Contemplations of an octogenarian mathematician*

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Dear friends and colleagues,

Let me begin with introducing myself. My name is Peter Roquette. I am Professor Emeritus, which means retired professor, of Mathematics. I am retired since 16 years. Thus I am, so to speak, a living fossil in this Institute. In German universities it is a tradition that after retirement the professor remains a member of the faculty lifelong – without however being obliged to give lecture courses. Although I have always liked lecturing, I have now given up except for occasional invited talks here and there to people who are interested in the history of mathematics. The subject of my present research is the history of mathematics. (More precisely, the history of algebra and number theory in the first half of the 20th century.) If you are interested to know more about my work then you may consult my homepage.[1]

When I was asked to meet you here and to talk to you at this occasion, I was happy to agree. However, it soon turned out that it was not so easy to decide upon the subject of such talk. What should I discuss with you today? You have come to Heidelberg from all parts of the world in order to meet the Laureates, who represent the leading people in the mathematical sciences. I am not a Laureate. But I am a member of the Mathematical Institute in Heidelberg, and since you have come to us you may be interested to hear more about our Institute. Note that "Institute" in this connection means an organizational entity of our university, which in the English speaking countries is often called "department".

In the preceding hours you have been informed about the present work and projects of the various research groups of our Institute. You will have observed that in mathematical research our department is well positioned when compared with other institutions of similar kind. The University of Heidelberg has recently been placed as the highest ranking university in Germany:

[1] www.roquette.uni-hd.de

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Although I am not convinced of the value of such rankings I may say that the quality of mathematics in Heidelberg has deserved a good share in the outcome. Hence you may be interested in how this came about during the past. Therefore I shall include some remarks about the history of our Institute.

Our university is quite old: it was founded in 1386, more than 625 years ago. In fact, today Heidelberg is the oldest still existing German university. But don't be afraid: I will not bother you with the university's history of 600 years. One of the reasons is that I do not know much myself about this long period. Moreover, the structure of universities in earlier centuries was quite different from today's. In fact, it was in the 19th century only, that the university began to organize itself anew, parallel to the enormous expansion at that time of the sciences. The university became a place not only of learning but also of research. In that period the Mathematics Institute was established as a separate institution of Heidelberg University.[2]

You can find the bas-relief of the founder of the Institute in the coffee room behind this auditorium. His name was Leo Königsberger (1837 - 1921). He was a remarkable man in many respects.

[2] At first it carried the name of "Seminar" in place of "Institute".

Leo Königsberger (1837-1921)

If you wish to know more about him you may consult the internet under the heading: "*Homo Heidelbergensis mathematicus*" which you can find by Google. There you will also find information about many perople who in the course of history have been connected to mathematics in Heidelberg.

But why should one be interested in the history of mathematics? Why should a mathematician look back into the past, while the problems to be tackled in the future are vast and require mental strength and perseverance?

Let me talk about my own experience:

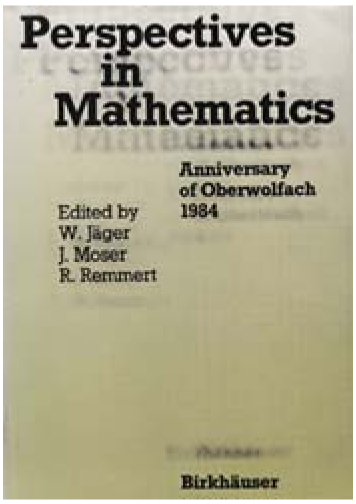
When, as a young student, I entered university and had to choose my field of study, then without any hesitation I decided to do mathematics. I was just eager to learn more from this wonderful science. Looking retrospectively to that time it seems to me that, unconsciously, my decision was, at least in part, influenced by the fact that mathematical results are *absolutely true*. Once a mathematical theorem has been correctly proved it remains to be true forever; it is added to the stock of mathematical discoveries which has piled up through the centuries. And it can be used to proceed still further in our pursuit of knowledge. The validity of a mathematical theorem is assured by its proof, it is not subject to further considerations, as it is for instance in physics, where new experimental results may force to abandon the whole theory and render it obsolete.

In other words: Mathematical theorems can be absolutely trusted and do not depend on opinion or prejudice, on political or religious indoctrination.

But later, in the course of my mathematical life I became aware that there is more in the picture of mathematics. Indeed there is constant change and movement. Recently I have found a text which, in my opinion, describes the situation well. Hence I would like to cite this text in its original:

> *The mere proof of validity of a theorem is in general not satisfactory to mathematicians. We also wish to know "why" the theorem is true, we strive to gain a better understanding of the situation than was possible for previous generations. Consequently, although a mathematical theorem never changes its content, we can observe, in the history of our science, a continuous change of the form of its presentation.*
>
> *Sometimes a result appears to be better understood if it is generalized and freed from unnecessary assumptions – or if it is embedded into a general theory which opens analogies to other fields of mathematics.*
>
> *Also, in order to make further progress possible it is often convenient and sometimes necessary to develop a framework, conceptual and notational, in which the known results become trivial and almost self-evident, at least from a formalistic point of view. So when we look at the history of mathematics we indeed observe changes, not in the nature of mathematical truth, but in the attitude of mathematicians towards it.*

(Well, I have to admit that the above citation is from my own text which I had forgotten and recently rediscovered as a contribution in the book "Perspectives in Mathematics" which had appeared in 1984.)



Let me give an example. You all know (or should know) Euler's beautiful formula for convex polyhedra:

$$v - e + f = 2\,,$$

v, e, f being the number of the polyhedron's vertices, edges and faces respectively. This is indeed true und remains true forever. But today it is not uncommon to interpret this formula in the framework of topology. For any closed orientable differentiable manifold its *Euler characteristic* χ is defined as an alternating sum, similar to the left hand side of the above formula, as

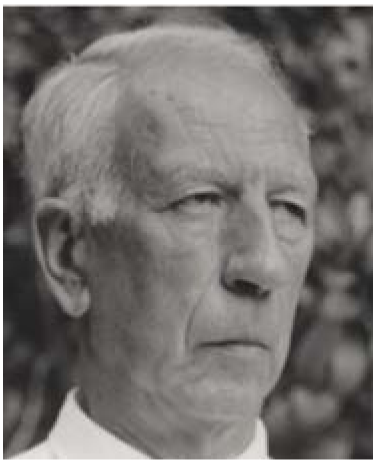
$$\chi = h_0 - h_1 + h_2 - h_3 \pm \cdots$$

h_n being the rank of the n-th singular homology group of the manifold. Indeed this turns out to be a topological invariant. There are several formulas connecting χ to other topological invariants, for instance the Riemann-Roch theorem, the Gauss-Bonnet formula, and more. In the case of a surface we have

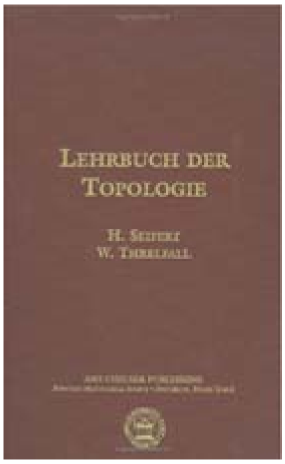
$$\chi = 2 - 2g$$

where g stands for the genus of the surface. On first sight these results seem to be of a quite different nature than Euler's polyhedron formula, belonging to the category of topological spaces and their homology groups, instead of the geometry of Euclidean 3-dimensional space. But nevertheless it is generally accepted that Euler's formula can best be understood in this more sophisticated framework. And the work of re-interpreting and generalizing Euler's formula is still going on.

It was a long way, more than 200 years, from Euler's polyhedron formula to the topological definition of the Euler characteristic and the investigation of its properties. Along the way a new area of mathematics came gradually into being, namely topology. For us it may be not without interest to note that mathematicians from Heidelberg had an important influence on this development of topology.

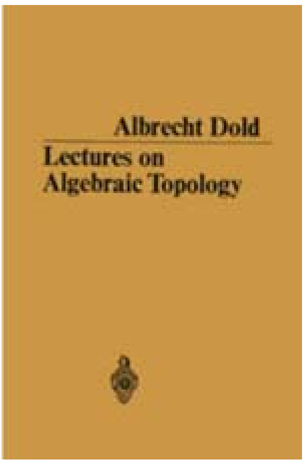


Herbert Seifert (1907-1996)



The topology book of Seifert and Threlfall has become a classic; it had enormous influence in the early times of topology, it had a number of editions and an English translation. After the death of Threlfall it was Seifert who during the 1950s built in Heidelberg a strong topological school.

The successor of Seifert was his former student Albrecht Dold who made Heidelberg one of the centers of topology worldwide.



Albrecht Dold (1928-2011)

Dold's book is considered as a modernized and widely enhanced follow-up of Seifert-Threlfall.

Let me cite another example, this time from arithmetic: You all know (or should know) the formula for the quadratic reciprocity law:

$$\left(\frac{p}{q}\right) = \left(\frac{q}{p}\right) \qquad \text{if } \ p \ \text{ or } \ q \equiv 1 \bmod 4$$

for odd prime numbers p, q. Here, $\left(\frac{p}{q}\right)$ denotes the quadratic Legendre symbol which indicates whether p is a quadratic residue modulo q or not; accordingly the symbol takes the value 1 or -1 respectively.

In simplicity, beauty and curiosity the quadratic reciprocity law does match Euler's polyhedron formula. Gauss has given 5 different proofs. He did this not because the theorem would thus be 5 times as true, but in order to learn more about the structure of prime numbers. Since then many generations of first class mathematicians have worked on it and tried a better understanding. Today the quadratic reciprocity law is embedded into the general reciprocity law of class field theory, discovered by Artin. This general law establishes a connection between the arithmetic of an arbitrary algebraic number field (of finite degree) and its absolute Galois group. More precisely:

The arithmetic of the number field is represented by its idele class group C (this is a refinement of its ideal class group); let $\overline{C}$ denote its maximal compact factor group. Let G denote the absolute Galois group of the field (this is the projective limit of the Galois groups of its finite extensions); and let $\overline{G}$ is maximal abelian factor group. Then Artin's reciprocity law establishes a canonical isomorphism between these compact groups:

$$\overline{C} \quad \xrightarrow{\approx} \quad \overline{G}\,.$$

Again, on first sight Artin's general result looks quite different from Gauss's quadratic reciprocity law. Nevertheless, after a long and extended development mathematicians are convinced, that Artin had found the viewpoint from which the quadratic law and its various offsprings can best be understood. But there is still further research going on, trying to explain in more detail and in more generality the highly complex structure of field arithmetic.

I am happy to report that also in this case, mathematicians from Heidelberg have contributed to this endeavour. I have in mind F. K. Schmidt.

F.K.Schmidt (1901-1977) with M.Kneser (1928-2004)

I could not find a good picture of F. K. Schmidt alone, hence I show here a photo of him together with Martin Kneser. The photo is dated 1952. It shows both on a boat ride on the Neckar river in Heidelberg. At that time Martin Kneser was assistant to F. K. Schmidt.

F.K.Schmidt was not one of the main protagonists but he has given valuable contributions to the development of class field theory. In the 1950s he established algebraic number theory as part of the research program in Heidelberg, which became the nucleus of what you see here today.

It may not be without interest that one of the Laureates who are present in this meeting, is a "descendant" of F. K. Schmidt. I mean Gerd Faltings whose academic advisor Nastold (1929-2004) had obtained his Ph.D. with F. K. Schmidt. You can verify this by consulting the homepage of the "Mathematics Genealogy Project".

One could add many more examples to the above two, showing that large parts of mathematical acitivity consist of evaluating, generalizing and combining known results and methods. Sometimes this is done to a point where the former notions and the past mathematical language are not any more easily understood in our days, and it needs experts to explain. In any case, we see that the mathematical science is not a static affair but it is continuously changing and expanding.

But I do not want to be misunderstood. I do not mean that mathematical research activity is predominantly occupied with reworking the past. On the contrary: Most of today's research is the answer to new problems which are set by external demands. But it turns out in many cases that the methods to tackle these new problems arise from the study and advancement of methods in the past. As an old Chinese proverb, attributed to Confucius, expresses:

Study the old to understand the new.

This is the apology for doing historic research.

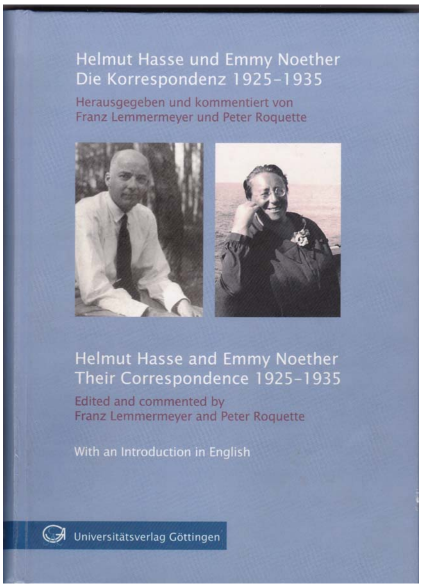
Let me recall how I came to be interested in historic research.

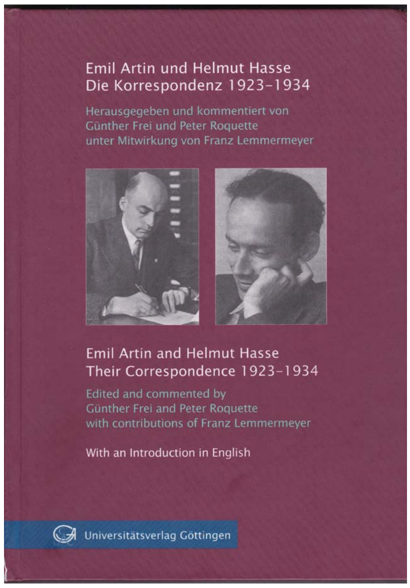
Once in the 1990s, after my retirement, I visited the archive of Göttingen University and found there the *Nachlass* of Helmut Hasse (1892-1979) containing his correspondence, manuscripts, his diary and more. Hasse had been my academic teacher and I was familiar with his published mathematical work. But in his Nachlass in Göttingen I discovered much more information about his way of doing mathematics than can be extracted from his published papers and books. Hasse had been an ardent letter writer. There were about 10.000 letters which Hasse had exchanged during 5 decades of his life with many mathematicians. With his correspondence partners he had exchanged ideas, problems, opinions, conjectures, results and more. I began to be interested and finally realized that these letters are a treasure for historic research. They show the gradual development of mathematics. Well, this refers to the work of one person, Hasse, only. But since he had many different correspondence partners one also gets a broader view of what mathematicians of his generation had discussed.

Reading Hasse's letters I learned the exciting story about how some of the mathematical highlights of his generation had been found, not yet in the elegant form of his publications but by hard work, trial and error, using analogies and bold conjectures.

I started to work gradually through all this, part of it jointly with my friends and colleagues Günther Frei and Franz Lemmermeyer. From time to time we published our findings, thus rendering at least some part of Hasse's Nachlass accessible to the mathematical community. This project is by no means completed yet. If you are interested you can see from my homepage what we have been able to publish until now, some part in the form of books and some as papers in mathematical journals. There are two highlights which may be worthwhile to mention:

1. *The correspondence between Hasse and Emmy Noether (published 2006),*
2. *The correspondence between Emil Artin and Hasse (published 2008).*





The Hasse-Noether letters in the first book offer us a live picture of the evolvement of Hasse's Local-Global Principle and how he worked hard to show that simple algebras over number fields are cyclic, jointly with Emmy Noether and Richard Brauer. Moreover, we obtain information about Emmy Noether's relation to the so-called "Noether boys", which is the nickname for the many young mathematicians who had gathered in Göttingen around her and later carried her ideas all over the world.

I did not meet Emmy Noether, she died in 1935. But as a young student I was briskly drawn into the sphere of her influence. For, in my 2nd semester I attended a course on representation theory and algebras which was given by one of the "Noether boys", Heinrich Grell. In retrospective I realize that he just read us the lecture notes from Noether's course which he had formerly attended in the 1920s. He grossly overestimated our capacity, since we were in our second semester, not having had any algebra course before that. Today I know that he practiced precisely Noether's way of teaching, namely challenging her people with demanding and difficult problems. This was a good lesson for me.

When I told this story to Auguste Dick, the first biographer of Emmy Noether, she conferred upon me the status of an "honorary mathematical grandson" of Emmy Noether. (In reality I am a "mathematical grandson" of Kurt Hensel, since he was the Ph.D. advisor of Hasse and Hasse was my Ph.D. advisor.)

The second of the above mentioned books contains the Artin-Hasse correspondence. It allows a glimpse into Artin's way of doing mathematics. We can participate in the evolution of Artin's reciprocity law, together with its consequences, and Artin's L-functions. It is my pleasure to announce that soon this book will be available also in English translation, thanks to the support of the Mathematics Center Heidelberg (MATCH) which strives to keep the memory of Emil Artin alive.

Let me tell you the story how I first met Artin; this was in Princeton in 1954. I was a young post-doc and had obtained what was called a "grant in aid" from the Institute of Advanced Study. We arrived in Princeton somewhat early before the term started. I did not dare to disturb the great master in his home and therefore decided to wait until the term of the university would start and I could meet him in his office. But soon, some days after our arrival, when I had been out of the house and just returned, I found Artin there. He had heard about our arrival in town and came to see whether he could be of help to us settling down in the new environment. Since I had been out of the house my wife had offered him some drink but he refused and, instead, volunteered to help her doing the dishes, which was what she just was occupied with. Well, I can confirm that Emil Artin was very good and efficient in doing dishes.

I am telling this story in order to show that Artin was open and easily accessible for all people, including the younger ones.

Artin's lectures were absolute pearls of thought. Once we were invited to a party at the Artins. At some point in the evening he would cease participating at the conversation, he got up and paced up and down the room without regard to the visitors any more. We were told that in this way he was preparing his next lecture and should not be disturbed. And indeed, next morning his lecture was beautiful again.

Let me return to Heidelberg and this Institute. I have mentioned already two names of mathematians whose work in the 1950s was the nucleus of the later development until today: Herbert Seifert and F. K. Schmidt. But in this connection I have to mention also two other names:

Gottfried Köthe who introduced functional analysis into the palette of research here. He came to Heidelberg in 1957. Those were the early days of functional analysis and he had a great influence on its further development in Germany.

Gottfried Köthe (1905-1989)

Hans Maass (1911-1992)

Hans Maaß (1911-1992) did complex function theory and automorphic forms. He has introduced non-analytic automorphic forms which today are known as "Maaß forms". He became professor in Heidelberg in 1948.

Now I have mentioned four mathematicians who have laid the foundations of the Institute's main activities of today:

Herbert Seifert, F. K. Schmidt, Gottfried Köthe, Hans Maaß.

Their main activities in this respect can be dated to the 1950-1960s. You may wonder why I have chosen just this period; certainly there were interesting people and activities before and after that period. But in that particular period there was a great expansion and new orientation of German universities, matching the general expansion of the Sciences in the world. This was reflected in Heidelberg in the expansion of the research areas to be worked on, but also in the expansion of staff. In the year 1956 the Mathematics Institute had already four professors and some more teaching staff, whereas formerly there were just two and sometimes only one professor. In addition, the University grew also in the size of the campus. Whereas formerly the university buildings were scattered throughout the old city, now a new campus was being built on the outskirts, just here where you are staying today.

Mathematics was one of the first buildings to be completed on this campus, that was in the year 1956. At that time this building was considered to be one of the most modern mathematical institutes in Germany, equipped with all what mathematicians needed in teaching and research (there were however no computers yet available). The building was spacious and in any case sufficient for the size of the Institute.

Since then, in the course of time the Mathematics Faculty in Heidelberg has greatly expanded (as have also the other scientific institutes) and this our building which you are in at present, is much too small. Mathematics offices, library and computer laboratories are scattered all over the campus. Hopefully, in some years the Heidelberg mathematicians will obtain a large building, sufficient for today's needs, where they have working space for all.

But I would like to emphasise that the quality of a department does not depend directly on the size of the faculty, nor of the size of the buildings or the amount of money available. The quality of the mathematics at the department solely depends on the quality of its people. History shows that also small departments, even just a single professor working alone, can give substantial and groundbreaking contributions to our science. One should never forget this while running for more and more financial support.

This is what I wished to say to you today. I wish you more interesting days in Heidelberg at the Laureate Forum, and I hope that in the future you will remember the Heidelberg Mathematical Institute as an interesting place with interesting people. I would be glad to meet you again some time in the future.