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Stable Groups

Bruno Poizat

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Stable Groups

Bruno POIZAT

GROUPES STABLES

Une tentative de conciliation
entre la Géométrie Algébrique
et la Logique Mathématique

نور المنطق والمعرفة

NUR AL-MANTIQ WAL-MA'RIFAH

Mathematical Surveys and Monographs

Volume 87

Stable Groups

Bruno Poizat

American Mathematical Society

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Groupes Stables

Translated from the French by Moses Gabriel Klein

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ABSTRACT. The subject of this book is the study of classical algebraic objects—groups, fields, rings—with the additional conditions of stability of the theory. It turns out that this additional property makes the objects very similar to the corresponding algebraic objects (algebraic groups, finite-dimensional algebras, etc.), establishing a deep connection between logic (model theory) and algebraic geometry. For graduate students and researchers working in logic and in algebra.

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العلم اوله مر مذاقه
لكن آخره احلى
من العسل
السلام

Contents

A foreword to the English edition

This is a book that I like.

The first thing is that it is extremely well written. Even under the disguise of a translation—by a gifted translator, as far I can judge from my weak knowledge of English—you can feel the shivering flesh of the French original.

Scientific French, what a beautiful language! Well, I know that beauty is a cultural trait, and that there is no linguistic basis to qualify a language as more beautiful than others. But can you imagine this book written in another language?

Well intentioned people have told me that it is quite rude to address a person in a language he/she cannot understand. If this were true, the community of mathematicians would rate high in the scale of rudeness, considering the number of times some of its members spoke to me in English. Listen: I have done my best to be understood by my readers, and used for that the only language in which I can offer this best; if you feel that you may be interested in what I say, then you must take a step towards me, and learn something of my idiom. My hope is that the present translation will help you to reach my original words, if you happen to be a speaker of English.

Yes, I believe that the plurality of languages in use for communication in science has a value per se, that some effort should be spent to maintain it and even to develop it, and that the menial inconvenience that it may generate is a small price to pay for the production of well written textbooks. I have no French nationalist feelings, nor a nostalgia for the time when French had a more dominant position than in our present. I am working concretely for the future, and my writing in French is doomed by practical considerations: for instance, I use consciously the fact that there is still little room for French in the domain of international scientific publishing.

Naturally, I would have preferred that this second edition be a plain reproduction of the first, but the editor of the AMS Publications that approached me (Sergei Gelfand) was firmly opposed to this eventuality. He also made clear from the beginning that the beautiful pictures that adorn the original will not be included, justifying his brutal decision by a call to American folklore (copyright problems) according to which anything is fair provided it follows the law of commercial morals (to illustrate the point, Walt Disney produced a film on Notre Dame de Paris without mentioning Victor Hugo as a co-author of the scenario, his works being long ago in the public domain).

The second thing is that it is extremely carefully planned. The chapters follow each other in a natural order, the proofs of the theorems are adequately devised, to the point that, in some cases, they have been extended later to wider contexts by other authors. The excellency of its construction has caused a major problem concerning the present edition; this book is now obsolete in many places, because its subject has undergone a drastic evolution in the meantime (to which the book itself contributed!). On the other hand, it seems difficult to submit it as a revision, because its different pieces are so intimately imbricated that altering one of them would shake the whole building.

Finally, it was decided to reproduce it as it was, with the only addition of this foreword and of a postscript where you find a few comments and references that actualize the text (but do not claim to account for all the present developments of the subject), and correct its ideological orientation; the postscript signals also all the alterations of the original that go beyond the mere correction of a misprint (there is an incredibly small number of mistakes in the text, for a book of this kind). The postscript has its own list of references, given between { }; by contrast, the references of the main text are given between [ ]. I hope that, with the help of this limited critical apparatus, the book will prove itself not only a mere historical document, but also an instrument of work to be efficiently used.

As you see, my book has been affected by the passing of time. This is why I allow myself to express such unconditional praise of my own work, without the slightest feeling of immodesty, because I am no more the man who wrote it during the years 1986–87. When I read it again thirteen years later, while reviewing the translation, it became apparent to me that I have lost the power on the words that were mine then, and that I am no longer able to write a text of this intensity.

This time, I have no picturesque details to tell you, for your enjoyment, concerning its publication: I did not renew the experiences of my first book, and I simply did not approach any professional, and sold the first copies in a meeting in Trento, in July 1987, only three weeks after having completed the typing of the manuscript (the original edition was of 400 copies). I only regret that, in the period of my maximal literary efficiency, I missed the support of an enlightened scientific publisher.

I am certain that this publisher would have spared me a lot of trouble, not only from the burden of the commercial diffusion of the book but would have tamed my instinctive taste for gratuitous provocation. This taste is so naturally rooted in me that even now, being an old man, I still do not understand the negative reaction of people that come unwarned in contact with the book. It contains nothing, after all, that you cannot find in your daily newspaper, or on the posters framed at your favorite bus stop, and would be easily admitted (if not considered as twaddles of the childish variety) for illustration in any work of literary fiction. What is so sacred in mathematics (and I consider my scientific activity as just a part of my normal life), to attach to my book an essence of scandal and submit it to the general reprobation?

Although the progression of my academic career has made me a kind of monument of sufficiency and respectability, it is not without a touch of jubilation that I have joined the restricted club of cursed writers, such as Charles Baudelaire and Oscar Wilde, who have been prosecuted on the basis of immorality (I can even safely assume that I am the author of the only immoral mathematical textbook of all time!). Unfortunately, the punishment of the guilty is no more efficient as it was in their time, and I cannot claim a crown of martyrdom for the cause I was defending.

What I have understood of my sins is that they were, in order of increasing magnitude: (i) that the book was published by myself, and not by a respectable and well-known publisher; (ii) that an inscription in Arabic was reproduced on the cover; (iii) that I have illustrated the book with pictures of nude women; (iv) that it was written in French.

I am ready to offer my apologies to any person that my irresponsible behavior has offended, hoping that I have caused to them no harm more serious than a superficial irritation. I will offer no regrets, and hoping that they will forgive me, considering a last thing, that this book has been extremely useful. Not only because it contains material found nowhere else (e.g. the axioms for ranked universes in the Introduction, that has been reproduced verbatim in Borovik and Nesin's books, the identity of "Borovik groups" and "Groups of finite Morley rank", etc.), but also because it has been used as a textbook by some young men and young women, a couple of them having found their way in the field of research in mathematical logic. I hope that this translation meets the same auspicious fate.

Les Brotteaux, December 2000,
B. P.

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Postscript: Thirteen years later

Introduction

Don Quixote. I thank Angel Loureiro and Enrique Casanovas for providing this quotation of Cervantes in its original spelling.

Cherlin's Conjecture, which was conjectured also by Zil'ber in [**Zil77**], is still pending, but the problem has changed in the meantime. What I call here Zil'ber's Conjecture, which I effectively recorded from Zil'ber during my visit to Kemerovo in 1986, has been disproved by Ehud HRUSHOVSKI in {Hru 92}.

The **Nirvana Principle** (which is more widely known under the technical name of "Zil'ber Trichotomy Conjecture") is definitely false; see {Hru 93}. Nevertheless, it is valid in the frame of "Zariski structures", which is much more restricted than finite Morley rank, as shown in {HZ 93}; this last fact has dramatic consequences for the applications of Model theory to Algebraic geometry (for a painless initiation to Hrushovski's works on the subject, see {Goo 97}).

It is no longer believed that the Cherlin-Zil'ber Conjecture could be derived, if true, out of pure model-theoretic nonsense, as suggested in this introduction. There is presently more hope in what some call Borovik's program for an inductive classification of the simple groups of finite Morley rank based on the shape of their Sylow 2-subgroups: there is a lot of Algebra in it, inspired by the classification of finite simple groups from a revisionist perspective. This approach has recently produced many solid works, and for a start one could read the textbook by Alexandre Vasilievich BOROVIK and Ali Azizog NESIN {BN 94}, where a more limited acception of the phrase "bad field" was introduced, since the too general one proposed in this introduction was rendered obsolete by the failure of the Nirvana principle: a bad field is now a field of finite Morley rank with a proper infinite definable multiplicative subgroup. It is not known for certain that such fields exist, although some good reasons in favour of their existence are given in {Poi xxa}.

Chapter 1

1.1. Fields of Morley rank $\omega^\alpha.n$, for arbitrary α and n, are constructed in {Poi 99}.

Theorem 1.13, step 1. According to a result of Otto Kegel, {Keg 67}, there are no infinite uniformly locally finite simple groups.

1.5. Last paragraph: a stable, nonsuperstable, omega-categorical theory has been constructed by Hrushovski; see Wag 94. But the method has failed to produce exotic groups: all presently known stable omega-categorical groups are abelian by finite.

1.6. This old theorem of Thomas, blended with a new one by Wagner {Wag xx}, has generated a result which conforms to the ideology of the introduction in {Poi xxb}, concerning the definable subgroups of $\mathrm{GL}_n(K)$, for K any field of finite Morley rank.

Chapter 3

3.1. As mentioned above, we are no longer chasing aurochs with stone axes.

Lemma 3.5. This is a very special case of a deep and unexpected theorem of Frank Wagner concerning the fields of finite Morley rank, {Wag xx}.

3.4. It would be more traditional for the definition of the hypocenter and of the hypercenter of an infinite group, to expand the central series transfinitely. In the present case, it is not that important, according to Corollary 3.15.

Corollary 3.24. Did you notice that there is a gratuitous affirmation in the proof? Why G has finite exponent? But do not worry, the gap has been filled in {AFG 91}, and, indeed, there are no infinite groups of finite Morley rank with finitely many conjugacy classes!

Corollary 3.28, Proposition 3.29. In the statement of the two results and the proof of the second, the french edition quotes the exact words of Galois.

Theorem 3.31. A group of this kind has been constructed by Olshanskii and some of his students, but none of them are known to have finite Morley rank.

Chapter 4

4.1. As observed by Simon Thomas, the conjecture after Corollary 4.2, in the case of simple groups, is in fact an equivalent to Cherlin's Conjecture; see {Poi xxb}.

4.5. End of the section: a group of generic exponent 3 has exponent 3; by contrast there exist groups of generic exponent n , but not of exponent n, for every $n \bullet 7$. None of these groups is known to be stable; see {Jab 00}.

Theorem 4.13. In the proof, the definition of V has been lightened somehow.

4.7. End of the section: the two fields conjecture was disproved by {Hru 92}.

Chapter 5

Corollary 5.2. A final sentence has been added to the proof.

Lemma 5.16. Groups satisfying the conclusion of the lemma has been called R-groups by Frank Wagner, who has extended for them many properties of superstable groups; see {Wag 97}.

Chapter 6

6.2. Berline's Conjecture was disproved in {Poi 99}.

Chapter 7

7.3 and the last paragraph of **7.2** have been altered, since while speaking French I failed to see how to extend rigidity to the context.

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Bachmann's book was quoted in a Russian translation!

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