Reminiscences from Hilbert's Göttingen

Richard Courant

Editor's note: Richard Courant was born in Lublinitz, Germany on January 8, 1888. He went to Göttingen in 1907 and wrote his dissertation under the direction of Hilbert, receiving his degree in 1910.

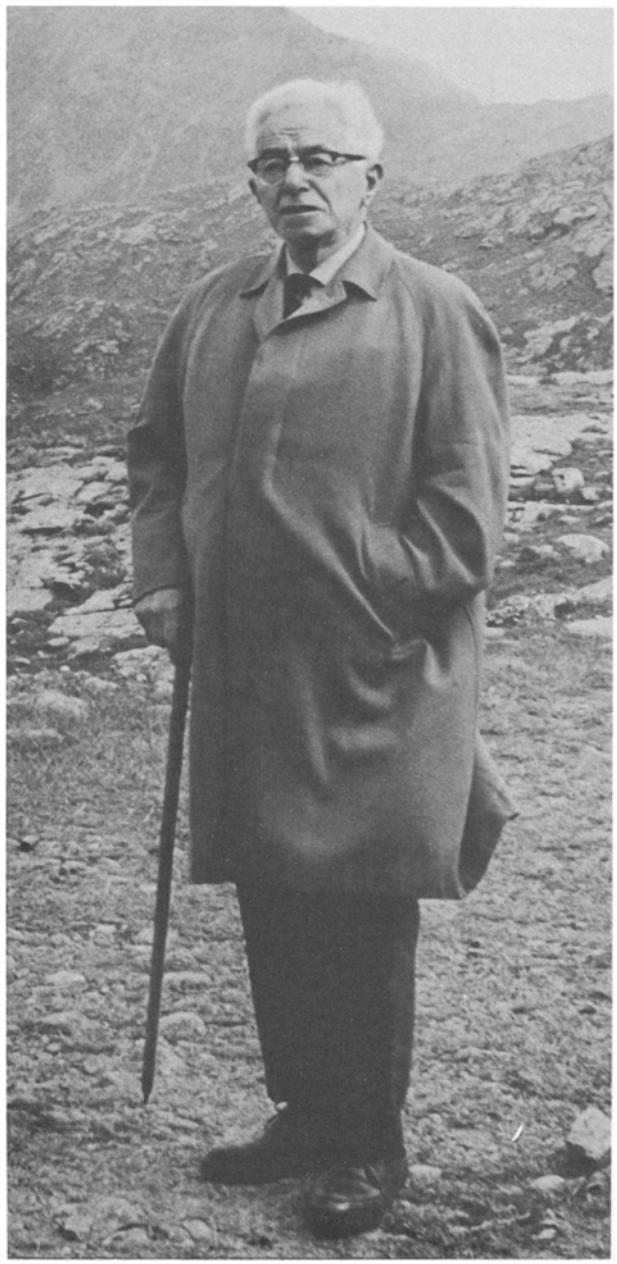
From the beginning Courant was enchanted by Göttingen and the kind of mathematical life which it represented. Forsaking other opportunities, he lectured at Göttingen, became Professor, and finally director of the Mathematical Institute there. He had a profound influence on the Institute and its development until 1933 and the advent of the Nazi regime.

After a one year visit to Cambridge, Courant went to New York University in 1934, first as a visiting professor and then as director of the Graduate Center for Mathematics. He was instrumental in establishing the Institute for Mathematical Sciences at NYU and served as its director from 1953 until his retirement in 1958. He died in 1972.

To merely say that Courant remembered Göttingen with fondness is inadequate; he had a deep, passionate conviction that the sort of mathematical life-style represented by Göttingen during the early part of this century was an ideal to which all mathematical institutes should aspire. The man he felt was most responsible for this greatness was David Hilbert.

The following article is an edited transcript of a talk delivered by Richard Courant at a Colloquium in the Department of History of Science and Medicine at Yale University on January 13, 1964. It is an informal and very personal view of the place and the man he most admired – Göttingen and Hilbert.

I would like to start with a few general remarks about the History of Science. I think scientific life is a very tender plant, more vulnerable than most other ingredients in the flow of history. Decay and flourishing of science, just as it does for art and music, depends very much on unstable and short-range human factors. Of course, I cannot discuss in the philosophical manner broad questions of general history, but I want to confine myself to discussisng, by way of specific illustrations, one of the single important points for the development of science. This point is the decisive role which great individuals have played, and probably will also play in the future, for the progress of scientific life. Individuals around whose radioactive personalities, fruitful scientific life crystallizes.



Richard Courant (1888–1972)

During the era since the French Revolution, a sizeable number of such great scientific personalities has been largely responsible for the enormous development of science and

technology in Europe, and by a chain reaction, outside Europe. It is true that the level of excellence and achivement in each case has invariably decayed after one or two generations. Also, it is remarkable that the centers of scientific activities have rapidly migrated from place to place and from country to country. Yet the overall progress from the time when the Ecole Polytechnique was established during the French Revolution to our own era of commercialization, public relations, and showmanship, has been very great indeed.

If the young generation wants to resist the forces of decay, which are present in every civilization and must be resisted in every phase of civilization, then it seems to be vital that a sense of historical understanding and tradition be preserved and that some awareness of the role of leaders from the not too distant past should be kept alive (I think of leaders such as Rutherford, Niels Bohr, Harald Bohr, Arnold Sommerfeld, Ludwig Prandtl, and many others).

I personally had the great fortune of a close personal and scientific association with some of these outstanding personalities. Since my days as a graduate student I belonged to the unique scientific center which had developed at the University of Göttingen. It had been initiated and guided by Felix Klein and, in my time, was filled with infinite energy and devoted enthusiasm by David Hilbert, until the moment when the Nazis broke the back of this very unique scientific center.

The Mathematics Institute in Göttingen was not isolated at all; it was the organic center of a broad effort in the sciences reaching far beyond mathematics. It is perhaps fitting for me to describe unsystematically, informally, and personally some features of this old Göttingen as they come to my mind, and mostly attached to the name, to the personality of that great mathematician and leader of the younger generation, David Hilbert.

Felix Klein (1849–1952)

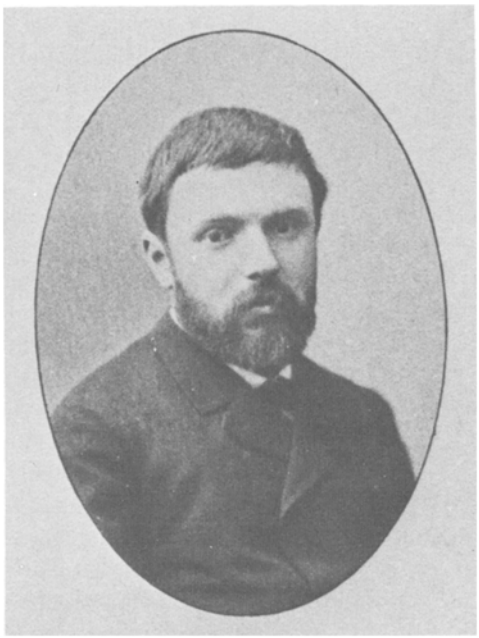
Otto Toeplitz (1881–1940)

I myself came to Göttingen in the late summer (I think it was the year 1907) advised by a mathematician whose name is probably known to many of you, Dr. Otto Toeplitz. He was a Privatdozent in Göttingen at the time. Many in the group of students around me shared, to a degree, extreme poverty. Some lived in attic rooms in which the water froze in the very primitive washstands and had to be broken painfully in the morning if one was anxious to be at least a little bit clean. One was not able to afford a warm meal every day. But all these things were not a deterrent for a high wave of enthusiasm which enveloped everybody and radiated through the whole circle around Hilbert.

When I came to Göttingen, I luckily found almost immediate access to this circle. I met, in the first days of my stay, people such as Carathéodory, Erhard Schmidt, Toeplitz, Hellinger, Alfréd Haar, Hermann Weyl (an aloof young student who did not quite belong to the clique into which I was drawn in the beginning – cliques also existed at old Göttingen), and Erich Hecke, who followed me from the University of Breslau. Very soon, Harald Bohr appeared for longer periods as a frequent visitor and became a very close

friend. Many Hungarians and many Americans also came. A very outstanding personality among the students (post-doctoral students, one would call them) was Theodore von Kármán who, at the time when we met him, did not carry the title of nobility. He was elevated and his family knighted only during the time we spent at Göttingen together. There were many visitors, including Mittag-Leffler, Poincaré,

Ernst Zermelo (1871–1953)



Henri Poincaré (1954–1912)

Hermann Minkowski (1864–1909)

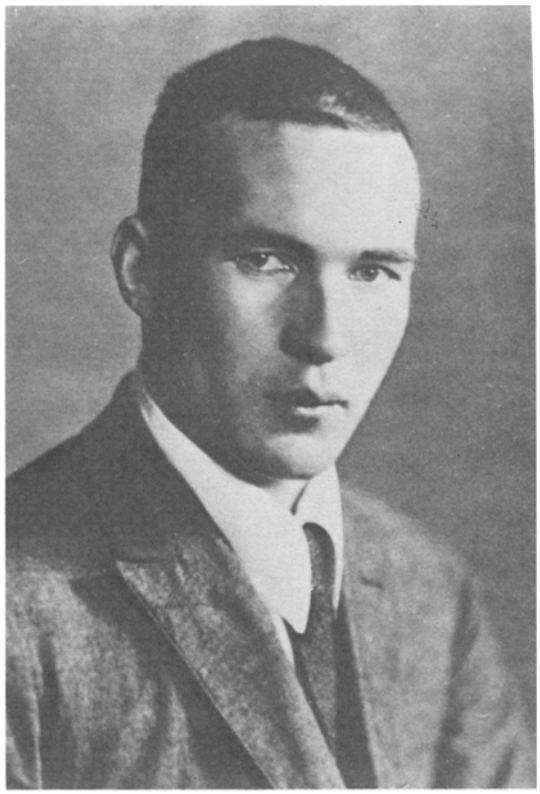
Carl Runge (1856–1927)

P. S. Alexandrov (*1896) Uryson
L. E. J. Brouwer (1881–1966)

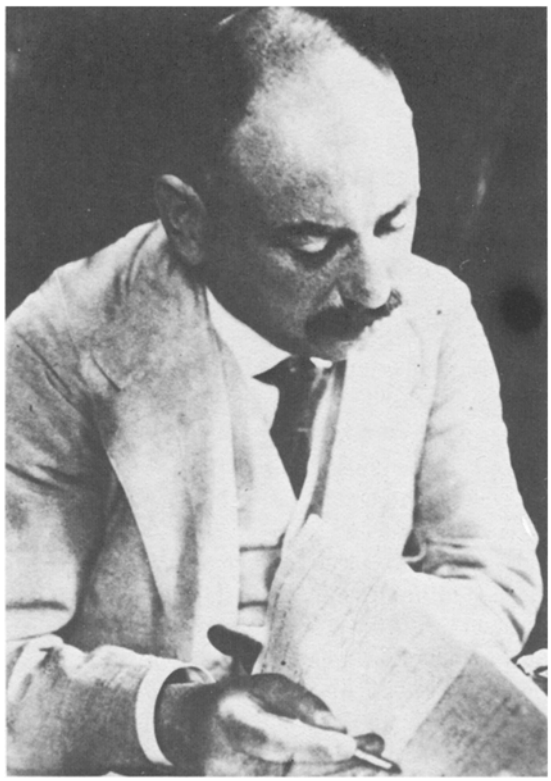
Holmgren, Steinhaus, Schauder, Levi-Civita, and others. There were Bernays, Koebe, Zermelo, Felix Bernstein, Prandtl, Debye, Schreier, and Paul Ehrenfest, one of the leaders in physics at the time.

The Senior Faculty consisted of Hilbert, Minkowski, Klein, and Runge. Klein had, to a certain extent, retired, but he was a very active organizer and a master of Göttingen affairs. Later, after the interruption by the First World War, a new crop of brilliant people turned up: among them were James Franck the physicist, Heisenberg, Max Born, the astronomer Kienle, the biologist-geneticist Kühn, Pascual Jordan, Pauli, and others who just don't come to my mind at the moment. Very regular and intense visitors who were really part-time members of the group, came every year: for example, Paul Alexandrov from Moscow, his student and protegé, Kolmogorov, Uryson, who died tragically on one of his wild swimming expeditions in the sea, and L. E. J. Brouwer, the pioneer of modern topology who unfortunately seems to be almost completely forgotten in the modern development. There was also Lexis, the initiator of modern mathematical statistics which was not considered such a big, all-embracing scientific endeavor then. He was in Göttingen and had contact, of course, with the mathematicians even though he was an economist. Also present were philosophers such as Husserl; the very interesting personality of Nelson played a very great role, and there was very much interaction between groups of mathematicians and groups of philosophers. It was really an extremely colorful and intense group of people, all in more or less close contact with each other.

After Minkowski's death, Edmund Landau was called to Göttingen. I was an assistant of Hilbert at that time, but all the deep secrets of faculty policy were discussed between Hilbert, his friends, and his assistants. He liked to get advice from his wife and from his assistants, but not from his colleagues. There was a big question: whom should one call to Göttingen as a succesor of Minkowski? Minkowski died



Andrei Nikolaevitch Kolmogorov (*1903)



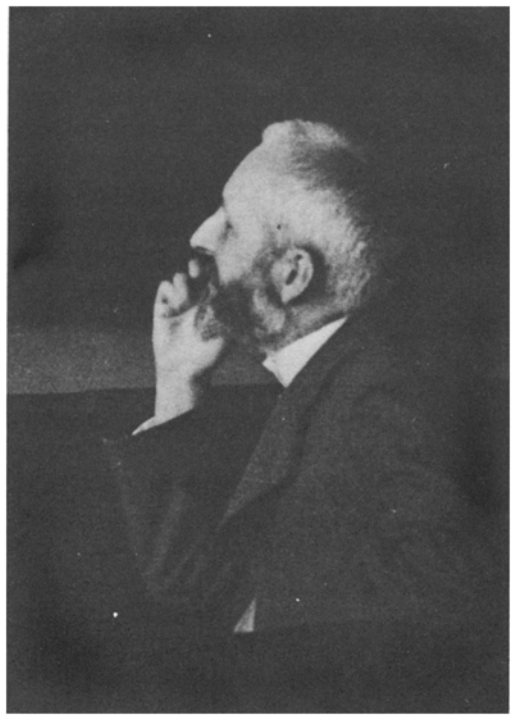
Edmund Landau (1877–1938)

very tragically, very suddenly, and unnecessarily from the consequences of appendicitis which was not diagnosed. There were three candidates – it is interesting for people today to known how responsible scientists at that time acted – three candidates of first rank: one was Perron, the second was Hurwitz, and the third was Landau. Now, Hurwitz was rather ill and didn't want to move from Zürich. The question was between Perron and Landau. All their papers and everything that they had done was carefully scrutinized, not by one, but by quite a number of competent people of the faculty. It was a toss-up, and finally the decision went for Landau. Landau was called with the very explicit justification: of the two, Perron and Landau, Landau is less agreeable and less easy to handle, and it would be very important for the faculty not to have "yes" men, people who toe the line; and that is indeed how Landau came to Göttingen, and not Perron. It was very interesting and probably a very wise principle which could be very well used in many other cases today.

Of course, I received my doctor's degree with Hilbert; I had wanted to be a physicist, but the physicists at that time were very uninspiring people, and the attraction that Hilbert and Minkowski exercised was overwhelming. Just when I came Hilbert and Minkowski decided they wanted to learn something about physics, and something had to be done. There was a new wave in physics. Einstein had just published his first papers on special relativity, and there was also a very good theoretical physicist in Göttingen, Abraham, author of the first modern textbook on electrodynamics. One wanted to study Maxwell's theory of electrodynamics. and would like to see what was its mathematical core. So Hilbert and Minkowski started a physics seminar in which everybody had to give a talk on some feature of electromagnetic theory, emphasizing the mathematical connection between concepts and proofs. This was a very fruitful enterprise, and it was the very first thing I came in contact with in Göttingen. It was the famous seminar in which Minkowski discovered the connection of Einstein's special theory of relativity and the Lorentz group. So the whole Minkowski theory originated as a seminar. From week to week it emerged before our eyes; it was extremely inspiring for everybody who participated. The contact between mathematics and physics was established for Hilbert then, even though it reached further back into the history of Hilbert's development, and it was with Hilbert to his last years.

It is very difficult in a short talk to explain what was so fascinating and inspiring about Hilbert. Hilbert was a very typical product of the good old times, older than the times that I really experienced, when the middle-class lived quietly and peacefully. It was between wars. Economically everything went very well, not only in Germany but everywhere in Europe, and Hilbert grew up in East Prussia, in Königsberg, the same place where Kant was born and lived all his life. He was the son of a higher judge and there were no prob-

Adolf Hurwitz (1859–1919)



Oskar Perron

lems, either economic or personal; there was only devotion to his intellectual pursuits, which was mostly mathematics. As a young boy he composed poems and wrote stories. That was soon forgotten. He was completely absorbed by science and became a Privatdozent in Königsberg. A Privatdozent is somebody who is allowed to lecture to students, without being paid directly. (The sociological situation of this institution is a very interesting one, but it would lead too far ... .) He was sitting in Königsberg with his wife. They had no money, but were comfortably off and could live without starving. Of course, every Privatdozent at that time had one main preoccupation; namely, keeping book about every full professor: how old they were, how well, and what were the chances that some openings would occur. I remember how Hilbert and his wife described, dramatically and completely naively, how they would sit each morning reading the newspaper at breakfast just for news about the state of health of professors of mathematics all over Europe. It was a very healthy time, so Hilbert remained in Königsberg for 12 years even though he had already achieved very major scientific successes. He read in the papers one day that a mathematician named Meyer had died, and knew that all this would start an avalanche – his place would be taken by somebody else, and that place would be open. Indeed, Hilbert came to Göttingen at that time.

In the Göttingen society, if you read old chronicles, a Göttingen professor was a demi-god and very rank-conscious – the professor, and particularly the wife of the professor. Hilbert came to Göttingen and it was very, very upsetting. Some of the older Professors' wives met and said: "Have you heard about this new mathematician who has come? He is upsetting the whole situation here. I learned that the other night he was seen in some restaurant, playing billiards in the backroom with Privatdozents." It was considered completely impossible for a full professor to lower himself to be personally friendly with younger people. But Hilbert broke this tradition completely, and this was an enormous step toward creating scientific life; young students came to his house and had tea or dinner with him. Frau Hilbert gave big, lavish dinner-parties for assistants, students, etc. Hilbert went with his students, and also everybody else who wanted to come, for hour-long hikes in the woods during which mathematics, politics, and economics were discussed. It was a very close contact. This was the way in which I later met Uryson, Alexandrov, and Kolmogorov for that matter; one day there came along some strange-looking people, Russians, that had just arrived. It was my first meeting with Alexandrov, Uryson, and Kolmogorov, and we became good friends afterwards. It was a very loose and natural personal contact.

One could also go and visit Hilbert in his garden. He spent his whole time gardening and in between gardening and little chores, he went to a long blackboard, maybe twenty feet long, covered so that also in the rain he could walk up and down, doing his mathematics in between

David Hilbert (1862–1943)

digging some flower beds. All day one could observe him, I happened to have a student room on the fifth floor from which I could look out of my window and see Hilbert in his garden. He had a bicycle and practiced little stunts on it. It was a very harmless and pleasant life, alone with colleagues and students, and very inspiring for everybody who had contact with him.

Many people did have contact with him. First, he was a uniquely inspiring lecturer. His lectures were not perfect in a formal way, and it happened quite often that he hadn't prepared quite enough, so at the end of the hour he would run out of material and had to improvise, which made him stumble and fumble. His friends and students made fun of him and gave him all kinds of ironical gifts for his birthday, to help him stretch the content. He also made mistakes and got stuck in his proofs, and so you had the chance to observe him struggling with sometimes very simple questions of mathematics, and finding his way out. This was more inspiring than a wonderfully perfect performance lecturing.

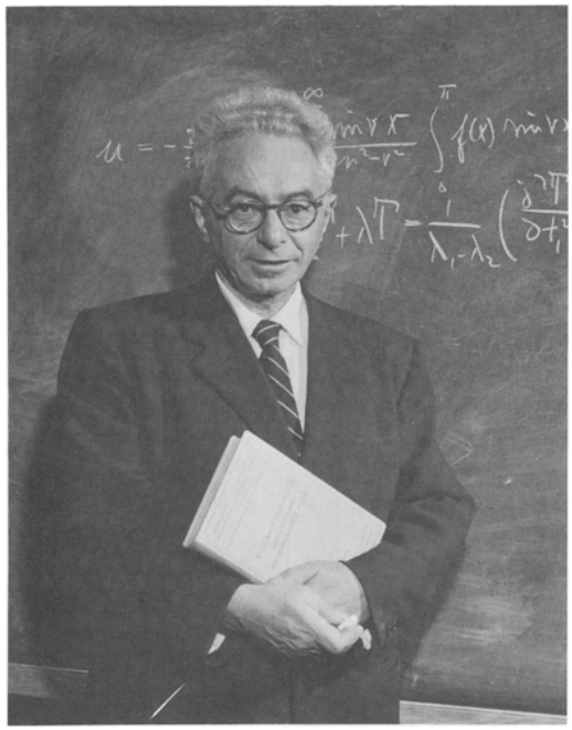
The most impressive thing (thinking about it now) is the great variety, the wide spectrum of his interests. Hilbert,

when in Königsberg, won his first acclaim for the proof of a theorem which solved an old problem with which most of the older algebraists had struggled. It was a problem of invariants; the question was how to prove that under certain circumstances there is only a finite number of independent invariants. Many mathematicians had tried to prove it and it was one of the great challenges of the time. Then, just like playing with the Columbus egg, Hilbert proved this theorem by turning the problem around. He dismissed the task of constructing this finite system of invariants, proving their existence. In fact he proved that, assuming there are infinitely many independent invariants, one can find a contradiction. This was called an existence proof and Hilbert's existence proof really disposed of the whole problem as it was posed. But it wasn't really what people had in mind, and certain people said: "Oh, but that, what Hilbert did, was not mathematics, it was theology." This reaction has remained alive in many people who maintain that such purely existential considerations, which are not constructive, should not be considered as full-valued mathematical contributions. He himself felt that one should really find a constructive proof; he never quite gave up, and finally found a constructive proof. Thus his first great achievement was in algebra.

When he came to Göttingen he turned to the theory of algebraic numbers, and the result of his enormously original and intense struggle was the famous book on algebraic numbers. It was published as a report in the Mathematical Society and has become the bible on which the development of algebraic number theory really rested afterwards. An enormous achievement! He did very many things in number theory, but he was not the kind of person who stuck to one narrow field or even a wide, broad field. He wanted to penetrate the whole spectrum of mathematical sciences.

I cannot give you a biography, or a chronology of all the things he did in mathematics. I cannot discuss them here, but every one of them merits a very detailed discussion and narration. His famous, and one of his strongest achievements, was his proof and his treatment of the so-called Dirichlet principle. In number theory his treatment of Warings problem stimulated the development of number theory in England (Hardy, Littlewood, and also Ramanujan); it was a very great tour de force which had an enormous influence on many people.

Hilbert was not a scholar in the sense that he knew everything that happened in the world. He did not read every paper nor have a little catalogue in which he could find out everything that existed. On the contrary, it was one of his strengths, but also one of his shortcomings, that he listened very carefully and caught inspiration, but then frequently forgot from where his inspiration came. There are two important instances of this. Once he was traveling in a railroad coach with some colleagues from a congress when he learned that a mathematician called F. Schur had discovered that the Euclid system of axioms was not complete, that he had added some more axioms and that the whole question of the foundations of geometry was still open. Hilbert immediately paid attention. After a few weeks he had forgotten what he heard and started studying foundations of geometry with the result that he wrote the most definitive book on the foundations of geometry, which is one of the



Richard Courant

Ivar Fredholm (1866–1927)

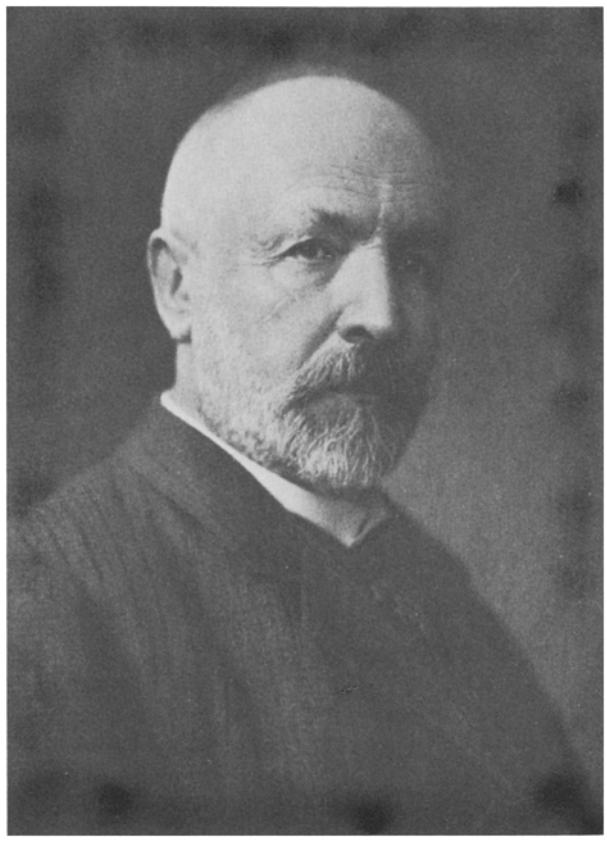
great classics of mathematics of this era. Of course, it went far beyond Schur and beyond anything anybody else had done, but when later reminded that he had heard this about Schur, Hilbert could not recall it anymore.

A similar thing happened with the theory of integral equations, also after a mathematical congress. (At this time I must admit that mathematical congresses still did make some sense. Times have changed. At such a congress, not 3,000 but maybe 200 people participated.) Hilbert learned from somebody on the railroad that a man in Sweden, Mr. Fredholm, had done something very interesting on integral equations. Hilbert was reminded of what he had learned from papers by another Swedish mathematician, Helge von Koch, and also from what Poincaré had written about infinite systems of equations. It stirred up some latent energy in Hilbert; he forgot the source of his enthusiasm very quickly and started writing his final, basic, and very important papers on integral equations. So indeed, Hilbert's theory of integral equations, one of his greatest achievements, was triggered by a bad memory, I would say.

It is quite interesting that a good memory and profound and broad knowledge can be a great impediment. Tycho Brahe knew so much and he had so many data that he could not make the discoveries which Kepler, who knew much less, could make because he did not know all the sordid details. Columbus could discover America only because he was so deeply ignorant that he didn't know that this was not the way to go to India. Everybody with some education at the time could have known that Hilbert had a little bit of this spirit of aggressive adventure in him. "Never mind what all these people have done, I will do it independently." This was very much all right, but it did create in Hilbert's students and assistants a feeling of neglect. A certain duty exists, after all, for a scientist to pay attention to others and give them credit. The Göttingen group was famous for the lack of a feeling of responsibility in this respect. We used to call this process – learning something, forgetting where you learned it, then perhaps doing it better yourself, and publishing it without quoting correctly – the process of "nostrification". This was a very important concept in the Göttingen group.

Enormously important for Hilbert all through his life was the variety in aspects of mathematics. He was a most concrete, intuitive mathematician who invented, and very consciously used, a principle; namely, if you want to solve a problem first strip the problem of everything that is not essential. Simplify it, specialize it as much as you can without sacrificing its core. Thus it becomes simple, as simple as it can be made, without losing any of its punch, and then you solve it. The generalization is a triviality which you don't have to pay too much attention to. This principle of Hilbert's proved extremely useful for him and also for others who learned it from him; unfortunately it has been forgotten.

Hilbert had an enormous respect for, and deep belief in, the need to consider concrete individual problems, and the methods and challenges they generate. On the other hand, he was completely open to the most abstract and almost theological mathematical considerations. I remember a very typical dictum of his. (My historical knowledge is not good enough, so I will mention a fictitious year.) He said, "1872 was one of the most remarkable years in mathematics. First, Cantor started his general theory of sets by discovering the non-denumerability of the real number system. That was a very great achievement. And another thing that impressed me just as much was that Otto Staude discovered the construction of the ellipsoids in space as a generalization of the construction of the ellipse with fixed points and a thread, only much more complicated, of course." Staude's discovery was a very ingenious and very wonderful piece of geometry and Hilbert deemed it just as important as the theory of sets. Probably it is; basically and philosophically this kind of mathematics is just as important. But the development of fashion has not vindicated Hilbert. If you ask a student now, maybe he will know how the ellipse can be constructed, the so-called "Fadenkonstruktion", but as for the ellipsoid I think you will get one answer out of 500 questions, and a correct answer out of 2,000 questions. It was very deeply Hilbert's conviction that this is



Georg Cantor (1845–1918)

Otto Staude

what mathematics means; you have to have these different poles in the spectrum.

Hilbert was one of the earliest and most passionate defenders of Mengentheorie, the theory of sets. I remember once when Henri Poincaré came to Göttingen shortly before his death to give a number of very interesting talks on different topics; one was propagation of electromagnetic wave around the earth, and another was on the foundations of mathematics. It was a violent attack against Cantorism and against the principle of choice and theorems such as the one about well-ordering. Zermelo had just proved the fact that every set can be well-ordered and was sitting near him at his feet. Poincaré wanted to be polite (he could be devastatingly impolite if he tried to be friendly) and he thundered against the Cantor attitude and against the trend in mathematics to do something in this direction. He said, "Even the almost ingenious proof of Mr. Zermelo has to be completely scotched and thrown out of the window." Zermelo, who was a very passionate and very strange fellow, was in despair and fury and at the dinner the same day he would have shot Poincaré if he had been a little bit more skillful, but he was a very clumsy person. But for Hilbert all of mathematics, even the most extreme theory of sets, was equally acceptable. He attacked any attempt to establish barriers, such as: "This is a far as you are allowed to go, but that you must not do", and called it a hyper-Prussian attitude. (In old Prussia you could find everywhere in parks and streets big posters saying that it is forbidden to go here or there, it is forbidden to spit on the pavement, and so on.)

Hilbert was furious that anything not criminal should be forbidden. Many of his personal fights in life came from this passionate idea that one must be allowed to follow one's own interest and one's own conviction. I will tell you in this connection a personal incident. One of the regular visitors and friends of Göttingen was L. E. J. Brouwer, the great Dutch topologist, who really for the first time broke through and brought topology into the framework of rigorous, deductive analysis. A very interesting man! He was a nationalist. I mean a nationalist in general who would be a nationalist wherever he was. After the first World War he became somehow a very great German nationalist even though he was a Dutchman. The first International Congress after the war was in Toronto; the second was in Bologna. The allied countries wanted to make a gesture of conciliation and invited the German mathematicians and asked Hilbert to be the president of the congress in Bologna. Hilbert accepted but a group of nationalists became excited, and it all became a question of honor. It was a fantastic fight. You can still read it in the old newspapers. One of the leaders in this fight against international cooperation was Brouwer. Hilbert became so furious at Brouwer: "He cannot forbid me to go to Bologna and do what I want; such nationalists we cannot use." Hilbert was very ill at the time so Harald Bohr had to write the letter to Brouwer for him, throwing Brouwer out of the editorship of the Mathematische Annalen. It was a big insult: at that time to be an editor of this distinguished journal did not mean anything, but to be thrown out as an editor, that was really something. That precipitated a war and the hostility permeated much of the mathematical life. It was all because he thought no one should forbid something that was obviously reasonable. Hilbert fought tooth and nail.

I would like to make a few more remarks about the broadness of the spectrum of Hilbert's interests. As we have already said Hilbert was very much interested in physics and his friend Arnold Sommerfeld had established the great center of theoretical physics in Europe at Munich. Hilbert said, "I must learn something about physics," (relativity theory had just started) "and I want to learn something about quantum theory, but to learn it I must have some assistant as a private teacher. These mathematicians are too abstract to explain physics." Thus he always had an assistant from Sommerfeld sent to him. The best and the most distinguished of these was Otto Stern who later got the Nobel Prize. He was not very mathematical but he understood the need for and the essence of simplification. For years he was Hilbert's physical mentor. Hilbert learned much about physics and it was his great desire to be at least aware of everything that happened. Then Niels Bohr came to Göttingen. (Hilbert could not learn anything from Niels Bohr – it was a problem in itself to communicate mutually with Niels Bohr.) Hilbert had about him the very best people. At that time Max Born and Franck had come to Göttingen and Hilbert founded a special seminar with the physicists

on the Structure of Matter. The term "structure of matter" really comes from Hilbert's seminar. Many physicists came and all the questions that pertained to the problems of quantum theory and various phases of quantum mechanics and so on were discussed at the seminar. Heisenberg was an important member, Jordan, Born, and Pauli. This was really a very heroic time of modern theoretical physics. The seminar and Hilbert's inspiring interest played a very much greater role in this than the normal art-historical physicist of today knows, or even has the slightest idea about.

Hilbert had also many other interests which he tried to follow up. There was in Göttingen a wonderful biologist and one of the early geneticists in Germany, Mr. Kühn. Hilbert persuaded Kühn to give a special lecture for interested students of theology or philology or anything, and in particular for the group around Hilbert. Hilbert was one of the most enthusiastic and regular students in this course on genetics, but of course this was shortly before the Nazis broke out and terminated all this. Hilbert also was interested in philosophy, in history, in everything. He was always willing to admit that there were some boundaries which he had not yet crossed and he tried to learn something about it. He was older than 50 years when he discovered music. By some chance, somebody gave him a good – relatively good – record-player and he bought records, without looking at them. One was by Mozart, and he noticed somehow that it was different and became interested. After half a year he was a connoisseur of classical music and went to every concert.

He was completely open, open to criticism and open to different points of view and every student. Everybody who had contact with him felt that although he was such a mental giant and such a really great force in science, one could talk to him on an equal footing – if one had something to talk about. This gave Hilbert an enormous hold on very many people.

It was interesting to observe Hilbert as a teacher. Many people wanted to get their doctor's degree with him and, of course, examinations had to be given sooner or later; this was not taken very seriously. Somebody would hand in a doctoral thesis on a subject proposed by Hilbert. If it wasn't written well, and it was easier to do things by himself rather than study what the person had written, then Hilbert handed it to some assistant. Very quickly every assistant noticed that Hilbert didn't read the thesis so they also did not read it. By this method very many theses were accepted by Hilbert, and he produced a very large number of Ph.D's, some of them with impossibly wrong and silly theses. (All these theses are collected in volumes and it is really very strange to see them.) Two girls, for example, who were at first very badly treated by Hilbert and didn't understand anything, came and visited on Sunday and brought wonderful liverwurst for the dog; that turned Hilbert's heart. It didn't make him study in detail their theses, but they got their degrees anyhow after a rather short time. Such things happened all the time.

Also, in the oral examinations it was difficult to find out whether the candidate really knew something or not. There is an old, old tenet in Roman law, "In dubio, absolvo" (when in doubt, the accused has to be acquitted). Hilbert was very often in doubt, and it was very difficult to fail an examination with Hilbert. So standards of uniform excellence in this respect did not exist. On the other hand, of course, some of the most wonderful papers came out as theses for Hilbert; for example, Erhard Schmidt's doctoral thesis is one of the gems of the mathematical literature, but that was up to the candicate. As for other standards, as I told you in the story about Brouwer and the Mathematische Annalen, Hilbert took the editorship of the Mathematische Annalen very seriously, and the violence with which he rejected papers was completely without any sympathy. As long as he and Carathéodory and such people were editors, it was a very high-ranking, maybe the highest-ranking, mathematical journal in the world, and it really meant something to have a paper printed there. This responsibility was taken very seriously. As for the responsibility of examining students, he always made fun of the French system in which a real attempt is being made to get a so-called objective judgement of the student; that did not make sense to him.

Let me make a last remark about the students. There was around Hilbert, and altogether in Göttingen, a relatively large group of students who really lived in complete dedication to the task of learning and studying, but without looking for jobs or thinking of jobs; for most of the mathematicians very little remained except to become high shool teachers. It was not quite as bad as being a high school teacher in a small midwestern or western town in the United States (allegedly, it's no longer so), but there was really very little else but the teaching profession. Nonetheless, the students were very dedicated and closely connected to each other. They had much contact with the faculty, with assistants, and with each other. They spent very much time debating scientific and philosophical matters, trying to solve the mysteries of life and to find out the reason for political, ethical, and religious principle. It was really a very wonderful atmosphere of seriousness. Nevertheless, people had a very good time, at sports and so on. It was, of course, a hard time, people lived modestly; but the point is that when the number of students increased, it was certainly no longer possible to eliminate those who didn't belong to the intellectual society; and so, gradually, class distinctions grew between those who had contact with assistants and the faculty, and an anonymous mass of people who didn't get a foot on the ground. This gradually, but rather swiftly, generated a split between different layers of academic student societies and had certainly something to do with the success of the Nazis. The disenfranchised students who studied but didn't get anywhere saw others invited to din-

ner in the houses of the professors and included in swimming parties with the assistants and felt that they "did not belong". They formed, gradually, a large body of dissatisfied, sometimes quite intelligent, elements. When the Nazis came this was a wonderful reservoir for them to avail themselves of intellectual help.

I do think this difficulty – that the university students found little fulfillment of their dreams of education and intellectual profession – had something to do with this; the single point in the development came in 1933 when suddenly, to the great surprise of the faculty and older students, in many of the classes and in the seminars and at the university institutions, students turned up – but you didn't really known them – with the insignia of the Nazi Party. It was a very great surprise, and of course, can be quite simply explained by the fact that there was this separation.

Well I do think that I have talked too long, but of course, I have said very little of substance and I have only scratched the surface of this quite interesting phenomenon that was old Göttingen. I have not said anything about applied mechanics. There was also in Göttingen the greatest of all scientists in the field of mechanics of the old era, Prandtl, a very unassuming and kind of awkward man who had the most profound influence on mechanics; the whole field of modern aerodynamics and fluid dynamics was really established by him. Nor have I mentioned the style of Landau who was the most extreme abstractionist. (Twenty years later, he would probably have been a card-carrying member of the Bourbaki society.) He wrote a famous book on the foundations of calculus, which is very admirable, but quite absurd as a book for students. He had deep contempt for everything that had anything to do with applications. Prandtl once wrote a most fascinating and interesting paper on grease and oil, long molecules, and how they function in engineering problems for lubricating. It was a very great achievement but, of course, this lubricating oil makes spots on your trousers if you are not careful. Landau was very careful in this respect. Whenever anybody talked about something that remotely reminded him of applications he would assert, "Ah! Schmieröl!" You see, this in Landau's mouth became an invective against Prandtl and the people in mechanics.

So, you see, all parties, all trends were represented in this old Göttingen, but there was no open fight between them. Friendly fights as there are in each faculty and in each group, but by and large they were constructive.

Edited for publication by John Ewing