Walter Friedrich Unplugged: His 1963 Interview in East Berlin

Translated and annotated by

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Keywords: X-ray diffraction; Crystallography; History of Physics; History of Science

Prefatory Remarks

Walter Friedrich (1883-1968; Fig. 1) was the first person to build a device that successfully achieved the diffraction of X-rays by a crystal, and he was the first to observe the diffraction pattern produced by it. This epochal accomplishment simultaneously demonstrated the wave-like nature of X-radiation and the translational symmetry that defines crystallinity. Had Friedrich performed this study today, he might have been named a co-winner of the Nobel Prize. Instead, Friedrich's status as a postdoctoral researcher on temporary loan from Arnold Sommerfeld to a Privatdozent, Max von Laue, situated Friedrich on the lower rungs of the German academic hierarchy. Consequently, Laue was named the sole recipient of the Nobel Prize in Physics in 1914, and Friedrich's contributions seem eclipsed by the likes of the Braggs père et fils, Paul Ewald, Peter Debye, and Laue himself. As evidence, the English language Wikipedia includes an entry for *Friedrich Walter*, an Austrian hockey player of the 1940s, but nothing for Walter Friedrich, who instead is memorialized only in the German Wikipedia.

Laue always was generous in recognizing Friedrich and his other junior collaborator, Paul Knipping, Sommerfeld's graduate student at the time. Laue gave Friedrich and Knipping senior authorship of seminal papers on X-ray interference (Friedrich et al. 1912, 1913) and sole authorship of two others (Friedrich 1913a,b); he acknowledged them in his Nobel lecture; and he offered them a share of his prize winnings. Nevertheless, Friedrich's important place in history has been obscured by additional factors: Two years following the diffraction experiments, Friedrich left solid-state physics to focus on biological radiography, a field in which he made pioneering discoveries but remained outside the orbit of the physics community. Moreover, in 1947 Friedrich accepted an appointment at the University of Marburg, which was located within the Soviet sector of Berlin, and he thus became a citizen of the German Democratic Republic, further limiting his activities with western scientists. Finally, Friedrich was a modest man who habitually downplayed the importance of his creative contributions relative to those of Laue.

As the historical record shows, however, early X-ray diffraction theorists – including Laue and the Braggs -- were mired in deep misapprehensions. Even if one is not persuaded by all elements in Forman's (1969) attempt to debunk the mythology surrounding the birth of X-ray diffraction, it is clear that the premonitions of Laue and William Lawrence Bragg were well-divorced from physical reality. Laue proposed that stimulation of a crystal by an X-ray beam would induce heavy atoms to fluoresce characteristic X-rays, whose interactions with an *electron lattice*[1] would yield a diffraction pattern. That is why Friedrich's first experiments led to the placement of photographic plates to the sides rather than behind the crystal – of triclinic

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[1] This term is a direct translation of the German word *Elektronengitter* used by Friedrich in his interview. Readers will be aware that the use of the word *lattice* in this way is incorrect according to the crystallographic definition (see the discussion at https://www.iucr.org/news/newsletter/etc/articles?issue=142706&result_138339_result_page=7). It would be preferable to translate this perhaps as a *periodic array of electrons*. However, we retain it in this article rather than attempt to rewrite Friedrich's historical useage (Comment from Newsletter Editor).

chalcanthite ($Cu_2SO_4.5H_2O$), hardly the first mineral an omniscient crystallographer would choose to authenticate the diffraction process. Conversely, WL Bragg's (1912) intuition that X-rays reflect from planes of atoms remains a spectacularly useful but erroneous metaphor. Paradoxically, in his first effort to demonstrate the mirroring of X-rays by atomic planes within a muscovite wafer, he placed a photographic plate *behind* the crystal, not in the reflective Bragg-Brentano geometry so familiar to us today. Experimentation was the only path that could lead from the fog of theory towards an approximation of truth, and Friedrich's proficiency with X-ray instrumentation gained from his six years as a doctoral student with Röntgen was crucial to the success of this transformative research.

Friedrich recounted the story of the discovery in several published documents, including a decadal anniversary paper in *Naturwissenschaften* (Friedrich 1922) and a reminiscence in the same journal 27 years later (Friedrich 1949) – both in German. His impressions were not included among the personal remembrances in the semicentennial volume edited by Paul Ewald (Ewald 1962). Thomas Kuhn and colleagues spearheaded an effort in the 1960s to "find and preserve primary source materials for the study of the history of quantum physics" (Kuhn et al. 1967), and these archives include approximately 100 interviews, available online through the American Institute of Physics. Perhaps to correct the omission in Ewald (1962), Gustav Hertz, Théo Kahan, and John L. Heilbron traveled to East Berlin to interview Walter Friedrich in May 1963. Hertz (1887-1975) was a postdoctoral assistant to Heinrich Rubens at the University of Berlin in 1912; he was collaborating with James Franck on inelastic collisions of electrons in gases, for which he won the 1915 Nobel Prize in Physics. At the time of the interview, he was *ordinarius* professor at the University of Leipzig and chairman of the Physical Society of the GDR. Théodore Kahan (1904-1984) was a French physicist who authored two dozen books on radioactivity and quantum physics. John Heilbron (b. 1934) was working on his doctoral studies with Thomas Kuhn at the time of this interview. He is a Professor of History and Vice-Chancellor emeritus at UC-Berkeley, and he has won many awards for his research into the history of science, including the George Sarton Medal in 1993. He is a member of the Royal Swedish Academy of Sciences.

Both Knipping and von Laue had passed away at the time of the interview – Knipping was riding a motorcycle that was hit by a car in 1935 and Laue was driving a car that was hit by a motorcyclist in 1960. Friedrich was, therefore, able to speak without fear of ruffling feathers, but there are no salacious revelations in this 1963 interview. Indeed, those familiar with the many outstanding reviews of the X-ray diffraction revolution (e.g., Ewald 1962 and 1969; Eckert 2012; Authier 2013) will recognize much of what Friedrich has to say. Nevertheless, the interview offers genuine rewards: one senses his keen intelligence, wry humor, and self-deprecating modesty. Despite his obvious affection for Laue, Friedrich emphasizes that these discoveries were not predetermined by theorists. In addition, those of us who have advocated for a greater appreciation for the role that mineralogists played in developing notions of ordered atomicity will appreciate the affectionate description of Paul von Groth, the professor of mineralogy at the University of Munich (Heaney 2020). As a physicist in Munich in the early 1900s, Friedrich resided at one epicenter of quantum mechanics, and his research intersected with many of the luminaries who set the stage for the birth of modern physics, including Röntgen, Sommerfeld, and von Laue. These reminiscences bring these characters back to life.

Acknowledgments

This interview was conducted as part of the Archives for History of Quantum Physics (AHQP) project, and we thank Prof. Heilbron for sharing his memories of the AHQP with us. The original recording was remastered onto 10" reels and sent to the American Philosophical Society. We gratefully acknowledge the American Institute of Physics (AIP) for allowing the reproduction of this full interview, designated as: Friedrich, Walter on 1963 May 15: in German. Niels Bohr Library & Archives, American Institute of Physics. One Physics Ellipse, College Park, MD 20740. Original document, transcription, and translation are available online at https://repository.aip.org/islandora/object/nbla:266548#page/1/mode/2up. PJH acknowledges the support of a travel grant to Munich by Penn State Global Programs and NSF Grant EAR-1925903. The authors thank Prof. A.M. Glazer for his editorial handling of this contribution.

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Interview with W. Friedrich
Conducted by: Gustav Hertz, John L. Heilbron and Théo Kahan
Place: East Berlin, East Germany
Date: May 15, 1963
Tape No. 69, Side 1 (a)

WF: I was involved with X-rays already as a high school student, and I still have an invoice from the magistrate of my hometown for 8 X-rays, 28 Deutschmarks. I have a receipt that says: "W. Friedrich, high-school student." That was in 1902. I was a student in Geneva, and I actually wanted to be a musician there. I had always played the violin, and I studied with a famous violinist[2]. But my father asked: "Well, how are you going to be a great violinist?" I said, "I don't know yet." He said, "There are people in your class who play better than you!" I admitted, "Yes, there are." "So, you won't be a great violinist."

Then I resignedly returned to my old love of physics and continued my studies in Munich with Röntgen (Fig. 2).[3] I was with Röntgen for six years for my studies and as an assistant. I also completed a dissertation on X-rays with Röntgen. After finishing my doctorate, I was an assistant with Arnold Sommerfeld[4] for three years. It was something like 1909 to 1913; I don't remember the exact period. There I also met Professor Ewald[5], who I believe is now in America[6], and also Professor von Laue[7], who came from Berlin at the time and became a Privatdozent in Munich to continue working with Sommerfeld there. Ewald was working on a doctoral thesis with Sommerfeld on the subject: "The optical behavior of electro-magnetic waves when passing through an *electron lattice*." He had some difficulties in performing the mathematical derivations and asked Max Laue for assistance, and Laue helped him with it. At this time, Laue came up with the idea: If the X-rays have a wavelength as short as was assumed based on diffraction experiments with narrow slits, then they would have to give X-ray interference patterns when passing through an *electron lattice*, through a crystal. This was discussed for about a quarter of a year in the cafés, as was the custom in Munich at the time.

TK: Who was there?

WF: Ewald and Laue.

TK: You were there too?

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[2] Identified as Henri Bateau on the manuscript and Jacques Thibaud in his entry in the German Wikipedia
[3] Wilhelm Röntgen (1845-1923), Professor of physics at the University of Munich from 1900-1920, discovered X-rays in 1895 and received the first Nobel Prize in Physics in 1901.
[4] Arnold Sommerfeld (1868-1951), Professor of physics and director of the Theoretical Physics Institute at Munich, was a mentor to many Nobel laureates.
[5] Paul Peter Ewald (1888-1985), doctoral student with Sommerfeld, the developer of the dynamical theory of X-ray diffraction (including the "Ewald sphere"), and founder of the International Union of Crystallography in 1944
[6] Ewald was hired at the Polytechnic Institute of Brooklyn as head of the Physics Department in 1949 and retired in 1959.
[7] Max von Laue (1879-1960), the recipient of the 1914 Nobel Prize in Physics for the discovery of X-ray diffraction, was a Privatdozent with Sommerfeld.

WF: No, I wasn't there at all. I heard about it from their later conversations. I had a job studying X-rays with Sommerfeld. I had an X-ray machine, at that time, one of the most intense and powerful X-ray machines, and I had to do some work on particular properties of X-rays and Planck's quantum theory, okay? Sommerfeld had certain ideas about Brehmsstrahlung radiation, whether or not it was controlled by quantum processes, etc. I was there shortly after these discussions. And we talked about it [X-ray interference] for a long time, and the strange thing is that none of the people who were at the meetings, including Röntgen, and Sommerfeld -- also Mie[8] when we were skiing, and Starke[9], (and Wald and Jort)[10] -- these were the main people at the time -- they didn't think it would work. And I almost have forgotten Willy Wien[11] from Würzburg, even though he had worked with X-rays at his Institute.

And it was kind of forbidden to do an experiment. "This is not possible; it is superfluous; the thermal vibrations will prevent it [X-ray interference] from happening." No, people have forgotten, but as we now know, there were statistical problems too. There was only a certain probability that the locations and positions of atoms according to the space lattice were regular, and it seemed like a longshot, you know, that we would observe interference; as we later proved, we needed long exposure periods. And we talked about it, and I said to Laue, "Well, let's do it in the back [of the lab], let's do it in the evening," all right? [Chuckles]. And since, as I said, during the day I was busy with my work with Sommerfeld, my seminar assignments -- you know what it is like in an institute -- we brought in another student, Paul Knipping[12]. He had just finished his Ph.D. work and was writing it up and had some time.

We built a very simple apparatus, a diaphragm through which X-rays of 1 mm or 2 mm in diameter passed, a large lead box in which was locked a plate holder, and a crystal holder, which was just a tripod (Fig. 3). Since we had misconceptions about the nature of these interferences, we first thought -- and we should have known better, by the way -- that it was the characteristic radiation of the crystal that triggered the interference. That is why we examined copper sulfate, because we thought we were generating the characteristic radiation of copper. Today, we know that this radiation is not at all coherent – just as fluorescent radiation is not optically coherent. As such, no interference could have come from the copper radiation.

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[8] Gustav Mie (1868-1957), Professor of theoretical physics at the University of Greifswald from 1902-1917 who developed equations to describe scattering of electromagnetic waves by colloidal particles ("Mie scattering")

[9] Identified as [H.] Starke in the original manuscript and presumably refers to Oskar Hermann Starke, extraordinary Professor of physics at Greifswald, who discovered secondary electron emission.

[10] Added parenthetically in the original manuscript but identities are uncertain

[11] Wilhelm Wien (1864-1928), who studied black-body radiation (Wien's displacement law) and won the 1911 Nobel Prize in Physics

[12] Following his X-ray doctoral studies with Sommerfeld, Knipping worked in the Siemens Laboratory in Berlin on Coolidge type X-ray tubes. He was employed with the Kaiser-Wilhelm-Institut für Physikalische und Elektrochemie in Berlin-Dahlem. Eventually he became a Professor in X-ray physics at the Technical University in Darmstadt until his death in 1935.

Anyway, because I knew about X-rays, I thought. “Well, let’s just irradiate the crystal for ten hours.” So we irradiated for ten hours. At that time, we were still using the gas-containing tubes that had to be regenerated. We used a palladium tube, which is quite permeable to hydrogen, and the tubes had to be regenerated over and over again. We waited for ten hours. And in the evening it was around 11 p.m. that I took the plate out and put it in the developer tray, and then suddenly a black blob emerged, a black spot from the unscattered X-rays. In addition, there were ring-shaped patterns that were quite irregular, of course, because firstly, the mineral belongs to an irregular crystal system, okay, and also because it was not oriented. And next to these patterns, there was the shadow of a pitcher [Laughter].

TK: A pitcher?

WF: A mug, a Munich mug, a beer mug (Fig. 4). And how did that come about? Because the non-directional radiation, the scattered radiation, cast a shadow of a so-called screw-ring. The crystal was inserted into this screw-ring. This shadow appeared on the plate as a body, short and wide, and the handle on the ring also stuck out. Now, of course, you can imagine my great surprise. I went to Knipping in the morning, and we went to Laue and then finally to our boss and showed it to him.

TK: Your boss was Sommerfeld?

WF: Yes, Sommerfeld. We also showed it to Röntgen, etc., and they were quite surprised. Of course, we immediately tackled other things -- first of all, a regular crystal, and considerations of characteristic radiation led us to sphalerite [a variety of ZnS].

TK: For what reason?

WF: Because we thought the characteristic radiation of zinc was the radiation that would yield interference patterns. We had a jeweler cut a beautiful plate from a sphalerite crystal and grind it perpendicular to the four-fold symmetry axis, and we mounted it in the apparatus. We now had a goniometer as part of it, etc.; the instrument was now much more complicated and finished. Also, different sections of sphalerite, cut in different directions, were examined. We took another picture with a long exposure time. And behold, the beautiful four-fold symmetry was there (Fig. 5).

Now you can imagine the great joy! But here was the situation: Max Laue had intuited the phenomenon but did not yet grasp the theory. That’s why we had no idea what it would look like! We expected it would be a blurry thing, and we had set up plates in all directions and were now surprised that suddenly there were very sharp interference spots.

Now Laue sat down straight away and tried to do the right thing as an optical physicist according to the classic theory of optics. That was the difference between him and Bragg[13], isn't it true? He [Laue] developed the theory from the old theoretical formulas about interference, these empirical formulas, and in this way, he was now able to derive equations, which then contributed to the interpretation of this interference phenomenon. He found that there were different wavelengths involved. He did not specify the wavelengths directly but invoked a parameter λ/a, where a is the lattice constant. In any case, this was the presumption made by Laue. Later it was said that we planned it in this order, that his theory emerged, and then the experiments were performed. In reality, the experiments came first, and then the theory followed. That is a mistake, which may give rise to some misinterpretations of the entire history. That's how it was.

Now it was easy. Next, the threefold symmetry axis was examined, then the two-fold symmetry axis, then isomorphic crystals, also regular crystals. Then the question of thermal vibrations came up. We just made things less complicated - no cooling, etc. of the crystal - by using a diamond crystal. At that time, the theory of specific heat told us that diamond is one of those solids that, at room temperature, has the same properties that other bodies exhibit at low temperatures. So we got a diamond plate from Röntgen, which he had obtained from Ereus [the Ginsberg Company] from Hanau[14]. The experiments with this diamond plate showed that the interference was much more pronounced, not only in the direction of the beam, but also in the direction away from the beam, so that, you know, it became apparent that thermal vibrations have an effect, but that this influence is not so great because solid matter is essentially static[15]. And there's only a change in intensity –

[2 voices obscure dialog]

WF: - the change in intensity revealed the appearance of new interference spots that would not otherwise have occurred during normal exposure times. And so it is, on such occasions, you can discover something else when you are not looking for it. For example, I wanted to take a picture, or rather, we wanted to take a picture. The height of the crystal was a bit high in the goniometer because of the optical setting, and it had to be lowered so that the X-rays could pass through it. The crystal was glued to the mount with adhesive wax, and the X-rays passed through the adhesive wax instead of through the crystal, which was not intended. And what did you see on the photograph? Rings, a ring, right?

And we thought about it for a long time and finally came to the conclusion that these were either [direct images of] the large molecules that composed the adhesive wax or, more likely, that the wax is a liquid crystal; we would not call the wax a solid crystal. A physicist in Karlsruhe,

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[13] William Lawrence Bragg (1890-1971), co-winner of the 1915 Nobel Prize in Physics with his father W.H. for their transformative studies of X-ray crystallography

[14] Presumably, this refers to the Haraeus Group, an industrial chemistry company founded in 1851 that refined techniques for melting platinum and manufacturing quartz glass.

[15] Curiously, they missed the fact that diffraction from diamond went against Laue's original supposition of the need for secondary X-rays (*IUCr Editor*)

you know him better than I, [Otto] Lehmann[16], described these crystals. And we now recognize that wax is completely disordered, and as a result, the interference pattern exhibited rotational symmetry. This is the same thing that Debye[17] later achieved with his powder patterns, where the crystals adopt all possible orientations, all possible directions. Consequently, the probability that interference occurs in a certain direction is represented by rotational symmetry, i.e., a ring. Well, that was published a bit later.

TK: May I ask a question? The length of time from the – from the moment that Laue's suspicions began --

WF: Half a year.

TK: - and the first photograph? Half a year?

WF: Nobody believed that!

GH: Half a year, too strange.

WF: Of course, nobody believed that! We went skiing. They had a rough idea in, let's say something like 1911, and then in 1912, the pictures were taken. But behind the scenes, you know. It's like, if you think about it, this nice idea wouldn't have been confirmed at all if I didn't have an X-ray machine, and if I hadn't worked with X-rays, and if we hadn't done it behind the scenes, it would have gone nowhere.

TK: Undercover work! [Laughter]

WF: Then it would have been discussed, over and over, "Well, you see, it's written here who did it."

GH: What did Laue think about it? Did he think that it made no sense?

WF: No. Laue thought it had to work. From the first, he didn't accept the objections concerning thermal vibrations because of his enthusiasm for the idea. That was the main reason that we thought: "For God's sake, a lot of people have been skeptical of things and later it was done, so let's do it, we have everything here!" The X-ray machine was here, my experience in working with X-rays, and so on. As a result, it all went very well. It was done in a few weeks.

And above all, I always emphasize that it was an extremely nice collaboration, you know? I encountered some difficulties later.[18] It is true that Professor von Laue was awarded the Nobel

[16] Otto Lehmann (1855-1922), head of the Institute of Physics in Karlsruhe, performed early studies on liquid crystals.
[17] Peter Debye (1884-1966), winner of the 1936 Nobel Prize in chemistry, made major contributions to both theoretical and experimental solid-state physics, including the Debye-Scherrer powder camera mentioned here.
[18] Perhaps this statement refers to the letters from Stark

Prize for it. He shared it with us[19]; of course, Stockholm did not recognize us, but he gave us credit. I received letters from Stark[20], you know: "Don't put up with that, etc." Yes, yes, yes. I have always taken the stance that I wouldn't have done it without the idea! I made the discovery, but I wouldn't have done it on my own. As a result, there was never any kind of animosity between us. On the contrary, we were friends until his death (Fig. 6).

TK: You didn't have any difficulty with Knipping, did you?

WF: No, no.

TK: I was told a great story that he became very angry and destroyed the equipment. Did such a thing happen?

WF: Why would he? He later passed his exam and was here at A.E.G.[21] He worked here, and then he came to Darmstadt, where he established a small institute. He was hit by a car and didn't live long. It's strange, isn't it, that all the people who worked there were involved in an accident. Sommerfeld was hit by a car, Laue was hit by a car, or at least had a car accident. The only thing missing was that I… [Chuckle]

JH [in English]: Can Professor Friedrich tell us a bit about the history of the quantum during his time in Munich? How Sommerfeld got interested in the quantum and so on?

TK [Translating]: Colleague Heilbron would like to know if you can tell us something about the quantum hypothesis in Munich, about the changing views on quantum theory.

WF: We know, of course, in Sommerfeld's Institute that Sommerfeld was very busy with quantum theory, and, as I said earlier, the experimental work that I started in his Institute was directed towards quantum theory, just as my Ph.D. thesis was. My doctoral thesis was focused on measuring the azimuthal intensity distribution of X-rays. [Illustrating with his hands]. So when the cathode rays come from here, and this is the anode, right, there is a particular distribution, which is called the azimuthal distribution. And this distribution depends on the speed of the cathode rays. This is not a symmetrical distribution, but it goes in the direction of the cathode rays, so to speak, where the maximum is. And if not, if this were an asymmetrical distribution, it would have to be assumed. And that's what Sommerfeld had already started thinking about, and he wanted to have this verified by a new study, where you could separate and then measure this intensity distribution so that one would have a clean image of the Bremsstrahlung radiation.

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[19] Laue shared the financial portion of the Prize with Friedrich and Knipping.

[20] Spelled without the "e", this likely refers to Johannes Stark, a Professor of physics at Aachen University from 1909 to 1917 whose work on the shifting and splitting of spectral lines of atoms and molecules due to the presence of an external electric field earned him the Nobel Prize for Physics in 1919. His staunch Nazism resulted in a four-year suspended sentence for crimes against humanity following World War II.

[21] Allgemeine Elektricitäts-Gesellschaft, the General Electric of Germany founded in 1883

GH: When was the law for the short-wave limit actually found?

WF: It was found later using X-ray spectroscopy by the Braggs, wasn't it? Röntgen, for example, didn't understand much of Laue's theory, you know, because Röntgen was an experimental physicist, and when he saw the integration of such and such, you can imagine that a theory developed purely on the basis of theoretical physics was difficult for him. But when he heard Sir William Bragg's perspective on the theory, akin to how the colors from thin films appear, how the coloration on the surface of a drop of oil is caused, that was immediately clear to him. That the crystal is built up like a book from layers, and when the X-rays hit the surface, a few are reflected from the top layer, and then a few from a little bit further down, giving rise to certain path differences and interference effects: that was much more vivid for him. And now, based on their theory, the Braggs built a real X-ray spectrograph, and they found the diffraction limit. That limit, of course, has been a pillar of quantum theory, you know, because it was related to v^2 of the electron.

GH: I think the gentleman also wanted to know whether you remember the initial discussion about the Bohr model, how it became accepted, whether you remember something about this among the Munich circle of scientists. It is always very difficult to remember this afterward.

WF: Yes, yes, it is always very difficult. It was clear, of course, that Bohr's work, in particular, was very well received by us, that there was a lot of discussion in the colloquia as well, especially in the Sommerfeld seminar -- a seminar that was more like a colloquium because it was not so much about teaching material, but about new views and new work, etc. There was a lot of discussion about it, but as much as I can remember, things were a bit -- well, how can I say -- well, now I am supposed to find the right expression - so insufficiently theoretically founded, that it was a kind of fantasy, first of all, a fantasy structure that, in his mind, was perhaps the answer to the question that this model posed, but it wasn't really properly founded. In this regard, Sommerfeld was much too precise a theoretical physicist, who was likely wondering: "How is this actually theoretically sound?" But it was initially only a model, one perspective before the matter was further substantiated.

TK: When did you leave Munich, please?

WF: I left Munich for a strange reason because I then degenerated into medicine.

TK: Which year?

WF: 1914.

TK: You had already left Munich.

WF: I left Munich with the permission of Sommerfeld, initially for a year, because I had a lot of contact with medical professionals, and I heard from them that when using physical forms of energy, like X-rays, light rays, etc., they actually knew nothing about the basis of the effect, even the physical basics, the biological basics. And then I went to Freiburg. There at the women's clinic was a gynecologist, a gynecologist whose name was [Bernhard] Krönig[22], and he was very interested in this matter. And if you considered it, you know, so what do the people who use X-rays actually know of the effects, of the properties of X-rays? An engineer from the factory from which you bought your X-ray machine came and instructed you a bit. But if you know how to work scientifically and you delved into it, you could see a new field of biophysics that was ready to blossom. So we then together wrote a book on the biological and physical-biological basis of radiation treatment. These were all our own experiments, where all the properties and laws were to be found. And that has actually been the basis of bio-physics (Fig. 7).

Later in Germany, a former industrialist, Dessauer[23], championed this field; he was a great diplomat. Do you know what he did? I wouldn't have had that idea. He was a member of the Reichstag in the Zentrum Party and, at the same time, a member of the city council of Frankfurt. I knew him personally for a long time, and we were very closely acquainted with each other, and because he pondered a lot of scientific questions, he said: "Frankfurt must have a university!" He founded a university!

GH: Oh, he did?

WF: He did so with the help of the Reichstag, okay, where he was a Zentrum delegate, and also with the help of the magistrate. They founded a university in Frankfurt, and he was called a Professor of medical physics. That was how he went from factory director to Professor. [Chuckle]. But then he had an assistant, you all know him, Rajewsky, [Boris] Rajewsky[24], who is now in Frankfurt, was an assistant to Dessauer.

TK: May I ask another question? Who attended the Sommerfeld seminar, the Sommerfeld colloquium? Who were the physicists? Were there experimental physicists there?

WF: Yes, experimental physicists were also there, although very few, mostly because experimental physicists lacked the mathematical tools. Back then, it was not the way it is now. Incidentally, Sommerfeld was the first to push his students into theoretical physics, because before that was Leo Graetz[25] and -- what were their names -- oh God, they treated theoretical physics with all the variables in the old way, wrote formulas that not only covered the whole

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[22] Bernhard Krönig (1863-1917) was a Professor of Gynaecology at the Freiburg University Clinic, who with Friedrich pioneered the exploration of the effects of ionizing radiation on human tissue.

[23] Friedrich Dessauer (1881-1963) studied physics at the Technische Universität Darmstadt and the University of Munich, with a focus on the medical applications of X-rays. His face was severely disfigured from radiation damage, and he was imprisoned in the 1930s for his opposition to Hitler.

[24] Boris Rajewsky (1893-1974) was a Rector at the Goethe University in Frankfurt and was highly honored for his studies of the effects of radiation on organisms.

[25] Leo Graetz (1856-1941) was a Professor of physics at the University of Munich who studied the propagation of electromagnetic energy.

blackboard but needed several rows of blackboards. The first one who actually introduced modern theories of mathematical analysis was Sommerfeld.

TK: Debye was already there?

WF: Debye was my predecessor as an assistant.

GH: When I was a student with Sommerfeld, Debye was an assistant.

WF: Yes, yes, he was my predecessor, and Debye came to Aachen at the time. He was already habilitated and came to Aachen, and I became the successor and then came Ewald. Ewald was in Göttingen and then for a while in England. Then he came back to Berlin and Munich and then went back to England. My colleague was Lenz, the theoretical physicist from Hamburg, Wilhelm Lenz[26], who mainly had electrodynamics as a major subject. But Sommerfeld's first lectures, the very first simple, descriptive working methods that he taught the students so that they could employ them for theorizing -- vector analysis, for example -- were first delivered in Munich by Sommerfeld. And of course, it was problems such as those in analytical mechanics, where everything became very clear from the use of vectors, etc.

JH [in English]: It's most interesting that von Laue first heard of the crystal model from Ewald, and I wonder whether the lattice model was better known to the experimentalists?...

GH: The gentleman means whether experimental physicists were actually aware of the concept of a crystal lattice before the discovery?[27]

WF: No, no. We had a friend, old [Paul Heinrich Ritter] von Groth[28], who was the mineralogist, the crystallographer. The physicists always liked him because he was an extremely nice and friendly man, and his field was always used as an exam subject. The physicists were examined in mathematics, theoretical physics, experimental physics, and another subject. You could take chemistry or something else, and we always took mineralogy, because when we went to the collection of crystals and minerals, there on Neuhauser-Strasse in Munich, you met old Groth. He said to you: "Well, young friend, you are also interested in these things," and then he handed his book over to you, and when he heard that you took his subject, mineralogy, as a minor, he said: "Well, you don't know that much about mineralogy. If you are a little bit interested in crystals and individual classes, I will ask you about that." [Chuckle]. And that's why Groth himself was our advisor afterwards in the choice of the other crystals that we examined.

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[26] Wilhelm Lenz (1888-1957) rose to Professorships in physics at the Universities of Munich (1920), Rostock (1921), and finally Hamburg (1921-1956) and is recognized today as the originator of the Ising model.

[27] The notion that crystals embodied atoms or molecules organized by a space lattice was widely shared among mineralogists of the time. von Groth's own 1906 text approvingly cites the application of early space group theory to crystals by Leonard Sohncke, Professor of physics at the Technical University of Munich from 1886 to 1897.

[28] von Groth (1843-1927) was one of the most pre-eminent mineralogists of the day, having founded the journal *Zeitschrift für Krystallographie und Mineralogie* and developing a five-volume systematic mineral classification.

TK: He was a professor of mineralogy at the university?

WF: At the University of Munich, yes.

[Gustav Hertz leaves.]

TK: I want to ask a bibliographical question. As his doctoral thesis, Ewald asked Sommerfeld about the passage of electromagnetic waves in crystals, etc.

WF: In *electron lattices*.

TK: In *electron lattices* . Where did this dissertation appear?

WF: It was published in Munich, so it must have been printed in Munich.

TK: It was before the war, I think, and only an abridged version was published in the *Annals of Physics*. Where is the entire dissertation located?

WF: It has to be in Munich, in the archive, because he had to hand in several copies.

TK: When did he submit this dissertation?

WF: I don't remember that anymore. Well, it must have been between 1912 and 1914, I don't know anymore, around that time. If you just write to the Munich University Archives, you will receive that information.

TK: A very beautiful work.

WF: It's strange, isn't it, that Laue suddenly came up with the idea. Of course, he also dealt with X-rays, theoretically, didn't he, because they had a lot of analogies with optics because of how they originated and the Bremsstrahlung and natural radiation, etc. So of course he knew something about X-rays and the unsuccessful attempts to observe X-ray diffraction experimentally, as was done by Haga and Wind[29], who had done experiments with such narrow slits, that they could only ever propose an upper limit [for the X-ray wavelength]. If X-rays have a wave-like character, then the wavelength must be at most 10^{-9} cm.

JH [in English]: Was there any feeling at all at Munich in favor of the elder Bragg[30]'s particle ideas for X-rays?

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[29] Hermanus Haga (1852-1936) was a Professor of physics at the University of Groningen from 1886 to 1921, and he collaborated with Cornelis Wind (1867-1911) to determine the wavelengths of X-rays by passing radiation through a V-shaped slit that was 27 μm at the top and close to 0 μm at the base. The broadening of the detected intensities from the narrow portion of the slit was interpreted as evidence for X-ray diffraction.

[30] William Henry Bragg (1862-1942) developed some of the first effective X-ray spectrometers and firmly believed in a particle-like nature of X-rays until his son's experiments convinced him otherwise.

TK [Translating]: What did the physicists in Munich think of the Bragg corpuscular hypothesis?

WF: You didn't believe it, first of all, because some things were so terribly difficult to understand with this theory. Some properties of X-rays, for example, the energy of the electron that occurs when a -- . [Cigarettes offered and refused]. No, this theory was not correct. I have said this in my speeches about the discovery, and I have said so many times, not only in Munich but also before.

TK: Have your lectures on the history of the discovery appeared anywhere?

WF: Yes, they have appeared. That's when I made the point that it's a little bit humorous to hear that all the bosses didn't believe in crystal diffraction, and the student did it at night, you know? [Chuckle]. But it's actually the case that these things would not have been discovered at that time because the experiments were forbidden to be attempted by the bosses. Who was going to do it?

TK: Sommerfeld was also very negative?

WF: He was negative. Everyone was negative, and even Röntgen didn't believe it.

JH: Because of thermal vibrations.

WF: Yes, yes. You thought about the thermal vibrations, the atomic motions, they always move, there's no regularity.

JH: Did Epstein[31] play any part in those discussions?

WF: Epstein was a guest at the university at the time.

TK: He didn't take part in the discussions?

WF: Yes, yes, yes. Afterwards he went to America, Epstein did. In my opinion, he was a very clever person, Epstein. I don't know, wasn't he in Los Angeles?

JH: Pasadena.

TK: Do you have any memories of people who attended the Sommerfeld seminar between 1910 and 1912? So it was you and Ewald, yes? Laue was there too?

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[31] Paul Sophus Epstein (1883-1966) was a Russian-born mathematician and physicist who worked with Sommerfeld in Munich before ultimately becoming a Professor at Caltech. A member of the US National Academy of Sciences, he explained the Stark effect using quantum theory.

WF: Laue was there too, then Lenz, Epstein - wait, who else? That was a long time ago.

TK: Röntgen was there regularly?

WF: Yes, he seldom came. Röntgen did not want to hear much about the - let's say - exclusively mathematical physics, for the simple reason that he knew the theories, the foundations of the physical phenomena, the laws, quite well, but he didn't have the mathematical tools, you know. So he didn't understand it if you were using vectors, or possibly tensors – these things we thought were really great -- or matrices, right, he didn't understand that at all—[Chuckle].

TK: But he was familiar with Maxwell's theory.

WF: It was known to him, yes, yes, of course.

TK: Boltzmann[32] was in Munich, wasn't he? When was that? 1902?

WF: Yes, in 1902, I never knew him at all.

TK: Were a few Boltzmann traditions left behind?

WF: Only the armchair!

[Laughter]

TK: The style of the lectures, etc., was not maintained at all?

WF: No, I think that happened in Vienna later.

TK: And his assistants at that time, they were not kept?

WF: No, no. The chair was empty for a long time. Sommerfeld was called only because of Röntgen. Röntgen was of the opinion that a chair for modern theoretical physics was required. We absolutely needed it because the other physicists who were there, such as Leo Graetz -- I don't know if you are familiar with his book about physics and electricity, which was very nicely written, but of course old-fashioned. Röntgen had Sommerfeld fetched because he particularly valued Sommerfeld's theoretical work since it dealt with current problems in physics at the time, including quantum theory and related fields.

JH [in English]: Do you know what Röntgen's attitude towards the quantum theory was?

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[32] Ludwig Boltzmann (1844-1906), the father of statistical mechanics, was Chair of Theoretical Physics at the University of Munich from 1890-1894, at which point he moved to the University of Vienna, so the recollections are amiss here.

TK [Translating]: What was Röntgen's opinion regarding quantum theory?

WF: Oh, he supported it. I'm sure he supported it. Röntgen supported all theories that, let's say, had some prospect of realization, so that one could, so to speak, use the concept to explain physical behaviors. That impressed him. He said: "All attempts to make a theory that can explain a phenomenon, I appreciate that, even if I don't understand the mathematical context completely."

TK: Were Röntgen and Sommerfeld friends?

WF: Yes, yes, they were friends. Röntgen was much older.

TK: When did he die?

WF: He died when he was 76 years old, when might that have been? I gave a memorial for him.

TK: It was during the war, no?

WF: No, not during the war. Wait, when did Röntgen die?[33] I really can't say. I gave a commemorative speech for him in Munich. I do not know.

TK: One more question: Did Sommerfeld read about thermodynamics?

WF: Yes, yes, yes. He was familiar with the whole field of theoretical physics.

TK: Did he also read about Maxwell's theory?

WF: Yes, yes, and thermodynamics, and optics etc. Above all, he taught his students using (analytical) mechanics so that they learned how to treat physical problems because he always had the point of view: "What I want to show you in my lecture is not how to differentiate or how to integrate or solve a differential equation, but how to take the proper approach." He said: "What use is it if you can differentiate and integrate, but if you don't know how to treat a falling stone? How do I deal with the case laws theoretically if I don't know how to treat them? Which approach do I take?" You know that's how it was in his seminars: The approach was the important part. That's what he demanded of his students, who had already had the mathematical lectures so that they could differentiate and integrate. But they mainly learned to approach a physical problem, and that's why he started with mechanics.

TK: ...Please, were Sommerfeld's lectures published?

WF: No, no, never. He wrote a few nice books.

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[33] Röntgen died in 1923, one-and-a-half months shy of his 78th birthday.

TK: But later. Don’t you have any notes or records, with his lectures?

WF: No. When Sommerfeld arrived, I was almost finished.

JH: What were the relations between Sommerfeld and Debye?

WF: The relationship between Sommerfeld and Debye was very good, actually. He liked Debye very much. He encouraged him a lot. He brought him to Holland and then to Zurich.

TK: Aachen too.

WF: Aachen first. Yes, I was pleased that Peter Debye was there last year at the 50th-anniversary celebration of X-ray diffraction. Yes, we greeted each other happily—[Chuckle]. We had a very nice life together in Munich. As is customary, you go to a café after lunch, which is similar to Vienna. And we all met in the café, and there were, especially in the summer, tables that were made of stone – marble -- and once the physicists left after drinking their coffee, the whole table was painted with formulas and drawings and figures, etc. That was very nice. And then in the evening, after the colloquium, there was something else, we played skittles. We ate together, and we played skittles with Sommerfeld, you know, all physicists. That was a nice time. And also with the older students, who were working on their doctoral theses.

It was very difficult to do a doctorate with Röntgen. He gave very complicated assignments. Now, I’ll tell you the task Paul Knipping was given for a doctoral thesis, the elastic response of diamond at high pressures and low temperatures. A diamond crystal was loaded, and the strain was measured with an interference device, interference bands, you know? And then the pressure was released, and the bands moved back but not all the way back. So that was the elastic response. The whole apparatus was in a steel vat, and a pressure of 300 atmospheres was pumped in, and the whole thing was immersed in liquid air. Now do the experimental setup as a student. [Laughter]. He spent four years on his doctoral thesis.

I also did work using X-rays. Yes, you had to make the X-ray tube yourself. So you went to the glassblower and got it done, and you had to make the pump and do everything yourself.

TK: What were the relationships like between Sommerfeld and his students? You just said that Ewald was having trouble with his doctoral thesis, and that he was very happy to discuss his issues with Laue. What was the relationship between Sommerfeld and his students? When Sommerfeld assigned a task to one of his students –

WF: Then the student didn’t want to appear stupid, and as a result, he asked others for help. [Laughter]. That was the reason, okay, that he didn’t go to Sommerfeld. Otherwise, Sommerfeld would have said: “You don’t know that?!” So the student went to Laue.

JH: Later, Sommerfeld had a reputation for talking at great length with his students.

WF: He did, sure, sure. But when he gave you a job to do, he'd say, "You can do it." But if you couldn't, then you didn't go to Sommerfeld but to someone else, with a little diplomacy. [Laughter].

Röntgen was also a very strict gentleman. Oh! He gave me 30 glass plates one evening and said: "Mr. Friedrich, would you measure the thickness of these 30 glass plates tonight?" With an interferometer, you could measure it to the nearest micron. I measured 25 and thought I would measure the last five the next morning. So I arrived at my laboratory at 8 a.m., and His Excellency Prof. Röntgen is already sitting there. [Chuckle]. He is already sitting there, and he says: "Well, Mr. Friedrich, have you measured my plates?" [GH returns, Friedrich repeats the anecdote up to here.] I said: "I measured twenty-five last night, and now I want to measure the last five." "Well, how late did stay up measuring?" I replied, "Well, until midnight." And then he said: "Well, and what did you do then?" [Laughter]. That's how it was!

Or, during the Carnival period -- Munich celebrates the Carnival - you know what that is, Carnival?[34] - and so on Ash Wednesday I didn't arrive at the Institute at eight in the morning, and as a result, he promptly sent his mechanic to find out where I was. And there I am - I was still in bed - so I quickly got dressed on Wednesday morning at half-past eight and went to work. And he said: "I thought the Carnival was over! Why didn't you come to work? We can't have these lazy dogs!" [Laughter]. He said to me, "Well, next time—"

[END OF INTERVIEW]

[34] The Christian Carnival celebration in Germany begins with the feast of the Epiphany (Three Kings Day) and ends on Shrove Tuesday, the day before Ash Wednesday.

Figure 1 -- Walter Friedrich in 1962.
[From German Wikipedia:
https://de.wikipedia.org/wiki/Walter_Friedrich_(Biophysiker)#/media/Datei:Bundesarchiv_Bild_183-A0919-0015-001,_Walter_Friedrich.jpg]

Figure 2 -- Wilhelm Conrad Röntgen in 1900.
[From https://commons.wikimedia.org/wiki/File:Roentgen2.jpg]

Figure 3 -- Friedrich's experimental apparatus for the first X-ray diffraction experiment. Copyright: Deutsches Museum in Munich

Figure 4 -- The first diffraction pattern of chalcanthite, taken by Friedrich and Knipping and signed by Laue, in the shape of a Munich beer mug.

Figure 5 -- The first published X-ray diffraction pattern along the 4-fold axis of sphalerite (Friedrich et al. 1913a). Open access article from *Annalen der Physik*

Figure 6 -- Max von Laue. From Wikipedia: https://en.wikipedia.org/wiki/Max_von_Laue

Figure 7 -- Bronze bust of Walter Friedrich cast in 1964 by Maria Shockel-Rostowskaja, located on the biomedical Campus Berlin-Buch GmbH. From German Wikipedia: https://de.wikipedia.org/wiki/Walter_Friedrich_(Biophysiker)#/media/Datei:Maria_Schockel-Rostowskaja_-_B%C3%BCste_Walter_Friedrich.jpg