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Boekbespreking Development of Mathematics 1950-2000

High level vulgarization of mathematics

Het jaar 2000 staat te boek als het jaar van de wiskunde. Het Clay instituut heeft een lijst opgesteld met belangrijke wiskundige problemen, net als in 1900 door Hilbert is gedaan. Uitgeverijen zoals Springer en Birkhäuser hebben feestelijke gedenkboeken gepubliceerd waarin een panoramisch beeld van de wiskunde wordt gegeven. Vele wiskundigen hebben aan deze gedenkboeken meegewerkt, met als gevolg buitensporig dikke boeken.

Een van die overzichtswerken is 'Development of Mathematics 1950–2000'. De titel maakt nieuwgierig: Wat is er de laatste vijftig jaar gebeurd? Geeft dit boek een goede impressie van de recente ontwikkelingen? T.A. Springer geeft een kritische bespreking.

This book is a sequel to a book with a similar title (*Development of Mathematics 1900-1950*), published in 1994, with the same editor and publisher.

The two books have rather different characters. The first book was an unsystematic outgrowth of a Symposium organized by the Luxembourg Mathematical Society in 1992. The aim of the second book, as I see it, is to present a somewhat systematic review of what happened in mathematics in the second half of the 20th century. A worthy but ambitious undertaking, as the period has seen an exponential growth of mathematical production.

The book is presented as an attempt at high level vulgarization of mathematics. Is it a successful attempt? Before answering that question I have to give some idea of the contents of the book.

Overview

The core of the book consists of 36 reviews, of variable lengths, about half of them in French. Besides, the reader finds lists of the invited one-hour addresses at the International Mathematical Congresses, of the invited addresses of the Bulletin of the American Mathematical Society and of the Russian Mathematical Survey.

At the end of the book there are three interviews of R. Langevin with A. Douady, M. Gromov and F. Hirzebruch. They are quite interesting, reflecting three very different personalities, each with his own views.

Making a technical evaluation of all contributions is an impossible task for one reviewer. However, my mathematical life — such as it is — spanning more or less the period covered in the book, I expected to find familiar material. This is indeed the case, although none of the contributions is on topics with which I am really technically conversant.

Ideally, I think, a contribution should be written by an expert in its area and be largely intelligible for laymen in the area. They should be able to get some feeling for what has been going on and what have been the most striking developments in the last fifty years. With such an ideal contribution in mind let me now turn to some of the material.

Some examples

The book starts off with two contributions by V. Arnold (the order of the contributions is alphabetic), *Dynamical systems* and *Singularity theory*. These contributions seem to approach the ideal pretty closely. Arnold starts off quite simply, but manages to present a great deal of material very clearly, with many illustrative pictures.

Another contribution which impressed me — in an area where I am a stranger — is the one by P.-A. Meyer: *Les processus stochastiques de 1950 à nos jours*. This is a historic survey with hardly any formulas but with very detailed references. My impression is that for stochastic laymen (and maybe also for experts) this is an enlightening report.

Other enlightening contributions are by M. Berger: *Les mathématiques durant le deuxième demi-siècle: géométrie riemannienne*, P. Dolbeault: *Variétés et espaces analytiques complexes*, C.O. Kiselman: *Plurisubharmonic functions and potential theory in several complex variables* and J. Sjöstrand: *Microlocal Analysis* . These are contributions in areas which to me are somewhat far away. A little closer is a — very good — contribution by M. Karoubi: *Rapport sur la K-théorie (1956-1997)*.

But some contributions are far from ideal, for example the one by F.W. Lawvere: *Comments on the development of topos theory*. It is pretty unhelpful to an ignorant reader, as there is no explanation of the basic notion of a topos (which should have been placed right at the beginning, I think).

Technical progress

The contributions of the book demonstrate that mathematical technique made enormous progress in the last fifty years. One usually takes this for granted. But when I tried to look at things from the perspective of the 1950's I realized how impressive the progress really is. Here are some illustrations, coming from my private interests.

In my mathematical youth I was intrigued by analytic number theory (without getting into it seriously). There is a very good contribution on that topic by E. Fouvry: *Cinquante ans de théorie analytique des nombres*. Fouvry describes technical tools which have been developed and discusses a number of concrete successes of analytic number theory, results which in 1950 were completely inaccessible. For example, that the generalized Riemann hypothesis holds for almost all Dirichlet L-functions, and also the description of the distribution of primes which are sum of a square and a fourth power.

Another subject in which I got interested in my youth is algebraic geometry (and in later years I got fairly familiar with some parts of it). In 1954, at the International Congress in Amsterdam, I attended the Symposium on Algebraic Geometry. Looking today at the Proceedings of that Colloquium one realizes what enormous progress has been made. Things which looked very hard or had shaky foundations in 1954 are now common knowledge: rational equivalence, Weil conjectures. Sheaf theory, which had just appeared in 1954, is now a familiar tool.

The book contains an instructive contribution by C. Ciliberto on the geometry of algebraic varieties. The contribution discusses some of the progress of algebraic geometry. But, probably because of length limitations, it is sometimes rather sketchy.

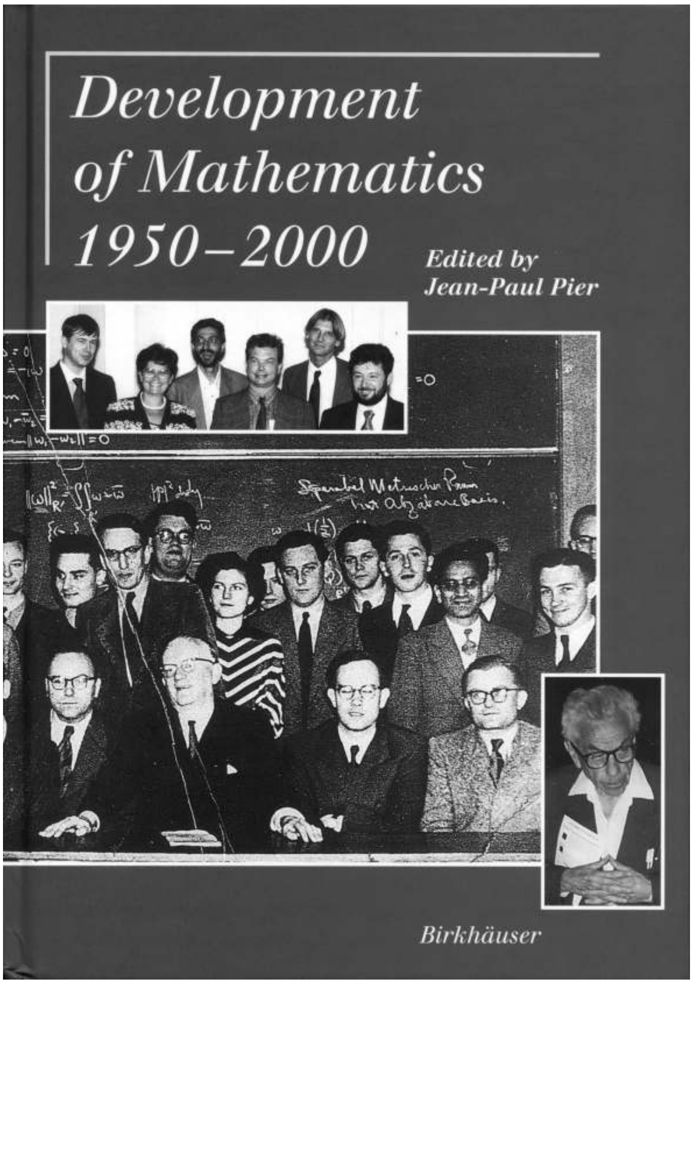
There is one other contribution about algebraic geometry, namely by M.-Fr. Roy: *Géométrie algébrique réelle*. In my opinion other topics from algebraic geometry now hardly touched upon in the book — for example the theory of motives, moduli spaces or arithmetic algebraic geometry — would have equally deserved a contribution.

Crossing borders

My later research interests — in algebra and Lie theory — are not represented in the contributions. But some items rang a bell. One of these brings me to a digression.

There are two contributions on model theory, by B. Poizat: *Autour du théorème de Morley* and by J.-P. Ressayre: *La théorie des modèles, et un petit problème de Hardy*. I am a layman in that area, but over the years I have been struck by the power of model theory, which is sometimes able to establish results which are beyond the capabilities of 'ordinary' mathematicians. I became aware of that power in the sixties, when Ax-Kochen (and Ershov) established the first general results about a conjecture of Artin, using model theory.

Let K be a local field and let f be a homogeneous polynomial in n indeterminates of degree $d > 0$, with coefficients in K. Then the conjecture was: if $n > d^2$ the equation $f = 0$ has a non-trivial solution in K^n. It had been proved by Lang if K has characteristic $\neq 0$ (i.e. if K is a finite extension of a field of formal power series in one variable over a finite field), but it remained open for some time for K of characteristic 0 (i.e. if K is a finite extension of a p-adic field). In the sixties counterexamples appeared (already for $d = 4$). But then model theory



intervened. It enabled one 'to pass from characteristic p to characteristic 0', and led to the following result, for $K = \mathbb{Q}_p$ (the field of p-adic numbers): given p, there is a d_p such that Artin's conjecture holds for $d \geq d_p$. As far as I know, there is no 'arithmetic' proof of such a result, and there do not seem to be good estimates for d_p.

In Poizat's contribution this application is mentioned in passing, as only superficial aspects of model theory are involved. But in my opinion such examples of border crossing between different areas in mathematics are highly interesting and would deserve some attention in the present book. (Poizat's contribution describes other instances, but a bit too brief for a layman. Ressayre's contribution, though, does discuss in detail an application of model theory in mathematics.)

In connection with the phenomenon of 'border crossing' I quote a remark made by Arnold in his contribution on singularity theory (observing that the ADE-classification appears in different areas). He mentions "the rather mysterious unity of all branches of mathematics".

As to passage from characteristic p to characteristic 0, several examples have appeared in the last decades, for example in the theory of perverse sheaves, in representation theory and in the use of Frobenius splittings. It is a phenomenon which deserves some attention. I would have liked to see a contribution discussing methodical phenomena of such a nature.

Bibliographical matters

There is a list of names of about 2500 cited people. Chauvinistically, I observed that a little more than 2% of the names are Dutch. The compiler of the book obviously had trouble with the vagaries of Dutch names, leading to some inconsistencies: De Bruijn is listed twice (under B and under D), Van Dantzig is listed under D, but Van der Waerden under V. Moreover, one also finds garbled Dutch names (Lojienga).

The book contains about 200 pictures of mathematicians. These come from a collection at the Eidgenoössische Technische Hochshule in Zürich. But the book contains no subject index.

Criticism

I have mixed feelings about the book. On the one hand it contains a wealth of interesting material, mostly very well presented. On the other hand — and this is my main criticism — the choice of the topics is rather haphazard. One would expect the editor to have some master plan for the distribution of various areas in the book. What is it? Why eight contributions on probability and statistics and none in topology or in Lie groups, and only one in algebra? Why no separate contribution on numerical analysis? (One does find a few pages on numerical work in the contribution of R. Temam, *Some developments on Navier-Stokes equations in the second half of the 20th century* and some computations in number theory are discussed in the contribution of J.-L. Nicolas, *Arithmétique et cryptographie*.)

To be more specific I mention a few serious omissions. In the book group theory is practically absent. There is one probabilistic contribution involving groups, *Marches aléatoires sur les groupes* by Y. Guivarc'h. But there is nothing about finite groups or Lie groups and nothing about representation theory. This is very strange.

As for finite group theory, there is the classification of finite simple groups, a monumental piece of work (perhaps not yet well understood). This is an achievement in algebra which certainly ought to have a place in a review of mathematical successes in the second half of the 20th century. The more so as partial results, like the solvability of finite groups of odd order, are understandable to any mathematician (although still very difficult to prove). Also, some sporadic groups would merit a description.

The period covered by the book has also witnessed spectacular developments in representation theory. As examples I mention Harish-Chandra's work on the representation theory of real Lie groups and the fact that the theory is involved in the "Langlands program", one of the most significant developments in number theory. But Harish-Chandra's and Langlands's names are mentioned only once in the book, in a review of French mathematics by the late J. Dieudonné. (Also, as far as I can see, Dieudonné's contribution is the only one mentioning modular forms). Moreover, representation theory and Langlands's work provide beautiful examples of 'border crossings' and Arnold's 'unity of all branches of mathematics'.

Conclusion

Finally, I return to the question posed at the beginning, whether this attempt at high level vulgarization of mathematics has been successful. My answer is a qualified 'yes'. Taken as a whole the book, in spite of its flaws, presents a lot of interesting material. But the reader should be aware of its incompleteness.

Development of Mathematics 1950–2000, edited by J.-P. Pier. x + 1372 pages, price €208,51. Birkhäuser Verlag, 2000. ISBN 3-7643-6280-4