Remembering Peter Debye in Munich

Paul P. Ewald

In commemoration of the 100th anniversary of Peter Debye's birthdate last year, I offer my recollections of working with him while I was a graduate student at Munich. When I came to Munich as a student, Debye had been Arnold Sommerfeld's assistant since the time Sommerfeld had occupied the chair of mathematical physics at the Polytechnic School in Aachen. Debye came to Aachen from his home town, Maasstricht, which lay near Aachen but in Holland, across the frontier. I don't know whether Debye lived with his parents and commuted daily or whether he lived in lodgings in Aachen. Anyway, Sommerfeld, with his keen sense of recognizing the aptitude of young students, made Debye his 'assistant' and accepted the call to the University of Munich on the conditions that he be given the small Institute of Theoretical Physics that Ludwig Boltzmann had occupied and that he could bring Debye with him as his assistant. The building for the institute, to be located behind the main university building and fronting on the Amalienstrasse, was still in the planning stage, but Sommerfeld was assigned provisional space in a large building, jutting out into the Kaufingerstrasse. This building had become state property when the former owner, the Jesuit Order, was expelled from Bavaria.

The entrance to the institute was impressive: after entering the building through wide, high doors, you had to go up very wide stairs slanting at a low angle, so that you were tempted to take every other step. At the first floor you passed a number of stuffed animals—a bear and others that had not found room inside the Institute of Zoology, which was housed on the first floor. When you came to the second floor, you had to ring to obtain entrance. The door was opened by Boltzmann's mechanic, Herr Sinz, who was the keeper of the collection of apparatus that Boltzmann had left behind when he accepted the call to Vienna some four years before Sommerfeld's arrival.

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At the time of his appointment, Sommerfeld had announced an experimental course for his students. Debye was in charge of this course. When I came to Sommerfeld after being enthused by his short course on hydrodynamics in the summer term of 1908, I asked for a practical problem. Debye thought it would be good if I produced and analyzed the electrode-less luminescence of a partly evacuated glass sphere. This luminescence is produced by a current along the equator, but outside the sphere. Among the material in the Boltzmann collection was a suitable sphere of about 70 cm diameter, and also a fair-sized induction coil, as well as a Toepler mercury pump for evacuating the sphere.

So I set to work. The Toepler pump worked slowly and I left it acting overnight. One morning, when I returned to the setup, I found the room flooded: some repair had necessitated turning off the water supply on a lower floor and, on being turned on again, water had rushed through the Toepler pump and filled half the sphere. This happened twice, and good-natured old Sinz took the glass sphere and everything else and subjected it to prolonged drying. Finally the whole experiment was again in a working condition—the induction coil and two or three turns of wire surrounding the sphere and a spectrograph focused on the interior of the sphere. It turned out that the faint luminescence produced in the sphere was not strong enough to be analyzed by the spectrometer. Besides, the long the end of my endeavor.

Some four years later I saw a belated inaugural lecture given by the newly installed professor of physics at the Polytechnic School in Munich, Jonathan Zenneck. It was a marvelous lecture, held in the large lecture theater seating 300 to 400 students. After demonstrating some other astounding experiments, Zenneck had the same type of large glass sphere—but he evacuated it with a modern Gaede pump; instead of a few turns of copper wire, he had a solid copper bar of about 2 or 3 cm^2 cross section surrounding the sphere; and instead of the induction coil, he had a resonance transformer. Zenneck turned down the general lighting of the big lecture room and closed the circuit. In a single flash the remnants of gas in the sphere lit up to blinding power, and for minutes afterward the whole big auditorium was filled with a gradually decaying light. This was the phenomenon I had attempted with my substandard means—a striking example of the meaning of technical physics.

Peter Debye in a photograph by Francis Simon. (Courtesy AIP Niels Bohr Library)

But let us return to the old Akademie. My friend Demetrious Hondros had discussed with Debye my suggestion of inaugurating a colloquium for the quicker instruction of physics students by asking them to report on recent papers. Debye took this up with Sommerfeld, who agreed in principle but would not take part so as not to frighten the students.

After the first two or three meetings, however, the format changed when Debye invited Fritz Reiche to come

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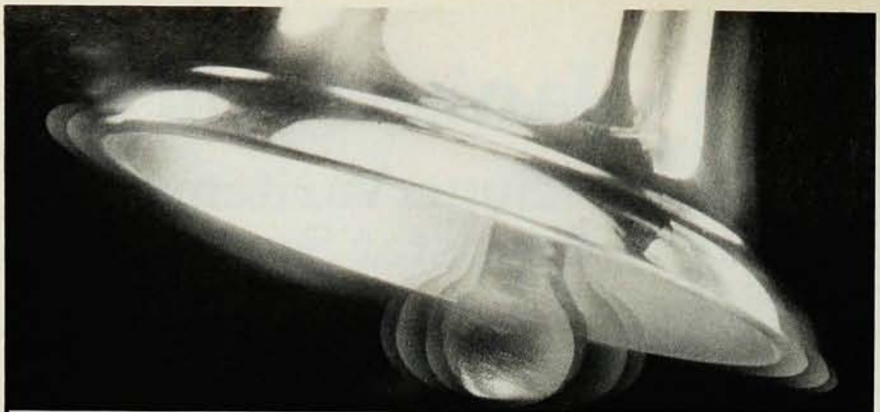

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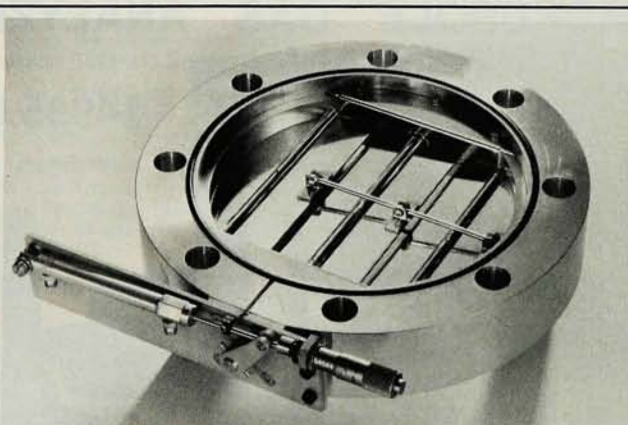

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from Berlin to tell of his work on the color sequence in the rainbow. His talk was long and formal, and he used Bessel functions, of which I knew nothing. At the end of his explanation Debye stood up, went to the blackboard and, writing down the integral representation of the Bessel functions, showed how Reiche's results could be obtained in a much clearer way. This was the first instance when Debye really impressed me. Actually, Debye had promised to write a book for the publishing company of B. G. Teubner on his study of the rainbow and related phenomena—a book that never appeared.

Debye gave me great help when I was working on my doctoral thesis on the double refraction produced in an orthorhombic crystal lattice. Sommerfeld and I discovered simultaneously, but independently, a strict method of summation for all the wavelets issuing from the atoms. But then came the much more difficult problem of determining the field exciting the oscillations of any one of the atoms. To find this, one had to subtract from the entire field the part that issued from the atom itself. This could not be done using the description of the field found by the Sommerfeld–Ewald method, because the expression of this field diverged at the place of the atom. This lack of convergence left me trying all kind of dodges in vain.

Help came from Debye during a skiing vacation in Mittenwald attended by Sommerfeld's group from Munich, Gustav Mie from Freiburg, and Willy Wien from Würzburg. We enjoyed the deep snow carpet covering the mountains during the day and supper in the evening in the primitive hotel. But the hotel had only one lamp bright enough to read by. This source of illumination was claimed in advance by those wanting to discuss problems with each other. The evening came for which Sommerfeld had reserved the light source for Debye and me. I had to explain to Debye our success in finding the total optical field and our exasperation in not being able to subtract the part of the dipole itself—the on-dipole as we called it. Debye frowned for a short while, then he said: "you have to use the integral representation of the dipole field which was given by Riemann and then you can separate this integral over theta functions by using the transformation relating the argument and its inverse. That will solve your problem." It did indeed, and, young and inexperienced as I was, in my thesis I never thanked Debye, nor, as a matter of fact, Sommerfeld, for their help. □