ROCKY MOUNTAIN
JOURNAL OF MATHEMATICS
Volume 32, Number 4, Winter 2002

ABELIAN GROUPS IN HUNGARY

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Time has not yet come to assess the contributions to the development of abelian group theory by individuals or by groups of researchers in the last half of the century, but I think I can claim with confidence that there was a substantial influence by Hungarian mathematicians. It started in the late 1940's, when a significant development of the theory of abelian groups took place in Hungary.

It can be traced to a famous conjecture by H. Minkowski (1896) on filling the n-dimensional Euclidean space by n-dimensional cubes. If we fill the space such that every point is covered by a cube and no two cubes have interior points in common, then – the conjecture said – there are cubes sharing $n-1$-dimensional faces. (There is an equivalent formulation in terms of inequalities for linear forms.)

For about half of a century, no real progress was made toward the solution of the problem. In 1942, the problem was reformulated by G. Hajós as a factorization problem on finite abelian groups and then solved in this form. His result asserts that if a finite abelian group A (written multiplicatively) is a direct product of cyclic subsets (i.e., subsets of the form $\{1, a, a^2, \dots, a^{k-1}\}$ for some $a \in A$ and integer $k \geq 2$), then one of the subsets ought to be a subgroup [**14**].

The proof was based on rather complicated arguments on the integral group ring of the group A. The prominent algebraic number theorist, László Rédei got interested in the problem and tried to simplify the proof [**20**]. To a certain extent, he succeeded in his effort. His student, Tibor Szele, obtained a more relevant shortcut in the argument [**24**]. They both were puzzled by the fact that the problem had apparently no close link to the fundamental theorem on finite abelian groups, and they were diligently searching for such a connection – in vain. Even today there is no hard evidence of the existence of a link of any kind between the fundamental theorem and Hajós' result.

Received by the editors on July 26, 2001, and in revised form on October 17, 2001.

This was not only the beginning of abelian group theory but also the starting point of abstract algebra in Hungary. The country had a strong tradition in many branches of analysis, thanks to our pioneers: Leopold Fejér, Frederic Riesz and Alfred Haar, as well as to their students, many of them leading mathematicians at universities all over the world, John von Neumann, George Pólya, Gabor Szegö, Marcel Riesz, Paul Turán, Béla Sz. Nagy – to name a few. Extensive research was done in topology, graph theory, mathematical logic, etc., but hardly anything in algebra. Much earlier, Julius König and Michael Bauer had a number of papers in classical algebra (the former even had a frequently quoted book), and Joseph Kürschák was the founder of classical valuation theory, but they died before World War II and did not have any followers. Abstract algebra was nonexistent in Hungary. As a result, no guidance was available for students interested in abstract algebra; we had to learn it from books and articles. Undoubtedly, we benefited tremendously from the congenial, research-oriented atmosphere and the support of many established mathematicians, though the "abstract nonsense" was frowned upon by several application-oriented colleagues.

Both Rédei and Szele were captivated by the Hajós problem and very soon – being fascinated by the beauty of the subject – they started to learn about abelian groups in general and to investigate special types. At the University of Szeged, several other algebraists, including J. Szendrei, I. Szélpál joined in. Soon the focus was shifted to the investigation of the influence of the ring structure on the underlying additive group. Szele's remarkable talent began to unfold, and his interest soon developed into a lifelong commitment to the theory of abelian groups. When he was appointed to the algebra chair at the university in Debrecen, he intensified his research. The big boost came when he discovered a paper by L.Ya. Kulikov on abelian groups of arbitrary cardinality, published in Russian in 1945. He immediately arranged its translation into Hungarian, and the handful of typewritten copies which circulated among interested algebraists became a main source of ideas for research in abelian group theory. Kulikov's paper was an important milestone in the theory, shifting the focus to uncountable groups. A major topic was Kulikov's theory on direct sums of cyclic groups of arbitrary cardinality. It was Szele who immediately recognized the overwhelming importance of basic subgroups which was used by Kulikov primarily for describing torsion-complete p-groups, and along with his students he started an in-depth study of the properties of basic subgroups.

Szele was filled with ideas which he shared with everybody who was willing to listen. His enthusiasm attracted several young mathematicians to the subject. His students, A. Kertész, L. Kovács, S. Gacsályi, J. Erdös, M. Erdélyi and Z. Papp, were busy with working on various problems raised by him. The results spread over several branches of the theory.

As a professor, Szele had to come often from Debrecen to Budapest to attend meetings at the Department of Education or at the Academy. But his afternoons were always devoted to a more enjoyable activity: discussing mathematics. He used to come to my parents' home where we moved to a remote room, closed the door behind us and spent endless hours discussing our own and our students' works in progress, trying to prove theorems on the spot, calling each other's attention to interesting results we read in recent publications, swapping information on new developments, and above all exchanging ideas on various subjects: groups, rings, lattices, etc. The only person who dared to enter our work sanctuary was my mother who supplied us with strong espresso coffee (necessary for any kind of mathematical activity in Hungary) and refreshments. Szele left only when he had to rush to catch the night train back to Debrecen.

At that time, my research interests were the ideal theory of commutative rings and the theory of partially ordered groups. But I was not immune to Szele's persuasive enthusiasm, and soon I found myself in the small, but very active, circle of abelian group theorists. My interest in the subject was reinforced during discussions with another very good friend of mine, András Kertész, a student of Szele, with whom I also had often marathon afternoon sessions. In addition, as the technical editor of the new journal Acta Mathematica Hungarica, I had the privilege of having immediate access to papers on abelian groups submitted for publication which I studied with increasing interest.

Abelian groups were not strangers to me: I had encountered them earlier in the form of ordered groups. They were, of course, all torsion-free. The first problem I attacked in abelian group theory originated from Szele: to extend Kulikov's criterion (or its Kertész version [**16**]) on direct sums of cyclic p-groups to the mixed case [**4**]. With him and his student A. Kertész we had several joint projects; most of them reached the publication stage. The majority of the problems were suggested by Szele, he always had a large supply of unanswered questions. In the problems we decided to work on jointly, he usually made initial progress toward their solutions; then we worked together when he visited me and later by correspondence, and at the end I had to give the final touch by pushing the results as far as possible. I cannot tell how grateful I am to my friend Szele for introducing me to this wonderful subject, and for his leadership in our cooperation.

To get the right picture, I have to point out that we had to rely on each other to a great extent. At that time we algebraists in Hungary were quite isolated. The chance to travel abroad was practically nil: the few possibilities to visit countries in the eastern block in the exchange programs between the academies were – in practice – almost exclusively open only to the members of the Academy. (My first foreign trip was in 1954 to East Germany after Heinrich Grell, of Humboldt University in Berlin, known for his work in ideal theory and a student of Emmy Noether, expressed his interest in meeting me.) Our contact with the Western world was virtually nonexistent. Reinhold Baer arranged a visit for Szele, but he was denied an exit visa. Amazingly, we had no contact with the Russian abelian group theorists, either. Our foreign relations were limited to Poland and Czechoslovakia – which turned out to be very important in view of later developments. Of course, correspondence by mail was always available; we took advantage of it to some extent, but one has to realize that for us beginners it was not easy to approach leading experts in the capitalist world who were not known personally to us, especially when such a correspondence was not favored by the authorities.

The lack of personal contact with leading researchers explains why we failed to focus more closely on the most intriguing problems of the subject. In retrospect, I feel that some problems we dealt with were not as relevant as we thought at that time they were (I mean problems like describing groups all of whose pure subgroups are summands or all of whose subgroups are endomorphic images). On the other hand, I have no doubt that they were instrumental in developing new machinery and in gaining a better understanding of the group structures. Some of the early publications look today very easy or unexciting – thanks to the very development they helped to unfold.

In spite of the unhealthy isolation and various restrictions, substantial progress was made by the small, devoted Hungarian group. To wit, I will mention several results which are fundamental for the theory of abelian groups.

I start with the new area inaugurated by Szele: the additive groups of rings. He called attention to the importance of the role played by the underlying additive structure [**25**]. His pioneering papers were published in 1949–50. In this direction the most frequently quoted result is a generalization of Hopkins' well-known theorem (Fuchs-Szele [**11**]): A right (or left) artinian ring is noetherian if and only if it does not contain a subgroup isomorphic to $\mathbf{Z}(p^\infty)$ for any prime p. Equivalently, if its annihilator ideal is finite. We succeeded in getting a complete survey of the additive structures of artinian rings. Subsequently, my student, Ferenc Szász [**23**] added a final touch by showing that every right artinian ring is the ring-direct sum of two right artinian rings: one has torsion additive group, while the other has a torsion-free additive group; and the latter must have a right identity.

There were several papers on the additive structures written by Hungarian mathematicians. In this connection let me mention my paper [**6**] on the additive structure of rings, in which *inter alia* it is shown that the basic subgroup completely determines the multiplication in any ring on a p-group. This is one of my theorems Kurosh liked the most.

To complete my report on additive groups, let me abandon the chronological order and jump ahead to mention a result from 1962–63 which is perhaps the best evidence of the impact the additive group can have on the ring structure. I refer to my paper with I. Halperin [**10**]; we joined forces to show that every (von Neumann) regular ring can be embedded as an ideal in a regular ring with identity. I emphasize that we had to rely heavily on properties of the additive group in order to construct a universal regular ring, over which every regular ring is a unital algebra.

In 1950, Szele developed a theory of abelian groups that ran parallel to E. Steinitz' theory of fields [**26**]. This is actually an approach to the study of injective envelopes of abelian groups: *essential* extensions of abelian groups correspond to *algebraic* extensions of fields. Subsequently, Kertész initiated a study of systems of linear equations over injective groups which he extended later to modules over semisimple rings [**17**].

Szele also published several papers on the basic subgroup of p-groups which he considered as one of the most important concepts in the theory. In this direction, his major contribution is the theorem stating that a basic subgroup of a p-group is always an endomorphic image [**27**]. Both Kovács and Papp published interesting papers on basic subgroups [**18**], [**19**].

We studied carefully the fascinating Prüfer-Ulm-Zippin theory of countable p-groups. Szele and I could not stop praising it, and we discussed it at numerous occasions. Regretfully, we failed to find any reasonable extension of Ulm's theorem to a larger class of p-groups. But at least I was successful in generalizing Zippin's theorem on the existence of p-groups with prescribed Ulm sequence to arbitrary cardinality [**5**], unaware that L.Ya. Kulikov was working on the same problem (this was stated as an open problem by Kurosh in his group theory book). Kulikov and I published essentially the same result, using slightly different conditions on the Ulm sequences. My paper was published a few months later. Kulikov included also mixed modules over the localization of the integers at a prime p, my presentation on p-groups was much shorter.

Since Reinhold Baer established the existence of indecomposable torsion-free groups up to the continuum (1937), there was no progress in the construction of indecomposable groups of larger cardinalities. Bognár [**1**], a topologist, gave a simple construction for finite rank indecomposable groups; his method, based on rigid systems, is the best I can think of. Both Szele and I were puzzled by the continuum as an upper bound, but were unable to surpass it nor to prove that torsion-free groups beyond the continuum are decomposable. After a while, Szele conjectured that the latter alternative was true, but he could not substantiate it. In 1957, suddenly three papers appeared almost simultaneously, establishing the existence of indecomposable groups of cardinality $2^{2^{\aleph_0}}$. The construction by Sąsiada [**22**] was sketchy; it was based on the then unpublished theory of slender groups, while the constructions by Hulanicki [**15**] and myself [**7**] were based on the combination of the ideas of de Groot [**13**] and Bognár [**1**]. (My paper published later in 1959 claiming the existence of arbitrarily large indecomposable groups was incorrect.)

Unfortunately, Szele did not see the refutation of his conjecture on large indecomposable groups; in 1955 after a short, but severe, illness he passed away at the age of 37. It was a great human tragedy, an immense loss for the theory of abelian groups. I had the sad task of preparing his unfinished manuscripts for publication. He carried a wealth of ideas into his grave. His distant plan was to write a book on abelian groups which would go far beyond the material covered by Kaplansky's little red book.

I have to return to an earlier topic and to comment on our contact with foreign algebraists involved in our subject. The contact with Czech algebraists (especially with the young V. Dlab) was not as strong and fruitful as our interaction with several Polish mathematicians, above all with J. Łoś, A. Hulanicki, S. Balcerzyk and E. Sąsiada. We had a chance to meet them more frequently (Łoś, as a member of the Polish Academy, visited Hungary several times), and as a result, there was a continuous flow of ideas back and forth between us – this is manifest in several publications as well. Just one illustration: in 1955, Łoś and I were attending the meeting of the Czechoslovakian Mathematical Society in Prague. One morning we decided to skip the talks and to discuss mathematics while exploring the city. *Inter alia*, I described to him the results in the latest manuscript by Gacsályi [**12**] characterizing pure subgroups and direct summands via systems of equations. He immediately realized that this has far-reaching consequences on compact abelian groups: in this very moment, the seed of pure-injectivity was planted in his head. A more extensive cooperation with Łoś took place when he explained to me his "schlanke Gruppen," and we worked out the details of the theory of slender groups during my visit to Toruń in 1958.

My first chance to attend an international group theory meeting came about in 1956 when I was invited to a meeting in Oberwolfach. Due to administrative difficulties (both exit and entry visas), I arrived late, only on the last day of the conference, missing a large portion of the talks. My talk was the last one; it was on the additive groups of rings. I was thrilled and excited to meet many giants of the theory of groups: Reinhold Baer, Bernhard and Hanna Neumann, Helmuth Wielandt, Kurt Hirsch, Friedrich Levi and many young group theorists, including Bertram Huppert. I stayed two extra days to be able to spend more time with Baer, the Neumanns and the others. While hiking in the Schwarzwald forest and collecting mushrooms, we had ample time to talk about mathematics as well. Both the Baers and the Neumanns were extremely nice to me; I had a most enjoyable time in Oberwolfach. I also had a long walk in the forest with Friedrich Levi, the nestor of abelian group theorists. Besides mathematics, he also told me about his escape from Germany to India. I learned about the new trends in group theory and returned to Hungary with new ideas. At the same time, I realized how limited our scope was in our isolation.

I have benefited from this trip tremendously, mainly by establishing contact with the Neumanns and with Reinhold Baer. The correspondence with them became more frequent with time. Both Bernhard and Hanna visited us in Budapest. Baer subsequently invited me to almost all the meetings and workshops he organized in the Research Institute. A few years later, in March 1961, the first meeting on abelian groups took place in Oberwolfach. Friedrich Levi and I were the organizers, while Reinhold Baer, Jean Dieudonné, Wolfgang Krull, Tony Corner, Bernard Charles, Horst Leptin were among the participants. I was pleased that both Dieudonné and Krull considered abelian group theory important enough to come to the meeting.

As I mentioned earlier, we did not have any contact with Russian abelian group theorists. This is hard to explain, since several Hungarian algebraists studied in the Soviet Union, even with Alexander Kurosh, but nobody in group theory. Szele corresponded with Kurosh; he even arranged a Hungarian translation of the first edition of Kurosh's book on group theory – it was the only book on groups that had an extensive chapter on abelian groups. But it was only in the late 50's when I could meet Kurosh: first when I visited Moscow in 1957 and shortly afterwards, when he visited Hungary. Kurosh was full of energy, very enthusiastic and talkative, an extremely impressive mathematician. Then I understood why he could develop a large algebra school in Moscow, and why he had so many prominent students.

He was my main contact during my visit to the Soviet Union in the exchange program between the two academies. His office was on the 13th floor of the huge building of the university on the top of the hill, overlooking the city. Our meetings were most interesting and stimulating. He was extremely knowledgeable and well-informed, and it was a pleasure to discuss with him any topic. We had lots in common, and in a matter of days, we developed a warm friendship. One day, he opened one of his notebooks in which he recorded several results from the papers he read, along with his comments. He opened the pages where some of my papers were featured, quoted results which he especially liked, mentioned others, and then he added his comments – this was quite a treat for me. I was also fortunate to meet a number of Soviet algebraists, including A.I. Mal'cev, L.A. Skornyakov, I.R. Shafarevich, B.N. Delone and several group theorists: O.N. Golovin, L. Kaloujnine, E.S. Lyapin, A.P. Mishina. Needless to say, I was most eager to meet the leading Russian abelian group theorist. Through my interpreter I requested that the Soviet Academy arrange a meeting with L.Ya. Kulikov. Next day, when she informed me that I could not meet him because he was sick, I immediately told her that I was ready to visit him in the hospital (provided that such a visit was medically feasible). Before she could open her mouth, Kurosh jumped in by saying "Kulikov ist krank" and added a gesture cutting further discussion of the subject. I had the pleasure to meet Kulikov only a few years later, in 1963, at the conference in Tihany, Hungary.

I would like to say a few words about the background of my old book, *Abelian Groups*, that was published in Hungary at the end of 1958 [**8**]. I shared Szele's view that a more detailed book was needed on abelian groups which would go far beyond Kaplansky's beautiful, but incomplete exposition, and much beyond the material in Kurosh's group theory book. Szele's project was doomed when he passed away. At that time Hungary was one of the handful of leading research centers in abelian groups, and had all the manpower to produce a more substantial book in the subject. I was not yet prepared to write such a text, but I ruled out the idea of preparing a volume with someone else (Szele would have been a perfect partner, but none of the prospective coauthors had a style compatible with mine). During the years of intensive research on abelian groups, I developed several new proofs and found a number of previously unnoticed links between different results. These were insufficient for papers, but I thought it would be nice if they could be made available to students interested in the subject. When in the summer of 1957 I got rid of heavy administrative duties, I thought it was time to think seriously of a book project. After checking with the Hungarian Academy, I learnt that they were willing to publish such a volume and, consequently, I decided to start working on it, at least to find out how I liked it. I enjoyed the challenge and decided to continue.

My goal was "to give a fairly complete and detailed account of the present status of the theory with special emphasis on results concerning structure problems." In 1958, such a program was still feasible in one volume. I tried to develop a sound foundation and to include almost everything which I thought at that time to be of importance for the subject. Some of the results could be included only in the exercises, but I tried to avoid relegating proofs to the exercises. Not counting minor revisions, I completed the manuscript in less than a year, though I was confronted with several serious problems, mostly in connection with the organization of the material. Each theorem should find its most natural place, both by content and by method of proof – these two requirements are often incompatible. I was especially concerned with the choice of proofs. It was – and still is – my strong conviction that the best approach is always via the simplest, direct avenue that offers the best insight into the essence of problem. I tried to adhere to this principle, but I know that I have not always succeeded. I also tried – perhaps harder than necessary – to avoid proofs that needed reference to a later part of the book. In the final version, some proofs were modified after receiving Baer's and Kertész' comments on the manuscript. I expressed my hope that the 86 unsolved problems listed in the book will influence some of the more advanced students to begin research in this field.

I received several suggestions from Reinhold Baer, like including applications of the then newly developed theory of homological functors; he even furnished me with a preprint of his forthcoming paper. In accordance with my aim, my focus was on their structural properties as abelian groups; as a consequence, I failed to take full advantage of the methods of homological algebra. On the other hand, I was pleased to be able to include Łoś' theory of slender groups.

I was very happy when the book was well received; I must admit, I did not expect such a favorable reaction. In a few months, the 1000 printed copies were gone, though the publisher had no distributors in the English speaking countries. The British publisher Pergamon Press bought the copyright from the Hungarian publisher and reprinted it two or three times in the 1960's. Soon I started to receive letters and manuscripts with solutions to the problems listed in my book.

A few of my students in Budapest (E. Fried, G. Grätzer, E.T. Schmidt, F.A. Szász, R. Wiegandt) were interested in various problems on abelian groups, but they soon deserted to other research areas (lattices, universal algebras and ring theory). In a way I did not mind it, because this gave me the opportunity to get acquainted more closely with other branches of algebra. Our weekly seminar at the university in Budapest (held jointly with the research institute of the Academy) was a lively forum where both finished works and research in progress were openly discussed. Groups, abelian groups, semigroups, rings, modules, lattices, ordered structures, universal algebras – you name it – were the seminar topics. The spirit was cordial, the atmosphere congenial and everybody was open to criticism and unrestricted comments after the presentations. I still vividly remember these inspiring seminars with nostalgia.

Interestingly, none of Szele's students stayed with abelian groups, either: A. Kertész became a ring theorist, L. Kovács went to study noncommutative groups with B.H. Neumann. Actually, J. Erdös has not changed his area of research; after writing a few nice papers on torsion-free abelian groups (e.g., [**3**]) he stopped publishing.

At that time I was the only mathematician in Hungary who kept publishing regularly on abelian groups. In one of my papers [**9**] I introduced – in the current terminology – the cotorsion groups, simultaneously with D. Harrison and R. Nunke. My interest was shifting towards partially ordered structures, and I was kept busy with completing the manuscript of an introductory book.

In the academic year 1961/62, I was visiting Tulane University. Paul Conrad was my main contact; with him and with his students I had long conversations on lattice-ordered groups and rings. I was pleased to be able to attend Alfred Clifford's impeccable presentations on semigroups. It was a dream to meet the leading American abelian group theorists, first at the annual meeting of the American Mathematical Society in Cincinnati, and later during my visits to several universities as colloquium speaker. It was a great pleasure to make friends with many of them; I still proudly enjoy their friendship. My visit culminated in the Las Cruces conference in the first week of June 1962, where a most enthusiastic group of talented young abelian group theorists assembled; many of them are today the leading experts of the theory. I had the honor of giving three talks on recently solved and open problems on abelian groups.

A year later, in September 1963, another meeting took place, this one was of different flavor. The International Mathematics Union sponsored a meeting which was held in Tihany, a resort place at Lake Balaton. The Hungarian Mathematical Society suggested three topics for an international meeting: besides abelian groups, two topics in analysis to which Hungarian analysts contributed heavily. The IMU selected abelian groups, recognizing the role Hungarian algebraists played in the development of the subject.

This meeting is remembered as the first occasion when a larger number of abelian group theorists both from the West and from the East could meet to exchange their ideas and to discuss their works in progress. Ten participants came from the U.S. (including Carol and Elbert Walker, Dick Pierce, Ross Beaumont, Ronald Nunke, John Irwin, Joe Rotman, Samir Khabbaz, Frank Haimo) and five from the Soviet Union (L.Ya. Kulikov, M.I. Kargapolov, E.S. Lyapin, A.P. Mishina, L.A. Skornyakov). Among the speakers from other countries, we find Tony Corner, Jean Maranda, V. Dlab, K. Honda, Wolfgang Krull, B. Charles, J. de Groot, H. Leptin, A.D. Sands, A. Hulanicki, E. Sąsiada. We invited I. Kaplansky, R. Baer, S. MacLane, J. Dieudonné, A.G. Kurosh as well, but they could not come. The program was dominated by talks on the homological aspects, and it was clear that fresh air was invading the theory. The spirit was cordial, the atmosphere was congenial, though the communication was not always easy. (Some Russians were reluctant to converse in any language other than their native tongue, but – to our big surprise – they needed only a few hours to speak broken or even fluent English or German; these were the hours of wine-tasting.)

I was delighted to meet Kulikov, though the language barrier prevented me from having extensive discussions with him. However, we could converse through interpreters. He was very friendly and extremely nice, but it was clear that he had not been working intensively on abelian groups for several years prior to the conference. He challenged me to learn Russian – to reciprocate, he would learn English. We could not make a deal, since I foolishly insisted on Hungarian rather than English.

I am pretty much convinced that the New Mexico and the Tihany conferences were important milestones in the history of the theory. They marked the beginning of an upsurge in research in abelian group theory, culminating in several celebrated results in the 1960's and 1970's. This period was the golden years in abelian groups: it was not only the most productive period, but in these years more young researchers started their scientific career with abelian groups than ever before.

After the Tihany conference, I spent three more years in Hungary. My research was divided between abelian groups and ordered abelian structures. I especially enjoyed developing the Riesz interpolation property and its applications to functions spaces and algebras. In these years, abelian group theory was fading in Hungary; there was no genuine abelian group theorist in Hungary I could talk to.

What is the current situation in Hungary as far as abelian groups are concerned? For about 20 years the theory was dormant. In the mid 1980's, one of the topics has been revived: the factorization of finite abelian groups in Hajós's sense – the topic that kindled the interest in abelian group theory in Hungary. This is the only topic that is surviving today. Interestingly, this remained all the time an almost exclusive Hungarian theme. With the exception of the Scottish algebraist A.D. Sands, virtually all publications on this subject have Hungarian authors or co-authors. Hajós himself wrote a couple of papers on the problem in the 1950's, while Rédei authored more papers on the subject, see e.g. [**21**]. Currently, K. Corrádi and S. Szabó, members of a younger Hungarian generation, are publishing quite a number of papers on the factorization problem; see, e.g. [**2**].

The sixty years of history of abelian groups in Hungary we covered started with the factorization problem and ended with the factorization problem. Strangely enough, this topic has not had any noticeable impact on the development of the general theory; it served merely as a catalyzer. This is in sharp contrast to the two decades in between when the theory of abelian groups was flourishing in Hungary.

In conclusion, I should mention that a number of Hungarian authors have published important papers on noncommutative groups (J. Szép, P.P. Pálfy, etc.), and the fourth quarter of the last century marked increasing activity in the area. It would take us too far away from our subject to mention some of these results, so I will restrict myself to point out just one result by Rédei which served as an essential fact in the solution of the Burnside problem on groups of odd order. In a 1950 paper [Acta Math. **84**] Rédei proved that the only simple group with the property that all maximal subgroups of its maximal subgroups are abelian is the icosahedron group (of order 60).

REFERENCES

1. M. Bognár, *Ein einfaches Beispiel direkt unzerlegbarer abelscher Gruppen*, Publ. Math. Debrecen **4** (1956), 509–511.

2. K. Corrádi and S. Szabó, *A generalized form of the Hajós' theorem*, Comm. Algebra **21** (1993), 4119–4125.

3. J. Erdös, *Torsion-free factor groups of free abelian groups and a classification of torsion free abelian groups*, Publ. Math. Debrecen **5** (1957), 172–184.

4. L. Fuchs, *The direct sum of cyclic groups*, Acta Math. Acad. Sci. Hungar. **3** (1952), 177–195.

5. ———, *On the structure of abelian p-groups*, Acta Math. Acad. Sci. Hungar. **4** (1953), 267–288.

6. ———, *Ringe und ihre additive Gruppe*, Publ. Math. Debrecen **4** (1956), 488–508.

7. ———, *On a directly indecomposable abelian group of power greater than continuum*, Acta Math. Acad. Sci. Hungar. **8** (1957), 453–454.

8. ———, *Abelian Groups*, Publishing House of the Hungarian Academy of Sciences, Budapest, 1958.

9. ———, *Notes on abelian groups. II*, Acta Math. Acad. Sci. Hungar. **11** (1960), 117–125.

10. L. Fuchs and I. Halperin, *On the imbedding of a regular ring in a regular ring with identity*, Fund. Math. **54** (1964), 285–290.

11. L. Fuchs and T. Szele, *On Artinian rings*, Acta Sci. Math. (Szeged) **17** (1956), 30–40.

12. S. Gacsályi, *On pure subgroups and direct summands of abelian groups*, Publ. Math. Debrecen **4** (1955), 89–92.

13. J. de Groot, *Indecomposable abelian groups*, Proc. Kon. Nederl. Akad. Wetensch. **60** (1957), 137–145.

14. G. Hajós, *Über einfache und mehrfache Bedeckung des n-dimensionalen Raumes mit einem Würfelgitter*, Math. Z. **47** (1942), 427–467.

15. A. Hulanicki, *Note on a paper of de Groot*, Proc. Kon. Nederl. Akad. Wetensch. **61** (1958), 114.

16. A. Kertész, *On the decomposability of abelian p-groups into direct sums of cyclic groups*, Acta Math. Acad. Sci. Hungar. **3** (1952), 122–126.

17. ———, *The general theory of linear equation systems over semisimple rings*, Publ. Math. Debrecen **4** (1955), 79–86.

18. L. Kovács, *On subgroups of the basic subgroup*, Publ. Math. Debrecen **5** (1958), 261–264.

19. Z. Papp, *On the closure of the basic subgroup*, Publ. Math. Debrecen **5** (1958), 256–260.

20. L. Rédei, *Vereinfachter Beweis des Satzes von Minkowski-Hajós*, Acta Sci. Math. (Szeged) **13** (1949), 21–35.

21. ———, *Die neue Theorie der endlichen Abelschen Gruppen und Verallgemeinerung des Hauptsatzes von Hajós*, Acta Math. Acad. Sci. Hungar. **16** (1965), 329–373.

22. E. Sąsiada, *Construction of a direct indecomposable abelian group of a power higher than that of the continuum*, Bull. Acad. Polon. Sci. Cl. III. **5** (1957), 701–703.

23. F.A. Szász, *Über Artinsche Ringe*, Bull. Acad. Polon. Sci. **11** (1963), 351–354.

24. T. Szele, *Neuer vereinfachter Beweis des gruppentheoretischen Satzes von Hajós*, Publ. Math. Debrecen **1** (1949), 56–62.

25. ———, *Zur Theorie der Zeroringe*, Math. Ann. **121** (1949), 242–246.

26. ———, *Ein Analogon der Körpertheorie für abelsche Gruppen*, J. Reine Angew Math. **188** (1950), 167–192.

27. ———, *On the basic subgroups of abelian p-groups*, Acta Math. Acad. Sci. Hung. **5** (1954), 129–141.

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