SIXTY YEARS OF MATHEMATICS*

Kurt A. Hirsch (1906 - 1986)

I left school in 1924 shortly after the end of the inflation in Germany during which we had to divide by 10^{12} the value of money from one day to the next. I didn't straightaway go to the university because I had no money. My father had died when I was 7 years old in 1913, and at that time there was hardly any support for students who needed financial support. So I became an apprentice to a bookseller for a year and a half, but I gave it up. I suddenly realized that I talked about books I hadn't read not only to customers - that's alright - but even to my friends, and so I gave it up.

Luckily, at that time, I had just discovered that the rector of the University of Berlin had a discretionary fund from which he could make awards to descendants of former rectors of the university. It so happened that my grandfather was a rector of the university in the 1870's and 1880's. And so I went to the rector and I addressed him, as it was proper, "Your Magnificence". (If you talked to the dean, you had to say, "Your Respectability".) And at the beginning of each semester, I got a certain sum of money from him which enabled me to study. I wasn't decided whether I should study mathematics or classics. I had been at school where we had both Latin and Greek on the one

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hand and mathematics on the other. And so for a couple of weeks I went to lectures in mathematics, classics and philosophy, and decided to go in for mathematics on the strength of Professor Segre's lectures which were quite fascinating.

I must say a little bit about the German university before 1914. Studying at the university at that time was a privilege of the rich. There were no grants and no stipends for needy students. There was a long tradition of freedom not only to teach on the part of the professors and lecturers but also freedom to learn on the part of the students. They were very independent. They went to lectures if they wanted to - as many as they could cope with - without any set course, and there was no advice, no counselling. You just had to take pot luck and see whether you could cope with the lectures. There was no set syllabus. You just went and listened to lectures and worked through them until you thought you were ready for the examination and then you presented yourself for the examination.

There were two kinds of examinations. One was called the state examination which led to a teaching career. If you went in for that, after four or five years of studying, you were assigned a mathematical topic on which you had to write an essay. It needn't be anything original but it should be a coherent account on a mathematical theme - something like our Master's degree if it is taken by thesis. The other one was to go in for a Ph.D., and if you went to a professor and said, "I would like to write a Ph.D. thesis", he would say, "Go ahead; why don't you?" He wouldn't give you a topic. He wouldn't advise you. It was up to you to search the literature and to find a suitable topic. Of course, once you had your first original result, you went and told the professor and from then on, he would certainly give you

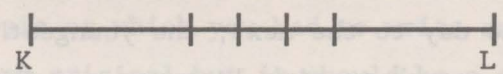
all the help he could.

As I said, teaching was for the privileged. That applied not only to the students but also to the lecturers. The first step was to become what was called a *Privatdozent*. He was allowed to announce lectures. He wouldn't get any money for it and only comparatively rich people with resources of their own could affort to be this. That went even for the rank of *Ausserordentlicher* (Extraordinary) *Professor*. The full professors had a good salary. Part of their salary came from the fees which the students paid for attending their lectures. At the beginning of the semester, you would go and book so and so many courses. They were written into your record book. For each course, you had to pay a certain amount of money, and for the well-attended courses (differential and integral calculus or analytic geometry) there were full audiences of something like 150 or 200 students. Quite a profitable fee for the professor. Well, the story went that for young people who went to the university it was so important that they should sit at the feet of the great man and listen to what he had to say.

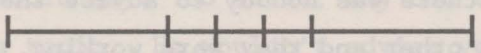
After the Second World War, fees were abolished and suddenly, from one day to the next, the youngest assistant lecturer was good enough to give lectures and the professors lost quite a substantial part of their salaries. Well, the scene changed a bit after the First World War. When the more mature students came back from the war they found the situation unsatisfactory - there was nobody to advise them, they had no contact with each other and they were working, as it were, in a vacuum. So they founded a loose organisation of mathematicians

and physicists (I talk only about Berlin) that was recognised and even a room was assigned to them. From 1923 or 1924 onwards, there was quite a flourishing mathematical-physical working community. The professors in Berlin at that time were Erhard Schmidt whom my colleagues will know from his theory of integral equations. Then there was the ill-fated Bieberbach and there was my own teacher Schur the algebraist and von Mises the applied mathematician. There was also a galaxy of other people who were not full professors: Segre whom I have mentioned, Heinz Hopf the topologist, John von Neumann who later became very famous, Löwner who worked in analysis, Hammerstein in integral equations, Robert Remak in group theory and many others. It was a very flourishing group and was quite comparable to the other place where mathematics was prominent, namely Göttingen.

Erhard Schmidt had a very broad Baltic accent. He came from the Baltic and was a very amusing lecturer. I remember one occasion when he lectured on set theory. Our blackboards were of the rolling type. You could write on the board and then turn the handle to make it go up and it eventually came down. What happened was he talked about set theory. He would draw a line



and put a point K and a point L on it and then started arguing about the related points. After five minutes or so, he would draw another diagram like this



and started arguing. This went on, and after a long while, the blackboard was all the way round. When he saw the first line, he

said, "Ah, there it is!" He didn't have to draw another line with all the points on it.

Bieberbach was a fascinating but lousy lecturer. He would say "A" but he would mean "B" and he would write "C" on the blackboard and "D" was correct. But if you really took the trouble to go through the lecture notes and worked it all out, you would really profit a lot from it. Later, of course, he became completely nazified, but I can tell you from my own experience one incident which shows that at least on the first of April, 1933, he was still quite a sane man. That was the so-called "Boycott Day", the day on which Jewish shops were boycotted and Jewish professors and lecturers were not allowed to enter the university. Everybody who was there had to make a little speech about the rejuvenation of Germany, etc., etc. And Bieberbach did this quite nicely and then he said, "A drop of remorse falls into my joy because my dear friend and colleague Schur is not allowed to be among us today." It shows that at that time he was really still sane, and he openly confessed his friendship with a Jewish professor. In fact, they had collaborated on some very nice papers together.

I must tell you a little bit about Schur. If my colleagues think they are overworked, let me tell you this. Schur always gave 2 courses, each with 4 lectures every week and 2 hours of exercises. That makes it 12 hours. In addition, he always gave an optional course of 2 hours a week on - well, I heard him on elliptic functions, summation methods and representation theory. (He was very versatile and he had an enormous command of mathematics.) He also had a weekly seminar on Monday afternoons which lasted 2 hours. That means his workload was 16 hours a

week. The two courses Schur gave were in algebra and in number theory. Each lasted two years (4 semesters) and they were staggered so that they didn't start with the elementary part at the same time. The algebra course began, as it was the custom at that time, with determinants and matrices, followed in the next semester by polynomial algebra, group theory and Galois Theory. In number theory, he would give a course in elementary number theory, followed by algebraic number theory, ideal theory, analytic number theory and then followed by usually something called special chapters in number theory. His exercises were quite excellent - very thoughtful, more of the type found in the books of Pólya and Szegö. The exercises would be set one week earlier and you had to hand them in the following week, and they would be discussed in the week after. It was Schur's custom that if somebody produced a particularly elegant solution of one of the questions, he would be called to the blackboard to produce his solution.

Well, I remember an occasion - I even remember what the exercise was: it was in interpolation and it was to find the explicit interpolation formula for a polynomial if not only the values of f(x) are given at $x_1, x_2, \ldots, x_n$, but also a certain number of derivatives $f'(x_i), \ldots, f^{(k_i)}(x_i)$, where the k_i would vary from place to place. I did find what I thought was an elegant solution and was quite sure that I would be called up to the blackboard. On the way to the university, I was stuck in the underground for an hour and a half, and true enough, when I finally came to the university, the others asked, "Where have you been? You were called to produce your solution." The occasion when I missed out on being called to the board left an indelible mark in my memory.

One day, Schur came to the university very excited because on the underground he had found a very short proof of a famous theorem in number theory - Stickelberger's Theorem - which says that the discriminant of a number field is congruent to 1 modulo 4. That proof was published somewht later in the *Mathematische Zeitschrift*. On the next page, a one-page proof (also by Schur) of the irreducibility of the cyclotomic polynomial, and on the next page, a one-page proof of the same theorem by Landau. He would call what is now called Schur's Lemma the *Verkettungsatz* (Linkage Theorem) and would prove it, not as we would do it nowadays in abstract fashion, but as a solution of the matrix equation AX = XB. He would set up the matrix X and partition it in blocks, etc. That was how he always proved it. He was not averse to modern ideas in algebra, but that was the way he had done it and that was the way he stuck to it. I got my Ph.D., my first Ph.D., in philosophy in 1930. It wasn't philosophy; at that time, there was a deep struggle among mathematicians about the foundations of mathematics and there were two factions: one of Hilbert and his students, who wanted to set up a completely formal theory of mathematics - the so-called formalists; and the other under the guidance of the Dutch topologist Brouwer - the so-called intuitionists. I was interested in tracing this struggle historically and found great similarity and contrast in the attitude of Leibnitz, which was formalistic, and that of Descartes, which was intuitionistic.

Because at that time I needed money (I was married and had a son), I started writing about matters mathematical or philosophical in a daily newspaper, the oldest daily paper in Europe, which dated back to the 17th Century - the so-called

Vossische Zeitung. The heading that was attached throughout the centuries said it was a paper of state and of learned matters. (After the Second World War, I had a very nice experience when I first visited Israel. Professor Fraenkel, whom you know as the author of the book on set theory, invited me to his house and there he had spread out on the table all the articles that I have ever written in the *Vossische Zeitung* on mathematics. He had taken them with him and kept them when he migrated. I was quite touched.) Then, as now, not many papers devoted much space to such matters. Normally only the top class paper did. In England, for instance, it was the *Manchester Guardian* and the *Times* that had regular reports on scientific and philosophical matters.

At that time, we were all under the impact of the appearance of the books by van der Waerden. A new spirit had come into algebra owing to the work of Artin, Emmy Noether and others. There were a number of my colleagues (Bernhard Neumann, Richard Rado, Fenchel, Peter Sperling and a few others) who formed a little circle to study this new kind of algebra we found in van der Waerden's books. Later, when Remak was no longer allowed to enter the university, he would invite us to his house (he was a man of independent means) and we would continue our studies. I am eternally grateful to him because though I was working at the *Vossische Zeitung* I did not lose interest in mathematics. At that time, I had read a paper by Schreier on infinite soluble groups and in van der Waerden's book there was a discussion on Emmy Noether's maximal condition. That gave me the idea of combining the two, namely to investigate soluble groups satisfying the maximal condition on subgroups - groups which are nowadays called polycyclic groups.

I was still in Berlin when the *Vossische Zeitung* came to an end in March 1934. For a year and a little bit more, it had struggled under Hitler - keeping up its prestige and also devoting more space to cultural matters rather than political matters. So I had a weekly full page in which I reported or had articles on progress in mathematics or the sciences or philosophy. I would write around and ask Heisenberg or Schrödinger to write and indeed, they did. Every week, there was at least one substantial article of lasting interest. Well, at the end of March, 1934, the newspaper folded up, and immediately I went to England. I had distant relatives in that country and it was natural for me to go there.

I was not decided yet whether I should continue with scientific journalism or with mathematics. It was due to Philip Hall that I chose to become a mathematician because I visited him in Cambridge in his room in King's College and I told him a bit of what we had done in Berlin in reading van der Waerden and also about the idea of investigating soluble groups satisfying the maximal condition. He encouraged me and he also arranged with a friend whom I had made and who was a fellow of King's College that I would be accepted as a research student, and that put me on my mathematical career. I got the Ph.D. from Cambridge in 1937. The custom was that there were two examiners. Mine were Philip Hall and Max Newman (M.H.A. Newman), and it was customary to have an oral examination to find out about the mathematical knowledge of the candidate. Well, I remember being asked two questions. The first question was what train I had to catch and the second question was whether I took sugar and milk in my tea. Otherwise, we just talked in a friendly way about mathematics.

The business about trains was this. I had been appointed at the University of Cambridge and was asked to give a course in algebraic number theory - a special course. Robert Rankin told me he attended this course and that I put him on his way; as it were, I aroused his interest in algebraic number theory. But I had also been appointed to my first permanent job, namely an assistant lectureship at the then University College of Leicester. So I spent half the week in Cambridge and half the week in Leicester. To get to Leicester from Cambridge you had to take first a little branch line and change train in Kettering and then go up to Leicester, and I had to do this to and fro. We lived far out at the end of Cambridge and rented a house for £1 a week, including rates. At the end of our garden, there was a level crossing at which the train had to stop. I had an agreement with the conductor that I could jump off the train and walk into my garden. Unfortunately this only worked on the way back from Leicester and not on the way to Leicester. I really had to go to the station.

Soon afterwards, the war started in September 1939 and much of the staff of the University of Leicester was called up. I remember I found myself suddenly teaching Honours student in German - middle High German. It's as if you were suddenly called to teach Chaucer. I had done middle High German at school and we had read the famous *Nieberlungenlied*. I remembered enough to be able to keep ahead of the students by one hour.

After the war, I went to Newcastle for a few years from 1948 to 1951, and then went to London. I must tell you something about London. When I went there, algebra was practically non-existent. The colleges in London had been evacuated to

various places all over the country. Towards the very end of the war, two influential mathematicians (both geometers of the old 19th Century algebraic geometry school) had gotten together in London and had drawn up a syllabus for the examination. At that time, the examination in London consisted of 6 compulsory papers and 2 advanced papers of topics of your own choice, and the 6 compulsory papers had 10 questions each. There were 30 questions in applied mathematics and 30 questions in pure mathematics. The 30 pure were divided into analysis on the one hand (15 questions) and geometry and algebra on the other (topology didn't exist yet). Of the 15 in analysis, there were 10 in the real and 5 in the complex. They had concocted a syllabus which would divide the 15 questions in geometry and algebra in the ratio of 11 to 4 - 11 tricky questions of 3-dimensional projective geometry (19th Century projective geometry) and 4 questions on algebra. Even these 4 questions on algebra had a sort of geometric flavour such as the transformation of the principal axes, which is an algebraic theorem and used in geometry.

When I was appointed to London, the first thing Harold Davenport did was to put me on what was called the Syllabus Revision Subcommittee, which met once a year to look at the syllabus and make recommendations for changes. At the first meeting, I made a reasoned plea for more algebra and less geometry, and the ratio was then changed to 5 to 10. Then the following year, like Oliver Twist, I came back for more. It was 6 to 9, and over the years, the geometry receded and algebra became more prominent.

I had the idea that it was more important for a group of mathematicians to talk the same language than to divide among the

various disciplines. Now, before the Second World War, if you went to a provincial university, you would find an algebraist, a number theorist, an analyst, a topologist or geometer, a statistician, etc. And they wouldn't talk to each other. They didn't talk the same language. In fact, it was not game for them to talk mathematics. Felix Klein, in his book on the development of 19th Century mathematics, tells that as a young doctor he went to England and visited all the famous people (Sylvester and Cayley and others), and one of them would talk to him about church architecture and another one would talk about the growing of roses, and nobody would talk any mathematics, and that prevailed for a long, long time.

Before the war, there was no meeting of all mathematicians in the country with invited speakers and at which you could also give a short talk about your results. There were only the monthly meetings of the London Mathematical Society. I remember that at the very first post-war annual British Mathematical Colloquium in Manchester in 1946, the geometer John Todd gave a lecture which ended up with two questions concerning geometric topics that could be phrased in algebraic terms. He said these questions were open questions. Philip Hall got up and gave him the answers outright, and Max Newman said to me, "Ah, you see, if a man from Trinity College, Cambridge, wants to talk to a man from King's College, Cambridge, they had to travel to Manchester first."

I put this policy into effect, and at Queen Mary College, whenever there was a vacancy (it didn't matter what it was for - analyst, geometer, statistician or what have you), I always saw to it that a group theorist would be appointed. So, over the

years, we got together a nucleus of group theorists and it got so bad that for some years if there was an advertisement in the papers for a post at Queen Mary College, nobody except group theorists would apply for the job. They knew that a group theorist would be appointed. This bore fruit because Queen Mary College in London is still a centre of group theory research. The policy I initiated paid dividends. As you heard, I had recently my 80th birthday, and there were celebrations at Queen Mary College, and it was very gratifying for me to see how many many people - top algebraists - passed through Queen Mary College. They were professors from all over the place: Paul Cohn in London, Philip Higgins first in King's College and later in Durham, Otto Kegel who was in Queen Mary College for a number of years and who is now in Freiburg, etc., etc. It was a very nice gathering of people who had been through Queen Mary College and also of many of my former students and Ph.D.s from all over the world. In fact, one of them came specially from St. Louis, Missouri; one from Utrecht; several from Germany. Well, that was my 80th birthday and that was also,you might say, the official end of my mathematical career.

I like to end with a few words of thanks. I've been in Singapore many times - it must be the sixth time now - and each time I had such a friendly reception and I felt so comfortable. Especially this time. I have been here for only two weeks. Everybody was so friendly and I think I like to take this opportunity to thank not only Tsu Ann but also everybody who has been in contact with me for the very warm welcome I have had on this and many other occasions. Thank you very much.

DISCUSSION

Question. Who inaugurated the British Mathematical Colloquium? Was it Max Newman?

Hirsch. In a way, yes. Max Newman, Bernhard Neumann and I and Graham Higman. Manchester was the choice because Bernhard Neumann was there. I had in the previous year given a talk to the British Association for the Advancement of Science on the theory of knots and suddenly found myself on the title page of a picture poster brandishing knots and models which I had made from ropes and rubber tubes. That was the first time for many years that mathematics was a topic at the British Association for the Advancement of Science. But the Mathematical Colloquium is now making its rounds through all the universities in the country. The first one was in Manchester and Max Newman was a very good organiser.

Question. Would you like to comment on a mathematician's fate?

Hirsch. Mathematician's fate! I only know that I toyed with the idea of having a cottage in the countryside and calling it "Aftermath". Mathematicians are a happy breed, as a rule. There are some exceptions. There were a few suicides among mathematicians, but by and large, they are even-tempered, I hope.

Question. How did you decide to go to England for your Ph.D.?

Hirsch. Well, in a way it was natural. My mother was born in Hamburg and there were close ties between Hamburg and England in commerce, in many cultural aspects. It was a natural decision. Later, so many people went to America but the British authorities at that time were reluctant to admit anybody who hadn't got anything to offer - that could be money or skills. I

had not much money but they let me in. But only for a year and I had to renew it from time to time. I went in April and started studying in October. I just managed to get a grant for the first year - a princely sum of £200 a year, and I and my family had to live on it. That was quite possible at that time. My first university job was an assistant lectureship which paid £250. There was also one thing. As a very young child, before my father died, we had an English tutoress in our family. I must have learned to talk English - I completely forgot it in the intervening years. And for some reason or other I never took English in school. That was good because the teaching of English at school was not so hot at that time. I remember that while I was in Cambridge waiting for my family who came much later in September, I went to the pictures regularly in order to learn English. My English is not flawless and the German accent is still very audible. What I know of English I learned in England and a good deal of it at the pictures.

Question. How did you get into translating texts from Russian into English?

Hirsch. That is a curious story. When I was at school, one of our teachers who came back from the First World War had been in Russian captivity and during captivity, he had learned Russian. He wanted to impart this to his students, and he offered a voluntary course in Russian. That was how I learned the rudiments of Russian, but I quickly forgot all about it after 40 years. Then after the war, the Russians ceased to write their papers in a western language with Russian summary or in Russian with a western summary, and the *Mathematical Reviews* wrote around frantically among reviewers to find out who could review Russian papers. I had some knowledge of Russian and offered my services,

and they sent me a two-page paper, and I had to look up every single word in the dictionary.

At least I knew the letters of the Russian alphabet. Also, since I had at school not only German but also Latin, Greek, French and Hebrew, I was used to tackling a language grammatically with passing sentences and I could find the structure of the sentences. That is a doctrine I always preach nowadays to my translators - pass the sentence and find out where the verbs and subjects are. So I had to look up every one of those words in the dictionary, but that was alright. I finally got their meaning and sent my review of this paper. Gradually not only did they send me other papers but I also became interested. There was a famous paper by Kurosh and Chernikov on unsolved problems in group theory which the Americans asked me to translate. I got more and more involved. The last book I translated was Shafarevich's *Basic Algebraic Geometry*.

I am still the editor of the *Russian Mathematical Surveys*. What I have to do there is to correct the mistakes which other tranlators have made. I have a panel of some 40 experts in various branches of mathematics but even the best translators make mistakes, and I am editing their translations. Even here, the papers follow me even though I have an assistant now. I have come to the conclusion that to make a successful translation of a Russian mathematical paper, the first prerequisite is a thorough knowledge of the mathematical content of the paper, otherwise you make horrible mistakes. In the early years, the Americans published translations from Russian with atrocious mistakes. The second prerequisite is that you could write a fluent style of English. A knowledge of Russian is quite immaterial.