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*Groups of prime power order. Vol. 2*, by Yakov Berkovich and Zvonimir Janko, de Gruyter Expositions in Mathematics, 47, Walter de Gruyter GmbH & Co. KG, Berlin, 2008, xvi+596 pp., ISBN 978-3-11-020419-3

Why study groups of prime power order? What are the interesting problems? What are the interesting results? What are the interesting techniques?

The crudest and most obvious goal is to produce a classification of all groups of order $p^n$, for all primes $p$ and for all $n > 0$; an impossible task. Nonetheless, refinements of this problem are of interest.

So while it is impossible to classify the groups of order $2^n$ for all $n$, it is a triviality (or a problem for number theorists) to classify the groups of order $p$ for all $p$; and indeed the groups of order $p^n$ have been classified for $n \leq 7$; see [22]. However, the difficulty of classifying the groups of order $p^n$ increases dramatically with $n$, so further progress in this style will be strictly limited. Moreover, there remains the question of what is meant by "classification". It seems that there is no clear cut definition of this term, and words like "useful" and "interesting" are likely to be needed.

So what can be said about the number of isomorphism classes of groups of order $p^n$ for fixed $n$? Here we have at least G. Higman's PORC conjecture [13], which asserts that, as a function of $p$, the number of isomorphism classes of groups of order $p^n$ is Polynomial On Residue Classes. That is to say, if the conjecture is true, one can take some integer $k$, depending on $n$, and give $k$ polynomials, indexed by the residues modulo $k$, such that if $p$ is congruent to $i$ modulo $k$, then the number of isomorphism classes of groups of order $p^n$ is $f_i(p)$, where $f_i$ is the polynomial (for this value of $n$) that corresponds to the residue $i$. The conjecture holds for $n \leq 7$, and seems a rather obvious conjecture to make; but the point of the conjecture lies not so much in its face value, but rather in the fact that if it is true (and perhaps if it is false) a proof of the fact would shed a great deal of light on the structure of groups of prime power order. In particular, if the conjecture has a constructive proof, with explicit bounds to the modulus $k$ and to the degrees of the polynomials $f_i$, then counting the isomorphism classes of groups of order $p^n$, for any fixed $n$ and all primes $p$, will have been reduced to a finite calculation. This would be a theoretical result, in that the calculation would in general not be feasible; but the insights on which the proof depended might make it possible to prove that various conjectures concerning $p$-groups (which we take to be finite, and hence to be groups whose order is a power of the prime $p$) have at most finitely many counterexamples of order $p^n$ for fixed $n$. There is some expectation that the conjecture may fail for $n$ approximately 10.

In this context, it should be mentioned that, in [12], the groups of order $2^6$ were classified, up to isomorphism, by M. Hall and J. K. Senior, and that the goal of classifying the groups of order $2^8$ was regarded as a first goal for computational algebra. It is easy to reduce this problem to a mechanical one, but it is not so

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easy to reduce the problem to a practical algorithm or to encode such an algorithm successfully. E. A. O'Brien constructed the groups of order $2^7$ and $2^8$ (see [21]), with B. Eick he constructed the groups of order $2^9$, and with H. Besche and Eick he counted the groups of order $2^{10}$ but did not construct them, there being $49,478,365,422$ such groups; see [2].

For a discussion of the PORC conjecture and the work of Higman, C. C. Sims, M. F. Newman, and C. Seeley on the asymptotic estimate of the number of groups of order $p^n$, see [3].

In a similar vein, a group of order $p^n$ and nilpotency class $c$ is said to have *coclass* $n-c$. The coclass project has got to the point of reducing the classification of 2-groups of fixed coclass $r$ to a finite calculation (see [7]), and it is conjectured that this is also possible for $p$-groups of coclass $r$ for any fixed prime $p$.

Apart from questions of classification up to isomorphism, what are the interesting questions one can ask about $p$-groups?

Perhaps the most famous problems concerning $p$-groups are Burnside questions. Is every $d$-generated group of exponent $p^k$ finite; and if not, is there a bound to the orders of the finite $d$-generated groups of exponent $p^k$? (The exponent of a group is the least common multiple of the orders of its elements.) Of course these questions can also be asked for exponents that are not prime powers. As is now well known, the answer to the first of these questions is, in general "no" (see [1]) and the answer to the second is "yes" (see [31] and [32]). These questions are extremely difficult and cannot be properly covered in general texts on $p$-groups. Finitely generated groups of exponent less than 5, or equal to 6, are known to be finite; but the question of whether every 2-generator group of exponent 5 is finite remains unanswered, despite a very determined and interesting attempt by Sims to prove that it is finite. The largest finite 2-generator group of exponent 5 is now easy to construct by machine, and the largest finite 3-generator group of exponent 5 and the largest finite 2-generator group of exponent 7 have also been constructed. These have orders $5^{34}$; $5^{2282}$ (see [27]); and $7^{20416}$ (see [23]), respectively. Extensive calculations with 2-generator groups of exponent 8 have indicated nothing that is easy to understand beyond the fact that these groups can be very large.

Other important questions concerning $p$-groups are questions of automorphisms. One can ask first about the Sylow $p$-subgroup of the automorphism group of a $p$-group $P$. A natural conjecture is that, with the obvious exceptions of cyclic groups and groups of order $p^2$, the order of the automorphism group of a $p$-group is a multiple of the order of the group. One might hope that analysis of this question would lead to more detailed results and conjectures; but the question remains unanswered. If the conjecture is true, then the outer automorphism group of every $p$-group of order greater than $p$ must contain an element of order $p$; W. Gashutz, in [9], proved this to be the case. (An automorphism of a group $G$ defined by conjugating by an element $g$ of $G$ is said to be *inner*, and the set of all inner automorphisms of $G$ form a normal subgroup $\operatorname{Inn}(G)$ of the automorphism group $\operatorname{Aut}(G)$ of $G$, and the quotient group $\operatorname{Aut}(G)/\operatorname{Inn}(G)$ is the *outer automorphism group* of $G$.) Further, Eick proves in [6] that for any $r > 0$ there are at most finitely many counterexamples to the conjecture amongst the 2-groups of coclass $r$. She does indeed prove more, namely that the 2-adic value of $|\operatorname{Aut}(P)|/|P|$ tends to infinity with the order of $P$, where $P$ is a 2-group of fixed coclass. One may also consider $G/N$, where $G$ is the automorphism group of $P$ and $N$ is the subgroup of $G$ that centralises $P/\Phi(P)$, where $\Phi(P)$ is the Frattini subgroup of $P$; that is to say, the subgroup of $P$ generated by all commutators and $p$th powers. So $P/\Phi(P)$ is an elementary abelian group of order $p^d$ for some $d$, and $G/N$ thus embeds in $\mathrm{GL}(d,p)$. It turns out that every subgroup of $\mathrm{GL}(d,p)$ arises in this way; see [5]. The less obvious question of considering the fixed points of automorphism has proved a far more profound problem; see [18].

A particularly difficult problem arises in the theory of presentations of $p$-groups by generators and relations. If $P/\Phi(P)$ is of order $p^d$, it is easy to see that $P$ can be generated by $d$ elements, and by no fewer. Also, if the $p$-multiplicator $H_2(P,\mathbb{Z}/p\mathbb{Z})$ is of rank $r$, then it is easy to see that $r$ relators will be needed in any $d$-generator presentation of $P$ and that $r$ relators suffice, on any $d$ generators, provided that one works in the category of pro-$p$ groups. But, in the category of discrete groups, it is not known whether $r$ relators always suffice. It is clear that the relators would have to be chosen with care. Given any finite $d$-generator $p$-group, with $p$-multiplicator of rank $r$, one can try to find a $d$-generator $r$-relator presentation, either using hand calculation or by coset enumeration or the Knuth–Bendix procedure, perhaps complemented by the $p$-quotient algorithm when one of the former two algorithms has been used to prove that the group is a $p$-group, but fails to determine its order; and one may succeed. All three algorithms are described in [26]. But if, for some $p$-group, such a presentation does not exist, it is hard to see any way in which one could prove this fact. Indeed, it is generally hard to prove that a given set of $r$ relators that defines the given $p$-group in the category of pro-$p$ groups does not define the group in the category of discrete groups. Sometimes this is possible. For example, if $p$ and $d$ are chosen in such a way that there is an infinite $d$-generator group of exponent $p$, then the largest finite $d$-generator group of exponent $p$ (that is to say, the restricted $d$-generator Burnside group of exponent $p$) cannot be presented by any number of $p$th powers as a discrete group, but it can be so presented as a pro-$p$-group. This is easily proved, but only when given the fact that the restricted Burnside problem has a positive solution, while the unrestricted Burnside problem has a negative solution. Other examples are known: for example, a finite discrete presentation of an infinite group with no finite homomorphic image, such as G. Higman's group $\langle a,b,c,d; b^a = b^2, c^b = c^2, d^c = d^2, a^d = a^2\rangle$, when regarded as a pro-finite presentation, presents the trivial group. For a discussion of this and other examples, see [4].

Another well-known problem, now solved, is the dimension problem. If $P$ is a finite $p$-group, and $g$ is in the $i$th term of the lower central series of $P$, then $g-1$ lies in the $i$th power of the augmentation ideal of $P$. Is the converse true? There are five published proofs of this result known to the reviewer and a beautiful counterexample, due to E. Rips; see [10]. Naturally, the counterexample is correct.

A more important, and still unresolved, problem is the group ring isomorphism problem. If $P$ and $Q$ are $p$-groups, and if the group rings of $P$ and $Q$ over the field of $p$ elements are isomorphic, does it follow that $P$ and $Q$ are isomorphic? This problem has received, and continues to receive, a great deal of attention, but appears to be still open; see, for example, [24].

A most intriguing conjecture, raised in [14], now answered negatively, was the Hughes conjecture. If $G$ is any finite group, let $\mathrm{H}_p(G)$ be the subgroup of $G$ generated by the elements of order not dividing $p$. So clearly, $\mathrm{H}_p(G)$ is trivial if and only if $G$ is of exponent dividing $p$, and it is easy to write down examples when $\mathrm{H}_p(G)$ is either $G$ or a subgroup of $G$ of index $p$. The question is, are these the only three possibilities? The conjecture was answered positively in [15] for groups that are not $p$-groups, and negatively for 5-groups in [29]. But this is just the beginning of the story; we shall return to this theme later.

The general theory of $p$-groups is concerned with the interaction between the commutator and power structure of the group. Thus P. Hall, in [11], introduced the concept of a *regular* $p$-group, this being a $p$-group in which the equation $(ab)^p = a^p b^p c$ can be solved for $c$, given any $a$ and $b$, where $c$ is required to be a product of $p$th powers of elements of the derived subgroup of $\langle a, b\rangle$. If $n \leq p$, then all groups of order $p^n$ are regular; and one sees, in an attempt to classify the groups of order $p^n$ for some small $n$ and all $p$, that the cases $p > n$ are easier in that the groups are then all regular. However, inspired by work of Lazard [19], a more important condition, that of a powerful $p$-group, was introduced and developed by A. Lubotzky and A. Mann [20]. A $p$-group is said to be *powerful* if the subgroup of $P$ generated by the $p$th powers of elements of $P$, if $p$ is odd, or the fourth powers if $p = 2$, contains the derived subgroup of $P$. Note that this is an interesting property for all primes, including the case $p = 2$, the definition having been adapted in this case to avoid triviality (otherwise all 2-groups would be powerful); but a 2-group is regular if and only if it is abelian. The introduction of the concept of powerful $p$-groups has done more than anything else to make the beautiful and influential account of the theory of $p$-groups in [16] out of date.

At this point one has to observe that the study of $p$-groups has been to some extent overshadowed by the theory of pro-$p$-groups. Now a pro-$p$-group, being an inverse limit of $p$-groups, is much the same as an indexed family of $p$-groups, supplied with suitable homomorphisms; so one might equally well say that the most popular way of studying $p$-groups is by studying pro-$p$-groups. Thus the proof of the coclass conjectures, of which the strongest concerns $p$-groups, depends heavily on theorems concerning pro-$p$-groups, and the theory of powerful pro-$p$-groups is closely related to the fundamental concept of $p$-adic analytic groups. So one studies $p$-adic analytic pro-$p$-groups, $F_p[[t]]$-analytic pro-$p$-groups, branch groups, groups defined by automata, and amazing groups such as the Nottingham group, which appears to belong to a collection of sporadic pro-$p$-groups. This is a very vibrant area of research. In particular, we should like to understand the hereditarily just-infinite pro-$p$-groups, which play a role loosely equivalent to the role played by simple groups in finite group theory. (A *hereditarily just-infinite* pro-finite group is one with no nontrivial closed subnormal subgroup of infinite index.) But we have no firm handle on these groups, and can do little more than construct beautiful examples.

So much for theorems and problems. What techniques are used? It might be said that, in the early days, $p$-group theory was something of a cottage industry, the basic requirement being an ability to count. For example, one proves that a nontrivial $p$-group has a nontrivial centre, and hence is nilpotent, by counting. As the subject has spread its wings, a greater and greater collection of serious techniques has been used. The most commonly used is the theory of Lie algebras. Lie algebras can be associated with $p$-groups or with pro-$p$-groups in many ways; for example, Aner Shalev's brilliant proof [25] of the coclass conjectures makes strong use of Lie algebras, in combination with refinements of the theory of powerful $p$-groups. Far more surprisingly, du Sautoy in [8] introduced zeta functions to prove an important theorem in the theory of $p$-groups of finite coclass, thus finally demonstrating that the subject has come of age. Everything gets involved: number theory, homological algebra, representation theory, Brauer groups, computers.

Given this rich, beautiful, and rapidly expanding scene, how is one to write a book on the subject? In fact we are here reviewing two books out of a promised three, according to the publisher's blurb on the back covers. In an attempt to make the books self contained, the authors have moved back towards the cottage industry style of $p$-group theory, without quite getting there, as they assume a knowledge of representation theory. However, they turn their backs resolutely on Lie algebras, and all other serious tools. This principle is taken to such an extreme that in Section 21 it is not proved that the Schur multiplier of a group is well defined. Presumably this is because the proof requires the consideration of free groups; so the reader is referred to the literature, it being considered more important to keep the exposition elementary than to keep the promise, at the end of the foreword, that the books are self contained.

One might, as a result, expect the books to be short of ideas, but nothing could be further from the truth. The authors suggest no fewer than 1400 research problems. In the view of the reviewer, this is rather too many. If a problem is published, people will expect a solution to the problem to be published as well. If all these problems are solved in the next few years, where will the solutions be published? Do the authors intend to read them all? Some of the problems are rather vague. Take at random Problem 1200: Study the $p$-groups whose cyclic subgroups are characteristic in their centralisers. There is no objection to asking a rather imprecise question ("Study…"), except that it could rise to a number of papers, but there is an objection to studying some oddly defined class of groups without knowing why. If the authors have some insight that studying this class of groups will bring new and significant insights into group theory, then well and good; but if not, then not.

It is customary, when writing a review of a book, to mention and perhaps to comment on its contents. In this case, it is not easy to carry out this task in an instructive way. These books together contain 118 sections, of which 26 are relegated to appendices, and together contain over 1000 pages. The effort required to read them is mitigated by the fact that Sections 35 and 81 and 91 are identical, apart from some trivial differences of a typographical nature. Against this, the book contains errors that make the reader's task harder. It is in the nature of a work of this kind that it should have some of the properties of an encyclopaedia, and, as one does not read an encyclopaedia systematically from Aaron's beard to Zygote, so one should be able to pick out a section from these books and understand the section without reading all the previous sections. So let us take Section 79 "Nonmodular quaternion-free 2-groups", and see what we can learn. A $p$-group $P$ has been defined as being modular if $HK = KH$ for any two subgroups $H$ and $K$ of $P$. It seems then reasonable to first read Section 73 "Classification of modular $p$-groups". Here we learn that a $p$-group $P$ is modular if and only if $P$ is an extension of an abelian normal subgroup by a cyclic group in a particularly restricted way, or $P$ is the direct product of a quaternion group of order 8 and a (possibly trivial) elementary abelian 2-group. So the restriction to nonmodular 2-groups in Section 79 is a minor detail, the modular case having been dealt with. A group is said to be quaternion free if it has no section isomorphic to the quaternion group of order 8. A reason for choosing Section 79 is that we are told, in the first paragraph of the section, that "The original proof of the corresponding classification theorem, given in [Wil2], depends on the structure theory of powerful 2-groups. In addition, in the proof of Lemmas 10 and 13 in [Wil2] there are some gaps. Our new proof of the classification theorem is completely elementary and does not involve powerful $p$-groups. Nevertheless, the proof is very involved and reaches probably a deepest result ever proved in the finite 2-group theory." In fact the reference should be to [Wil1] in their bibliography, and is [30] here.

In view of the depth of the proof, it seems reasonable to start by understanding the statement of the classification theorem in question. It turns out that this class of groups splits into three subclasses. The first two consist of some simply defined groups that are abelian-by-cyclic, and the third consists of groups that are certain types of extension of an abelian group by a group isomorphic to $M_{2^{k+1}}$ for some $k \geq 3$. It remains to decide what is meant by $M_{p^m}$, and as this is not defined in the section, we turn to the index, usefully reproduced in both volumes, where we learn that "$M_{p^m}$ is a non-abelian $p$-group containing exactly $p$ cyclic subgroups of index $p$." Rather than searching further for the precise definition, let us guess (correctly, it transpires) that $M_{2^m}$ is the split extension $C_{2^{m-1}} : C_2$ where the 2-cycle acts on the $2^{m-1}$ cycle by raising any element to the power $1 + 2^{m-2}$. Groups in the above three subclasses are here called Wilkens groups, and these are seen to be nonmodular and quaternion free. Then a minimal counterexample will have all its subgroups quaternion free, and hence they will be Wilkens groups or modular. Turning to the details, we are soon referred back to the previous section, on "Minimal nonmodular $p$-groups". Here we read, in the third sentence, "Hence there is $N \lhd G$ such that $G/N \cong D_8$-free but each proper subgroup of $G$ is $D_8$-free." This has to be read as "Hence there is $N \lhd G$ such that $G/N \cong D_8$; but each proper subgroup of $G$ is $D_8$-free." Before long we are referred back to Section 36, where Theorem 36.1 leads the way. This theorem is a simple result about metacyclic groups. After acknowledging that the result is essentially due to Blackburn (but no reference is given) we read, "Here we prove this result in slightly another form (our proof is shorter)." The theorem is then stated, followed by a remark, corollaries, but no obvious sign of a proof. It appears that the proof is the empty set. Looking for the proof, we observe that the section consists of two parts, numbered oddly (pun intended) as 1° and 3°. So the proof must be in the first part, and is eventually tracked down. It is 11 lines long and has a footnote that tells us, "In this proof we use only Lemma 36.5, which is independent of all previously proved results." So now the length of the proof is 15 lines, or 18 lines if we include the statement of Lemma 36.5.

And so we continue, working our way towards our goal. It is indeed a long road, and at the end we understand that, with some simple and well-described exceptions, all finite 2-groups have the quaternion group of order 8 as a section.

I should like now to compare these volumes with E. Khukhro's book [18], and to refer occasionally to B. Huppert's book [16]. I take [18] in part because it is a modern book, in the same series as the books under review, and for other reasons that will become clear; and I refer to [16] because, though now rather old, it is a book that all group theorists have read.

First there is the matter of the quality of the presentation. Khukhro's book [18] is beautifully written, with one typographical error (p. 221, line 9); thanks are expressed to J. C. Lennox for "language polishing". On the other hand the books under review, as in the above quotations, are full of linguistic errors and misprints. But it must be said that the mathematical obscurities found by the reviewer have been relatively harmless, in that the sense was either clear or could be recovered in well under an hour. The linguistic issue is another matter. It seems unreasonable to lay the very heavy burden of learning to write English well on the shoulders of mathematicians who do not have English as their native language. It is, in the reviewer's opinion, unfortunate that almost all mathematics is now published in English, but the trend cannot be reversed. So what is to be done for mathematicians who cannot write English well? Either we accept poor English, or someone needs help. One might expect, with a book, and perhaps with a journal article, that a copyeditor would help. The poor English in the quoted extracts would be very poor for a native English speaker, but might be regarded as quite competent when written by someone who is not. That being said, the authors' command of English is not that bad. They have realised, for example, unlike many who should know better, that the verb "to have" is always transitive, and hence to write "we have that" is always wrong.

There are presentational problems with these books that are harder to forgive. More than half of the second volume is copied from articles the authors have published elsewhere. Articles have been republished in books before. Newton, in Book 3 of *The Principia*, reproduces verbatim a paper by J. Machin. But one should ask oneself how surprised one would be to find in [16] that a section had been copied, or translated literally, from a published paper. One also has to express some surprise at three complete sections of these books being essentially identical. Another issue is the use of references. Taking p. 222 of Volume 1, we find a lemma ascribed to "Wiegold, Berkovich". The Wiegold paper is presumably [Wie2] in their bibliography, but which, if any, of the 33 papers by Berkovich in the bibliography is intended is hard to guess. Also on the same page we have references to the Schur-Zassenhaus theorem, Taussky's theorem, and Grün's lemma. Of these, the first is not covered in these volumes, the second is traced to Proposition 1.6 with the help of the index, and the third is a mystery. The index tells us to look in Section 1, where the reviewer could find no reference to Grün. Moreover, the result whose proof here relies on Grün's lemma is completely trivial, and not related to what is generally known as Grün's lemma. Again, the Hughes subgroup $\mathrm{H}_p(G)$ is defined on p. 219 of Volume 1 but is not named, and there is no relevant reference, although [15] appears in their bibliography. Such issues with references recur throughout these volumes. On a final point of style, the authors are constantly congratulating themselves on having produced shorter, more elementary, and generally better proofs than were given in the original articles (generally written by others). It is of little value to the reader to be told that the original proofs were longer, more obscure, and worse.

Turning from the packaging to the content, one might contrast the section of the second volume under review on quaternion-free groups with the discussion of the Hughes conjecture in [18]. The former consist of some 20 pages of detailed but elementary argument in the section in question, and it relies on a good deal of earlier material, as compared to the 15 pages of Wilkens' original paper. However, the newer proof is advertised, as we have seen, as being more elementary, in that it does not require the (surely elementary) theory of powerful groups. By way of contrast in [18], taking the whole of Chapter 7, we have 27 pages of mathematics in which we consider the resticted Burnside problem, multilinear identities in Lie algebras, and other major tools, that can in no way be regarded as elementary. And curiously enough, the latter is easier to read than the former.

So we have two concepts of what a book should be. On the one hand, we have a book (or rather two volumes), parts of which appear to have been thrown together with little thought or simply recycled from old papers; parts of which are riddled with misprints; filled with unconsidered problems (you cannot consider 1400 problems and survive); containing a rambling bibliography which is in no way comprehensive, but which includes many papers that are (I believe) not referred to in the text, and some that are not referred to when they perhaps should have been. The books are, on occasion, self-indulgent, and by refusing to use any serious machinery, they paint a view of the nature of the subject that most of the serious practitioners would reject. On the other hand in [18] we have a book that is beautifully written, contains no self congratulation, and makes one proud and glad to be a mathematician. One might also contrast the problems in the volumes under review with the famous Kourovka note book.

And yet, there is perhaps a place in the world for these volumes. A beginning graduate student could do worse than work through the section discussed above on quaternion-free groups, and look for improvements. There is no sharp divide in group theory between the elementary and the nonelementary aspects of the subject, and one has to learn to construct elementary arguments. These elementary arguments are not trivial arguments, and they contain simple ideas that can be learnt and used. It may also be that some of the results in the book may be useful, even if they are not obviously interesting. No one rushed naked through the streets of Syracuse because they had analysed the 2-groups that do not have the quaternion group of order 8 as a section, but it may be useful to know the result. On the other hand, the beginning graduate student, on reading this material in Khukhro's book, may be inspired but also perhaps a little intimidated. And while the bibliography of the volumes reviewed has not been assembled according to any particularly obvious principles, and articles by the same author appear to be ordered at random, to have 24 pages of references is of use.

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