commutative group variety, with identity element given by the unique point at infinity. The complex points $E(\mathbb{C})$ are isomorphic, as a Lie group, to $S^1 \times S^1$; consequently, if $n \geq 1$ is an integer, then the n-torsion points $E[n](\mathbb{C})$ form a finite abelian group abstractly isomorphic to $(\mathbb{Z}/n\mathbb{Z})^2$. Since the group operations of E are defined over $\mathbb{Q}$, these points are defined over the subfield $\overline{\mathbb{Q}} \subset \mathbb{C}$ of algebraic numbers. We write $L_n/\mathbb{Q}$ for the number field generated by the x, y co-ordinates of the non-trivial n-torsion points.

Then $L_n/\mathbb{Q}$ is a Galois extension, and its Galois group $\mathrm{Gal}(L_n/\mathbb{Q})$ acts on $E[n](\mathbb{C})$. Choosing a basis for this free $\mathbb{Z}/n\mathbb{Z}$-module gives a Galois representation

$$\overline{\rho}_{E,n} : \mathrm{Gal}(L_n/\mathbb{Q}) \to \mathrm{GL}_2(\mathbb{Z}/n\mathbb{Z}).$$

Our discussion up to this point would apply equally well for any elliptic curve over $\mathbb{Q}$. However, something very special happens for our curve, associated to a non-trivial solution to the Fermat equation in degree p. Indeed, in this case the number field L_p associated to the p-torsion points turns out to have very little ramification, essentially because the discriminant $\Delta(E)$ of E is (up to powers of 2) a p^{th} power.

We can make this precise by computing the invariants N, (k, t) attached to the representation $\overline{\rho}_{E,p}$ (which is of S-type). Assuming, as we may, that $a \equiv 3 \bmod 4$ and $b \equiv 0 \bmod 2$, we find that $N = 2$, $(k, t) = (2, 0)$. Serre's conjecture implies that the representation $\overline{\rho}_{E,p}$ should be associated to a system of Hecke eigenvalues occurring in $H^1(\Gamma_1(2), \overline{\mathbb{F}}_p)$. This is a contradiction! Indeed, the space $H^1(\Gamma_1(2), \overline{\mathbb{F}}_p)$ is 1-dimensional, and the unique system of Hecke eigenvalues it carries is not associated to a representation of S-type (in fact, it is associated to a reducible Galois representation).

Of course, Fermat's Last Theorem was proved first by Wiles in 1993, more than 10 years before the proof by Khare and Wintenberger of Serre's conjecture. Wiles' proof introduced a vast number of new ways to study the relation between Galois representations and automorphic forms, many of which appear again in an essential way in the work of Khare–Wintenberger. However, the route that Wiles followed to Fermat's Last Theorem is essentially the one we have outlined above: he proved the modularity of the elliptic curve E, hence of the representation $\overline{\rho}_{E,p}$, at level $N = N(E)$. Ribet's level-lowering results, alluded to earlier, imply the modularity of $\overline{\rho}_{E,p}$ at level $N = N(\overline{\rho}_{E,p}) = 2$, leading to a contradiction.

This is an appropriate moment to explain what it means for a general elliptic curve E over $\mathbb{Q}$ to be modular. In fact, it is simpler from our point of view to explain what it means for a Galois representation

$$\rho : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{Q}}_p)$$

with coefficients in the algebraic closure of the field $\mathbb{Q}_p$ of p-adic rationals to be modular. Exactly as in the case of $\overline{\mathbb{F}}_p$-coefficients, the cohomology groups $H^*(\Gamma_1(N), \overline{\mathbb{Q}}_p)$ receive actions of the pairwise commuting Hecke operators T_ℓ, defined for each prime $\ell \nmid Np$. We say that ρ is modular of level N and weight[2] $(2, 0)$ if there exists a simultaneous eigenvector $v \in H^1(\Gamma_1(N), \overline{\mathbb{Q}}_p)$ for the Hecke operators T_ℓ such that for all $\ell \nmid Np$, ρ is unramified at ℓ and we have the equality

$$\mathrm{tr}\, \rho(\mathrm{Frob}_\ell) = \text{eigenvalue of } T_\ell \text{ on } v.$$

One appealing feature here is that these cohomology groups, together with their Hecke operators, are defined over $\mathbb{Q}$: they arise by base extension from the vector space $H^1(\Gamma_1(N), \mathbb{Q})$. If $v \in H^1(\Gamma_1(N), \overline{\mathbb{Q}})$ is a simultaneous eigenvector for all of the Hecke operators T_ℓ ($\ell \nmid N$), then the eigenvalues generate a number field $K = \mathbb{Q}(\{a_\ell\}_{\ell \nmid N})$. Associated to v is a *compatible system* of Galois representations $\rho_\lambda : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{Q}}_p)$, one for each prime p and choice of embedding $\lambda : K \to \overline{\mathbb{Q}}_p$.

If E is an elliptic curve, then for any prime p we can glue the representations $\overline{\rho}_{E,p^n} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\mathbb{Z}/p^n\mathbb{Z})$ together into a representation $\rho_{E,p^\infty} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\mathbb{Q}_p)$. These representations form a compatible system, and we say that E is modular if one (equivalently, all) of them is modular in the above sense.

Modularity lifting theorems

The most important innovation in Wiles' work is probably the concept of the modularity lifting theorem. To explain this, we first recall that the topology on $\overline{\mathbb{Q}}_p$ is defined by the p-adic absolute value $|\cdot|_p$. The set of elements of absolute value at most 1 is a subring, denoted $\overline{\mathbb{Z}}_p$, and the set of elements of absolute value strictly less than 1 is an ideal in this subring, denoted $\mathfrak{m}_{\overline{\mathbb{Z}}_p}$. The quotient $\overline{\mathbb{Z}}_p/\mathfrak{m}_{\overline{\mathbb{Z}}_p}$ may be identified with $\overline{\mathbb{F}}_p$.

If $\rho : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{Q}}_p)$ is a continuous representation, then we may conjugate ρ to take values in $\mathrm{GL}_2(\overline{\mathbb{Z}}_p)$, and reduce modulo the ideal $\mathfrak{m}_{\overline{\mathbb{Z}}_p}$ to obtain a representation $\overline{\rho} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{F}}_p)$. The character of $\overline{\rho}$ is determined by that of ρ.

We have defined what it means for $\overline{\rho}$ to be modular, and also what it means for ρ to be modular. It is natural to ask how these concepts are related. One direction is easy: if ρ is modular, then so is $\overline{\rho}$, as can be shown by considering the reduction modulo p of classes in $H^1(\Gamma_1(N), \overline{\mathbb{Q}}_p)$. Much harder is to go in the opposite direction. In general, there are many more systems of Hecke eigenvalues occurring in $H^1(\Gamma_1(N), \overline{\mathbb{Q}}_p)$ than the analogous group with $\overline{\mathbb{F}}_p$-coefficients. This reflects the existence of plentiful congruences between modular forms, and is the source both of the difficulty of the problem and of the power of its solution.

Wiles proved the first modularity lifting theorem, stating that for a representation ρ satisfying some technical conditions, the modularity of $\overline{\rho}$ implies the modularity of ρ. (These technical conditions usually take the form of a global condition on $\overline{\rho}$, for example that it is irreducible, and some necessary local conditions on ρ, for example that $\rho|_{D_p}$ can be realised inside an abelian variety.)

To prove the modularity of an elliptic curve E using such a theorem, one needs to choose a prime p (with the aim of proving ρ_{E,p^∞} is modular) and verify the modularity of the residual

[2] One can consider other weights (k, t); this amounts to replacing $\overline{\mathbb{Q}}_p$ by a non-trivial coefficient system, just as we have done above in the mod p case.

representation $\overline{\rho}_{E,p}$. Wiles chooses $p = 3$, and makes use of the following two remarkable coincidences: first, that the reduction homomorphism $\mathrm{GL}_2(\mathbb{Z}_3) \to \mathrm{GL}_2(\mathbb{F}_3)$ has a splitting $s : \mathrm{GL}_2(\mathbb{F}_3) \to \mathrm{GL}_2(\mathbb{Z}_3)$; second, that the resulting representation $s \circ \overline{\rho}_{E,3} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\mathbb{Z}_3)$ has soluble image, and so can be shown to be modular using earlier work of Langlands–Tunnell (in particular, Langlands' proof of cyclic base change and descent for automorphic forms on GL_2). Neither of these two facts holds for any prime $p > 3$. Combining these observations with his modularity lifting theorem, Wiles is able to prove the modularity of semistable elliptic curves E, provided that $\overline{\rho}_{E,3}$ is irreducible.

To treat the remaining case, Wiles introduces another famous technique, the "3–5 switch". To prove the modularity of an elliptic curve E such that $\overline{\rho}_{E,3}$ is reducible, he introduces an auxiliary elliptic curve A with the property that $\overline{\rho}_{A,5} \cong \overline{\rho}_{E,5}$ and $\overline{\rho}_{A,3}$ is irreducible. This is possible since $X(\overline{\rho}_{E,5})$, the modular curve which parameterises those elliptic curves A such that $\overline{\rho}_{A,5} \cong \overline{\rho}_{E,5}$, is isomorphic to $\mathbb{P}^1_{\mathbb{Q}}$, and therefore has infinitely many rational points (once again, this would be false if 5 was replaced here by any larger prime.) The modularity of A follows using the argument of the previous paragraph. Finally, applying the modularity lifting theorem with $p = 5$ we deduce the modularity of E.

The proof of Serre's conjecture

As evidenced by Wiles' proof, modularity lifting theorems become especially potent in the presence of compatible systems of Galois representations. In fact, this combination formed the basis of the proof of Serre's conjecture by Khare and Wintenberger, in which the "3–5 switch" becomes a "p–P"-switch, where p, P are primes which become arbitrarily large.

Let us sketch the earlier proof by Khare [7] of the $N = 1$ case of Serre's conjecture, i.e., the modularity of S-type representations $\overline{\rho} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{F}}_p)$ which are unramified outside p. The argument is by induction on the prime p; in fact, it is enough to show the truth of the conjecture for infinitely many primes.

The base cases of the induction is the case $p = 2$. As we saw earlier, $H^1(\Gamma_1(2), \overline{\mathbb{F}}_2)$ is essentially trivial, leading to the expectation that the conjecture is vacuously true in the case $p = 2$: there are no irreducible representations. This is true, and was proved directly by Tate by analysing the discriminant of the number field cut out by a putative irreducible representation $\overline{\rho}$. This is a more sophisticated version of the elementary deduction, from Minkowski's bound, that there is no number field $L/\mathbb{Q}$ which is unramified everywhere.

What about the induction step? Suppose that the conjecture is true for a given prime p, and fix a second prime $P > p$ and an irreducible representation $\overline{\rho} : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{F}}_P)$. The first step is to lift $\overline{\rho}$ to a compatible system of representations $\rho_\lambda : G_{\mathbb{Q}} \to \mathrm{GL}_2(\overline{\mathbb{Q}}_\lambda)$. Taking a p-adic member ρ_p of this compatible system, one hopes to verify the residual modularity of $\overline{\rho}_p$ (by induction) and then apply a modularity lifting theorem to deduce the modularity of ρ_p. We can then appeal, as in the case of the compatible systems of Galois representations associated to elliptic curves, to the fact that the modularity of a single member of a compatible family is equivalent to the modularity of every member.

Carrying out this argument in practice is subtle because of the technical conditions imposed by modularity lifting theorems, and would not have been possible without important refinements of the modularity lifting theorems in Wiles' original in work by many authors. Another key ingredient is a technique for constructing lifts of $\overline{\mathbb{F}}_p$-representations $\overline{\rho}$ to $\overline{\mathbb{Q}}_p$-representations with prescribed local behaviour (for example, with the same conductor N as $\overline{\rho}$). The existence of such lifts is a key consequence of Serre's conjecture; Khare and Wintenberger turned this on its head and established it, using modularity lifting theorems, on the way to proving the full conjecture.

Symmetric power functoriality

We now discuss applications of these kinds of techniques to a different problem. Symmetric power functoriality refers to a special case of Langlands' functoriality conjectures, which suggest a beautiful set of relations between automorphic forms on different reductive groups. These relations should reflect, through Langlands duality, the relations between the Langlands dual groups. The most basic relations are those associated to the symmetric powers of the standard representation of GL_2.

Both the shape and the importance of these conjectures can be motivated by considering the case of modular elliptic curves over $\mathbb{Q}$: in this case, they are related to the now-proved Sato–Tate conjecture. If E is an elliptic curve over $\mathbb{Q}$, then for all primes $p \nmid N(E)$, the curve E has good reduction, and it makes sense to consider the set of $\mathbb{F}_p$-points of E. Write n_p for the cardinality of this set; then Hasse's theorem implies the estimate

$$|p + 1 - n_p| \leq 2\sqrt{p}.$$

The Sato–Tate conjecture[3] concerns the distribution of the normalised error terms $a_p = (p + 1 - n_p)/2\sqrt{p} \in [-1, 1]$ as the prime p varies: it states that the numbers $\{a_p \mid p < X\}$ become equidistributed as $X \to \infty$ with respect to the Sato–Tate measure $\frac{2}{\pi}\sqrt{1 - t^2}dt$.

Serre identified how one might hope to prove the Sato–Tate conjecture, using a method inspired at some level by the Hadamard–de la Vallée Poussin proof of the prime number theorem. The crux is to consider the so-called symmetric power L-functions associated to the elliptic curve E.

Recall first that the L-function associated to an elliptic curve E over $\mathbb{Q}$ is defined as an Euler product[4]

$$L(E, s) \doteq \prod_p (1 - a_p p^{-s} + p^{1-2s})^{-1},$$

where s is a complex variable. Hasse's theorem implies that this product converges absolutely in the right half-plane $\mathrm{Re}\, s > 3/2$. In fact, the resulting holomorphic function admits an analytic continuation to the whole complex plane; this is a consequence of the modularity of the elliptic curve E. The famous Birch–Swinnerton-Dyer conjecture relates the group $E(\mathbb{Q})$ of rational points of E to the leading coefficient in the Taylor expansion of this function at the point $s = 1$ [14].

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3 This statement is valid provided that E does not have complex multiplication (CM). When E does have CM, a different measure must be used, reflecting the existence of the curve's additional symmetries.

4 Some care is needed to define the Euler factors at the primes $p|N(E)$; we elide this detail here.

For each $n \geq 1$, we may equally define the n^{th} symmetric power L-function as follows. Let $\mathrm{Sym}^n : \mathrm{GL}_2 \to \mathrm{GL}_{n+1}$ denote the n^{th} symmetric power of the standard (identity) representation of GL_2. Let $t_p \in \mathrm{GL}_2(\mathbb{C})$ be a matrix with characteristic polynomial $\det(X - t_p) = X^2 - a_p X + p$. Then we define

$$L(E, \mathrm{Sym}^n, s) \doteq \prod_p \det\left(1 - p^{-s}\,\mathrm{Sym}^n(t_p)\right)^{-1}.$$

(When $n = 1$, Sym^1 is the standard representation and $L(E, \mathrm{Sym}^1, s) = L(E, s)$.) Once again, this Euler product converges absolutely in a right-half plane $\mathrm{Re}\, s > 1 + n/2$. Serre's observation was that if all the symmetric power L-functions can be shown to admit an analytic continuation to the whole complex plane, non-vanishing on the line $\mathrm{Re}\, s = 1 + n/2$, then the Sato–Tate conjecture follows.

These properties of the symmetric power L-functions follow from the Langlands conjectures! Indeed, $L(E, \mathrm{Sym}^n, s)$ would be precisely the standard L-function associated to an automorphic representation of GL_{n+1}, which deserves to be called the symmetric power lifting of the automorphic representation of GL_2 associated to E. These standard L-functions are known to have the required analytic continuation and non-vanishing properties.

The Sato–Tate conjecture for elliptic curves has now been proved. It turns out that the necessary analytic properties of the symmetric power L-functions can be proved to follow from the *potential automorphy* of the functorial lifts. More precisely, it is enough to show that the symmetric power L-functions admit meromorphic continuation and are holomorphic and non-vanishing on the appropriate line; these properties follow from the automorphy of the Galois representations $\mathrm{Sym}^n \rho_{E,p^\infty}|_{G_{M_n}}$, for some (inexplicit) totally real number field $M_n/\mathbb{Q}$. This was established in a series of works (culminating in [1]) based on Taylor's technique of potential automorphy and relying on contributions to the Langlands program by many other mathematicians.

This year, James Newton and I proved the automorphy of the symmetric power L-functions of elliptic curves; this shows in particular that they have the expected analytic (as opposed to merely meromorphic) continuation to the entire complex plane. More generally, we showed that for any automorphic representation of GL_2 which contributes to the cohomology of congruence subgroups of $\mathrm{GL}_2(\mathbb{Q})$, all of the symmetric power lifts exist, as predicted by Langlands' conjectures [8, 9].

We now sketch the proof of this result in the essential case of automorphic representations of level 1 (equivalently, which contribute to the cohomology of $\mathrm{SL}_2(\mathbb{Z})$ for some choice of coefficient system); this includes the important case of the representation generated by Ramanujan's Δ-function

$$\Delta(q) = q \prod_{n=1}^{\infty} (1 - q^n)^{24}.$$

Our proof is based on the existence of the Coleman–Mazur eigencurve $\mathcal{E}_p$ [6], which can be defined for any fixed prime p, and is a kind of a universal p-adic family of systems of Hecke eigenvalues. The eigencurve $\mathcal{E}_p$ is a 1-dimensional p-adic rigid analytic space, which admits a morphism

$$\mathcal{E}_p \to \mathcal{W}$$

to the space $\mathcal{W} = \mathrm{Hom}(\mathbb{Z}_p^\times/\{\pm 1\}, \mathbb{G}_m)$, called weight space. The eigencurve $\mathcal{E}_p$ has a dense set of "classical points" corresponding to pairs $(\{a_\ell\}_{\ell \nmid p}, \alpha_p)$, where $\{a_\ell\}_\ell$ is a system of Hecke eigenvalues appearing in some group $H^1(\mathrm{SL}_2(\mathbb{Z}), \mathrm{Sym}^{k-2}\, \overline{\mathbb{Q}}^2)$ and α_p is a root of the Hecke polynomial $X^2 - a_p X + p^{k-1}$; the image of this classical point in weight space is the character $x \mapsto x^{k-2}$.

The density of these classical points is a reflection of the fact that systems of Hecke eigenvalues can be put in p-adic families, in which the Hecke eigenvalues vary continuously (in the p-adic topology) as a function of the weight k. The non-classical points of $\mathcal{E}_p$ can be interpreted as arising from the systems of Hecke eigenvalues appearing in p-adic "overconvergent" cohomology groups.

The first step in our proof is to show that (for fixed n) the automorphy of the n^{th} symmetric power lifting is a property which is constant on irreducible components of $\mathcal{E}_p$: put another way, we can analytically continue the functorial lift along irreducible components of the eigencurve. We are also able to establish the existence (using modularity lifting theorems) of *some* modular forms for which a given symmetric power lift exists.

Each irreducible component of the eigencurve contains infinitely many classical points, so this shows at least that for each n, infinitely many modular forms admit a symmetric power lifting. This does not yet solve the problem completely, since the irreducible components of the eigencurve, and its global geometry more generally, remain mysterious.

We have not yet specified a choice of prime p. We now choose $p = 2$. Buzzard and Kilford were able to compute a large part of the 2-adic eigencurve $\mathcal{E}_2$, namely the part 'close to the boundary of weight space' [5]. When $p = 2$ the group $\mathbb{Z}_p^\times/\{\pm 1\}$ is free and $\mathcal{W}$ may be identified with the open p-adic disc $\{|w| < 1\}$. By the boundary of weight space, we mean the annulus $\{1/8 < |w| < 1\}$. Buzzard–Kilford showed that above this boundary annulus, the geometry of the eigencurve in fact becomes very simple: a countably infinite collection of open annuli, each of which maps isomorphically to the boundary of weight space.

To finish the proof, we need only to show that each of this infinite collection of boundary annuli inside $\mathcal{E}_2$ meets an irreducible component over which the symmetric power lifting exists. This we can achieve by combining our freedom to analytically continue along components with the freedom to move between the two classical points corresponding to the two roots of the Hecke polynomial $X^2 - a_p X + p^{k-1}$: above the boundary of weight space, this has the effect of jumping between different boundary annuli in $\mathcal{E}_2$.

We conclude with a concrete numerical consequence of the Buzzard–Kilford theorem: if $n \geq 3$ and $\chi : (\mathbb{Z}/2^n\mathbb{Z})^\times \to \overline{\mathbb{Q}}_2^\times$ is a primitive character such that $\chi(-1) = 1$, then the space of cuspidal modular forms of level 2^n, weight 2, and character χ has dimension 2^{n-3}, and the 2-adic valuations of the eigenvalues of the U_2 operator are the numbers in $(\frac{1}{2^{n-3}}\mathbb{Z}) \cap (0, 1)$, each appearing with multiplicity 1.

This statement, a beautiful generalisation of the triviality of $H^1(\Gamma_1(2), \overline{\mathbb{Q}}_2)$ which underpins the proof of Fermat's Last Theorem, is the essential starting point for our proof of symmetric power functoriality for holomorphic modular forms.

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Multiplicative Functions in Short Intervals, with Applications

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The understanding of the behaviour of multiplicative functions in short intervals has significantly improved during the past decade. This has also led to several applications, in particular concerning correlations of multiplicative functions.

1 Introduction

Let us start by defining the key players. A function $f\colon \mathbb{N} \to \mathbb{C}$ is said to be multiplicative if $f(mn) = f(m)f(n)$ whenever $\gcd(m,n) = 1$. We define the Liouville function $\lambda\colon \mathbb{N} \to \{-1,1\}$ by $\lambda(n) := (-1)^k$ when n has k prime factors (counted with multiplicity). For instance, $\lambda(45) = \lambda(3 \cdot 3 \cdot 5) = (-1)^3 = -1$. The function $\lambda(n)$ is clearly multiplicative.

It is well known that the average value of $\lambda(n)$ is 0, i.e.

$$\lim_{X\to\infty} \frac{1}{X} \sum_{n\le X} \lambda(n) = 0. \tag{1}$$

In other words, about half of the numbers have an odd number of prime factors and half of the numbers have an even number of prime factors. The result (1) is actually equivalent to the prime number theorem, asserting that

$$\lim_{X\to\infty} \frac{|\{p \le X : p \in \mathbb{P}\}|}{X/\log X} = 1, \tag{2}$$

where $\mathbb{P}$ denotes the set of prime numbers. In this article, the letter p will always denote a prime.

It will be very convenient for us to use $o(1)$ and $O(1)$ notations, so that $A = o(B)$ means that $|A|/B \to 0$ for $X \to \infty$ and $A = O(B)$ means that $|A| \le CB$ for some constant $C > 0$ depending only on subscripts of O. In this notation (1) and (2) can be written as

$$\sum_{n\le X} \lambda(n) = o(X) \tag{3}$$

and

$$\sum_{p\le X} 1 = \frac{X}{\log X} + o\left(\frac{X}{\log X}\right). \tag{4}$$

Before discussing the Liouville function further, let us define another important object: write $\zeta\colon \mathbb{C} \to \mathbb{C}$ for the Riemann zeta function which is defined by

$$\zeta(s) := \sum_{n=1}^{\infty} \frac{1}{n^s} = \prod_{p\in\mathbb{P}} \left(1 - \frac{1}{p^s}\right)^{-1} \quad \text{for } \Re s > 1, \tag{5}$$

where $\Re s$ denotes the real part of s. A series of the type $\sum_{n\in\mathbb{N}} a_n n^{-s}$ with $a_n \in \mathbb{C}$ is called a Dirichlet series. The ζ-function can be analytically continued to the whole complex plane apart from a simple pole at $s = 1$.

It is easy to see that $\zeta(s)$ has no zeros with $\Re s > 1$, and furthermore for $\Re s < 0$ the only zeros are the "trivial zeros" at negative even integers. The remaining zeros with $0 \le \Re s \le 1$ are called the non-trivial zeros. One of the most famous open problems in mathematics, the Riemann hypothesis, asserts that all these non-trivial zeros satisfy $\Re s = 1/2$.

The zeros of the zeta function are closely related to the behaviour of the Liouville function. This relation stems from the fact that, for $\Re s > 1$

$$\frac{1}{\zeta(s)} = \prod_{p\in\mathbb{P}} \left(1 - \frac{1}{p^s}\right) = \sum_{n=1}^{\infty} \frac{\lambda(n)\mathbf{1}_{n \text{ square-free}}}{n^s}, \tag{6}$$

where $\mathbf{1}_{n \text{ square-free}}$ denotes the characteristic function of the set of integers that are not divisible by a square of a prime.

The function $\mu(n) := \lambda(n)\mathbf{1}_{n \text{ square-free}}$ is called the Möbius function and its behaviour is very similar to that of the Liouville function. Consequently, the zeros of $\zeta(s)$ correspond to the poles of the Dirichlet series $\sum_{n\in\mathbb{N}} \mu(n)n^{-s}$ that is closely related to the Liouville function.

One can show through (6) that (3) (and thus also the prime number theorem (4)) is equivalent to the fact that the Riemann zeta function has no zeros with $\Re s = 1$. The equivalence with the prime number theorem stems from the fact that, for $\Re s > 1$, one has

$$-\frac{\zeta'}{\zeta}(s) = \sum_{k=1}^{\infty} \sum_{p\in\mathbb{P}} \frac{\log p}{p^{ks}},$$

so the zeros of the zeta-function also correspond to the poles of a Dirichlet series that is closely related to the characteristic function of the primes.

In general, the Liouville function is expected to behave more or less randomly. In particular, we expect that it has so-called square-root cancellation, i.e. one has, for all $X \ge 2$,

$$\sum_{n\le X} \lambda(n) = O_\epsilon(X^{1/2+\varepsilon}) \quad \text{for any } \varepsilon > 0. \tag{7}$$

The conjecture (7) is in fact equivalent to the Riemann hypothesis, and proving (7) even with $X^{1/2+\varepsilon}$ replaced by $X^{1-\delta}$ with a small fixed δ seems to be a distant dream which would correspond to the Riemann zeta function having no zeros with real part $\ge 1-\delta$ for some fixed $\delta > 0$. The best result currently is that

$$\sum_{n\le X} \lambda(n) = O\left(X \exp\left(-\frac{C(\log X)^{3/5}}{(\log\log X)^{1/5}}\right)\right) \tag{8}$$

for some absolute constant $C > 0$. This follows from the Vinogradov–Korobov zero-free region for the Riemann zeta function that has been essentially unimproved for sixty years.

2 Short intervals

A natural question is whether the average of the Liouville function is still $o(1)$ if taken over short segments; one can ask how slowly H can tend to infinity with X so that we are guaranteed to have

$$\sum_{X<n\le X+H} \lambda(n) = o(H), \tag{9}$$

so that in the segment $(X, X + H]$ roughly half of the numbers have an even and half of the numbers have an odd number of prime factors.

The bound (8) together with the triangle inequality immediately implies (9) when

$$H \ge X \exp\left(-\frac{C(\log X)^{3/5}}{2(\log\log X)^{1/5}}\right).$$

However, one can show this for much shorter intervals. In 1972, Huxley proved prime number theorem in short intervals by showing that, for any $\varepsilon > 0$,

$$\sum_{X<p\le X+H} 1 = \frac{H}{\log X} + o\left(\frac{H}{\log X}\right) \quad \text{for } H \ge X^{7/12+\varepsilon}.$$

Subsequently, in 1976 Motohashi [9] and Ramachandra [12] independently showed that Huxley's ideas also work in the case of the Liouville function, showing that, for any $\varepsilon > 0$, (9) holds for $H \ge X^{7/12+\epsilon}$.

The proofs of these results are based on zero-density estimates for the Riemann zeta function, i.e. estimates that give an upper bound for the number of zeta zeros in the rectangle

$$\{s \in \mathbb{C} \colon \Re(s) \in [\sigma, 1] \text{ and } |\Im(s)| \le T\}$$

for given $\sigma \in (1/2, 1]$ and $T \ge 2$.

Using the multiplicativity of $\lambda(n)$ in a crucial way, (9) was recently shown to hold for $H \ge X^{0.55+\varepsilon}$ for any $\varepsilon > 0$ by Teräväinen and the author [7]. However, this result is still very far from what is expected to be true – random models suggest that (9) holds in intervals much shorter than $H = X^\varepsilon$.

One can ask what about (9) in almost all short intervals rather than all (we say that a statement holds for almost all $x \le X$ if the cardinality of the exceptional set is $o(X)$). In this case, the techniques based on the proof of Huxley's prime number theorem get one down to $H \ge X^{1/6+\varepsilon}$ for any $\varepsilon > 0$; whereas assuming the Riemann hypothesis, Gao has proved it (in an unpublished work) for $H \ge (\log X)^A$ for certain fixed $A > 0$.

All the results discussed so far, with the exception of the very recent work [7], have their counterparts for the primes. Given these similarities and the so-called parity phenonenom, it is natural that until recently the problems for the primes and for the Liouville function have been expected to be of equal difficulty.

However, in the recent years this expectation has turned out to be wrong; in [2], Radziwiłł and the author made a breakthrough on understanding the Liouville function in short intervals by proving the following.

Theorem 1. *Let $X \ge H \ge 2$. Assume that $H \to \infty$ with $X \to \infty$. Then*

$$\sum_{x<n\le x+H} \lambda(n) = o(H) \tag{10}$$

for almost all $x \le X$.

Note that H can go to infinity arbitrarily slowly here, e.g. $H = \log\log\log\log X$, so this unconditionally improves upon Gao's result that was conditional on the Riemann hypothesis.

While our method fundamentally fails for the primes, it does work for more general multiplicative functions (see [2, Theorem 1]):

Theorem 2. *Let $X \geq H \geq 2$. Let $f\colon \mathbb{N} \to [-1,1]$ be multiplicative. Assume that $H \to \infty$ with $X \to \infty$. Then*
$$\left|\frac{1}{H}\sum_{x<n\leq x+H} f(n) - \frac{1}{X}\sum_{n\leq X} f(n)\right| = o(1)$$
for almost all $x \leq X$.

For many applications, it is helpful to also have a result for complex-valued functions, and such an extension can be found in the recent pre-print [3], where we also extend our result in other directions, as we will explain below.

3 Applications

Already in [2] we presented several applications of our general theorem such as

Corollary 3. *For any $\varepsilon > 0$, there exists a constant $C = C(\varepsilon)$ such that, for all large x, the interval $(x, x + C\sqrt{x}]$ contains x^ε-smooth numbers (i.e. numbers whose all prime factors are $\leq x^\varepsilon$).*

Corollary 4. *There exists a constant $\delta > 0$ such that the Liouville function has $\geq \delta X$ sign changes up to X.*

Starting from [4] our work [2] has led to several applications concerning correlations of multiplicative functions. Chowla's conjecture from the 1960s concerning correlations of the Liouville function asserts that, whenever $h_1, \ldots, h_k$ are distinct, one has
$$\sum_{n\leq X} \lambda(n+h_1)\cdots\lambda(n+h_k) = o(X).$$
This is in line with the general philosophy that additive and multiplicative structures are independent of each other.

Chowla's conjecture can be equivalently stated as saying that, for any $k \geq 1$, each sign pattern $(\varepsilon_1, \ldots, \varepsilon_k) \in \{-1,1\}^k$ appears in the sequence $(\lambda(n+1), \ldots, \lambda(n+k))_{n\in\mathbb{N}}$ with density $1/2^k$.

Given the analogues between the primes and the Liouville function, Chowla's conjecture can be seen as a "Liouville variant" of the notoriously difficult prime k-tuple conjecture asserting an asymptotic formula for the number of prime k-tuples $(n + h_1, \ldots, n + h_k) \in \mathbb{P}^k$.

Since already the twin prime conjecture that n and $n + 2$ are both primes infinitely often is completely open, a natural starting point is to try to show that, for any $h \neq 0$, one has
$$\sum_{n\leq X} \lambda(n)\lambda(n+h) = o(X).$$
In [4] Radziwiłł, Tao and the author managed to show this for almost all shifts h from a very short range, i.e.

Theorem 5. *Let $X \geq H \geq 2$. Assume that $H \to \infty$ with $X \to \infty$. Then*
$$\sum_{|h|\leq H}\left|\sum_{n\leq X}\lambda(n)\lambda(n+h)\right| = o(HX).$$

To prove this, we extended Theorem 1 to twists by linear phases $e(\alpha n)$ where $e(x) := e^{2\pi i x}$. More precisely, we showed that

Theorem 6. *Let $\alpha \in \mathbb{R}$ and let $X \geq H \geq 2$. Assume that $H \to \infty$ with $X \to \infty$. Then*
$$\sum_{x<n\leq x+H} \lambda(n)e(\alpha n) = o(H)$$
for almost all $x \leq X$.

Theorem 5 follows from Theorem 6 through Fourier analytic techniques. Note that the case $\alpha = 0$ of Theorem 6 corresponds to Theorem 1. Subsequently, Tao [14] used Theorem 6 alongside a novel entropy decrement argument to prove a logarithmically averaged variant of Chowla's conjecture for a fixed shift in the case $k = 2$:

Theorem 7. *Let $h \neq 0$. Then*
$$\sum_{n\leq X}\frac{\lambda(n)\lambda(n+h)}{n} = o(\log X).$$

Both this and Theorems 5 and 6 have variants for much more general multiplicative functions. Remarkably, Tao [13] was able to utilise the general version of Theorem 7 to prove the long-standing Erdős discrepancy problem from combinatorics:

Theorem 8. *For any $f\colon \mathbb{N} \to \{-1,1\}$, one has*
$$\sup_{k,N\in\mathbb{N}}\left|\sum_{n\leq N} f(kn)\right| = \infty.$$

Later Tao and Teräväinen [16, 17] managed to solve all the odd order cases of the logarithmically averaged Chowla conjecture (without needing Theorem 6).

Theorem 9. *Let k be odd and $h_1, \ldots, h_k \in \mathbb{Z}$. Then*
$$\sum_{n\leq X}\frac{\lambda(n+h_1)\cdots\lambda(n+h_k)}{n} = o(\log X).$$

(When k is odd, one can trivially dispose of the condition about h_j being distinct.)

The even cases $k \geq 4$ of the logarithmic Chowla conjecture remain open. Tao [15] has shown that the complete resolution is equivalent to two other conjectures, the logarithmically averaged Sarnak conjecture and the logarithmically averaged local higher order uniformity conjecture for the Liouville function.

Sarnak's conjecture roughly asserts that, for a bounded sequence $a(n)$, one has
$$\sum_{n\leq X} a(n)\lambda(n) = o(X)$$
whenever $a(n)$ is of "low complexity", whereas the higher order uniformity conjecture is a vast generalisation of Theorem 6 that allows α to depend on x and also replaces the linear phase $e(\alpha n)$ by much more general nilsequences that are the characters of the higher order Fourier analysis. The definition of nilsequence is so involved that we do not give it here, but instead we mention two special cases.

First, the higher order uniformity conjecture includes the claim that the Liouville function is locally orthogonal to polynomial phases. More precisely, for any $k \in \mathbb{N}$ it asserts that, for almost all $x \leq X$,
$$\sup_{P(y)\in \mathrm{Poly}_{\leq k}(\mathbb{R}\to\mathbb{R})}\ \sum_{x<n\leq x+H}\lambda(n)e(P(n)) = o(H), \tag{11}$$

where $\text{Poly}_{\leq k}$ denotes the set of polynomials of degree at most k. Secondly, the conjecture includes the claim that, for almost all $x \leq X$,

$$\sup_{\alpha,\beta} \sum_{x<n\leq x+H} \lambda(n)e(\lfloor \alpha n \rfloor \beta n) = o(H). \tag{12}$$

The fact that the phase is allowed to depend on x makes the problem much more difficult, and indeed in the recent progress [5, 6] on this conjecture (in the range $H \geq X^{\varepsilon}$) the main ingredient is to show that if, e.g., (11) failed for many x, then the corresponding polynomials yielding the supremum must be related to each other in certain way.

In [6] Radziwiłł, Tao, Teräväinen, Ziegler and the author were able to establish the higher order uniformity conjecture for $H \geq X^{\varepsilon}$; consequently for instance (11) and (12) hold for almost all intervals of length $H \geq X^{\varepsilon}$.

Unfortunately, in order to deduce the logarithmic Chowla conjecture, one would need to establish the higher order uniformity conjecture in much shorter intervals of length $H \leq (\log X)^{\varepsilon}$.

However, the result in [6] still has some interesting applications: first, it yields a new averaged version of Chowla's conjecture (as a special case of a result for more general patterns):

Corollary 10. *Let $k \in \mathbb{N}$. For $H \geq X^{\varepsilon}$,*

$$\sum_{|h|\leq H} \left| \sum_{n\leq X} \lambda(n)\lambda(n+h)\cdots\lambda(n+(k-1)h) \right| = o(HX).$$

Secondly, we obtain that the Liouville function has super-polynomially many sign patterns. More precisely, if one writes

$$s(k) = \Big| \{(\varepsilon_1, \ldots, \varepsilon_k) \in \{-1, 1\}^k : \exists n\colon (\lambda(n+1), \ldots, \lambda(n+k)) = (\varepsilon_1, \ldots, \varepsilon_k)\} \Big|$$

for the number of sign patterns of length k, then

Corollary 11. *For any $A \geq 1$ there exists a constant $\delta = \delta(A)$ such that $s(k) \geq \delta k^A$ for every $k \in \mathbb{N}$.*

The previous record [8] had $A = 2$.

4 Refinements and further applications

In a recent pre-print [3], Radziwiłł and the author revisited the problem of multiplicative functions in short intervals. As explained above already, the work in [2] led to further progress and many applications. However, there are certain drawbacks in it as well. In [3], we extended the results to sparsely supported functions, improved the quantitative bounds and extended to the complex case with the correctly twisted main term.

A key application of these new developments concerns the distribution of norm forms in short intervals. Let us discuss the simplest possible case, the characteristic function $\mathbf{1}_{n\in\mathcal{N}}$ of the set $\mathcal{N}$ of numbers that can be represented as a sum of two squares. Then it is well known that $\mathbf{1}_{n\in\mathcal{N}}$ is multiplicative and furthermore

$$\mathbf{1}_{p^k\in\mathcal{N}} = \begin{cases} 0 & \text{if } p \equiv 3 \pmod 4 \text{ and } k \text{ is odd;} \\ 1 & \text{otherwise.} \end{cases}$$

Hence $\mathbf{1}_{p\in\mathcal{N}} = 0$ for essentially half of the primes, which implies that the density of $\mathcal{N}$ is asymptotically $C/(\log X)^{1/2}$, i.e.

$$\sum_{\substack{n\leq X \\ n\in\mathcal{N}}} 1 = C\frac{X}{\sqrt{\log X}} + o\left(\frac{X}{\sqrt{\log X}}\right)$$

for certain constant $C > 0$. In other words, the average gap of two elements of $\mathcal{N}$ is of size $\sqrt{\log X}/C$. Consequently, one cannot expect $\mathcal{N}$ to be regularly distributed in intervals shorter than this.

In [2] we obtained a quantitative version of Theorem 2 but even it is completely trivial for sparsely supported functions such as $f(n) = \mathbf{1}_{n\in\mathcal{N}}$ and so does not tell us anything about the behaviour of $\mathbf{1}_{n\in\mathcal{N}}$ in short intervals. But fortunately, the method can be adapted to this situation and we proved

Theorem 12. *As soon as $h \to \infty$ with $X \to \infty$, one has*

$$\Big| \sum_{x<n\leq x+h(\log X)^{1/2}} \mathbf{1}_{n\in\mathcal{N}} - Ch \Big| = o(h) \tag{13}$$

for almost all $x \leq X$.

Previously Hooley [1] and Plaksin [10, 11] had shown that there exist constants c_1 and C_1 such that, as soon as $h \to \infty$ with $X \to \infty$, one has, for almost all $x \leq X$,

$$c_1 h \leq \sum_{x<n\leq x+h(\log X)^{1/2}} \mathbf{1}_{n\in\mathcal{N}} \leq C_1 h.$$

Their methods were based on an asymptotic formula for

$$\sum_{n\leq X} r_K(n) r_K(n+h),$$

where $r_K(n)$ are the coefficients of the Dedekind ζ-function for $K = \mathbb{Q}(i)$ ($r_{\mathbb{Q}(i)}(n)$ counts the number of representations of n as a sum of two squares).

Such an asymptotic formula is known for $K = \mathbb{Q}(i)$, but is completely open for non-quadratic number fields. Hence Hooley and Plaksin's methods have no chance of generalising to higher degree number fields.

In [3] we only use multiplicativity and get much more general results: we say that an integer n is a norm-form of a number field K over $\mathbb{Q}$ if n is equal to the norm of an algebraic integer in K. In the case $K = \mathbb{Q}(i)$, the norm forms are simply the sums of two squares. In [3] we have a much more general version of Theorem 12 for norm forms of number fields of any degree.

5 How do we attack short intervals?

Let us next discuss a common strategy for attacking arithmetic questions in short intervals. We would like to show that

$$\sum_{x<n\leq x+H} \lambda(n) = o(H)$$

for almost all $x \leq X$.

A typical way in analytic number theory to pick up the condition $x < n \leq x + H$ is to use the contour integration formula

$$\frac{1}{2\pi i}\int_{1-i\infty}^{1+i\infty} \frac{y^s}{s} ds = \begin{cases} 0 & \text{if } y < 1; \\ 1 & \text{if } y > 1. \end{cases} \tag{14}$$

This formula follows by moving the integration far to the right in case $y < 1$ and far to the left in case $y > 1$; in the second case we obtain the main term 1 from the residue of the pole at $s = 0$.

Applying (14) twice (when $x, x+H \notin \mathbb{N}$, but this small technicality is easy to deal with), we have, for any $x \le X$ and $H \le X$,

$$\sum_{x<n\le x+H} \lambda(n) = \sum_{n\le 2X} \lambda(n) \left(\mathbf{1}_{\frac{x+H}{n}\ge 1} - \mathbf{1}_{\frac{x}{n}\ge 1}\right) = \frac{1}{2\pi i}\int_{1-i\infty}^{1+i\infty} \sum_{n\le 2X} \frac{\lambda(n)}{n^s} \cdot \frac{(x+H)^s - x^s}{s} ds + O(1). \quad (15)$$

Objects of the form

$$F(s) = \sum_{n\le N} \frac{a_n}{n^s}$$

are called Dirichlet polynomials and they are very important tools in analytic number theory.

Recall that we aim to prove (10) only for almost all $x \le X$. Thus it suffices to show that

$$\int_1^X \left|\sum_{x<n\le x+H} \lambda(n)\right|^2 dx = o(H^2 X). \quad (16)$$

Using (15) one can show that in the language of Dirichlet polynomials this essentially reduces to the claim

$$\int_{-X/H}^{X/H} \left|N(1+it)\right|^2 dt = o(1), \quad (17)$$

where

$$N(s) := \sum_{X/2<n\le X} \frac{\lambda(n)}{n^s}.$$

A fundamental tool for studying mean squares of Dirichlet polynomials is the mean value theorem for Dirichlet polynomials which gives that, for any complex coefficients a_n and any $T, N \ge 2$, one has

$$\int_{-T}^{T} \left|\sum_{N/2<n\le N} \frac{a_n}{n^{1+it}}\right|^2 dt = O\left(\left(\frac{T}{N}+1\right)\frac{1}{N}\sum_{N/2<n\le N} |a_n|^2\right). \quad (18)$$

Let us motivate this in the case of coefficients with $|a_n| = 1$ for all n in which case one simply gets the bound $O(T/N + 1)$.

The term $O(T/N)$ reflects the expected average behaviour – from a random model one expects that for a typical t one has square-root cancellation, i.e. something like

$$\sum_{N/2<n\le N} \frac{a_n}{n^{1+it}} \asymp N^{-1/2}$$

leading in the mean square to $O(T(N^{-1/2})^2) = O(T/N)$.

On the other hand, the term $O(1)$ reflects possible peaks of the polynomial – for some values of t one might have

$$\sum_{N/2<n\le N} \frac{a_n}{n^{1+it}} \asymp 1; \quad (19)$$

surely this holds, e.g., if, for some t_0, one has $a_n = n^{it_0}$ for every n. At any rate, (18) is in general best possible.

If we now apply the mean value theorem (18) to the left hand side of (17), we obtain the bound

$$O\left(\frac{X/H}{X} + 1\right) = O(1) \quad (20)$$

which barely fails to produce the desired $o(1)$; this bound $O(1)$ for the left hand side of (17) gives the trivial bound $O(H^2X)$ for the left hand side of (16). Note that since this mean value theorem argument did not utilise any properties of $\lambda(n)$ except that $|\lambda(n)| \le 1$, it had no chance of leading to $o(H^2X)$ for (16).

Now it is the second term on the left hand side of (20) that is not $o(1)$. In the case of $a_n = \lambda(n)$, it is known that (19) cannot happen; for any $|t| \le N$ we have

$$\sum_{n\le N} \frac{\lambda(n)}{n^{1+it}} = O\left(\frac{1}{(\log N)^{1000}}\right)$$

by a known zero-free region for the Riemann zeta-function. Hence there seems to be some hope.

Often when one deals with Dirichlet polynomials, it is helpful if there is some bilinear structure, i.e. one can write the relevant Dirichlet polynomial (in our case $N(s)$) as a product of two or more Dirichlet polynomials. There are several classical ways to obtain such a decomposition, such as the identities of Vaughan and Heath-Brown.

These techniques work equally well for the primes and the Liouville function, and this is one of the reasons why many results are of similar quality in these two cases.

In our case, when we study very short intervals (such as $H = X^\varepsilon$ and smaller), it is of benefit to have a decomposition where one of the factors is very short.

Indeed, a crucial step in the proof is to use the multiplicativity of $\lambda(n)$ and take out a small prime factor utilising the fact that almost all integers $n \le X$ have a prime factor from the interval $(P, Q]$ as soon as $\log Q/\log P \to \infty$ with $X \to \infty$,

This can be done rigorously though, using either the Turán-Kubilius inequality or a Ramáre type identity. The latter gives better quantitative results and we use it in our research papers, but let us use the former here, which yields the following.

Lemma 13. *Let $X \ge H \ge 2$ and $3P \le Q \le H^{1/2}$. Then*

$$\sum_{X<n\le X+H} \lambda(n) = \frac{1}{\sum_{P<p\le Q} 1/p} \sum_{\substack{m,p \\ X<mp\le X+H \\ P<p\le Q}} \lambda(mp) + O\left(\frac{H}{\left(\log \frac{\log Q}{\log P}\right)^{1/2}}\right).$$

Here

$$\sum_{P<p\le Q} \frac{1}{p} = \log \frac{\log Q}{\log P} + O(1)$$

is a normalising factor corresponding to the average number of representations that n has as mp with $p \in (P, Q]$.

Note that utilising this idea that almost all integers $n \le X$ have a prime factor from $(P, Q]$ as soon as $\log Q/\log P \to \infty$ with $X \to \infty$ fundamentally fails in the case of primes, as primes $p > Q$ never have such a prime factor.

6 Intervals of length $H \ge X^\varepsilon$

In this section we sketch the proof of Theorem 1 in case $H \ge X^\varepsilon$. We start by applying Lemma 13 with

$$P = \exp((\log X)^{3/4}) \quad \text{and} \quad Q = \exp((\log X)^{7/8})$$

so that

$$\log \frac{\log Q}{\log P} = \frac{1}{8} \log\log X.$$

Hence by Lemma 13 it suffices to show that, for almost all $x \le X$, one has

$$\sum_{P<p\le Q} \sum_{x<mp\le x+H} \lambda(mp) = o(H \log\log X). \tag{21}$$

Note that here by multiplicativity $\lambda(mp) = -\lambda(m)$. We split the summation over p into dyadic ranges $p \in (P_1, 2P_1]$, so that we wish to show, for any $P_1 \in (P, Q]$,

$$\sum_{P_1<p\le 2P_1} \sum_{x<mp\le x+H} \lambda(m) = o\left(\frac{H}{\log P_1}\right);$$

summing this over $P_1 = 2^j$ with $P < 2^j \le Q$ gives essentially (21).

We can run a similar argument as in the previous section but with $N(s)$ replaced by

$$P_1(s)M(s) := \sum_{P_1<p\le 2P_1} \frac{1}{p^s} \sum_{X/(4P_1)<m\le 4X/P_1} \frac{\lambda(m)}{m^s}, \tag{22}$$

so that we need to show that

$$I := \int_{-X/H}^{X/H} \left|P_1(1+it)M(1+it)\right|^2 dt = o\left(\frac{1}{(\log P_1)^2}\right). \tag{23}$$

Now (18) still fails to do this, but we have the additional advantage of having a bilinear structure. The known zero-free region for the Riemann zeta-function yields

$$|P_1(1+it)| = O\left((\log X)^{-1000}\right) \tag{24}$$

for every $|t| \le X$. Using this we get that

$$I = O\left((\log X)^{-2000} \int_{-X/H}^{X/H} |M(1+it)|^2 dt\right).$$

Now we are in the position to apply (18) to the polynomial $M(s)$, giving the bound

$$I = O\left((\log X)^{-2000}\left(\frac{X/H}{X/P_1} + 1\right)\right) = O((\log X)^{-2000}) \tag{25}$$

since $P_1 \le H$. Hence (23) follows.

7 Shorter intervals

When $H \le \exp((\log X)^{2/3})$, a new issue arises: to make the last step in (25) work, we need to have $P_1 \le H$. However, for such short $P_1(s)$, we do not know (24) for all $|t| \le X$ any more. Fortunately, in [2] we were able to develop an iterative argument to rescue us.

Let us explain the rough idea. For simplicity, we pretend that Lemma 13 implies that, for $j = 1, 2, \ldots, J$, we have

$$N(s) = P_j(s)M_j(s) := \sum_{P_j<p\le 2P_j} \frac{1}{p^s} \sum_{X/(4P_j)<m\le 4X/P_j} \frac{\lambda(m)}{m^s},$$

with $P_1 = H, P_{j+1} = P_j^{\log P_j}$ for $1 \le j \le J-1$ and $P_J = \exp((\log X)^{3/4})$. For those t for which $|P_1(1+it)| \le P_1^{-1/10}$ the earlier argument works.

For those t for which $|P_1(1+it)| > P_1^{-1/10}$ and $|P_2(1+it)| \le P_2^{-11/100}$ we note that $1 \le (|P_1(1+it)|P_1^{1/10})^{2k}$ with $k = \lfloor \log P_1 \rfloor$ and in this case it suffices to show that

$$P_1^{k/5} P_2^{-11/50} \int_{-X/H}^{X/H} |P_1(1+it)^k M_2(1+it)|^2 dt = o\left(\frac{1}{(\log P_2)^2}\right)$$

which follows from the mean value theorem which is efficient as $M_2(s)P_1(s)^k$ has length about X.

Now we are left with t, for which $|P_2(1+it)| > P_2^{-11/100}$. Continuing the recursion, we are eventually left with t for which $|P_{J-1}(1+it)| \ge P_{J-1}^{-1/8}$, say. But now P_{J-1} is so large that this can only happen rarely, and we can use (24) for $P_J(s)$ together with a large value theorem for Dirichlet polynomials.

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Interview with Abel Laureate 2020 Hillel Furstenberg

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

Professor Furstenberg, first we would like to congratulate you on being awarded the Abel Prize in mathematics for 2020, which you share with Gregory Margulis. You have received the Prize, and here we cite the Abel Committee, "for pioneering the use of methods from probability and dynamics in group theory, number theory and combinatorics." Can you first of all tell us when you became enamoured with mathematics, and when you discovered that you had an exceptional talent for mathematics?

Perhaps I should say that I had a head start in mathematics – if you include adding and multiplying as mathematics. Let me give you the background story. I was born in Germany in 1935, and at roughly the age of five I came to the United States, my family having escaped Nazi Germany shortly after the Kristallnacht in November, 1938.

I lived with an uncle who had a poultry farm. I went to a rural school, which I think had only four classrooms. When I was in the kindergarten, I was in the same class as the first grade and second grade pupils. So it was easy for me to get a little bit ahead of where I should have been, or where I might have been. That was one aspect. Another aspect was that I had a sister who was three years older than I was, and she always kept me ahead. When the class was learning addition, I was learning multiplication. When they were learning fractions, I was learning algebra. I was always a little bit ahead, and of course you feel good about things that you are better at than most of the pupils in the class.

When I was in high school, I really enjoyed the Euclidean geometry that was taught there. I guess I enjoyed the challenge of geometry exercises. You are able to do things your way. You do not have to follow definite rules, it is about your own thinking. If it is clear and logical, you get to the right answer. I enjoyed that.

We learned about imaginary numbers when I was in high school. I thought I could make my name in mathematics if I proved that, when using imaginary numbers like $\sqrt{-1}$, it was going to lead to some contradiction in mathematics. I filled pages and pages of calculations and of course it didn't get anywhere, but it was a good experience just doing the calculations. I think it was a good experience to be a little frustrated when you want to show something, but it doesn't work out. But still, you don't feel bad about it. I think that it was pretty clear that I was enjoying mathematics.

So you enjoyed problem solving?

Yes, absolutely. I should mention a friend of mine who became a prominent mathematician at Harvard – Shlomo Sternberg. He and I were in the same class at high school. Both of us heard about an interesting and challenging problem in Euclidean geometry: given a triangle, assume two of the angle bisectors have the same length. Prove that the triangle is isosceles. It is obviously trivial to do it the other way around. That is a rather difficult problem which I would have trouble doing today. Anyway, we came out each with our own solution to this after some time.

Did the two of you announce that you had solved this problem?

It so happened that the high school I went to was in the same building as Yeshiva College, and at that time there was a journal called *Scripta Mathematica*, which contained articles about recreational and historical aspects of mathematics. It was a good journal at that time, but it no longer exists. At any rate, the editor of that journal was Professor Jekuthiel Ginsburg, and he had an office in the same building as our high school.

Shlomo and I plucked up our courage and went up to Professor Ginsburg and showed him that we had solved this problem. He took it upon himself to encourage our interest in mathematics. In particular, he encouraged me further. He gave me opportunities to advance in mathematics and, at the same time, earn money. Our financial situation at home was not that good. My mother was widowed when we were on our way to the United States, and it was clear that I should be earning money to help out.

What Professor Ginsburg did was to give me a job with the journal. I would help out with the graphics, I would translate articles written in French and German to English. In many ways he encouraged me, and I owe a lot to him. He communicated to me and to his students the innate beauty of abstract mathematical ideas. Over and beyond the mathematics I learned, I experienced the love of mathematics blended with human kindness, an experience I can only wish I could replicate for others. Anyway, with this background I think it is pretty clear that mathematics was a direction that I wanted to go into.

While you were an undergraduate student at Yeshiva College you published three papers, two of which appeared in American Mathematical Monthly. We will focus on one of these papers titled "On the infinitude of primes". The paper is only half a page in length, but that belies its originality. Furthermore, perhaps this paper gave you the motivation to further explore the interplay between topology and dynamical systems on the one hand and number theory on the other hand? Could you elaborate on this?

Let me just say that I was recently asked what made me think about putting a topology on the integers. I didn't have the answer right away. Afterwards, I realised that at that time I had been learning about p-adic integers with the p-adic topology. So you could put a non-trivial topology on the integers, where integers which are normally very far away now are very close. For example, $n!$ is very close to 0, and so on. So I had this topology on the integers in which all non-empty open sets were infinite. By looking at it carefully you could prove that there must be infinitely many primes; otherwise one could show that there exists a non-empty finite open set.

Let me be more explicit. In our topology, two-sided infinite arithmetic progressions form the basis of the topology; so they play the same role as open subintervals of the unit interval (0,1) in the usual topology. I hoped we could pursue the analogy further, and regard the set of integers also as a measure space with full measure 1. Assuming the measure is translation invariant (as on the unit interval), an arithmetic progression $a+d\mathbb{Z}$ would have measure $1/d$.

More generally, the measure of a "measurable" set would be its density. So this idea of looking at something happening in the integers as taking place in a measure space came in a natural way from the early paper, "On the infinitude of primes".

You graduated from Yeshiva College in 1955, having been awarded both a B.A. and an M.Sc. Then you went to Princeton University to study for your doctorate in mathematics, supervised by Salomon Bochner. Your Ph.D. thesis, "Prediction Theory", was submitted in 1958, and in 1960 an elaboration of your thesis was published in Princeton Annals of Mathematical Studies under the title "Stationary processes and prediction theory".

What fascinated us greatly was that – as a by-product or offshoot of that work, really – you proved Hermann Weyl's celebrated equidistribution theorem by using dynamical systems. This was the first time that had ever been done, right?

As far as I know, yes. I didn't know of a precedent for that. Looking at number theoretic issues dynamically, I think that was a first.

Could you elaborate?

As you said, it came about in a rather indirect way, in studying prediction theory. First of all, the idea of looking at, say, the integers as a measure space leads to the next step of looking at it dynamically. In ergodic theory, one looks at a measure space with a transformation that preserves the measure. For the integers endowed with the density measure, translating a set by adding a constant also preserves the measure. So the idea of thinking of something happening on the integers as of dynamical nature is not unnatural.

The motivation for me of working on prediction theory was related to my interest in harmonic analysis. Norbert Wiener, one of my mathematical heroes, had done some very profound work on Tauberian theorems in harmonic analysis. The latest thing that he had worked on was his prediction theory in a form closely related to harmonic analysis. In his theory, the "future", i.e., the next reading, was given as a random variable, that is, as a function defined almost everywhere on a measure space, not really well defined at specific points.

And so the question that arose, and that I wanted to answer, was: Suppose you're given exactly a certain past, past meaning something that happened yesterday, the day before, and so on up to minus infinity, so to speak. This I called the past. Given the past you would like to say exactly what's in the future, but you usually cannot say that. If, for example, what you are looking at is coin tossing, then all you could say is that with probability one half the next reading would be heads, and with probability one half it would be tails. So, going into the entire future, what you want to define is a stochastic process that will answer: What are the probabilities of what's happening in the future given what you have had in the past. There are certain situations in which you can do this. That's what is elaborated in my thesis.

How does the ergodic theorem enter the picture?

In fact, what I was doing was inverting the ergodic theorem. Let me explain. The first step in my construction, and this is how prediction theory connects with stationary processes, is to look at the past and to associate with it a stationary process. In other words, you want to look at this as a typical sequence of some stationary process. A stationary process arises by evaluating the function at hand on a probability space on which there is a measure-preserving transformation, the latter representing change in time. So the statistics of the process today and tomorrow are the same as the statistics will be a week from now and the day after, and so on.

The point is that I found a method of going from the individual sequence to the process, which is inverting the ergodic theorem. In fact, if you apply the ergodic theorem to stationary processes you can say in terms of expectations what is happening at almost every sample sequence. Our idea was to go from what I would like to be a sample sequence to the process of which it could be looked at as a typical sequence. This represents a reversed point of view. The idea is, you assume densities are defined in the past; let's say it's a plus one, minus one sequence. You notice that plus ones occur, say, 2/3 of the time in the past, and minus ones occur 1/3 of the time, and they occur together 1/7 of the time, and so on, for every combination there is a well-defined frequency of occurrence. Knowing this you infer the statistics from the given sequence, and from this statistics you build the stationary process.

Are there alternative ways of looking at this?

Yes, there is a more constructive way of building the space on which the stationary process is defined. In fact, just take the sequence itself and look at all its translates, and then take the closure in the product, or Tychonoff, topology. That gives you a compact space. On that space you define a measure based on what is "happening", so to speak, on the sequence you started with.

I mention this way of doing things because this has a precedent in Bochner's approach to almost periodic sequences, or more generally, to almost periodic functions. What he did was to say that almost periodic functions have a certain property: if you look at the closure of the translates of one such function then you get a compact space in the *strong* topology. That it is compact in the weak topology is immediate, but that it is also compact in the strong topology turns out to characterise almost periodic functions. So this idea of embedding a single sample sequence in a whole family of sequences is due to Bochner, although I didn't know it at the time I was his graduate student.

Let's go back to your thesis where you, as we mentioned already, were able to prove Hermann Weyl's equidistribution theorem by using dynamical arguments. We understand that this came as an offshoot, so to speak, of the main thrust of your thesis. At any rate, did this inspire you to look at other Diophantine approximation problems to see if you could solve these by using dynamics?

Yes, it did encourage me to look for dynamical systems relevant for other number theoretic problems. You may ask: How did my prediction theory led me to such dynamical systems?

The basic idea is a little bit subtle and somewhat technical. For us, to predict is, given the past reading through yesterday, to produce a stochastic process, indexed by time 0, 1, 2, etc., representing in probabilistic terms what will happen today and in the future. Moving ahead one day in time, we want to produce another stochastic process, also knowing today's reading. The new "future stochastic process" is just the old one, conditioned on today's reading and re-indexing 1, 2, 3 etc. to 0, 1, 2 etc. So, if a point in our probability space comprises a pair (two-sided, past + future actualised sequence, future process) our dynamic transformation takes this point to the pair (shifted sequence, future process conditioned on 0-reading). Dynamically this is a complex example of a skew product dynamical system.

Can you give us an example of a skew product?

Our space is a two-torus (the surface of a doughnut) which is formed by moving a vertical circle along a circular path returning to its initial position. We think of this as a bundle of circles above a base circle. A classic example named for Anzai and Kakutani describes a transformation of this torus whereby the base circle is rotated by a fixed angle, and the vertical circles move accordingly, each one rotated by an angle depending on its location on the base circle. For the example of Anzai–Kakutani you can show, using ideas from ergodic theory, that every orbit is equidistributed when the base circle is rotated by an angle α, an irrational multiple of π.

This is interesting. It gives you the equidistribution of $n^2\alpha \pmod 1$, $n \in \mathbb{Z}$, originally proved by Weyl by his methods of trigonometric series. So this gives a dynamic proof of Weyl's theorem. That encouraged me to look in general at a sequence as a sample sequence of some dynamical system, and then study the dynamical system and see what you can say.

Is it fair to say that this way of thinking gave you the idea of how to prove the Szemerédi theorem, which says that a subset of the integers with positive upper density has arithmetic progressions of arbitrary length?

Yes, you are basically correct. Let me put it this way: looking at the integers as a measure space, where adding one to each integer is a measure preserving transformation, we're given a set of positive measure, and I want to show that I can return several times to that set using the same number of steps. That I return once – in the standard probability space context – is the Poincaré recurrence theorem, but what I want is what is now called multiple recurrence. Given any natural number n, there exists a number m such that I return to the given set n times using consecutively m steps. This phenomenon of repeated recurrence is the measure theoretic version of the Szemerédi Theorem.

This is a prime example of what is now called the *correspondence principle*. Something going on in the integers corresponds to something going on in a measure space. You prove the measure theoretic thing, and then you get the number theoretic thing. In my thesis this principle does not appear explicitly, but it is implicit. The idea of how to go from an explicit past to a process is basically the correspondence principle. That's really the first time that the principle was used in a way that could be called a *principle*.

It is noteworthy that Green and Tao in their proof of the celebrated result that the primes contain arithmetic progressions of arbitrary length, while making no explicit use of ergodic theory, are influenced in their approach by the novel methods you apply in proving Szemerédi's theorem, specifically your correspondence principle.

In 1981 you published a book titled "Recurrence in ergodic theory and combinatorial number theory". It is

a marvellous book which has enthralled many, including the two of us. It describes in exemplary clarity how one can apply dynamical systems – both topological and ergodic – to number theory and Diophantine approximations, thereby proving some highly non-trivial results.

We will return to your proof of the multiple recurrence theorem, but first it might be useful that you tell us what an isometric extension is, and how this concept is the key building block in your proof from 1963 of the structure theorem of so-called distal flows in topological dynamics.

The Anzai–Kakutani example that I mentioned earlier will give a good illustration of what an isometric extension is. So, you have a big topological dynamical system on the two-torus, and a smaller one on the base circle. We say that the big system is an extension of the smaller one in the sense that looking at two coordinates (x,y) describing the torus, you now look at the first coordinate x which parametrises the base circle. That x coordinate moves according to a certain rule; in our case, x goes to $x+a$, and the y goes to some other y. So sending (x,y) to x is a factoring of the big system to a smaller system. But it is factoring in a special way, which is an example of an isometric extension, in the sense that two points on the torus that sit over the same point of the base circle maintain a fixed distance from each other as the x coordinate moves. The big system is an example of a so-called distal system, meaning that if two points are distinct they don't get closer than a certain amount, which depends upon where the points are.

Obviously, if a dynamical system is isometric, meaning that distances are preserved under the dynamics – we assume the underlying space is a compact metric space – then the system is distal. For a time it was an open question if distality implies isometry, perhaps in a different, but compatible, metric. This is in fact correct if the space is zero-dimensional – in particular, if the space is the Cantor set – and was proved by Robert Ellis in 1958. However, one can show that the Anzai–Kakutani example is distal, but not isometric, so the converse is not true. What you can show is that an isometric extension of a distal system is again distal.

So, to get examples of distal systems you take successive isometric extensions, even infinitely many, of a given distal system. These were the only examples of distal systems that I knew about, so I asked myself: Maybe that's it, there are no other examples? I proved that fact, and that became the structure theorem for distal systems.

Is there an analogous structure theorem for ergodic systems in the measure theoretic setting?

That's the crucial point in the proof of the ergodic version of the Szemerédi theorem! Firstly, you have something analogous to what I described in the topological setting, which you could call a distal ergodic system. Every ergodic system has as its base a distal factor, which might just be a rotation. But that does not necessarily exhaust the whole system, because an ergodic system is not in general distal. The next – and final – step to get the ergodic system you are looking at is a so-called relatively weak mixing extension. The notion of weak mixing means that things get very mixed up, and there is a relative notion of that. So the most general ergodic system is obtained by a relative weak mixing extension of a distal system.

What is that good for? Well, in this way I can prove the Szemerédi theorem in its ergodic version using that structure, by proving it bit by bit: proving that it's true for distal systems, which means it's true for rotations and isometric extensions, and then showing that if it's true for a given system, then it's true for a weak mixing extension of that system.

Could you tell us how you became aware of the Szemerédi theorem in the first place, as well as the origin of your proof of that theorem?

That was sort of accidental. The year 1975 was the first year of the Institute for Advanced Studies at the Hebrew University in Jerusalem, and the application by the Mathematics Department to have a special year devoted to ergodic theory was accepted and funded. We invited ergodic theorists from around the world to attend, like Donald Ornstein, Daniel Rudolph, Jean-Paul Thouvenot and many others. Then there was Konrad Jacobs, who was one of the early authors of a book on ergodic theory. But at that time he had stopped being interested in ergodic theory and had become interested in combinatorics instead, and so was not a member of our ergodic theory group.

Anyway, Jacobs was invited to visit, and he suggested to the organisers to give a colloquium talk on some aspects of combinatorics that he found exciting, including, as it turned out, the Szemerédi theorem. I was really not interested in the topic of his talk, but I felt that out of respect for the speaker, I should attend the lecture.

At that time I was basically aware of the correspondence principle that we talked about, so hearing of Szemerédi's theorem from Jacobs, it was natural to translate it into ergodic theoretic terms. Also, at that time I had information about what you can say about weakly mixing systems. It turns out that for weakly mixing systems, you have recurrence more or less in any pattern you want. In particular, you have recurrence along an arithmetic progression. So if the system is weakly mixing, you are finished. The other extreme case relative to weak mixing is rotation on a compact group. Again, it's almost immediate that recurrence occurs along arithmetic progressions. What was needed to nail the proof was a structure theorem, combining the two modes of behaviour. With the help of colleagues, Benjamin Weiss and Yitzhak Katznelson, I succeeded in proving the necessary structure theorem.

The story you have just told us seems to be an example of cross-fertilisation between different mathematical perspectives. Going to department colloquiums, even though the topic is vastly different from one's own interests, can open one's eyes to see what one can do.

Oh, yes, absolutely. I certainly learned a lesson from that. Actually, at my retirement there was a conference held in Jerusalem, and I was asked to speak on "Probability in Mathematics". I was thinking about my own career, and it struck me that I should really call my talk "The improbability of my mathematics". There were so many things that came together in my mathematical life. For instance, had I not looked at distal systems, I wouldn't have known how an appropriate structure theorem for ergodic systems might be formulated.

Incidentally, there was another accident that I should mention. I was in fact not going to be able to go to the colloquium talk by Konrad Jacobs. My youngest son had just been born a few months earlier, and at the time of the colloquium, I was to be assigned babysitter. Fortunately, my eldest child, my daughter, happened to be free at that time, and she came and took my place. Had she not, I would not have heard this colloquium talk.

Before we leave the subject of your ergodic proof of the Szemerédi theorem, we should emphasise that this has spawned a lot of generalisations due to many people, including you and various co-authors. One of the most spectacular of these generalisations is your and Katznelson's proof in 1991 of the density version of the Hales–Jewett theorem, which is a fundamental result in Ramsey theory. The proof is achieved by means of a significant extension of the ergodic techniques that you had pioneered in your proof of the Szemerédi theorem, and the result did not seem to be available by any other means than ergodic theory. However, in 2012 a so-called Polymath group of mathematicians published a new proof avoiding ergodic theory arguments. That is, they **do** ***admit that some part of their proof is inspired by ergodic methods. Do you have any comments?***

It wasn't unnatural that one would find a combinatorial proof for a combinatorial theorem. But I think that in every proof (as with Gowers' proof of the original Szemerédi theorem), one decomposes the behaviour to a random component and a regular component.

You introduced another concept which has been immensely important, namely that of a boundary. But perhaps before we get there, could you say something about random walks and how it is related to the boundary concept?

First let me give you an example of random walk in a group. Say I am given a bunch of $m \times m$ matrices and I attach a probability for each of those. I decide to start multiplying these matrices randomly according to that probability distribution. So I get the matrices X_1, X_1X_2, $X_1X_2X_3$, and so on, and it turns out that it is, with some restrictions, rather easy to show that the norms of this sequence grow exponentially. But I want to know what is happening qualitatively. What I am looking at is a random walk inside a group of matrices and I want to look at some limiting behaviour. It's no longer true that with probability one there is a specific limiting behaviour.

What behaviour is there then that can be called upon? It turns out that the rows of these product matrices come closer and closer together as you go to infinity, and they tend to point in a certain direction. A different sequence of products of matrices would give you a different direction, so you wind up with a random direction in projective space, basically. If you look at the special case of 2×2-matrices this is the only kind of boundary behaviour that you can talk about, in the sense that you can ask which point on the projective line does the sequence of products converge to.

In higher dimension it turns out that the sequence of products of matrices converges to a point in a so-called flag space of the right dimension – a line sitting in a plane, a plane sitting in a 3-space, and so on. In some sense, I can attach a flag space of dimension $m-1$ to the group $\mathrm{GL}_m(\mathbb{R})$ of $m \times m$ invertible matrices. So it makes sense to talk about a random walk converging to a point. What is nice about this is that for many groups (e.g. semi-simple Lie groups), this boundary has an explicit presentation as a homogeneous space of the group.

This is what is called the Furstenberg boundary today? Can this boundary be characterised in another way?

Yes, it can be characterised abstractly in terms of the notion of strong proximality. Proximality means that it is opposite to distality – the notion we already have encountered. So, we have a compact space on which the group acts in such a way that any two points will get as close together as you like under the action of some group element. If you think about it, that entails that any k points can come close together under the action of a group element.

Strong proximality means that if you have any measure on the compact space on which the group acts, it will converge to a point measure – a Dirac measure – under the action of some sequence of group elements. It's not obvious that the two notions are different, but they are. Now the boundary of the group can be characterised as the universal strongly proximal action of the group.

Margulis, with whom you share the 2020 Abel Prize, writes somewhere, and we quote: "I learned about Furstenberg's work around 1974, and his boundary theory influenced me very much. In particular, my proof of the normal subgroup theorem concerning lattices in semi-simple Lie groups could not exist without that theory. I also consider the proof of the normal subgroup theorem as my best proof". So he credits you for supplying him with a crucial idea in his proof!

I am very happy to hear that! I might actually give someone else credit here, namely the probabilist Monroe Donsker, who put me in contact with a friend of his – his name was Peter Ney – who was editing a book on applications of probability in various mathematical fields. He came to me once when I visited the University of Minnesota and said: "You should find an application of probability to algebra for Ney's book". So I thought of the boundary theory, and I thought that it seems intuitive that a lattice subgroup of a group should be very close qualitatively to the group itself. In fact, these two ought to have the same boundary.

Using this idea, I could prove a very special case of a theorem of Margulis – his superrigidity theorem. So in that way I interacted with Margulis. That you could use the boundary to reflect, so to speak, properties of the group itself turned out to be useful.

You mentioned to us before that Gelfand's work on Banach algebras, and, in particular, on commutative C^*-algebras, was an important inspiration for you at some point. Could you elaborate on that?

There are two ways in which Gelfand's theory comes in. One way of proving the correspondence principle is by using Gelfand's proof that there exists an isometric isomorphism between an arbitrary commutative C^*-algebra and an algebra of continuous functions on a certain compact space, sometimes called the Gelfand space. Applying this to the algebra of bounded sequences produces the space in which the dynamics takes place.

In fact – it is sort of an anecdote but this really happened – I once gave a lecture on these things at Gelfand's seminar at Rutgers University. Gelfand liked, when you put forth a theorem, to understand that theorem by himself. He did not want to listen to the lecturer explaining the theorem. I had put the correspondence theorem on the blackboard: so given something on the integers, then there's a measure space, etc., etc. Gelfand turned to the class in the seminar and asked: "Why is this true?" He didn't know either, but he wanted someone to explain it. Everybody gave up. And then I said: "It's Gelfand's C^*-representation theorem; that's how you get this."

The other thing I would mention that what Gelfand – who certainly was one of my mathematical heroes – did, was to algebraicise and then prove the Wiener Tauberian Theorem. This was one of the things I learned when I was a student at Princeton, and it made a deep impression on me. Gelfand gave this new and marvellous proof invoking algebra, and, of course, at a certain point you need analysis. Now one might say that Gelfand's theory is also the basis of my original way of getting the boundary of a group. In fact, there is a connection between boundaries and harmonic functions, and you somehow build an algebra from harmonic functions by finding a certain way of multiplying these functions. The Gelfand space of that algebra is the boundary of the group.

Of course, what is important here is the connection between random walks and harmonic functions. In fact, you can look at harmonic functions by looking at probabilistic questions on random walks, and vice versa, you can go from harmonic functions to random walks.

In 1967 you introduced the notion of disjointness of ergodic as well as topological dynamical systems. This notion, which is akin to that of being coprime for integers, turned out to have applications to a wide range of areas, including signal processing and filtering questions in electrical engineering, the geometry of fractal sets, homogeneous flows and number theory. Could you comment on this?

Let me answer your question in the following way. The notion of disjointness in dynamical systems arises in connection with filtering. Specifically, filtering out noise from a signal consisting of a transmitted time series plus noise. Given that signal and noise have known stationary statistical behaviour, when can noise be filtered out entirely? A sufficient condition is "disjointness" of the underlying dynamical systems generating the signal and the noise. This notion, while originating in the ergodic context, also applies to topological dynamical systems and gives insight into the structure of various systems.

There is an incidental application to Diophantine approximation involving the dynamics of two transformations acting on a space; for example $x \to 2x \pmod 1$ and $x \to 3x \pmod 1$, where 2 and 3 are examples of "multiplicatively independent" integers. The theorem states that when both operations are applied – so higher rank actions – then the orbits are either finite or dense.

The underlying intuition was that the two actions are fundamentally distinct, so that the invariant sets of each will also be different, so that a common invariant set would necessarily be degenerate – either finite or the whole space. My 1970 paper titled "Intersections of Cantor sets and transversality of semigroups" is an effort to make this precise. In differential and algebraic geometry, the notion of transversality relates to the dimension of the intersection of the two manifolds. In our context, Hausdorff dimension is expected to play a similar role, and a number of conjectures are raised in the paper. A partial result is obtained for which the underlying idea is the construction – based on a given fractal measure – of a stationary process of measure-valued random variables.

You wrote a memoir titled "Ergodic theory and fractal geometry" in 2014, where you try to reignite interest in this subject. Can we ask you what the status of the conjectures you alluded to above is?

Very recently one of my main conjectures was proved by two mathematicians, Meng Wu and, independently, Pablo Shmerkin. To illustrate the result they proved one has the following corollary: The inequality

$$\dim\{2^n\alpha \pmod 1 \mid n \in \mathbb{Z}\} + \dim\{3^n\alpha \pmod 1 \mid n \in \mathbb{Z}\} \geq 1$$

holds for all α in $\mathbb{R}$ except for a set of Hausdorff dimension 0. Here "dim" is box dimension, which is Hausdorff dimension of closure. Obviously, if α is rational then it is in the exceptional set. The details of the use of ergodic theory in the context of fractals and their dimensions appear in the monograph you referred to.

At any rate, there is current interest in that aspect, that is, the connection between ergodic theory and fractal geometry. Actually, Wu's proof makes use of the ergodic theory of something called a CP process, which comes about naturally when you try to look at things as a kind of process. You're zooming in at a fixed rate. Now, what are you seeing on your screen? How does this picture change? Instead of numbers changing, there are pictures changing. And that could all be a stationary process. Every fractal generates some kind of stationary process which is interesting to look at. I am very happy about the current interest in all of this.

As we emphasised at the outset of this interview, you were the first to build a bridge between dynamical sys-

tems and number theory, in particular, combinatorial number theory and Diophantine approximations. This has spawned a lot of activity in this area, both among your colleagues and your students, and again their students. You must be very pleased by this development?
Certainly. One is proud of one's children, but maybe in a sense even more proud of the grandchildren. It shows there's a line going there. The same is true in mathematics: what your students can do, and what their students are doing, likewise with your colleagues, enables you to see and appreciate the ramifications of your work. The real prize, and what you really appreciate, is when people understand what you are doing and are continuing that.

We still have bunches of other questions on our notepads, but perhaps this is the time we should end this interview. We would like to end by quoting Harish-Chandra: "I have often pondered over the roles of knowledge or experience, on the one hand, and imagination or intuition, on the other, in the process of discovery. I believe that there is a certain fundamental conflict between the two, and knowledge, by advocating caution, tends to inhibit the flight of imagination. Therefore, a certain naiveté, unburdened by conventional wisdom, can sometimes be a positive asset."

It seems to us that you in your approach to mathematics embody what Harish-Chandra describes.
I'm happy if you think of it this way!

Due to the covid-19 pandemic, the prize ceremony for the Abel Prize 2020 had to be postponed. The interview was conducted remotely, with Professor Furstenberg at home in Israel.

On behalf of the Norwegian and the European Math. Societies, and from the two of us personally, we would like to thank you for this most interesting interview. We very much look forward to meeting you in person in Oslo at the next Abel Prize event.
I am looking forward to that myself, among other reasons in order to put things on the blackboard so that people will really be able to understand the things we have been talking about abstractly.

Interview with Abel Laureate 2020 Gregory Margulis

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

Professor Margulis, first and foremost we would like to congratulate you on being awarded the Abel Prize 2020, together with Professor Furstenberg, for your, and we quote the Abel Committee, "pioneering the use of methods from probability and dynamics in group theory, number theory and combinatorics."
It's a great honour.

We will come back to the background story in a moment, but just to place your mathematics: you received the Wolf Prize mostly for your contributions to algebra. Your NSF grants emphasise analysis, and the Abel Prize focuses on probability and dynamics. So where should we place you? What sort of problems attract you?
I mostly consider myself to be a geometer. Once I talked to Jacques Tits, and he said "I am a geometer and you are a geometer". Somehow, I am a geometer in the sense that my mathematical thinking is mostly based on imagination and intuition. There are different types of geometers.

Let's see if we can get back to geometry as we go along. Jacques Tits, of course, is a previous winner – in 2008 actually – of the Abel Prize. You were born in Moscow in 1946. When did your interest in mathematics begin?
Probably from an early age. My father was also a mathematician, but he was mostly interested in mathematical education and didactics, and he wrote his PhD, or candidate thesis, under the direction of Aleksandr Khinchin, who was a famous probabilist. But my father's dissertation was in didactics. He was, how can I put it, a candidate of pedagogical sciences.

You played chess, and you were included in the so-called mathematical circles run by the Moscow State University. What was life like for you as a kid in Moscow in the 1950s and 1960s?
It was okay. My family was relatively well off. My father worked at an educational institute, and his salary was relatively high. From the time I was seven or eight, this was in 1954, we had our own apartment, which was quite exceptional at that time. People usually lived in so-called communal apartments. So one family occupied a room, and several families lived in one apartment. My family actually encouraged me to do mathematics. My father probably realised that I had some mathematical talent. From an early age, I was able to multiply double-digit numbers.

So it was natural that you were included in these mathematical circles?
Included is not the right word. Somehow it was no competition. The mathematical circles probably started for me around seventh grade when I was 12 or 13 years old.

Was it hard work being in the circles?
No, the circles were okay. It was quite informal. I don't remember many details, but it was run by students, sometimes by graduate students, from Moscow University, and we discussed various problems. And we were encouraged to solve them. There was also supervision by some more senior mathematicians. At that time, the mathematical Olympiads were connected to these mathematical circles.

So it was expected that one participate in Olympiads?
Yes, it was. It was in Moscow, I think it was in the so-called City Olympiad. A little later it was for the entire country. Somehow, there was no competition to enter, but it was quite challenging for many.

We can imagine that you actually did quite well in these Olympiads, didn't you?
Yes.

And later, in 1962, at the age of 16, you participated in the International Mathematical Olympiad, where you won a silver medal.
Yes.

Let us talk about your early career as a research mathematician. You wrote your first mathematical paper before you were 20. And in 1968 you and your fellow student Kazhdan published a very influential paper. How did that play a role in your later life, for example, toward arithmeticity of lattices and so forth.
Actually, my first paper was written while I was attending the Dynkin seminar. It was about positive harmonic functions on nilpotent groups. Probably it has some influence even now. My joint paper with Kazhdan was about the existence of unipotent elements in non-uniform lattices, or what is also called the non-cocompact case. This was a conjecture by Selberg. But our paper was also directed towards a proof of the arithmeticity of non-uniform lattices in semisimple Lie groups. So, actually, this paper with Kazhdan was the starting point towards the proof of arithmeticity.

We will talk more about arithmeticity later. However, we will remark that the joint paper you wrote with Kazhdan caused quite some excitement, and Armand Borel talked about it in a Bourbaki seminar. You were in the Dynkin seminar when you wrote the paper on positive harmonic functions on nilpotent groups. However, your undergraduate and PhD advisor was Yakov Sinai, who, by the way, was the Abel Prize Recipient in 2014.
Yes, it was Sinai. I attended the Sinai seminar on dynamical systems. I did actually then publish two works which are related to dynamical systems. One of them was about Anosov systems on compact 3-manifolds and the conditions on the fundamental groups this entailed. The other was about the counting of closed geodesics on compact manifolds of negative curvature. And this work was done because I attended and was inspired by this seminar. Sinai became my advisor in the third year of my undergraduate studies, so around 1965. I finished my PhD thesis in 1970.

After you finished your PhD, you began to work at the Institute for Problems in Information Transmission. According to Dynkin, you had the luxury of spending most of your time there on your own research. Is that a fair assessment?
Yes, that's quite a fair assessment. I didn't get a position at the Moscow University or the Steklov Institute, but the Institute for Problems in Information Transmission was one of the institutes of the Soviet Academy of Sciences. By Soviet standards at that time, the Institute was relatively small, only 200 researchers, but by Western standards it would probably be considered a huge institution. My immediate boss was Roland Dobrushin, who was a famous probabilist and a mathematical physicist. There was also another group there, which was headed by Mark Pinsker. He was famous for his work on information theory. In a sense, I was lucky to be there, because my most well-known and widely cited paper on expander graphs, published in 1973, was written under Pinsker's influence. Expander graphs, which incidentally have many applications in computer science, were first defined by Pinsker. Their existence was first proved by Pinsker in the early 1970s. My paper gave the first explicit construction of an infinite family of expander graphs. The

work on expanders was in some sense done under pressure or from a sense of duty. But I was mostly working on discrete subgroups of Lie groups, on arithmeticity and super-rigidity, and so on. It was done in parallel. All my own independent research was not, to put it mildly, quite related to the main direction of the Institute.

We will get back in more detail to your work on lattices in Lie groups. But before that, if you were to rank the people in Moscow who influenced you the most when you embarked on your research career, who would you name?
Sinai, of course, and then Piatetski-Shapiro, Kazhdan and Vinberg.

Did Piatetski-Shapiro make you aware of Selberg's and his own conjectures about lattices in semisimple Lie groups? Did you get the problem from them?
Yes, in a sense. It started with Selberg, and then with Piatetski-Shapiro. Selberg only stated the conjecture for non-uniform lattices. For uniform lattices he didn't actually have the definition of an arithmetic subgroup. That was done by Piatetski-Shapiro. So this problem somehow circulated, mostly thanks to Piatetski-Shapiro. I first proved the arithmeticity for non-uniform lattices by following the strategy which is essentially due to Selberg and Piatetski-Shapiro. You start with the unipotent elements and do various pieces of quite intricate work. For some special cases, the arithmeticity of non-uniform lattices had been proved by Selberg. But in the general case, the proof is much more complicated and requires a lot of additional, non-trivial arguments. For me, it took quite a long time, something like two or three years, to write a detailed proof. Regarding the case of uniform lattices, there was no strategy before my work.

So it is fair to say that your strategies for proving the non-uniform case and the uniform case are vastly different?
Yes, they are vastly different. For the non-uniform case there are these unipotent elements, and there are some kinds of building blocks which allow you to get the structure of arithmetic groups. For uniform lattices there are no such building blocks. For non-uniform lattices the method of proof is of algebraic and geometric nature, but for uniform lattices you have to use transcendental methods.

Before we go on, could you explain to us what it means to say that a lattice is arithmetic?
First, in Lie group theory a lattice Γ is a discrete subgroup of a Lie group G with the property that the quotient space G/Γ has finite invariant measure. Or, as we say, Γ has finite covolume. A lattice Γ is uniform (or cocompact) if the quotient G/Γ is compact, and non-uniform (or non-cocompact) otherwise. Consider the group $SL_n(\mathbb{R})$ of real invertible $n \times n$-matrices with determinant 1, and the subgroup $SL_n(\mathbb{Z})$ of matrices with integral coefficients. This is the standard example of an arithmetic subgroup. It's a classical result that $SL_n(\mathbb{Z})$ has finite covolume in $SL_n(\mathbb{R})$. It probably goes back to Hermite and Minkowski.

Full studio at the Norwegian Academy for the interview which was conducted remotely due to the covid-19 pandemic, with Professor Margulis on line from Yale.

Armand Borel and Harish-Chandra generalised this to semisimple Lie groups: $G(\mathbb{Z})$ is a discrete subgroup which has finite covolume in $G(\mathbb{R})$, where G is a semisimple Lie group. I proved that, under certain conditions, any lattice in a semisimple (algebraic) Lie group G is arithmetic (precisely, the lattice must be irreducible and the real rank of the group must be greater than 1). However, one needs to extend the definition of arithmetic subgroups. One extension is that the subgroup Γ is commensurable with $G(\mathbb{Z})$, i.e. the intersection of Γ and $G(\mathbb{Z})$ has finite index in both Γ and $G(\mathbb{Z})$. Another extension, which was actually due to Piatetski-Shapiro, is that, vaguely speaking, there is some construction which comes from maybe a bigger group that maps onto the original group with compact kernel. Selberg probably did not know about this definition.

Can you give us the timeline of your proof of the arithmeticity of lattices in higher rank semisimple Lie groups?
For non-uniform lattices, the crucial step was an announcement in 1969. I wrote quite a long paper about this crucial step, which was finished in 1971, but because of some difficulties in getting it published, it did not appear before 1975. As for getting the arithmeticity result from this crucial step, that was finished in 1973.

As for uniform lattices, the initial inspiration came in 1969 or 1970, when I learned about Mostow's fundamental work on strong rigidity. Thinking about it, I realised at some point that it would be possible to prove the arithmeticity of uniform higher rank lattices if one could prove a statement which is now called superrigidity. I believe, and this was confirmed by Mostow, that superrigidity was a new phenomenon which had not been discovered before. The first proof of superrigidity was based on a combination of methods from ergodic theory and algebraic group theory. One of the important ingredients was Oseledec's multiplicative ergodic theorem. It involves methods which are actually quite far from the original formulation of arithmeticity. I was invited to give an address at the ICM Congress in Vancouver in 1974, but I was prevented from attending. Instead, I sent a report, later published in the Proceedings of that Congress, where I outlined a proof of arithmeticity in the uniform case.

At the next ICM Congress in Helsinki in 1978 you received the Fields Medal, but you were not allowed to attend, mostly due to the opposition of the top Soviet mathematical establishment at that time. Jacques Tits said in his citation of your work, and we quote: "Margulis has completely, or almost completely, solved a number of important problems in the theory of discrete subgroups of Lie groups, problems whose roots lie deep in the past and whose relevance goes far beyond that theory itself. It is not exaggerated to say that, on several occasions, he has bewildered the experts by solving questions which appeared to be completely out of reach at the time. He managed that through his mastery of a great variety of techniques used with extraordinary resources of skill and ingenuity."

I am perhaps not the right person to comment on that. However, Dennis Sullivan told me that when Jacques Tits gave a presentation at Collège de France – or maybe it was at IHES at Bures – of my proof of the arithmeticity of cocompact lattices, Armand Borel was extremely surprised that ergodic theory was a crucial ingredient in the proof. After all, the theorem was stated in arithmetic terms.

Could you comment on a later and quite different proof of the superrigidity theorem and its application to arithmeticity – both in the uniform and non-uniform case – using the work on boundaries by Furstenberg, with whom you share the Abel Prize?

It seems strange now, but when I worked on superrigidity I was not influenced by Furstenberg's work, because I was essentially not familiar with it. It is indeed strange, because many ideas and methods introduced by Furstenberg are very similar in style to what I used.

As I mentioned before, my proof of the uniform case is vastly different from my proof in the non-uniform case. For the uniform case, there are actually two parts in the proof. The first part is to prove the existence of equivariant measurable maps and the second part is to show the rationality of these equivariant measurable maps. I did the first part for the uniform case, and using certain integrability estimates my argument could be extended to the non-uniform case. Actually, for that case I had to use arithmeticity, or at least the crucial statement in my proof of arithmeticity, to obtain these estimates. For the existence of equivariant measurable maps, Furstenberg gave a different proof that is not based on the multiplicative ergodic theorem, but is based on his boundary theory, which is, in a sense, quite related to the multiplicative ergodic theorem. One of the origins of the multiplicative ergodic theory of Oseledec was previous work by Furstenberg and Kesten on products of random matrices dating back to 1960.

Incidentally, let me relate a story which has some bearing on your question. Around 1970, Furstenberg visited Yale. At that time, Mostow was working on strong rigidity, and Furstenberg was working on applications of boundary theory to the theory of discrete subgroups of Lie groups. Furstenberg and Mostow were good friends, but somehow at that time they did not pay much attention to each other's work. In retrospect it looks strange, because their works looked very closely related. Mostow told me later on several occasions that Furstenberg probably could have proved superrigidity if he had been aware of the problem. I mentioned that in my talk during one of the workshops in honour of Furstenberg. Furstenberg was present and he immediately said something like: "I would never have been able to do that".

You are on record saying that you consider the proof of the so-called normal subgroup theorem as your best proof. Could you tell us what the normal subgroup theorem says, and also tell us why you think this proof is so good?

The normal subgroup theorem says that if G is a connected semisimple Lie group of rank at least 2 with no compact factors and with finite centre, and if Γ is an irreducible lattice in G, then any normal subgroup N of Γ either belongs to the centre of Γ or has finite index in Γ. So a special case is, for example, $SL_3(\mathbb{R})$ of 3×3 matrices of determinant 1. Take a discrete subgroup Γ which has finite covolume. Then any normal subgroup of the lattice Γ is either central or has finite index in Γ.

The general proof is divided into two parts. You have this irreducible lattice Γ and a normal subgroup N. Consider the quotient Γ/N. First consider the case that Γ/N is an amenable group. This case can be treated using representation theory arguments, in particular Kazhdan's property T. Then consider the case when Γ/N is a non-amenable group. Somehow I realised that one can use algebras of measurable sets. A crucial tool was one of the initial lemmas occurring in Furstenberg's paper on his boundary theory. I cannot explain how I came upon the idea behind the second part of the proof – it was some sort of intuition. Also, the idea of subdividing the proof into two parts was quite new. Anyway, I consider it the best proof I have done because it is mostly based on intuition.

Were there any precursors, or did the statement of the normal subgroup theorem come out of the blue?

The statement of the normal subgroup theorem was known to be true in certain cases, for example for $SL_n(\mathbb{Z})$. The proofs were done by algebraic methods. Maybe it was natural to assume that the statement was true in general. However, the proof in the cocompact – or uniform – case was obtained partially by using measure theory, as I alluded to above. For example, one of the ingredients in the proof is the density point theorem in measure theory. So even though the theorem is stated in purely algebraic terms, the proof in the general case is mostly non-algebraic.

Another example where you prove results in algebra and Diophantine approximation by your sort of methods, which, to us at least, seem very surprising, is when you prove the Oppenheim conjecture. Could you explain to us what that is, and how we should think about it?
The Oppenheim conjecture is actually a quite natural conjecture. There is a classical theorem called Meyer's theorem which says that if you have an indefinite rational form Q in at least five variables, then it nontrivially represents zero over rational numbers. That is, you have a nonzero integral vector x such that $Q(x)=0$.

Oppenheim – he was British and a student of Dickson's in Chicago – worked on these rational forms in four variables. He published a paper in the Proceedings of the National Academy of Sciences in 1929, and there was a footnote where he formulated the conjecture. It can be considered as an analogue of Meyer's theorem for irrational forms: if you have an indefinite irrational form Q in at least five variables, then for every positive ϵ there exists an integral vector x, such that the absolute value of $Q(x)$ is less than ϵ.

So the image of the set of all integral vectors is dense in $\mathbb{R}$, is that what you say?
More or less. It was later realised that the conjecture could be strengthened. There was a lot of work on using analytic number theory methods. I think that the main progress had been done by Davenport and his coauthors, starting from 1946 up until 1959, where they proved this when the number of variables is at least 21. But it was mostly analytic number theory methods.

Davenport realised that the conjecture could be stated not just for dimension at least 5; for irrational forms it could be stated for dimension at least 3. For dimension 3 and 4 the statement of Meyer's theorem is not true for rational forms, but for its analog for irrational forms it *is* true. Later, in the mid 70s, Raghunathan realised that the Oppenheim conjecture can be reformulated as a statement in dynamical systems. Let's say G is $SL_3(\mathbb{R})$ and Γ is $SL_3(\mathbb{Z})$, and if you take H to be SO(2,1), then any bounded orbit of H in G/Γ should be closed. Actually, the Oppenheim conjecture can be stated for a very special case: if you take the form $Q(x,y,z)=x^2+y^2-\sqrt{2}z^2$, then $Q(\mathbb{Z}^3)$ should be dense in $\mathbb{R}$. And the proof of this special case is not any simpler than the general case.

You learned about this problem in Bonn, as we understand?
Yes, I visited Bonn for three months in '79 from the beginning of July. I met Gopal Prasad, who was a student of Raghunathan's, and he is now a well-known mathematician.

The proof was published in 1986, is that right?
Yes, the proof was published in '86. But I remember that I already gave some kind of oral presentation in '84.

Let's move on to what we briefly touched upon before, namely expander graphs. You said that it was Pinsker who introduced you to this topic. Could you be more specific? But first tell us what an expander graph is!
Intuitively, an expander graph is a finite, undirected graph in which every subset of the vertices that is not "too large" has a "large" boundary. This notion was first introduced by Mark Pinsker in his work on so-called concentrators. Pinsker's original definition was not the same as the standard definition one can find in textbooks today. Vaguely speaking, a regular and undirected finite graph with n vertices is an expander graph if for any subset A of m vertices, where m is less than $n/2$, A has $m(1+\epsilon)$ neighbours for some (small) ϵ. Actually, the definition is not just for one graph, but for an infinite family of graphs. That the graph is d-regular means that each vertex has exactly d neighbours.


The Abel Prize Laureate 2020 Gregory Margulis. © Dan Rezetti.

As I said earlier, this started with the work of Pinsker, who proved the existence of expander graphs by probabilistic methods. In fact, almost all graphs which come from his construction are expander graphs. But there were no explicit constructions. I realised that by using some group theory, especially involving property (T), I could explicitly construct an infinite family of expander graphs. This was probably unexpected for people working in computer science.

Later on you constructed even more examples of expander graphs, isn't that correct?
Yes, later on there were these graphs that came from using quaternions. This was in 1984 and at that time I was mostly interested in studying the girths of regular graphs, and finding upper estimates of the girth size. In graph theory, the girth of a graph is the length of the shortest cycle contained in the graph. There was some probabilistic construction due to Erdős and Sachs that gave an upper asymptotic estimate $2\log_p n$ for the girth of a $(p+1)$-regular graph with n vertices (this is simple), while the asymptotic lower estimate was $\log_p n$. Quite surprisingly, my explicit construction gave an asymptotic lower estimate $4/3\log_p n$. I believe that up until now there hasn't been any probabilistic construction which goes beyond $\log_p n$.

At the same time as I studied these explicitly constructed graphs I realised, based on some deep work by Deligne, that they were also expander graphs. Slightly later and completely independently, Lubotzky, Phillips and Sarnak basically gave the same construction, but with some variations. They also used the work of Deligne, and they called these graphs which come from this construction Ramanujan graphs, because it is related to some Ramanujan conjecture.

We have to talk about another problem that you solved, this was at the very beginning of the 80s, and we quote Mathematical Reviews: "Although it is not explicitly stated, this paper settles a long-standing problem of Banach on the uniqueness of invariant means on the n-sphere". We understand that this came to you in a flash, and it involves property (T) again. Could you tell us a wee bit about the story behind this?

I believe I was at a conference in Poland in 1980, in May or June. I met Rindler, an Austrian mathematician, who mentioned this problem by Banach and Ruziewicz about invariant means for algebras for measurable sets and the reformulation of the problem due to Rosenblatt, who explained that it would follow from the statement about small almost invariant sets. I immediately realised that this statement can be deduced from property (T) for certain subgroups.

Okay, if you start in dimension 5, say SO(5), then it contains certain arithmetic lattices, or *S*-arithmetic lattices, and it has property (T). I think – essentially around the same time – Dennis Sullivan gave another proof using a slightly different subgroup, but also using work by Rosenblatt.

Actually, the proof came to me almost immediately.

Is this typical, does this happen to you from time to time that all of a sudden you see the answer?

Maybe I had an answer before I had a question…

Fair enough.

This work became quite famous, but actually I didn't spend much time on it.

Can we ask you about your working style, because there is one thing that strikes us. Early in your career, you were the single author of nearly all of your papers, and then in the past thirty years, almost all of them have been joint papers. How do you explain that?

When I was in Moscow, essentially all my papers were written by myself. One notable exception was the joint paper with Kazhdan. Actually, for me it was quite challenging to write up papers, so it took a lot of time. But when I moved to the United States and to Yale in 1991, it was a completely different environment. I started to work with many mathematicians, mostly younger than me. I also started to work with my graduate students.

Was that fun, did you enjoy it?

Yes, working with graduate students was fun. I did not have graduate students in Moscow, but after I moved to Yale I had several graduate students. It was a rewarding experience to work with them.

We noticed that even last year, in 2019, you had a paper together with A. Mohammadi. So you are still working with younger fellows?

A. Mohammadi was my graduate student, who finished more than ten years ago. I had joint papers with him when he was a graduate student, and then we had several joint papers after that. Another graduate student of mine that I would like to mention is Dmitry Kleinbock. We have several joint papers. In 1995 I noticed a book in the maths department library by Sprindzuk, where he presents his proof of the Mahler Conjecture on transcendental numbers and Diophantine approximations. I asked Dmitry to look at it and see if there are relations between the subject of Sprindzuk's book and dynamics. Soon after, Dmitry gave a reformulation of the Baker–Sprindzuk conjectures in dynamical terms. After that we realised that a modification of methods used in the proof of non-divergency for unipotent flows can be applied to prove the dynamical reformulation of the Baker–Sprindzuk conjectures.

Now I am retired, and I had my last student finish this current year.

A final question: How do you rank yourself on a scale from theory builder to problem solver?

Probably I am more a problem solver than a theory builder. But I find this division rather artificial.

That brings us to the end of the interview. We want to thank you on behalf of the Norwegian Mathematical Society and the European Mathematical Society. Also, the two of us would like to thank you personally for this very interesting interview. Thank you very much!

Thank you!

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

Sir Vaughan F. R. Jones (1952–2020)

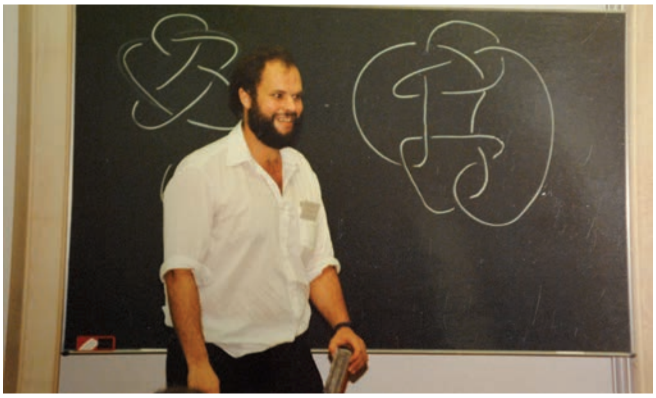
David Evans (Cardiff University, UK) and Sorin Popa (University of California at Los Angeles, USA)

Sir Vaughan Jones died aged 67 on 6 September 2020, following complications after a severe ear infection. An inspired and inspiring mathematician of exceptional originality and breadth, his enduring work brought together several disparate areas of mathematics, from analysis of operator algebras to low dimensional topology, statistical mechanics and quantum field theory, with major impact and unexpected, stunning applications, even outside of mathematics, as in the study of DNA strands and protein folding in biology. A crucial idea leading to these striking connections was his groundbreaking discovery in the early 1980s that the symmetries of a factor (an irreducible weak* closed algebra of operators on Hilbert space), as encoded by its *subfactors*, are *quantized* and generate quantized groups, a completely new type of structure, endowed with a dimension function given by a trace and an index that can be non-integral.

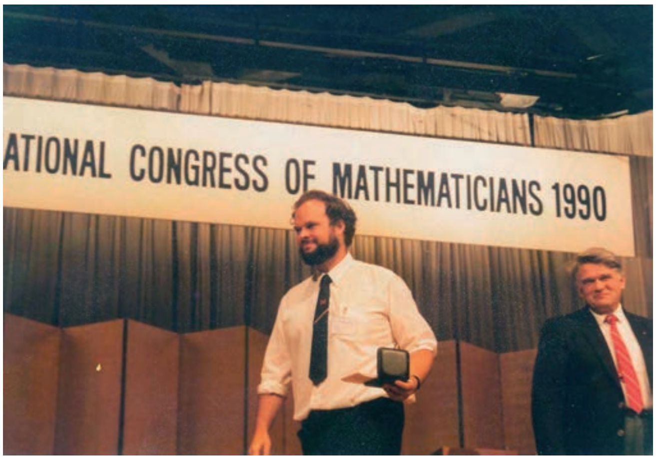
Vaughan Jones was born on 31 December 1952 in Gisborne, New Zealand. He was educated at Auckland Grammar School and the University of Auckland, where he earned a bachelor of science and a master of science with first class honours. He then received a Swiss government scholarship and completed his PhD at the University of Geneva in 1979 under the supervision of André Haefliger and Alain Connes, with his thesis awarded the Vacheron Constantin Prize. He was a Hedrick assistant professor at UCLA in 1980–1981, an associate professor at the University of Pennsylvania 1981–1985 and was then appointed full professor at UC Berkeley in 1985. From 2011 on, he held the Stevenson Distinguished Chair at Vanderbilt University, while also being professor emeritus at UC Berkeley.

Already in his thesis work, Vaughan Jones was interested in the classification of finite groups of automorphisms ("classical symmetries") of a class of von Neumann algebras called II_1 factors, following up on Connes' classification of single automorphisms. He developed a novel, algebraic approach, where the action of the group was encoded in the isomorphism class of a subfactor. Soon after, this led him to consider abstract subfactors together with a natural notion of relative dimension that he called *index*, and to study the values it can take. By late 1982, he had made a series of amazing discoveries. On the one hand, the index of a subfactor can only take values in the discrete set $\{4\cos^2(\pi/n) \mid n \geq 3\}$ or in the continuous halfline $[4, \infty)$. On the other hand, all these values can actually occur as indices of subfactors, and, indeed, as indices of subfactors of the most important II_1 factor, the so-called *hyperfinite* II_1 factor (the non-commutative, quantized version of the unit interval). The proof involved the construction of an increasing sequence of factors (a *tower*), obtained by "adding" iteratively projections (i.e., idempotents) satisfying a set of axioms which together with the trace provide the restrictions. Shortly after, Jones realised that his sequences of projections give rise to a one-parameter family of representations of the braid groups, and that appropriate re-normalizations of the trace give rise to a polynomial invariant for knots and links – the *Jones polynomial*.

This immediately led to a series of spectacular applications in knot theory, solving several of the Tait conjectures from the 19th century. More importantly, it completely reinvigorated low dimensional topology, igniting totally unexpected developments with an exciting interplay of areas, including physics, and a multitude of new invariants for links and 3-dimensional manifolds, altogether leading to a new brand of topology, *Quantum Topology*.



This revolutionary work also had a huge, far-reaching impact on the theory of II_1 factors and operator algebras, posing exciting new questions about the classification of subfactors and of the quantized groups they generate. Many outstanding results by a large number of people have followed. Jones was much involved in this development, notably finding the best way to characterise the group-like object arising from the tower of factors (the *standard invariant*) as a two-dimensional diagrammatic structure of tangles called *planar algebra* (1999), and then classifying them up to index 5, in a remarkable programme developed with some of his former students (2005–14). This, together with a quest to produce conformal field theory from subfactors, led Jones to a study of the Thompson groups and again to unexpected spin-offs for the theory of knots and links (2015–2020). In a parallel development which started in 1983, the connection was made with calculations by Temperley and Lieb in solvable statistical mechanics, triggering yet another series of connections with physics, statistical mechanics and conformal quantum field theory, where a similar dichotomy of discrete and continuous parts of the central charge occur.



Vaughan Jones was awarded the Fields Medal in Kyoto in 1990, and was elected Fellow of the Royal Society in the same year, became Honorary Fellow of the Royal Society of New Zealand in 1991, member of the American Academy of Arts and Sciences in 1993 and of the US National Academy of Sciences in 1999, and foreign member of national learned academies in Australia, Denmark, Norway and Wales. He received the Onsager Medal in 2000 from the Norwegian University of Science and Technology (NTNU). In 2002 he was made a Distinguished Companion of the NZ Order of Merit (DCNZM), later re-designated Knight Companion KNZM. The Jones medal of the Royal Society of New Zealand is named in his honour.

He had a strong commitment of service to the community. In 1994 he was the principal founder and director of the New Zealand Mathematical Research Institute, leading summer schools and workshops each January. He was vice president of the American Mathematical Society 2004–2006, and vice president of the International Mathematical Union 2014–2018.

Vaughan had a very distinctive and personal style of research in mathematics. His warmth, generosity, sincerity, humour and humility led him to thrive on social interaction, and for the mathematical community to significantly benefit from his openness in sharing ideas through every stage of development, from initial speculations and conjectures about the way forward to the discussion and explanation of the final results. His presence both at formal and informal events and his regular interaction with mathematicians, especially graduate students, including his own, of which he had more than 30, enriched all those who came into contact with him.

Vaughan regularly mixed his passion for skiing and kite-surfing with hosting informal scientific meetings at Lake Tahoe, Maui and his family retreat in Bodega Bay. His love for rugby was legendary, as was the fact that he wore an All Blacks jersey for his plenary at the ICM in Kyoto following the award of his Fields medal. His other major passion was music, especially choral singing and orchestral playing, shared intimately with his family and friends. Vaughan is survived by his wife Martha (Wendy), children Bethany, Ian and Alice and grandchildren. He will be dearly missed by his family and the many friends all over the world.

This obituary was first published in the September issue IMU-NET-103 of the Newsletter of the IMU (https://www.mathunion.org/imu-net/archive/2020/imu-net-103). It is reproduced here with the kind permission of the Editor, Martin Raussen.

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Who Owns the Theorem?

Melvyn B. Nathanson (City University of New York, USA)

Simple questions: If you prove the theorem, do you own it? Can you forbid others to use or even cite it? Can you choose not publish the theorem? Can you be forbidden to publish it?

What is a theorem? A mathematical statement may be true. It is true whether or not there is a proof. Without a proof, we do not know if it is true. A theorem is a true mathematical statement that has a proof.

Suppose there is a true mathematical statement, and you prove it. Now it is a theorem. It is "your theorem". In what sense might you own it? Can or should a theorem be considered the private property of its discoverer, who may or may not choose to publish? If you own the theorem, can you license it or rent it? Can you insist that anyone who wants to use or apply the theorem must pay you to do so?

If you publish the theorem in a refereed journal, or post it on arXiv, or explain it in a seminar, or submit it to a journal, then everyone knows you proved it. When does "your theorem" become part of the public library of proven mathematical truths that other researchers can freely use to prove new theorems?

If you need a result to prove a theorem, and know that the result is true but the discoverer has not announced or released it publicly, is it ethical (of course, properly citing the discoverer) to use that "unpublished" result in the proof? Is it ethical for you *not* to prove the theorem because it requires a result that is true but is being withheld by its "owner"?

Suppose you find out that someone has proved a theorem, but has not revealed it to the world. Maybe you have even seen the proof, and checked it, so you are sure that it is correct. Even though it has not been published, you know that it is a mathematical truth.

Can you use it in a paper, even though the discoverer might not want the result to be known? Does the prover of the theorem own it enough to prevent other mathematicians from using it?

The notion of "owning a mathematical truth" is, in part, connected with careerism in academic life. What might be called "vulgar careerism" is endemic and not necessarily vulgar. Many mathematicians hide what they are working on so others will not "scoop" them, will not use "their" ideas to prove a theorem before they do. Perhaps it is not sufficient to give proper attribution. Maybe the author is an untenured assistant professor who wants to deduce more results from the theorem, publish more papers, and get promoted. Maybe the author thinks it will lead to a proof of the Riemann hypothesis and earn the million-dollar prize from the Clay Mathematics Institute. Some mathematicians admit that they discuss their ideas about how to solve the Riemann hypothesis only after they are convinced that the ideas will not work.

It used to be that, every year, permanent professors in mathematics at the Institute for Advanced Study in Princeton would appoint a visiting member to be their "assistant." Long ago at the Institute, there was a permanent member who required his assistants to promise not to reveal to anyone what he was working on.[1] When I learned this, I was shocked. It was antithetical to everything I believed about science. I was also naive. I had not understood that for many people mathematics is a competition.[2]

In 1977, the National Security Agency decided that publication of cryptographic research would endanger national security, and wanted to require that professors who wrote papers in cryptography would have to send them for pre-publication review by the NSA and not submit them to journals without NSA approval. At first, the NSA hoped for voluntary compliance, but also considered making this a legal requirement. This did not happen, and, after considerable contentiousness and debate in the mathematical community, prepublication review by the NSA faded away and seems not to be an issue today [1, 2].

Secrecy in mathematics is less important than in other sciences. Mathematical results rarely have commercial value. Like many mathematicians, I don't care if my theorems are "useful". I only hope that I have not made mistakes, that the proofs are correct, that the "theorems"are theorems and are interesting. I upload preprints to arXiv as soon as they are written, before I send them to a journal. I am happy if someone uses my results. But this does not answer the central question. Mathematician A proves a theorem, and mathematician B learns about it. Maybe B reviewed A's NSF proposal. Maybe A submitted the manuscript to a journal and B refereed it. Can B use the theorem (as always, with proper attribution) in a paper before A has published it?

For me, the answer is clear. Here is an analogy. Legally, you cannot sequence a plant or animal DNA strand and patent the sequence because you did not create the sequence. God created it, and you only discovered it. Similarly, mathematical truths exist, and mathematicians only discover them. If you discover a theorem, you have the power, the privilege, and, perhaps, the right not to reveal it to anyone, but if, somehow, someone learns of your result, knows that a certain mathematical statement is true, then that person has the right to tell the

1 I was once André Weil's assistant at the Institute. He did not impose a secrecy oath.

2 I still do not understand why, for some mathematicians, getting medals in high school and college competitions is a core part of their self-esteem.

world and to apply it to obtain new results, with or without your consent.

Can you own a scientific truth? Can you hide a scientific truth? These are ethical questions, and, in the Covid era, not only in mathematics.

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Melvyn B. Nathanson is professor of mathematics at the City University of New York (Lehman College and the CUNY Graduate Center). His B.A. is in philosophy from the University of Pennsylvania, he was a graduate student in biophysics at Harvard University, and in 1972 was awarded the Ph.D. in mathematics from the University of Rochester. In 1972–73 he had an IREX fellowship to study with I. M. Gel'fand in Moscow State University, and in 1974–75 was Assistant to André Weil at the Institute for Advanced Study. He has had visiting positions at Rockefeller University, Harvard University, and Tel Aviv University. He is a Fellow of the American Mathematical Society.

Underrepresentation of Women in Editorial Boards of Scientific and EMS Journals

Alessandra Celletti (Università di Roma Tor Vergata, Italy) and Stanislawa Kanas (Rzeszów University, Poland)

One of the fundamental principles of the European Union is the equality between women and men (EU, Article 23 of the "Charter of fundamental rights of the European Union"). As mentioned in [1], "Equal participation of women and men in decision-making is a matter of fairness and is needed to strengthen democracy. It is also likely to benefit the EU's economic growth and competitiveness". The EU Gender Equality Strategy 2020–2025 presents new policy objectives and actions with the aim of reaching a 50–50 gender balance. This is an effort to contribute to the goal set by the EU of offering women and men equal opportunities to thrive by enabling equal participation in all aspects of society.

This note was written as a reaction to the fact that women are still underrepresented on editorial boards of EMS journals relative to their representation among researchers.

Gender gap in STEM subjects

There is clear evidence of a large imbalance in the participation of women in STEM fields compared to men, in particular at more advanced career levels. This imbalance is more acute in fields that are critical for national economies. In particular, according to data collected by the UNESCO Institute for Statistics (UIS), women represent less than 30% of the Research & Development workforce worldwide [2].

These data illustrate that women are globally underrepresented in STEM fields, both in the overall number of graduates (especially at the PhD level), and in research professions. The "UNESCO Science Report Towards 2030" indicates that gender gaps are more apparent in disciplines such as mathematics, engineering and computer science.

Figure 1 illustrates the gap between women and men, where the underrepresentation of women in STEM subjects translates into the loss of a critical mass of talent.

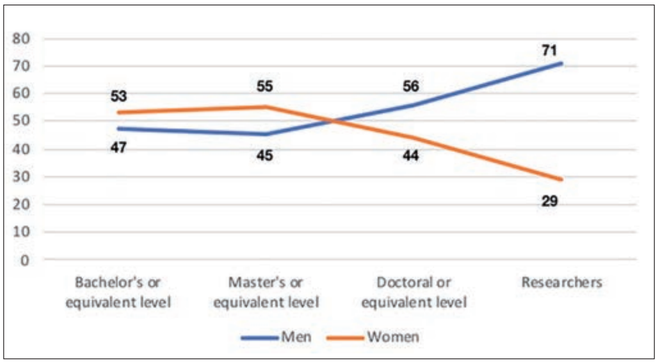

Fig. 1. Proportion of women and men: those that are graduates in tertiary education by programme level and those employed as researchers, 2014. Source: UNESCO Institute for Statistics (UIS) [2].

The UIS findings echo the National Benchmarking Survey 2017 commissioned by the London Mathematical Society [3] for more advanced career stages. Figure 2 exhibits a significant drop in UK female mathematicians from first graduate degree to academic staff. The data presented here is a snapshot of the year 2017, where the total number of lecturers, senior lecturers, researchers (i.e. staff on a research-only contract) and professors (including readers/associate professors as well as full professors) was 3910, of which 805 were women. This gives 20.6% of women academics in the mathematical sciences in the UK in 2017.

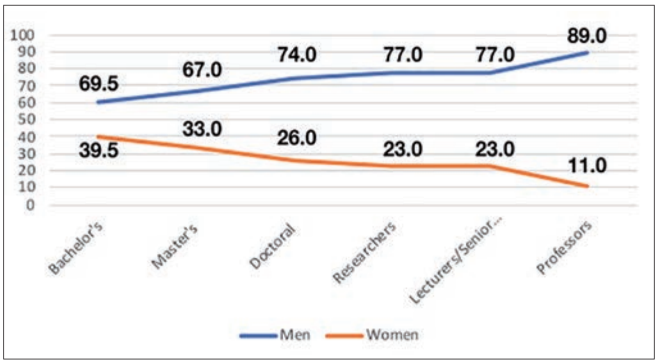

Fig. 2. Percentages of women and men in tertiary mathematics education in the UK in 2017, according to the Benchmarking Survey commissioned by the London Mathematical Society. Source: [3].

This proportion has not increased since the 2005 survey published in [4], which finds that in 2005, 20% of the mathematical sciences tenured faculty positions were held by women in Europe (Denmark, Finland, Iceland, Norway, Sweden, Austria, Belgium, France, Germany, Ireland, The Netherlands, Switzerland, The United Kingdom, Czech Republic, Estonia, Italy, Portugal and Spain).

Gender gap in scientific publication authorship

Recently, several articles have illustrated the persistence of a gender gap in science, and have sustained and informed the continuing discussions of possible reasons for this discrepancy.

For instance, it has become evident that gender stereotypes do affect the performance of women in mathematics – see [5], since it leads to psychological pressures and (cf. [6]), from a cognitive viewpoint, it reduces individuals' working memory capacity.

The *Gender Gap in Science* book [7] is a report of the 2017–2019 Gender Gap in Science project [8] which contains the results of a gender analysis of authorship based on several million publications. Figure 3 is a plot taken from [7] that captures a data analysis from zbMATH. Although the plot shows a steady increase in the proportion of women authors, at this rate we will have to wait until 2070 before reaching a balance.

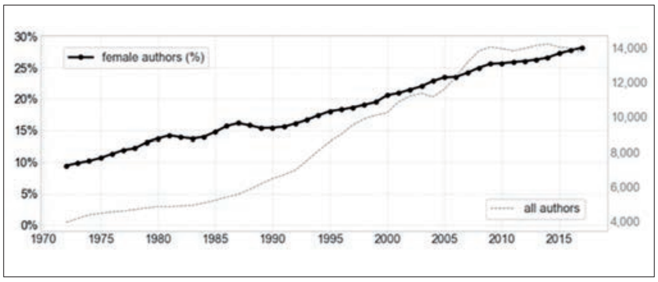

Fig. 3. Number of active (publishing) mathematicians since 1970 and percentage of them that are women. Source: [7].

The findings of the *Gender Gap in Science* book [7] are in line with the conclusions reached by the authors of [9], who analysed the representation of female authorship in 293,557 research articles from 54 journals covering the categories *Life Science, Multidisciplinary, Earth & Environmental* and *Chemistry* between 2008 and 2016. Their study indicates (a) that 29.8% of all authorships and 33.1% of the first, 31.8% of the co-authors, and 18.1% of the last authorships were held by women and (b) that in prestigious and highly competitive articles, women are underrepresented with respect to men.

As an illustration, we present the situation for the *Journal of the European Mathematical Society* in Figure 4.

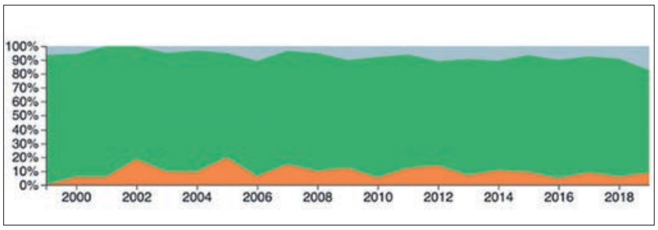

Fig. 4. Proportion of authorships in the Journal of the European Mathematical Society per gender and publication year. Female authors in orange, male authors in green, unidentified gender in grey. Graph obtained using the interactive tools of the Gender Gap in Science project [10].

A study in [11] presents data relating to the publication records of 168 life scientists in the field of ecology and evolutionary biology and supporting the fact that the median number of citations per paper gives no difference between women and men. This, according to the authors of [11], "argues against a quality versus quantity hypothesis". On the other hand, the data provide evidence that there are relatively few women that publish poorly cited articles.

Comparison of h-indices with the number of citations per publication gives the average number of citation of publications of researchers.

From these data, it emerges that female researchers produce higher quality output, whereas males tend to be below the expected productivity with respect to this metric. The conclusion of the authors of [11] is that "for a given level of productivity, females produce better quality work than males". They also conclude that females lead higher quality research compared to their male counterparts with high h-index (years 1996–2005).

Gender gap on editorial boards of scientific journals

The gender gap in science is also illustrated by the composition of editorial boards of scientific journals. Despite some progress in recent years, the underrepresentation of women on editorial boards remains an important challenge for the scientific community. This underrepresentation pinpoints the numerous obstacles that women still face on their way to reaching higher positions, and limits the global potential of our research community. There is evidence that having a high percentage of male editors leads to a higher percentage of male referees – see [12]. Of course, the refereeing activity is necessary for any scientist, especially for the young ones, since this also makes them feel respected in their field. The more prestigious the journal is, the more rewarding the refereeing experience is. According to some editorial comments in *Nature* (see [13, 14]), this journal has been aware of the gender gap and has been continuously working on improving the situation by applying different types of measures – see also [15]. One of the most notable consequences was the announcement of the first female editor in chief of *Nature* in 2018.

The gender representation on the editorial boards of 435 journals in the mathematical sciences, listed in the Thomson-Reuters Journal Citation Reports, has been studied in [16]. These journals, of which 35.6% are in pure mathematics, 43.9% are in applied mathematics and 20.5% accept articles from both fields, belong to 123 publishers. The number of editors in the studied group is 27.7% for pure journals, 51.9% for applied journals, and 20.4% for journals publishing both disciplines. For 91.1% of the data, the authors of [16] found an overall number of 86 countries associated with the editorship. The largest number of editors are the researchers from the US (33.6%), The United Kingdom (7.4%), France (6.7%), Germany (6.6%), Italy (4.5%), Canada (4.0%), Japan (3.8%), China (3.8%), Russia (2.4%), and Australia (2.0%).

The authors of [16] show that 8.9% of the 13067 editorships are held by women, 90.3% by men, and 0.8% are undetermined (see Figure 5a). This is to be compared and contrasted with the 20.6% of mathematical sciences tenured faculty positions held by women in 2017 in the UK [3], the 15% of mathematical sciences tenured faculty positions held by women in 2013 in the United States of America (see [17, 18]) and the 20% of mathematical sciences tenured faculty positions held by women in 2005 in Europe [4].

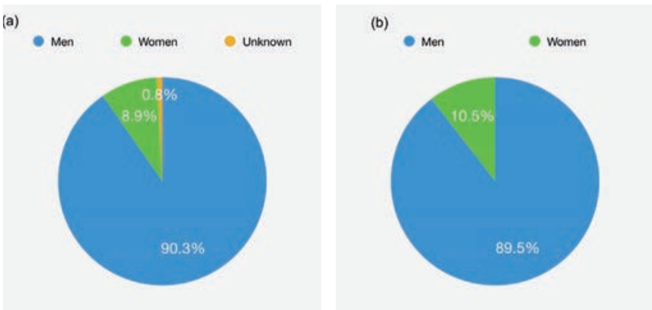

Fig. 5. (a) Gender representation on the editorial boards of 435 journals in the mathematical sciences, listed in the Thomson-Reuters Journal Citation Reports (2013). Adapted from source: https://doi.org/10.1371/journal.pone.0161357;
(b) Gender representation on the editorial boards of the 21 journals from the EMS Publishing House (2020).

As regards gender representation, numbers are not encouraging: the median journal editorial board only includes 7.6% of women, 62 journals have less than one-half percent women, while 51 journals (namely 11.7% of the total investigated journals) have no women at all.

The Women in Mathematics (WiM) committee of the EMS has independently found that the editorial boards of the 21 journals handled by the EMS Publishing House follow the trend of having a very low female representation (Figure 5b). The situation has changed from 8.9% (42/473) in 2018 to 10.5% (51/484) in 2020 (where the fractions refer to the number of women editors over the total number of editors). This means that in order to achieve the most conservative percentage of female editors so that it matches the proportion of female researchers in the mathematical sciences (estimated at 20%), the EMS Publishing House would have to invite an extra 58 women on board if the number of male editors were to be kept constant at 433.

If instead we wished to attain a threshold of 30% women editors, the EMS publishing house would have to invite an extra 135 female editors.

The discrepancy between the percentage of active women mathematicians and that of women acting as editors of scientific journals is totally unacceptable. This is by now a well-documented state of affairs which should be remedied as soon as possible. While we should continue to analyse and resolve the reasons behind this imbalance, we strongly believe the data make such poor reading that the publishers must make it their topmost priority to change the situation with regard to gender balance on editorial boards, thereby demonstrating their commitment to resolving this bias. The WiM committee has the experience to be a helpful resource for the publishers in such an important task.

We are optimistic, however, that the publishers are capable of developing more inclusive strategies of their own in the future.

This note grew out of discussions between all members of the Women in Mathematics committee of the EMS: Alessandra Celletti (Chair), Lisbeth Fajstrup, Stanislawa Kanas, Pablo Mira, Beatrice Pelloni (Past Chair), Elena Resmerita, Marie-Françoise Roy, Elisabetta Strickland, Anne Taormina and Katrin Wendland.

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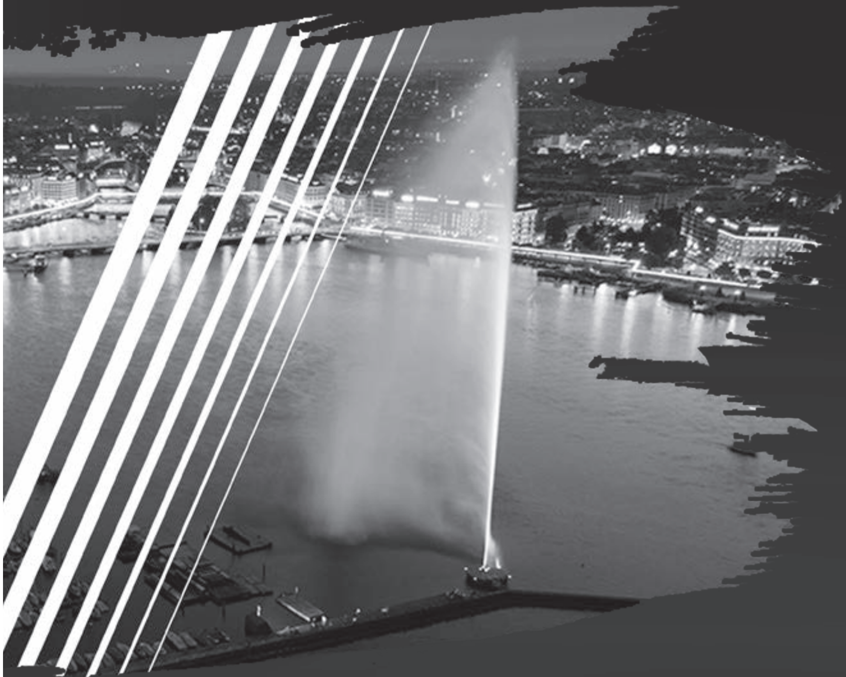
Stanislawa Kanas is Professor of Institute of Mathematics at the Rzeszow University. She is the President and founder of Polish Women in Mathematics and member of EMS Women in Mathematics Committee. She works actively for women, e.g. by initiating and organizing the international conference "On the trail of women in mathematics", delivering the lectures on famous women in mathematics, and supporting younger women studying and working in the field of mathematics.



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Working from Home. ~~2 Months~~ 8 Months and Still Counting…

Andrew Bruce (University of Luxembourg, Esch-sur-Alzette, Luxembourg), Katarzyna Grabowska (University of Warsaw, Poland), Dmitry Millionshchikov (Lomonosov Moscow State University, Russia), Vladimir Salnikov (La Rochelle Université, France) and Alexey Tuzhilin (Lomonosov Moscow State University, Russia)

My letter, 23/04/2020 (Vladimir Salnikov)

Dear friends and colleagues,
These days most of us are working from home and this seems to be a unique experience. We thought of writing an article about this topic...

As a reminder, these are the first lines of my letter to mailing list for scientific announcements, asking them to share the experience of (not) working from home and (not) spending time with the family in this context. We published the full text of it and several replies in the previous issue of the EMS Newsletter:
https://www.ems-ph.org/journals/show_abstract.php?issn=1027-488X&vol=9&iss=117&rank=7,
and we promised to continue in this one. I still hope it will not become a regular section, although I also received some feedback after the first publication like "Oh, I have stuff to share when/if I have time.", so let us see. I will be adding the dates again to give the proper context, and as promised before there are more "online" stories this time.

Andrew Bruce, letters from 24/04/2020 and 28/04/2020

Dear Vladimir,
Maybe not inspirational, but I do miss my blackboard and chalk. I am finding myself, in part due to this, a little less motivated than usual.

Another thing, maybe not completely mathematics related, is that I have explained to friends online about 5G more than once. Just some basic stuff about the electromagnetic spectrum, ionizing and non-ionizing radiation, how the power drops rapidly as you move away from the source. Some of my school friends are asking about science.

Best, Andrew

Hi Andrew!
Nice to hear from you! Indeed, it is probably tough to be stuck in the place [Luxembourg] where most people come to work and not being able to actually go to work… I am locked up in a kindergarten, so not very productive either… Hopefully it will end soon.

Thanks for sharing the 5G stuff. Me too, I had to explain some basic things to friends about "experts" on Youtube etc., and also what a reasonable clinical trial is. Sometimes surprised about supposedly well-educated people…

Anyway, good luck! Try not to get too desperate.
Yours, Vladimir

Hi Vladimir,
My situation is far more relaxed and peaceful than yours!

About the clinical trials, one old school friend is an antivaxxers. He has no concept of how science works, the amount of testing including clinical trials that are involved, and no idea that large-scale studies have and are being conducted on vaccine safety. It is madness, as all this information is available. However, the crap on Youtube needs a lot of filtering.[1]

Stay safe and enjoy this time at home, Andrew

Alexey Tuzhilin, letter from 24/04/2020

Dear Vladimir,
In my case, there has been no more change. Mostly I have been working from home, and I am still doing it now. The main difference is that I now use zoom for my lectures and seminars and do not lose time travelling to the university. In my opinion, it is more effective in the sense that you are mostly taking care of those students who are interested in education by themselves. The number of students attending my obligatory lectures has reduced in comparison to the usual amount. The reason, I think, is that the students are too shocked or impressed by the apocalyptic situation. In contrast, the number of people attending my special course has grown. In addition, this course has become international: some random people and some of my foreign friends have joined. When the isolation ends, I would like to continue my special course and seminar by means of zoom.

I do not think that we have any means of adequately explaining what maths is to non-maths people (except speculations to get some support). In my opinion, maths is similar to religion: to work successfully in it, you have to believe in maths, in its esoteric essence. And the only way to explain it is to draw a halo of mystery. To understand what is better, maths or cartoons, is similar to answering the question: should one believe in God, or live for momentary pleasure? Of course, it is better to have both. However, momentary pleasure can be obtained in many easy and quick ways, while the pleasure of maths is not obtained without some effort. How can we explain the pleasure of maths, its powerful purification of consciousness and the sparkle of pure mind as a result? How can esoteric people explain their state when they interact with mystery?

1 This discussion of fake news, strange opinions etc. … is very typical these days. With colleagues, we realized that probably now more than ever our role of educators is important.

That is why I simply continue to sit in front of a monitor full of different maths texts. It is my form of prayer, and my relatives appreciate that.

All the best, Alexey Tuzhilin

Katarzyna Grabowska, letter from 25/04/2020

Dear Vladimir,

Let me start by saying that it is very nice to hear from you!!! I wrote the following letter and then noticed that it is horribly long. Feel free to stop when you have had enough of it. Even before starting to read ;)

I will probably write a book about problems and observations I have made concerning remote everything, especially remote teaching. Let me then just point out two things:

1. My experience is that, whatever additional work or problem appears because of the isolation policy, it is expected to be done or taken care of by women. I have two children aged 16 and 19. They used to have a very independent lifestyle, including eating out almost every day. Usually at school, but sometimes in some kind of restaurant or cafe. Of course, now the school is closed, as well as restaurants, so I have to provide lunch every day, as tasty as possible and as healthy as possible. The family also needs some extras: a cake now and then… This takes up an enormous amount of time and moreover requires careful planning. All this is on me.

My daughter is supposed to take her Matura[2] exam this year. It has been postponed indefinitely. She was supposed to study in the UK, she has an offer e.g. from UCL, but who knows what will happen. This is of course all very stressful, and a mother is the person who should provide some comfort. And the same for other members of the family with their own problems and fears.

The next thing is that we used to have a cleaning lady, who of course does not come now, so cleaning is my next task. I can delegate, and I do, but it is still my responsibility to organise it.

Asia and I have been taking part in producing disposable masks for hospitals and other medical facilities, because in Poland there is still a problem with availability. This is voluntary work organised by a local scout organisation. Again, girls and women make up 90% of people doing it. As a result, I have not experienced anything like quarantine boredom! On the contrary, my to-do list is getting longer every day…

That was just the list of complaints, but that is how I feel now ;)

2. About remote teaching: my kids are fortunately grown up and quite good at studying. Moreover, their schools are reasonably organised. Piotrek finally has enough time to hone his coding skills, and that is what he does all the time.

I am, however, involved with helping refugee children with their homework. I have been doing it for some time, working with a family from Tajikistan. For them, remote teaching is a real challenge. They have one computer and one tablet for four children of different ages. They are not very good at Polish, especially the girls, since their social interactions are very limited due to their religion (they are Muslims). Additionally, the teachers are not very precise when setting the tasks for pupils. Maybe they themselves are just not comfortable enough in the digital world. Children definitely get overwhelmed by the number of difficult and unclear tasks. Explaining it all over Google Meet is sometimes really tricky. I have even started to record short films about basic primary maths like fractions. There are good resources with films like that e.g. Khan Academy, even translated to Polish, but it turned out that a more personalised approach is needed here. Here is my first film about the definition of a fraction, prepared for Jusuf who is 11 years old (the film is in Polish of course) https://youtu.be/gLE0nPKaBgQ

I also prepare appropriate films for my students. I am teaching calculus this year. As a result, I have the Implicit Function Theorem and Local Invertibility Theorem with the proofs all recorded. Tomorrow I will be playing with a bit of differential geometry, since I have to prepare short films about gradient, divergence, rotation and laplasian in different coordinate systems. If you want to see the example, here is the first part of the proof of the local invertibility theorem: https://youtu.be/cZd8K0bqg8I

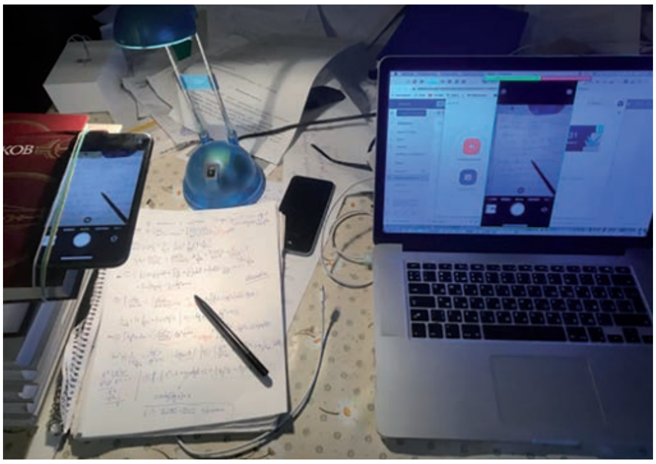
Ok. Enough of that.

Looking forward to hearing from you, Kasia

Dmitry Millionshchikov, letter from 24/04/2020

Moscow mathematician in self-isolation

The self-isolation[3] took us mathematicians by surprise, just like all the other professions. In Moscow, the developments were delayed by a couple of weeks, so we already knew about the success of the zoom software. Skipping the details, when you try zoom, you soon realise that you need a document camera or a tablet… what do you do if you don't have them? Within several days, they were all sold out in the IT shops; you could order one but the delivery time was about ten days, so not good for the next-day lecture. The picture below shows how encyclopedia volumes and a couple of rubber bands solved the problem.



[2] In Poland, end of high school.

[3] This is the word the Russian government uses for working from home, apparently to avoid all the social security issues.

Funny episodes

Almost immediately, I started the "Chronicles of a Dive Online Lecturer" on my web page. There I collect the funny events which have occurred during the online classes and zoom seminars, below are some of them.

I am often asked to share the experience: "Do you get any feedback from students during online lectures?" I answer, "I sure do!" I was recently giving a zoom lecture: inspired, with voice modulation… and I proved a theorem! The students were silent. I thought I was as convincing as ever, like Albus Dumbledore from the Harry Potter saga, pleased… Then I saw the zoom chat blinking in the corner of the screen: "Dmitry Vladimirovitch, we can't hear you!" Here is my feedback! I reconnected the micro and re-proved the theorem, no voice modulation this time.

During one of the first online classes, the screen suddenly started filling up with phalluses drawn in red marker pen. My voice: Ladies and Gentlemen, we have an intruder! There are not many of us, let us take a roll-call! Remove the enemies. Who is this "Vassily Utkin"? A football commentator, says one of my trusted students. Me: No, we don't need such commentators, it was his work. I kick out the fake "Utkin". Then I hear a teenager's voice: "Guys, please don't remove me. I won't lie, I'm a stranger too. But I like it a lot here. This is my second class, and I don't understand a word of it, but it's so cool!". But it is difficult to halt a cutting ax, so no more phalluses since then.

Today I was talking about sums and intersections of linear subspaces. Rainy weather, simple formulae and the total silence of the students inspired me to make associations and analogies with more motivation, and… a nonzero vector from the intersection became a person with double citizenship, showing one passport or the to sneak through the controls. The verification of the completeness of the system of vectors was compared to a sacred ritual, "like in The Adventure of Musgrave Ritual". And, wow, nobody had read that… "Dmitry Vladimirovitch, we have watched Sherlock, but we haven't read the book"… Well, homework: to read the "The Adventure of Musgrave Ritual" and "The Adventure of the Dancing Men" by Conan Doyle, and to be able to tell the scientific specialisation of Professor Moriarty.

I was giving a scientific zoom talk "in" Saint Petersburg (former Leningrad), and since the 22nd of April was the 150th birthday of Lenin, I decided to mention it in the talk. The topic was about the width of graded Lie algebras, and I started with an old joke.

N.B.: In Russian, the words "area" and "square" are the same, and the word "fortitude" may also mean "courage" and has the same root as "masculinity".[4]

"How do you find Lenin's square in Moscow?" – "Multiply Lenin's length by Lenin's width". Cadenza from analytical geometry: "Lenin's height = Lenin's volume / Lenin's square". Oh, how tough it is to joke online: you do not hear any reaction from the audience… The chairman of the seminar took pity on me, he told us how to find the Square of Fortitude in Saint Petersburg, which is round: "multiply the radius of fortitude squared by its pi".

Dmitry Millionshchikov has also written a long article about the challenges faced by the Russian higher education system during the pandemic. It is available (not translated) here: *https://www.forbes.ru/forbeslife/402941-tehniku-i-shtativy-priobretali-za-svoy-schet-kak-karantin-vyyavil-osnovnye*

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[4] For Russian-speaking readers, we give both jokes verbatim here: "Как найти в Москве площадь Ильича?" - "Да умножьте ширину Ильича на длину Ильича!" С каденцией из аналитической геометрии: "Высота Ильича=Объем Ильича/Площадь Ильича". Ох и плохо же шутить онлайн лектору - ведь не слышит он реакцию слушателей. Руководитель семинара пожалел меня и рассказал как в Питере найти площадь Мужества: "Умножить квадрат радиуса Мужества на его пи".

The Lithuanian Mathematical Society and Mathematical Life in the Country

Remigijus Leipus and Eugenijus Manstavičius (both Vilnius University, Lithuania)

The Lithuanian Mathematical Society (LMS) is a learned society unifying mathematicians – university and college professors, teachers and institute researchers. It varies from 200 to 300 in number. High school teachers, as well as personnel from industrial or business enterprises applying mathematical methods and people generally interested in mathematical knowledge, are likewise warmly welcomed. The LMS aims to develop research in theoretical and applied mathematics, support mathematical education and spread mathematical ideas and knowledge. This short presentation attempts to highlight the Society's activities in achieving these goals. The genesis of the LMS is rather specific; therefore, we deem it worthwhile to present it within the frame of the historical development of education and science in Lithuania.

The emergence of mathematics as a teaching subject in Lithuania traces back to its sixteenth century colleges. The culminating point was the foundation of the *Academia et Universitas Vilnensis Societatis Iesu* in 1579. Throughout the centuries, the university (sometimes bearing different names, ending up as VU) became a permanent fixture of the country, even if the palmy days were often interrupted by war and oppression. In the first quarter of the nineteenth century, mathematics teaching at VU was of European standard. The three chairs (of pure, applied and elementary mathematics) hosted visiting lecturers; at the same time, the VU enabled its university professors to visit research centres abroad. For example, the manuscripts written by Pranciškus Norvaiša (Narwoysz) during his stay at the University of Nancy have been found there in the library. This advancement come to an end in 1832 when the Russian Tsar closed the Alma Mater. The penalty for the uprising was spot-on: while European science was starting to grow rapidly and the first learned societies were spreading out at European centres, Lithuania was left without a research hearth. After the next deliberation attempt in 1863, the use of the Lithuanian language in official life was forbidden.

The first textbooks in arithmetic and geometry used in the secret Lithuanian schools had to be published in Prussia and transferred through the border at great danger. Soon after the abolition of the Lithuanian press ban in 1907, *The Lithuanian Learned Society*, containing a few enthusiasts in mathematics, was founded. In the independent state of Lithuania, during 1918–1939, much was undertaken to revive the system of education. The mathematicians were very active, having joined *The Society of Teachers of Mathematics and Physics*. In short, the original Lithuanian textbooks in mathematics covered all high school requirements. The first research papers were published by professors of the newly founded university in Kaunas. Professor Otto Folk, who had come from Munich for eight years, was the most active. Here we must recall that the Polish university named after King Stephen Báthory was functioning on the old university premises in Vilnius. The prominent mathematicians Anthony Zygmund and Jozef Marcinkiewicz, to name but a few, were holding lectures there.

The invasion of the Soviets in 1940 and the following deportation of the intelligentsia to Siberia put a halt to this smooth development. The Nazi occupation was no better. Many of the high school teachers and university professors who had survived fled to the West at the end of the war. In the first post-war years, the kernel of teaching staff at the VU comprised of only a few dedicated professors. The rise of the mathematical school and mathematical life in Lithuania is thanks to the efforts of Jonas Kubilius (1921–2011). Soon after the defense of his Candidate of Science Thesis (=PhD, 1951, advisor J.V. Linnik), he returned from Saint Petersburg and took up the lifelong leadership. A vast survey of Kubilius' contributions is the second author's obituary paper in *Acta Arithmetica* (157.1 (2013), 11–36). Let us outline more of the activities starting in the 1950s.

Enhancing education and research in mathematics was the main purpose. For younger enthusiasts, the focus was on the annual olympiads, which began in 1952. At universities, the individual work with bright students to attract them to doctoral studies was extended. The most gifted university students were directed to continue their studies in Moscow, Saint Petersburg and Novosibirsk. Implementing Kubilius' idea to concentrate the limited human resources on a few research branches like probability theory and mathematical statistics, a few of the VU graduates were sent to write dissertations in this field. So Vytautas Statulevičius (1929–2003) went to explore the Markov chains in the well-established mathematical schools headed by Linnik and A.N. Kolmogorov, while Bronius Grigelionis (1935–2014) made his first contributions to the theory of random processes in Kyiv under the guidance of B.V. Gnedenko. Later on, both influential Lithuanian mathematicians set up their own research schools.

In the 1950s, regular mathematical seminars and annual conferences by the teaching staff of the VU and researchers from the newly founded (1956) Research Institute of Physics and Mathematics began. That developed into the annual national conferences at which the teachers of mathematics were well represented. During the discussions at the first of them, held in 1958, the question about the necessity of a national mathematical society was raised and a working group to arrange its statute was appointed. The foundation of a national professional society in the former Soviet Union was by no means simple. The authorities were only ready to allow the formation of a branch of an all-union society. There was no such mathematical society at that time. Luckily, a short but slightly warmer political period followed. By that time, Kubilius had become a world-renowned scientist, already having written the founding monograph of probabilistic number theory. Witnessing its importance, we recall the following sentence from the review in the *Bulletin de la Société Mathematique de Belgique* (1967, vol. 19): *The work is an epoch-making event in its originality and in the effectiveness of the principle used*. Kubilius gained influence in the eyes of the local authorities; in 1958 he was appointed the rector of the VU. Due to the confluence of circumstances, Lithuanian mathematicians were granted their desired society, albeit under the formal patronage of the Lithuanian Academy of Sciences.

On 3 February 1962, the Statute of the LMS was registered. The latest edition of the Statute, accepted in 2014, anchored the LMS as an independent society. The consolidated efforts of the essential base of mathematicians in the country had a great impact. Kubilius was the indispensable president until his death. The authors of this article have had the duty and honour to take over this leadership (Manstavičius from 2011 to 2014, Leipus since 2014).

In the 1960s, the great lack of well-qualified mathematicians was still being felt in the evolving and newly opened institutions. Nevertheless, numerous gifted young scientists matured during this decade. Several young mathematicians with doctoral degrees received from the leading Soviet universities also joined their colleagues in their native country. Apart from research in number theory, probability theory and mathematical statistics, the number of papers in other branches of mathematics, in particular differential equations, numerical mathematics and mathematical modelling significantly increased. It was the LMS who constantly stimulated the collaboration among the research groups. The annual national

Participants of the 60th LMS Conf. at the General Jonas Žemaitis Military Academy of Lithuania, June 19, 2019.

conferences of mathematics were organised at all institutions in a round-robin fashion.

The LMS was one of the founders of the *Lithuanian Mathematical Journal* (1961). From 1973–2007 the journal was translated by Plenum and Kluwer publishers; since 2008 it has become a Springer edition. Editors-in-Chief were Petras Katilius, Jonas Kubilius and Mifodijus Sapagovas; since 2008 Vygantas Paulauskas has held this position. In addition, nowadays four international mathematical journals are issued in Lithuania and are seeking prestige. Many international conferences in mathematics have been organised in Lithuania. The LMS has intensified its help to the organisers, in particular, dealing with the fiscal formalities which are very strict in the country. The European Mathematical Society has supported: the 27th *Journée Arithmétique* (Vilnius, 2011), the 4th, 5th and 6th conf. on number theory dedicated to Kubilius' jubilees (Palanga, 2006, 2011 and 2016); and the 11th and 12th Vilnius' International Conferences on Probability Theory and Mathematical Statistics (2014, 2018). The latter was held jointly with the 2018 IMS Annual Meeting. Here, we must pay tribute to Statulevičius and Paulauskas as the former chairmen of the Organising Committees, the first author took command of them in the last few years. The contribution of the co-chairs of the Organising and Program Committees Erwin Bolthausen, Peter Bühlmann and Peter Jagers, as well as of all committee members including our colleagues Mindaugas Bloznelis, Rimas Norvaiša and Donatas Surgailis, to mention but a few, has been greatly appreciated. The 13th Vilnius conference is on the agenda for 2022.

Participants of the Vilnius 10th Conf. at the Cathedral, June 28, 2010.

Attention to the high school problems has not been decreased. Nowadays, the *Lithuanian Mathematics Teachers Association* (founded in 1991) is a faithful partner of the LMS. Apart from textbooks, many complementary material issues and popular books have been published. Lithuanian school children actively take part in the International Kangaroo Test, in a dozen regional olympiads. The winners of the annual national olympiads comprise the teams taking part in international events such as the *Mathematical Contest of Friendship in Honour and Memory of Grand Duchy of Lithuania*, the *Middle European Mathematical Olympiad* and *The Baltic Way* contest. The achievements at the *International Mathematics Olympiad* are comparable with that of countries of a similar size. Nevertheless, the lowering of the average level at middle schools is causing great concern. The continuing reforms and reduction of the teaching hours devoted to the subject of mathematics as well as the decreasing attention of the authorities and the low prestige of a teacher's position have had a negative influence.

Raising public awareness, the LMS and academic institutions have published several popular books devoted to the memory of praiseworthy personalities or to the history of mathematics in Lithuania. The LMS is ashamed to admit that the popular *Mathematical Journal* α+ω (1996–2003, Ed. Vilius Stakėnas) has terminated its existence. The most valuable are the activities (since 1983) of the *Lithuanian Henrikas Jasiūnas Museum of Mathematicians* named after its founder. The reader is welcome to visit its homepage: http://www.matmuziejus.mif.vu.lt/

From the Museum exposition; portraits of Kubilius by Vytautas Ciplijauskas and of Statulevičius by Vladas Karatajus.

The research achievements in the field of mathematics have won recognition in Lithuania. This can be witnessed by 12 National Science Prizes won by 22 mathematicians during 1991–2019 and two prestige Kubilius' Prizes of the Lithuanian Academy of Sciences (so far, the grantees were Manstavičius, Paulauskas and Surgailis). For its part, the LMS honours those contributing to science and education in the country by presenting the Zigmas Žemaitis Medal. Recently, the Prize of the LMS for mathematicians not older than 40 has been established. The first laureates were Vytautas Paškūnas (University of Duisburg-Essen), Kęstutis Česnavičius (Université Paris-Sud 11) and Paulius Drungilas (VU). In these times of continuing brain-drain, young Lithuanian students or those who already have positions abroad gather in their native country before New Year's Eve for a multidisciplinary workshop supported by the LMS. In 2019 the 8th one was held. Many of the participants are waiting for greater support from the government's side in terms of increased wages in academic positions.

In the last decade, the LMS has intensified its contact with the EMS by taking part in most of the European events. Recall that Kubilius and Grigelionis signed the founding agreement and happily toasted the genesis of the EMS at the Madralin meeting in 1990. The LMS joined the International Mathematical Union in 1995.

More information can be found on the LMS webpage: http://www.lmd.mif.vu.lt/

Zigmas Žemaitis Medal

Remigijus Leipus [remigijus.leipus@mif.vu.lt] is professor and director of the Institute of Applied Mathematics at the VU. He has been a president of the LMS since 2014. He has visited many universities throughout the world; in particular, he had a one-year Fullbright scholarship at Virginia Tech in 1993–1994, worked at the University of Liverpool in 1997–1998 and at the University of Utah in 2005. He collaborates with many partners in USA, France, China, etc. He was a chairman of the recent International Vilnius conferences on Probability Theory and Mathematical Statistics. Research interests lie mainly in probability theory, mathematical statistics and econometrics, since 2014 he is a full member of the Lithuanian Academy of Sciences.

Eugenijus Manstavičius [eugenijus.manstavicius@mif.vu.lt] is a professor emeritus at the VU. He served as president of the LMS from 2011 to 2014. He has held visiting positions at a dozen universities throughout the world; in particular, had a one-year professorship at the Paderborn University, worked on an individual EC project at the University of Bordeaux 1, collaborated with a partner in an NRC Twinning Program project at the Pennsylvania State University. His research interests lie mainly in probabilistic number theory and combinatorics. He is a holder of the National Science Prize of Lithuania and the Kubilius' Prize of the Lithuanian Academy of Sciences, to which he was elected in 2004 as the expert member, and since 2011 he is a full member.

Creation of the Standing Committee for Gender Equality in Science

Maria J. Esteban (CNRS and Université Paris-Dauphine, France)

SCGES Standing Committee for Gender Equality in Science

Some years ago, the International Council for Scientific Unions (ICSU) launched a call concerning the gender gap in science, to understand and measure it and to propose solutions to reduce it. Several members of ICSU, among them the International Mathematical Union (IMU) and the International Council for Industrial and Applied Mathematics (ICIAM) answered the call. The project was called 'A Global Approach to the Gender Gap in Mathematical, Computing and Natural Sciences: How to Measure It, How to Reduce It', and it was supported first by ICSU and then by the International Science Council (ISC), the result of a merger between ICSU and the International Social Science Council (ISSC).

A number of international unions and councils which took part in the project, and among them ICIAM and IMU, wished to act together to further promote gender equality in science by continuing and enlarging the work accomplished by that project and, in particular, by supporting women's and girls' equal access to science education and fostering equal opportunity and treatment for females in their careers. For this purpose, they have acted as founding partners for the establishment of a Standing Committee for Gender Equality in Science (SCGES). The founders are:

- International Astronomical Union (IAU)
- International Council of Industrial and Applied Mathematics (ICIAM)
- International Mathematical Union (IMU)
- International Union of Biological Sciences (IUBS)
- International Union of History and Philosophy of Science and Technology (IUHPST)
- International Union of Pure and Applied Chemistry (IUPAC)
- International Union of Pure and Applied Physics (IUPAP)
- Association for Computing Machinery (ACM)
- Gender in Science, Innovation, Technology and Engineering (GenderInSITE)

Further international organisations may join later as partners.

Among other things, the members of this Standing Committee are supposed to:

- Endeavor to promote gender equality in their own structure, proceedings and scientific discipline, noting the recommendations of the Gender Gap in Science Project.
- Share with SCGES, and especially with its chair, all relevant information that can help promote gender equality in science.
- Within the limits of its capacity, set up projects and initiatives to promote gender equality in science; for this purpose they may seek cooperation with other members of the Standing Committee.
- Decide whether to contribute to projects and initiatives led by other partners and endorsed by the SCGES, and consider the modalities of this contribution.
- Share communication relevant to gender equality in science with its members and through its networks by all means at its disposal, including social network accounts, website, newsletters and journals, electronic and in print.
- Call upon its member organisations or its representatives, if relevant, to set up national or regional initiatives to promote gender equality in science.

The committee was founded only recently and has only met for the first time in September 2020. Its newly created website can be found at the following address: https://gender-equality-in-science.org/

Maria J. Esteban is a CNRS senior researcher at Université Paris-Dauphine. Specialist in nonlinear PDEs and Mathematical Physics, she has been President of SMAI and ICIAM, and currently she is one of the ICIAM Officers as Past-President. More information about her activities and her CV can be found at her webpage https://www.ceremade.dauphine.fr/~esteban/MJEpage-engl.html.

ICMI Column

Jean-Luc Dorier (Université de Genève, Switzerland)

Review of ICMI studies: some initial findings

Merrilyn Goos[1]

The aim of the review of ICMI Studies is to obtain structured feedback from the wider ICMI community on whether the stated goals for ICMI Studies remain relevant and the extent to which these are being realised. Each ICMI Study is built around an international conference of invited experts in a specific field of contemporary interest in mathematics education, and results in a published Study Volume that communicates the main outcomes as well as proposals for future research and action. At the time of writing, 23 ICMI Studies have been completed, and an additional two Studies are in progress.

The first phase of the review comprised an online anonymous survey of past ICMI Study participants (for Studies 12 to 25). The survey asked the following broad questions:

- How relevant are current goals of ICMI Studies?
- To what extent are these goals being met?
- Is the time frame for completing a Study (up to 3 years) feasible for ensuring that the Study Volume is an up-to-date resource?
- What evidence is there of the impact of ICMI Studies on theory, policy, practice, research community development and individual careers?
- What is the distinctive contribution of a particular ICMI Study to growth of that field?
- What is the cumulative contribution of ICMI Studies to the field of mathematics education?
- To what extent are the Studies "international" in intention and enactment/
- How can participation and voice of developing countries be broadened in ICMI Studies?

There were 171 responses to the online survey, 41% of whom were male and 59% female. The geographical distribution, years of research experience and ICMI Study distribution of the respondents are shown in Figures 1, 2 and 3 respectively. Almost half (45.6%) of the survey respondents came from Europe, and a little more than one-quarter (27.5%) from the Americas, with 13.5% from Asia, 7.0% from Oceania, 5.3% from Africa and 1.2% from other regions. Half the respondents had more than 20 years of research experience. The number of respondents who had participated in each Study varied from 6 (for Study 16: *Challenging mathematics in and beyond the classroom*) to 51 (for Study 25: *Teachers of mathematics working and learning in collaborative groups*). While 71.3% of respondents had participated in only one Study, 12.2% had been involved in two, 8.8% in three, and 7.6% in four or more Studies. The respondents included Study Conference participants who had a paper accepted, conference co-chairs, IPC members, invited speakers and ICMI Executive Committee members.

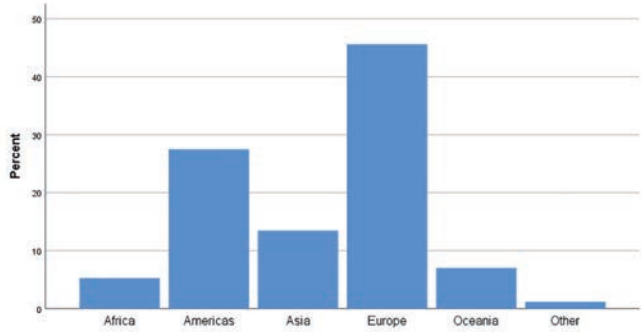


Fig 1. Geographical distribution of survey respondents.

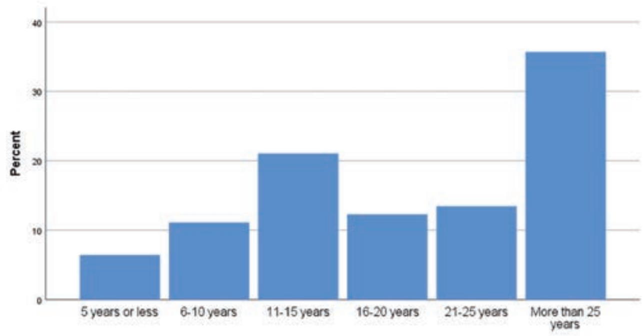


Fig. 2. Years of research experience of survey respondents.

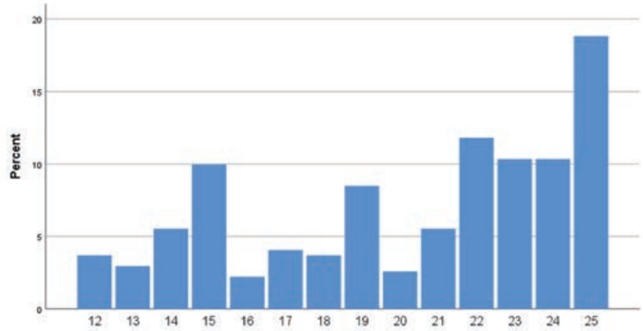


Fig. 3. ICMI Studies attended by survey respondents.

There was strong endorsement of the *relevance* of ICMI Study goals, with at least 65% of respondents rating all nine goals as being of either high or very high relevance. Based on these responses, the most relevant is Goal 1: To bring together international scholars (representative of

[1] This report made by Merrilyn Goos (ICMI vice-president) was first published in the July 20 ICMI Newsletter. It is reprinted here with her authorisation.

diverse cultural contexts, perspectives and backgrounds) to exchange knowledge, collectively reflect and discuss a specific theme, topic or issues in mathematics education (endorsed by 87.7% of respondents). More than three-quarters of respondents (76.1%) considered that ICMI Study *goals were met* to a large extent or in full.

In contrast to these positive assessments of the value of ICMI Study goals, the survey respondents were less certain of the impact that ICMI Studies have on *theory, policy and practice*. However, the Studies were thought to have substantial impact on *research community development* and, to a lesser extent, an impact on *individual careers* (65.5% and 39.2% of respondents, respectively, rated these as high or very high impact).

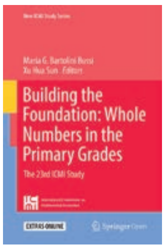
ICMI Study participants who responded to the survey identified many distinctive contributions of ICMI Studies to the field of mathematics education, in particular the fostering of international participation across diverse contexts, cultures and theoretical perspectives. Respondents also recognised ICMI's efforts to achieve greater inclusion of participants from low income or developing countries, while acknowledging the challenges of fully realising this intention.

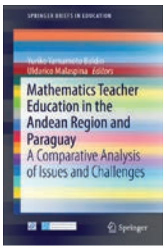
We would like to thank everyone who responded to the survey, and especially Dr George Ekol for his contribution to quantitative analysis of survey responses. In this article, we have deliberately refrained from presenting any commentary on the survey responses, because we would like to invite readers to contact us with your own interpretations. (Please send your views to both merrilyn.goos@ul.ie and jill.adler@wits.ac.za.) Your additional contributions will inform our analysis and discussion with the ICMI Executive Committee, as well as subsequent phases of the review that will involve interviews with key participants in past ICMI Studies.

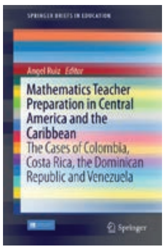
CANP – Open access publications (New!)

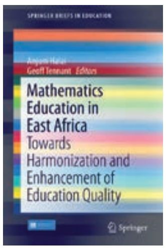
With the publication of ICMI Study 23, ICMI has decided to make relevant publications accessible to all (Open Access). Readers can find the Volume of ICMI Study 23 at https://www.springer.com/gp/book/97833196355457

ICMI has signed a contract with Springer to publish the upcoming ICMI Study Volumes (24 and 25) as open access as well as the existing books published by CANPs 2, 4 and 5, which will be available very soon.









Gender gap in science book now available

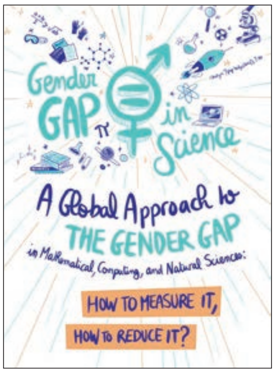
The Gender Gap in Science project's final report can be downloaded at: https://gendergapinscience.files.wordpress.com/2020/02/final_report_20200204-1.pdf).

This was a three-year project funded by the International Science Council (see https://council.science/) together with eleven scientific partner organisations to investigate the gender gap in STEM disciplines from different angles, globally and across disciplines. ICMI Vice President, Merrilyn Goos, was involved in several aspects of the study including the authoring of sections of the final report.

The study developed innovative methodologies and tools together with a set of recommendations addressed to different constituencies – instructors and parents; educational institutions; scientific unions and other organisations responsible for science policy – in order to reduce and possibly eliminate the gender gap. See the project website at https://gender-gap-in-science.org/ for details.

The Gender Gap in Science book is now available in hard copy format through the low-cost print-on demand service of IngramSpark. It can be ordered through many retailers worldwide (e.g., Book Depository, €10.41).

Here are the publication details for the book:



Authors: Colette Guillopé, Marie-Françoise Roy
A Global Approach to the Gender Gap in Mathematical, Computing, and Natural Sciences. How to Measure It, How to Reduce It?
Publisher: International Mathematical Union, June 2020.
Paperback, 244 pages.
ISBN 978-3000655333

ERME Column

Pedro Nicolás Zaragoza (University of Murcia, Spain) and Jason Cooper (Weizmann Institute of Science, Israel)

ERME topic conferences

European Society for Research in Mathematics Education (ERME) Topic Conferences (ETC) are organised on a specific research theme or themes related to the work of thematic working groups at CERME conferences. Their aim is to extend the work of the group or groups in specific directions, with clear value to the mathematics education research community. We report herein on a recent ETC.

INDRUM – International Network for Didactic Research in University Mathematics

As reported in the March 2020 issue, the Third Conference of the International Network for Didactic Research in University Mathematics (INDRUM 2020) was initially planned to take place in Bizerte (Tunisia), 27–29 March 2020. Due to the coronavirus pandemic, it was postponed to 17–19 September 2020, also in Bizerte. Finally, given the exceptional situation due to Covid-19, and the uncertainty about travelling, INDRUM 2020 was held in the form of an online conference, on 12–19 September 2020.

INDRUM 2020 is an ERME Topic Conference which falls within the activities of the research project INDRUM. Initiated by an international team of researchers in didactics of mathematics, this project aims to contribute to the development of research in didactics of mathematics at all levels and contexts of tertiary education, with a particular concern for the development of new researchers in the field and for dialogue with mathematicians.

Despite the adverse conditions, this conference attracted 189 registered participants from 33 countries, 4 continents, with time zones spanning from UTC-9 to UTC+9. There were 4 parallel thematic working groups, in which a total of 44 research papers and 5 posters were accepted for presentation and discussion. One of the thematic working groups was devoted to calculus and analysis; another was dedicated to modelling and the role of mathematics in other disciplines (for instance, engineering); a third was dedicated to number theory, algebra, discrete mathematics and logic; and a fourth was devoted to students' and teachers' practices. There was also a plenary panel concerning tertiary education in the digital age, which is the focus of this column.

INDRUM panels

As INDRUM activities aim to not only be of interest to researchers in didactics of mathematics, all INDRUM panels to date (in 2016, 2018, 2020) have not addressed very specific issues, but rather a broad range of themes that involve mathematicians as well as educators. Thus, the INDRUM 2016 panel, chaired by Marianna Bosch, was about the current state of interactions between mathematicians and research in mathematics education [1], and the INDRUM 2018 panel, chaired by Carl Winsløw, was about education and professional development of university mathematics teachers [2].

Introducing the INDRUM 2020 panel: Tertiary education in the digital age

Pedro Nicolás Zaragoza chaired the panel, and the panellists were Yael Fleischmann from the Norwegian University of Science and Technology; Ghislaine Gueudet from the University of Brest, France; and Said Hadjerrouit from the University of Agder, Norway.

Digital resources provide both teachers and students with a whole world of possibilities, and their potential is difficult to overestimate. Actually, the presentation of the panel started out by emphasising that, without digital information and communication technologies (ICT), not only would teaching have been impossible in many countries in recent months, but also the INDRUM conference itself could not have taken place.

Many issues arise concerning the use of digital resources in the teaching of mathematics at tertiary level. To begin with, the question of what can be considered a digital resource is interesting in itself, as different theories in didactics provide alternative conceptualisations of this notion, emphasising different possible roles played by these resources and depending on the kind of instruction these theories are interested in. Gueudet addressed this question, in connection with the recent evolution of mathematics education research in the study of digital resources and their use at university. Also, she wondered about which aspects of digital resources and their use are specific to tertiary level. Related to this, Fleischmann and Hadjerrouit considered the question of whether the instruction of some topics in mathematics can be improved thanks to digital means, and connected this to the question of whether digital resources are possibly more relevant to tertiary education than they are to secondary education. In his contribution, Hadjerrouit also addressed the problem of analysing the idea of digital resource, both from the technological point of view and from the didactic perspective of mathematics education. He also considered the relevance of digital means for tertiary education, and its reliance on many factors (intended educational ends, expertise of users, blending with other means, etc.). The idea that digital tools can be useful for some student-centred didactic paradigms was also tackled in Hadjerrouit's contribution.

Finally, the three panellists shared some insights regarding the impact of the Covid-19 crisis on teaching practices. For instance, the need to use unfamiliar digital resources to provide online or blended learning, the corresponding changes in teaching strategies, their effect on students' learning and consequences for the assessment of the course.

There are many issues that remain to be studied regarding the potential of digital resources and their possible use, depending on a given didactic paradigm, which are interesting for both researchers and practitioners in mathematics education. This panel served as a useful step in this direction.

ERME Thematic Working Groups

The European Society for Research in Mathematics Education (ERME), holds a bi-yearly conference (CERME), in which research is presented and discussed in Thematic Working Groups (TWG). The initiative of introducing the working groups, which we began in the September 2017 issue, will continue in the following issue of the newsletter.

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[2] Biehler, R., Jaworski, B., Rønning, F., Wawro, M., and Winsløw, C., Education and professional development of University Mathematics Teachers, *Proceedings of the Second Conference of the International Network for Didactic Research in University Mathematics* (INDRUM 2018, 5–7 April 2018), University of Agder and INDRUM, Kristiansand (Norway), eds. V. Durand-Guerrier, R. Hochmuth, S. Goodchild and N. M. Hogstad, pp. 12–13.

Pedro Nicolás Zaragoza is an associate professor at the University of Murcia's Faculty of Education. His research concerns both mathematics – primarily Homological Algebra and Representation Theory – and mathematics education – from the framework of the Anthropological Theory of the Didactic, with a special interest in the role played by reasoning in the genesis and development of mathematical knowledge.

Jason Cooper is an associate staff scientist at the Weizmann Institute's Department of Science Teaching. His research concerns various aspects of teacher knowledge, including roles of advanced mathematical knowledge in teaching mathematics and contributions of research mathematicians to the professional development of mathematics teachers.

On the Road to a Comprehensive Open Digital Mathematics Library

Darius Ehsani and Olaf Teschke (FIZ Karlsruhe, Germany)

In our previous column [BBHST], we discussed the feasibility of transforming scanned mathematics into formats which allow for automated digital processing. Obviously, the minimum requirement here is the existence of an openly available digital mathematical object, which is also what would be sufficient for most working mathematicians. Indeed, the comprehensive open digital availability of mathematics literature is the classical vision of the World Digital Mathematics Library (WDML), formulated more than 25 years ago. Since then, progress has been made through different approaches and policies. The aim of this column is to give a short overview on the current status.

Possible scope and extent

Given that mathematics is the language of exact science and is interconnected with so many diverse areas, it is almost impossible to precisely define the desirable extent of WDML. Throughout the decades, disciplines have undergone an evolution which is also reflected in publication patterns. Even supposedly uniform services like zbMATH have adapted their indexing policy several times – currently, it reads "published and peer-reviewed articles, books, conference proceedings as well as other publication formats pertaining to the scope defined by Mathematical Subject classification 2020 that present a genuinely new point of view"[1]. If this cannot cover all fields in which mathematicians are involved through their research activities, it hopefully reflects most of the needs of the community and allows for interconnection with digital libraries in other disciplines.

Another aspect is the application of the scope of the definition – even services with a fairly similar approach show significant historical differences. E.g., MathSciNet and zbMATH have been found to have a historical overlap of just about 60% [IT][2]. In the following, we will work with zbMATH data, but it should be taken into account that this may leave a considerable amount of publications omitted.

How much mathematics is digitally available?

Digitisation efforts in mathematics already started in the first years of the internet. At that time, they were often only identified by their url, which quickly lead to the well-known problem of dead links. zbMATH still contains a significant number of hard urls mostly dating back to these days, and experience shows that they are only occasionally useful (though they may sometimes help to trace back sources via the Internet Archive). For many years now, doi have been standard for accomplishing an (ideally) unique, sustainable referencing of digital publications. They are accompanied by stable IDs generated by large platforms and repositories such as arXiv, EuDML, Gallica, JSTOR, Math-Net.ru, or Project Euclid. This system of identifiers has proved its worth and is still incredibly reliable, although all kinds of issues occur on an individual basis[3]. Nevertheless, the number of working links remains relatively high; we checked the availability of digital objects given by these identifiers in the zbMATH database and obtained a success rate greater than 99.4%. This gives a much better picture overall in mathematics than the general analysis in [KB].

Thus, these IDs can be used for estimating the extent of digitally available mathematics. Below, we give the share of publications with sustainable digital identifiers in zbMATH.

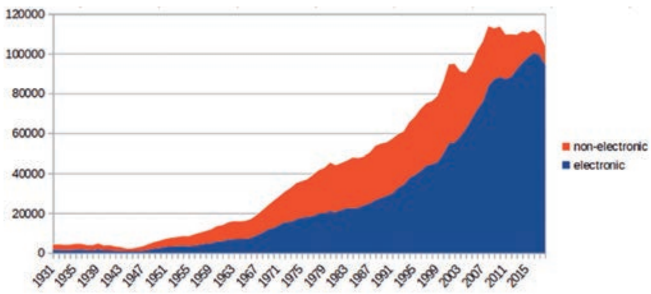

Fig. 1. Share of electronically available publications in zbMATH per publication year.

However, there is one important caveat: the number of publications does not tell the full story. Stable identifiers are much more prevalent for journals than for books. Historically, the latter have contributed up to 50% of the pages of published mathematical research (although this share has shrunk to about 14% recently), so the figures of digitally available pages look much less impressive. Based on [IT], we estimate that just above 60% of the

[1] Note that the formulation of the last addition is relatively new, triggered by the growth of (semi)trivial publications.

[2] Naturally, generic aggregators like Google Scholar differ by a much larger factor.

[3] To name only a few that popped up during our availability checks at the time of writing this note: All doi for historical content of a classical maths journal did not redirect properly after a change of the publisher; all doi of a publisher did not resolve in some browsers due to cookie issues; doi assigned to new articles not registered more than one year after publication; same doi given to different articles; different doi resolving to identical digital resources, doi leading to an official dead landing page after change to a non-CrossRed member.

roughly 130 million pages of maths research since 1868 are digitally available.

What is the share of open accessible publications?

Open Access issues have been a recurring topic during the last decades, and have also been frequently discussed in this column (see, e.g., [T] for the various shades of OA in mathematics). Three approaches have contributed to the open availability of the literature: genuine OA publications, DML platforms like EuDML, Project Euclid, or Math-Net.ru, and green OA repositories like arXiv or HAL[4]. Overall, the growing numbers for all three solutions look promising, but they come with certain caveats. First of all, taking the previous remark into account, they do not so far apply to a large chunk of the mathematical publications. Books are not only less frequently digitised, but also much more rarely available open access; hence, the number of open available math pages looks much less impressive than the number of OA publications.

Significant progress is only seen when we restrict to digitally available journal articles. Below, we give a figure of the numbers for all three kinds of OA approaches by publication years derived from zbMATH (note that recent years are not yet fully covered):

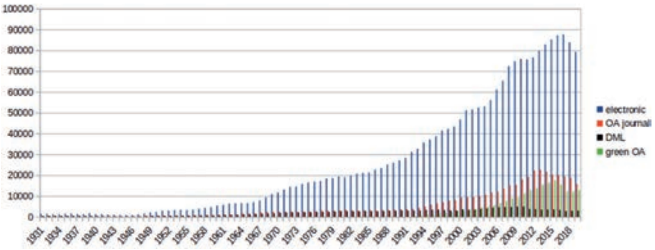

Fig. 2. Digitally available journal articles indexed in zbMATH, and those OA available directly via OA journals, green OA, and DML platforms.

This looks like a relatively successful picture, especially since one is tempted to count the three OA approaches cumulatively. However, the numbers do not tell us much about the overlap. E.g, arXiv overlay journals will automatically appear both as OA journal publications and green OA, and all other combinations are likely to happen as well. A detailed breakdown of the respective shares reveals a more granular picture (see Fig. 3).

A perhaps surprising takeaway is that historical publications in fact have a larger OA share than recent ones, mainly thanks to both the open DML platforms and open society journals with a rich tradition. There is a stable overlap of still existing OA journals available through the platforms, though, also driven by the existence of moving wall OA journals on the platforms (which explains the diminishing role of DML for recent publications). The shrinking share of OA for publication

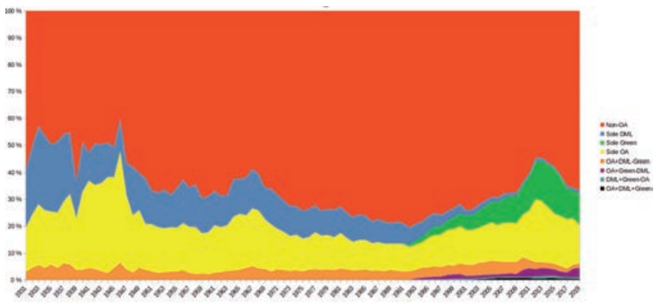

Fig. 3. Share of different OA resources for digitally available journal articles indexed in zbMATH.

years up to 1992 illustrates the concentration dynamics involving large commercial publishers. It is clearly visible that the creation of arXiv was a game changer in the 90s, reversing the trend. The relevant and stable share of papers both openly available at arXiv and at OA journals is not just due to overlay journals, but reflects a general OA-friendly community in several areas.

A more ambiguous trend is the sudden spike of papers in sole OA journals from about 2008 (and their decline after 2012). This reflects the boom of both APC and nickel OA journals which started around this time (see [T] for a more detailed discussion). Numerous examples in the following years indicate that their formal peer review process might not have always have been sufficient to live up to the classical zbMATH standards. This resulted in the tightened indexing policy mentioned above, coming into effect in 2017.

Such effects can be omitted when we restrict our analysis to core mathematics journals, which we define as journals indexed in zbMATH as Cover-to-Cover and belong to the top two internal categories [T]. They make up about 40% of zbMATH indexed electronic journal articles, with a growth of about 30% during the last decade (compared to about 50% overall). As discussed in [T], APC journals, which are responsible for most of the growth of sole OA journal publications, play almost no role in core mathematics journals. Hence, the figure for the relative share of the OA solutions looks a bit different when restricted to core math electronic journals:

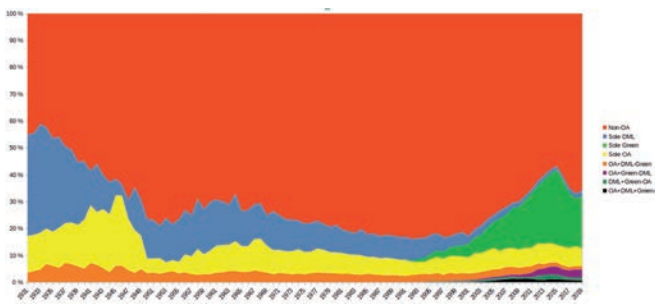

Fig. 4. Share of different OA resources for digitally available core math journal articles.

Here, we do not see an APC spike; on the contrary, the share of OA journal articles has been remarkably stable throughout the last decade. This does not imply stagnation – indeed, as we are all aware, there are numerous ini-

[4] An analysis of recent zbMATH publications with unpaywall [https://unpaywall.org] confirms that the arXIv is still by far the largest sole source for green OA in mathematics, although other repositories gained shares during the past years.

tiatives – it just says that they did not outpace the general growth. In comparison, the impact of green OA is even more significant here. It basically accounts for all progress made in the OA share during the past two decades.

The diagram may allow some conclusions about feasible approaches to further expand the share of OA publications. First of all, the strength of DML platforms, which already provide us with a large share of the literature until the 1960s, should also be used to facilitate the integration of more recent publications (note the dip from the mid-1960s until 2000 in the diagram!). This could be achieved by implementing broad moving wall policies, accompanied by both forcing suitable open licenses for this content and allocating resources for the platforms, which enables them to preserve it sustainably. A similar approach to enabling the integration of publications from OA journals would also be to enable DML functions for the recent literature, where the share is still relatively small.

In particular, this would address both the problem of possibly limited sustainability of sole OA journals[5] [LMJ] as well as limited machine readability (see, e.g., [KBS]). Hence, while there is obviously a need to expand DML services further, the platforms have not been much in the focus of recent OA initiatives, and the resources made available for them do not seem to quite match these tasks.

On the other hand, the progress of green OA seems almost undamped, and is the single most important driving factor eating into the share of non-OA publications. Since there are still no large indications of saturation, encouraging green OA via feasible platforms still seems to be the most effective measure to achieve broader OA in mathematics (note that this seems to be quite different from many other subjects). Perhaps the only visible tendency in green OA is a recently growing share of OA journal articles available as green OA (and a corresponding smaller share of sole green OA articles). While this is positive in general, since it provides more alternatives in a sustainable way, it may also indicate that the foundation of new OA journals in core mathematics during the last few years has mainly been addressing a community which is already quite OA-minded, hence achieving less with respect to reducing the overall non-OA share.

The conclusion of [T] that APC OA journals are no feasible way to propagate OA in core mathematics has only been reinforced once more by the diagram. Such enterprises have done nothing to significantly enlarge the OA share during the past years, although quite considerable funds have been made available in several countries in the past years by transforming subscription to APC resources. For core mathematics, implementation of policies supporting OA throughout APCs appear to be a misallocation of resources. It remains to be seen whether the implementation of transformative agreements like the "Project Deal" agreements in Germany have a broader impact in the future.

This leaves the question about how to open up the significant share of recent publications which will not be available by green OA in the foreseeable future. The subscribe-to-open model as recently backed by, e.g., the EMS Press https://ems.press/subscribe-to-open, appears to be a new and attractive model to address this issue. It will be interesting to see how its implementation will affect the OA share in the future.

5 This issue has also been discussed in a recent IMU Newsletter, but it should be noted that the two vanished OA journals listed in [LMJ] as mathematics journals did not fulfill zbMATH indexing requirements.

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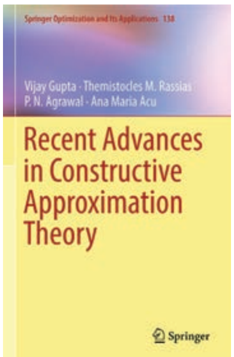
Dariush Ehsani has worked as a mathematician at several universities within the United States and Germany, specializing in the field of Several Complex Variables. In 2020, he joined FIZ Karlsruhe to facilitate the transition of zbMATH to an open access platform, with a special focus on the integration of digital mathematics libraries.

Olaf Teschke studied mathematics at Humboldt University Berlin, and completed his PhD in algebraic geometry there. He moved to FIZ Karlsruhe in 2008, and has been working there since 2009 as the head of the Mathematics Department and Managing Editor of zbMATH (including a short intermediate term as Editor-in-Chief). Since 2017, he has been serving as the Vice Chair of the EMS Committee on Publications and Electronic Dissemination. His main occupation is information infrastructure for mathematics.

Book Reviews



Vijay Gupta, Themistocles M. Rassias, P. N. Agrawal, Ana Maria Acu
Recent Advances in Constructive Approximation Theory
Springer, Cham, 2018
ISBN 978-3-319-92164-8

Reviewer: Gradimir V. Milovanović

This book is primarily designed to cater to the needs of graduate students, engineers and researchers working in the area of mathematical analysis and approximation theory with applications to numerical analysis, physics and the industry. In the past two decades, research on approximation by linear positive operators has caught the attention of many mathematicians across the globe.

Weierstrass (1885) laid the foundation of approximation theory by proving that a continuous real valued function on a closed interval can be approximated uniformly by a sequence of polynomials with real coefficients. Among many proofs of the theorem, the one given by Bernstein (1953) is the most simple and elegant one. He proposed a sequence of polynomials, known as Bernstein polynomials, for this purpose. These operators have been used in many branches of mathematics and computer science. Due to the applicability and importance of these operators, numerous modifications and generalisations have been defined and studied by a plethora of researchers in order to approximate functions in different function spaces.

In order to study the approximation properties of any sequence of positive linear operators, the first and foremost part of the study is the calculation of its raw and central moments. This can be achieved by obtaining the moment generating function of the operators. The first chapter of the book under review deals with this aspect in a very elegant and exhaustive manner. Quantitative estimates and the improvement of the rate of approximation by positive linear operators are also interesting topics treated. The second chapter of this book is concerned with various tools required to carry out such studies systematically. Recently, an extension of q-calculus, namely post-quantum calculus, was used to study various discrete and integral type operators. The third and fourth chapter are devoted to the compilation of the current literature on this topic.

In 1935, Grüss estimated the difference between the integral of a product of two functions and the product of integrals of two functions. Subsequently, Grüss type inequalities were established for various positive linear functionals and operators. An exhaustive account of the research conducted in this area is presented in the univariate case as well as the bivariate case in Chapters 5 and 6 of this book, respectively.

Lupas (1995) proposed the problem of obtaining estimates for the differences of positive linear operators. Gonska et al. (2006) obtained a general result regarding this problem.

Subsequently, several researchers have made important contributions to this topic. A detailed account of the research conducted in this direction is presented in Chapter 7 of this book in a very systematic and organised manner.

Kingsley (1951) initiated the study of approximation of functions of two variables by defining the bivariate Bernstein polynomials.

Pop (2008) obtained the rate of convergence by means of the modulus of continuity and proved the Voronovskaya type asymptotic theorem for these polynomials. Stancu (1963) introduced the bivariate Bernstein polynomials on the triangle. Subsequently, there was increased research interest in this topic and the bivariate generalisations of several sequences of linear positive operators were introduced and studied. The eighth chapter of this book deals with this aspect in great detail.

In 1934, Bögel introduced new concepts of Bögel continuous and Bögel differentiable functions and established some fundamental theorems in analysis. Dobrescu and Metei (1966) proved the convergence of the Boolean sum of the bivariate Bernstein polynomials to the B-continuous functions on a bounded interval. Badea and Cottin (1990) presented Korovkin type theorems for the generalised Boolean sum of linear positive operators. Subsequently, several researchers have focused on this topic. In Chapter 9 there is a detailed study of the contributions to this area of approximation theory.

The book has been written in a reader-friendly and lucid manner and should constitute an ideal textbook for students and researchers wishing to broaden their horizon in mathematical analysis and approximation theory.

Gradimir V. Milovanović [gvm@mi.sanu.ac.rs] is a University Professor in Mathematics and an Academician of the Serbian Academy of Science and Art, Belgrade, Serbia. His research interests are in Approximation Theory, Numerical Analysis and Special Functions (http://www.mi.sanu.ac.rs/~gvm/).

Letter to the Editor

Dear Editor:

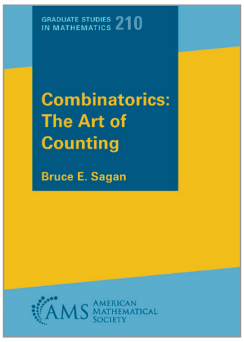
The day before yesterday saw the arrival of yet another EMS Newsletter, which as always was very interesting and read almost immediately in almost entirety, thank you.

My pleasure is, however, overshadowed by an issue which this time feels even more outrageous than usual: the Newsletter is a publication made by men, about men, presumably for a male audience. The only article which has women prominently appear is also the one with majority female authors, about education and childcare, essentially "female" topics. No women appear in the nice article about Alessio Figalli, which should be surprising if it wasn't so common; similarly for the Gotthold Eisenstein article. The Covid-19 article is very nice and careful, one of its few omissions being gender differences in Covid cases. Also the article about the AMU completely omits women; are there no female mathematicians in Armenia?

In summary, this Newsletter, like Newsletters before it, has a serious sexism problem, this time exacerbated by the fact that, oh yes, there is an article about women and written by women, but alas, it's about childcare, oh the irony! You must surely be able to do better; but seeing the gender distribution in the Editorial Team or even in the EMS Executive Committe itself, I'm not sure you are.

Best regards,
Ulrich Fahrenberg, École polytechnique

AMERICAN MATHEMATICAL SOCIETY



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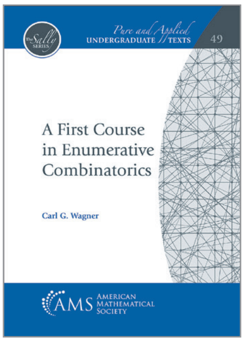

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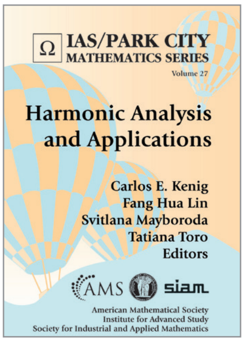

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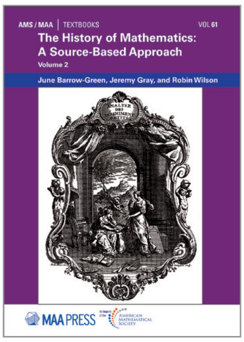

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