PART V

In Memoriam

Contents

My Development as a Physicist

AN AUTOBIOGRAPHY

*by Max von Laue**
(1879-1960)

It would seem that my essentially bookish nature was noticed by perceptive adults at an early age. In any case, when I was nine or ten years old, my grandfather, Theodor Zerrenner, who loved me dearly, made me a Christmas present of the comprehensive ten volume edition of Brehm's *Tierleben.* I remember looking at the beautiful illustrations many times and even today I retain a certain general knowledge of the principal divisions of the animal kingdom. This preoccupation, however, came at a time when a boy does not think of his future profession and I never felt any inclination to study biology. Later on my dear Mother may have mentioned Law as a profession, but that too did not last long for other things began to occupy my attention.

It happened in the Tertia of the Wilhelms Gymnasium† in Berlin—we had moved recently to this town (1891) because my father had been transferred there from Posen—that I heard at school, I cannot remember in what context, of the deposition of copper from copper sulfate solution by an electric current. The impression which this first contact with physics made on me was overwhelming. For several days I went about lost in thought and totally useless, so that my worried Mother asked me whether I was ailing. After finding out the cause, she saw to it that I visited the 'Urania' now and then. This was a society to popularize science, in whose house in the Taubenstrasse a large collection of physics apparatus was on show,

* Translated by P. P. Ewald and R. Bethe. The footnotes are those of the translators.— The German original has been re-published in Max von Laue, *Gesammelte Schriften und Vorträge,* Bd. III, pg. V–XXXIV, Friedr. Vieweg & Sohn, Braunschweig 1961. The two orations following here on the autobiography proper formed part of the original publication arranged by Laue in the book by Hans Hartmann, *Schöpfer des neuen Weltbildes* pg. 178–207, Athenäum Verlag, Bonn, 1952.

† The Gymnasium is the classical German secondary school, offering 9 years of Latin and 6 of Greek. Pupils in the 'Tertia' are 12 to 14 years old.

ready for simple experiments: following the printed explanation one had only to press a button in order to see some instructive demonstration. Only very much later, at the Physics Meeting in Koenigsberg in 1930, did I learn that I owed this impressive kind of instruction to Eugen Goldstein, the discoverer of Canal-rays; it was he who had arranged the display of demonstrations. I also listened to several of the lectures given in the 'theatre' of the 'Urania' and visited the astronomical observatory of the society in the Invalidenstrasse—but of these I do not remember any details. When I myself gave a talk in the 'Urania' in 1913 or '14 I recalled the old time affectionately.

We remained in Berlin only one and a quarter years. Then another transfer of my father took us to Strassburg in the Alsace. There I went to the famous 'Protestantische Gymnasium', and had indeed no reason to complain of the change. This institution, belonging to the Alsatian Church of the Augsburg Confession, was headed by a most understanding and well-meaning director, H. Veil, who knew that each individual has the right to follow his own law of development and who, spite of being a classical scholar with strong theological leanings, fully acknowledged the claims of mathematics and science, and more than once sided with me against teachers who would occasionally take offense at my mathematical and scientific bent. Long after leaving school I paid visits to this man, the last time at his place of retirement in Jena, where he died in 1938, high in his eighties.

The teaching faculty, as far as Veil had been able to pick it, also deserved on the whole high praise. There were, it is true, some old members, left over from the time before 1870, who could neither adapt themselves to the new political situation, nor change to another job. They went about rather embittered and were also, therefore, without influence on the students. For them, too, Veil tried to ease the situation, for example by excusing any teacher for whom it meant a conflict of conscience from giving the oration at the official celebration of the Kaiser's birthday. Veil never tolerated political oppression at his school and much less inflicted it.

Among the teachers I remember Professor Erdmann, a theologian, who, besides religious instruction, taught classical languages and German. He was perhaps the wisest of all my instructors. Two of his statements I recall to this day: In the upper forms the subject matter in German was, naturally, History of Literature. Erdmann, who liked to stray slightly from the prescribed course material, came to talk about Richard Wagner and the Ring of the Nibelungen. The 'Siegfried Ethics' of this musical drama threw him into a frenzy of excitement and he predicted terrible consequences if this should ever rule the conduct of man.—His other saying was: 'He who is burning for a great cause will never quite perish.' This I have found true in my own life.

Why do I report all this here where I deal with my development as a physicist? Because this development cannot be separated from my general mental development, least of all in those years when a boy changes into a youth and the foundations for all his later maturing are laid. I doubt also that I would ever have devoted myself completely to pure science had I not acquired at that time that feeling of intimate connection with the language and culture of the Greeks which the humanistic gymnasium and no other type of school produces. The joy of pure knowledge is, with few exceptions, after all acquired only from the Greek classics. If one attempts nowadays to train a scientific generation on a larger scale than in previous decades I have one formula to offer: send the boys to a humanistic gymnasium and let them have a good training in the Classics. And of course teach them also to read and to love the German classical authors. Ludwig Boltzmann, the great Viennese physicist who died in 1906, writes in the preface to his *Popular Essays* (1905): 'Without Schiller there might well exist a man with my nose and beard, but never myself.' This I endorse word for word.

I might mention in this connection that many of the boys who received their secondary schooling under Veil eventually became university teachers. Besides myself there were for instance: the historians Wolfgang Windelband, and Robert Holtzmann (both in Berlin), the physicians Friedrich Holtzmann (Karlsruhe) and Wolfgang H. Veil (Jena), the Lawyer Eduard Kohlrausch (Berlin), the chemist, Walter Madelung (Freiburg/Br.), and the physicists Erwin Madelung and Marianus Czerny (both in Frankfurt). But let me return to my subject.

Among all my teachers the one of greatest importance for me was Professor Göhring who taught mathematics and physics. He was an old bachelor with a thousand idiosyncrasies which would have made any other teacher impossible in front of a class of schoolboys. But in his case they were more than balanced by his mental superiority which fascinated even the worst rowdy in the class—and we were really not model pupils. To play tricks in front of him, impossible! Nobody ever even though of it. For that reason he never had to punish, rarely even to scold. The utmost I remember him saying were these words, addressed to a boy who had made himself a real nuisance: 'Huber grins; yes, Huber grins.' Huber did not grin again for a long time. Youth has a fine appreciation for intellectual greatness and is awed by it—provided it genuinely exists.

From this man I learnt my first mathematics, and I may say that I soon became one of his best students. We were taught the usual high school mathematics—algebra including quadratic equations, geometry according to Euclid and other ancient geometers, logarithms and trigonometry, etc. Much of it I have long since forgotten, and I would have to work up afresh the properties of the Appollonian circle if I needed them. But at this stage it is less the factual knowledge that is important, than the facility one acquires for scientific reasoning. 'Education is what remains after all school learning is forgotten' * is an often quoted saying of I don't know which great man. It was in this spirit that Göhring taught us. He soon recognized my mathematical gift and prodded me on with occasional praise.

My inability to do numerical work formed a strong contrast to my mathematical achievement. This went so far that when in the final school examination a number of mathematical problems had to be solved in a written class exam, the teacher,—it was no longer Göhring—referring to a trigonometry problem with numerical work, said: 'don't even try to do this one, you'd get the wrong answer anyway.' Only at the university and when I needed it for physics did I learn to do numerical work and by conscious attention to it acquired some dependability.

Under Göhring I also studied physics for the first time seriously. I do not think that the few experiments he could show with the inadequate facilities of the Gymnasium were nearly so significant, as was his ability to convey scientific thinking to his students. Besides, it was important that he referred us to suitable books. On his advice I acquired (in October 1896, as a note in the books indicates) the *Vorträge und Reden* by Helmholtz, and I still hear him recommending these two volumes to us in his Thuringian accent which knows no hard consonants: 'These are popular essays, but for a populus that isn't a bit dumb.' I studied these essays and discourses with the greatest avidity, read and re-read what I did not understand the first time, and I widened my horizon considerably through them and later through Helmholtz' *Lehre von der Tonempfindung*. Are not these both in form and content classics of physics literature? I did, however, not limit my reading of the lectures to those on physics. I also read the academic

* 'Bildung ist das was übrig bleibt, wenn man alles Gelernte vergessen hat.'

discourses and revelled in the thought of later becoming a university student. My favorite piece, however, was the magnificent autobiography which Helmholtz gave as a banquet speech on the occasion of his seventieth birthday. I even tried reading his philosophical talks—but with minor success.

In spite of all this, the years in the Secunda and Prima of the Gymnasium would not have had nearly so decisive an influence, had I not found among my schoolmates two of similar inclinations, Otto B. and Hermann F. We were drawn together mainly by our common interests, and were soon known throughout the school as the Mathematical Trefoil. One of us, Otto B., came from the highly intellectual family of a professor at the University of Strassburg. I saw very little of his parents or of his older brothers and sisters. But Otto B. had the extraordinary gift of fully absorbing what he heard from them, and, being interested himself, to pass it on in an interesting way. Also, he read much and with full understanding in the mathematical and scientific books which he found in his home, and he knew how to stimulate us to similar reading. In this way the three of us, by ourselves and with a kind of stealth, got acquainted with differential and integral calculus, browsed in the several volumes of Wüllner's *Lehrbuch der Physik*, and began experimenting on our own as boys will do. The experiments succeeded particularly well after we had managed to buy small induction coils which gave us fairly high tension. Early in 1896 Röntgen's famous *Communications*, which Hermann F. received from a book seller uncle promptly after publication, acquainted us with his great discovery. We managed to procure primitive discharge tubes and tried—in vain—to detect those mysterious X-rays, as they were then called. As sources for the primary current we used chromic acid and Bunsen elements, as the apartments of our parents were not connected to the city's electric power. Many a hole did the required chemicals burn in our clothes! I remember in particular a home-made galvanometer with many windings and an astatic pair of needles with thread suspension. This gave me the proof, which I had not been able to obtain from the experiments at school, that the discharge current of a Leyden jar deflects the magnetic needle.—We also often discussed optical phenomena, in particular, the interference of light and optical diffraction, which at that time were most mysterious to us. The great attraction of these phenomena lies in their being directly perceptible to our senses, without measuring instruments. My strong interest in Optics, which became important later in connection with X-rays, dates from these high school times.

These were stimulating hours which we had together, often in Otto B's attic room in the stately home of his parents in the Goethestrasse. The window offered a view over the University Gardens, far out to the bluish hills of the Black Forest on the horizon. These mountains, and also the Vosges, were often the goal of our excursions, lasting one or several days. Generally, we were not stay-at-homes but cycled through wide stretches of the Rhine plain and beyond, and swam in the little or the large Rhine near Kehl; I have often crossed the Rhine by swimming, with the help, to be sure, of its many gravel banks. No organization was needed to get us out of doors, the freedom accorded us by our elders at home and in school was enough. And this freedom was one of the main attractions of these hikes. What would they have been to us without our independent prior planning, our study of maps and time-tables, estimating our own stamina and, once on the way, the not always easy task of finding our path through the mountains in fog or snow. Our hikes exercised not only our body, but contributed also to our mental maturing.

Unfortunately, the Trefoil did not continue together up to the Abitur. A year earlier Otto B. transferred to another gymnasium in Baden, because of an inherited nervous affliction which progressively retarded him mentally, later prevented him from attending a university after he had passed the Abitur, and finally, in 1904, made him shoot himself. I have never forgotten him, and feel even today the great influence he has had on my work. Had he retained his health, he would have become an outstanding university teacher, an inspiration to his students, and perhaps a great scientist. He was buried in Strassburg.

In March 1898 I passed the final school examination, the Abitur. I had prepared for it to some extent, and felt a kind of serene cheerfulness in contrast with the excitement which took possession of most of my classmates as the date approached. The same thing happened in my later examinations, and at the last one, for the Teachers Certificate, which I took in Göttingen, in 1904, I felt slightly sarcastic at the importance which other candidates and even some of the examiners attached to this business. In the Abitur, I received 'Good' in Religion, Latin, Greek; 'Satisfactory' in German, French and History; 'Excellent' in Mathematics and Physics. The certificate contains a comment on German: 'Laue has come up to the requirements of the curriculum in German and has done good work occasionally. His intellectual potential is higher than his achievement in written or oral expression. The examination composition was satisfactory.' This was all too true. Schiller's moan: 'If the soul speaks, Oh! it's no longer the soul'* I have sympathized with all my life. Even worse, if I had to speak in a foreign language I suffered such torment that I could never give a fluent and correctly pronounced lecture.

Perhaps the reader missed English among the subjects of the Abitur. At that time there were no classes in English given at the German Gymnasium; this I have later felt to be the worst deficiency in my education. I learned English only after leaving school, from scientific journals and books which became more and more indispensable as time went on. I spent months in America and was compelled to use English there. Although I had very little practice in French after the gymnasium, I still speak French better than English. 'Was Hänschen nicht lernt, lernt Hans nimmermehr.'† Undeniably, a certain lack of personal aptitude also plays a part. In the gymnasium in Posen, from Sexta to Tertia, I heard much Polish spoken by my comrades, but I never managed to understand even a single word. In Strassburg I had a similar experience with the Alsatian dialect. Any language I tried required a conscious and determined effort. And for this reason it was a particular blessing for me that at the age when learning was easiest the gymnasium laid so much stress on grammar and expression; this emphasis compensated to a certain degree for my lack of natural ability.

Military service began a few days after the final examination, and brought an interruption of my intellectual training.‡ I could, however, register for the main lecture course in Experimental Physics during the second half of my military year, in the Winter Semester 1898/99. The course was given by Ferdinand Braun and I well remember his brilliant demonstrations and his elegant and often witty presentation. I watched and listened enthusiastically. Sometimes my military duties prevented me from arriving on time, and whenever this happened my entrance in uniform was quite a sensation and caused some unrest in the audience. This led to an amusing incident, characteristic of Braun, which I want to tell, all the more because Braun himself laughed heartily when he heard it from me at a physicist's meeting many years later.

At the end of the semester the students had to 'sign out'; this was a way of certifying their regular attendance. In view of the fact that

* 'Spricht die Seele, so spricht, ach, schon die Seele nicht mehr.'

† 'What Johnny doesn't learn, John never can'.

‡ By reaching the level of Obersecunda in a secondary school the student became an 'Einjährig-Freiwilliger' that is, his future compulsory military service was reduced from two or three years (depending on the branch of service) to one, with the option of receiving a commission as an officer of the reserve after further training.

Braun had several hundred students in his lecture course he was naturally not in a position to know; he therefore sat at a table in an antechamber of the lecture theatre signing his name mechanically and hardly looking up as the students laid their open books before him one after the other. Only when I approached in my uniform did he look up slightly and say with an ironical sigh: 'Yes, that *you* have attended I really *can* certify.

In later years, too, I tried hard to attend lectures regularly. Whenever I was obliged to miss one, I found it difficult to pick up the thread of the lecture, especially in mathematical courses. How other students could miss lectures, for example to fulfill fraternity obligations, was beyond my comprehension. My aim was Science.

Yes,—but what part of Science? This was my greatest problem in the first semesters. That my field lay somewhere among Mathematics, Physics, or Chemistry, of that I was certain since the Secunda. As a start I took many courses in all of these subjects, first in Strassburg, then (beginning in the autumn of 1899) in Göttingen. I attended probably more laboratory courses in Chemistry and Physics, than those who knew their goal and chose the shortest way. In Göttingen, under the influence of Woldemar Voigt, I clearly recognized my destination at last: Theoretical Physics. Next to Voigt's course, the published lectures of Gustav Kirchhoff contributed much to this decision. Otto B. had pointed them out to me when we were still at school and also Prof. Göhring had occasionally spoken of the first volume, the lectures on mechanics. But the decisive impression was my astonishment at seeing how much information about nature can be obtained by mathematical methods. Profoundest reverence for Theory would overcome me when it cast unexpected light on previously obscure facts.

Pure mathematics, too, did not fail to impress me, especially in the brilliant courses of David Hilbert. This man lives in my memory as perhaps the greatest genius that I ever laid eyes on. Mathematics represents the experience of truth at its purest and most immediate; on this rests its value for the formation of the human mind. One of my main delights, even at school, was a well rounded mathematical proof. And yet, mathematics has interested me only when I could find some physical application for it. Otherwise my endeavours in this field appeared to be 'swimming in empty space', or like exerting a force on a non-existing object. Other theoretical physicists differ in this respect and owe great results in physics to their preoccupation with mathematics *per se*. But I had to follow my own natural bent as I said, and come to terms with it.

Important as these and later courses were to me, I learned more effectively from books than from lectures. The spoken word never made as permanent an impression as what I saw before me in black and white. In reading you can stop wherever you like and think about what you have read. In a lecture you are bound to follow the lecturer's train of thought and the least inattention makes you lose the thread. Often enough the lectures only served to spur me on to delve into the appropriate books.

In these semesters, and even long afterward, Maxwell's *Theory of Electricity and Magnetism,* which had won full acknowledgment in Germany only a few years before, gave me particular trouble, and also great pleasure. Finally it happened to me as no doubt to everyone else; a new world opened up before me. What manifold and complex processes can be traced back to Maxwell's simple and beautifully harmonious equations—this insight is one of the most sublime experiences a man can have.

> 'War es ein Gott, der diese Zeichen schrieb
> Die mir das innre Toben stillen,
> Die Kräfte der Natur rings um mich her enthüllen?' *

That is what Boltzmann once quoted from Faust regarding these formulae. My predilection for optics suffered no damage from the strong impression Maxwell's Theory made on me; on the contrary, my admiration increased with the knowledge that optics formed part of the system of electrodynamics.

I remained in Göttingen for four semesters. Then during the winter of 1901–2 I went to the University of Munich. However, in those days a theoretical physicist could not find much in Munich. Boltzmann's chair had not been filled. I remember only Alfred Pringsheim's course on Theory of Functions and W. C. Röntgen's physics laboratory where he talked to me once at some length and was obviously pleased with my knowledge. Normally the assistants conducted the laboratory. But at that time I first became acquainted with the Alps in winter. I had been hiking there during summer vacations together with Hermann F. and Otto B. (who was at that time still physically quite able) and also had some ascensions behind me. But winter in the high

* 'Was it a god who traced these signs
That soothe the raging in my inner self
Revealing Nature's forces that surround me?'

mountains was something quite new and wonderful. It was a pity that skiing did not exist in Germany at the time. We therefore plodded along in the soft snow of the Rofangebirge, sometimes sinking in up to our hips, and when we wanted to push ahead to the Wendelstein after a sleigh descent from the Brünnstein we got into real trouble. We—that means the colleagues of the mathematics club to which I belonged in Munich, as earlier in Göttingen and later in Berlin; these scientific student societies maintained a fine esprit.

I learned to ski on the Feldberg under the tutelage of Paul Drude and Willy Wien, but not until 1906, and by then I was no longer young enough to become a real expert. I did gain enough skill to feel safe skiing cross-country, which after all is a satisfactory way to experience the great joy of this wonderful sport. Until well into World War I, each March found me skiing in Mittenwald with Willy Wien—Paul Drude lost his life in the summer of 1906—and the memory of this great, scientifically inspiring, noble, kind, and at the same time sports-loving savant, is one of the most beautiful of my life.

In the summer of 1902 I moved to Berlin. What drew me there mostly were my old school friends, Hermann F., who was there working for his doctorate in chemistry, and Otto B., who continued to make desperate efforts to complete his studies. Although I arrived at the end of June, the first eight weeks of the term having been swallowed up by a military exercise, I registered without difficulty.

I hurried to Planck's lectures on theoretical optics; I knew of him as the author of a textbook on thermodynamics, and knew also that he had accomplished much in this field. But I knew nothing as yet of his greatest achievement—the discovery, in 1900, of the Law of Radiation and the quantum-theoretical arguments leading to it; these results were not generally accepted at the time and hence they were little known. I could follow the lectures easily despite their advanced stage because I had heard the same material once before with Voigt, and I even managed to draw Planck's attention during the first exercise period by producing a rather elegant solution to the assigned problem. I also attended the 'small course' of O. Lummer on 'Special Problems in Optics', i.e., on interference phenomena, especially in ruled gratings, echelette gratings and plane parallel plates. He himself together with Gehrcke had just at that time introduced plane parallel glass plates in spectroscopy. Lummer's principal association was with the Physikalisch-Technische Reichsanstalt,* and for demon-

* Comparable to the Bureau of Standards in the U.S. and the National Physical Laboratory in Great Britain.

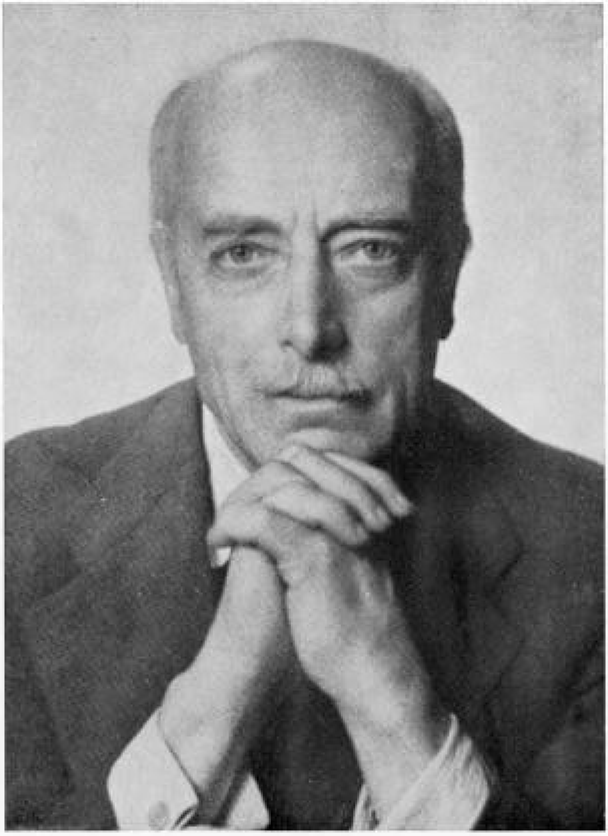
stration purposes, he brought to his lectures in the Physical Institute of the University some of the excellent apparatus of that institution. In these lectures I acquired the feeling for optics which became so very useful to me later on. Toward the end of the semester I went to Planck and asked him for a subject for a doctoral thesis. Referring to the above-mentioned lecture course he gave me 'Theory of Interference Phenomena in Plane Parallel Plates'. I worked on it until the summer of 1903. In July of that year I passed the doctoral examination *magna cum laude* with mathemetics and, as required, philosophy, as minor subjects.

Something should be said about this. Though I have never attended a course in philosophy, I have spent much time and thought on Kantian philosophy. To begin with, I read Kuno Fischer's presentation in his *History of Philosophy* and then I read and re-read Kant's *Critique of Pure Reason* and others of his works, especially those on ethics. The stimulation of this interest dates back to school days and came from Otto B., but only at the University, it would seem to me, did I mature sufficiently to understand philosophy. This transformed my life completely. Since then even physics seems to derive its real dignity only from the fact that it provides an essential resource for philosophy. My conception is that all the sciences must be grouped around philosophy as their common centre and that service to philosophy is their real purpose. Thus, and only thus, can the unity of scientific culture be preserved in the face of ever increasing specialization; without this unity our whole culture would be destined to disintegrate.

In those days the doctoral degree was still conferred in a truly solemn ceremony. Among other things the Dean read an oath which says in part: 'Te solemmiter interrogo, an fide data polliceri et confirmare religiosissime constitueris, te artes honestas pro virili parte tueri, provehere atque ornare velle, non lucri causa neque ad vanam captandam gloriolam, sed quo divinae veritatis lumen latius propagatum effulgeat.' * I have endeavoured to keep this oath.

While working on my thesis I still attended Planck's lectures on thermodynamics and his most impressive lectures on theory of gases and heat radiation. Boltzmann's principle of the relation between entropy and probability, Wien's displacement law with its proof given in final form only by Planck, and then the audacious derivation of the radiation law from the hypothesis of finite energy quanta—they

* 'I ask you solemnly whether by the given oath you undertake to promise and confirm most conscientiously that you will defend in a manly way true science, extend and embellish it, not for gains sake or for attaining a vain shine of glory, but in order that the light of God's truth shine bright and expand.'



Max von Laue (1879–1960).

were revelations to me at the time. There was in addition a magic charm which emanated from the personality of this great man and fascinated every one of his students. All this combined to give me the feeling that the University of Berlin was my proper spiritual home.

The University, not the town. I have always had an aversion to large cities. Therefore, for the continuation of my studies which I considered an absolute necessity, I preferred the typically provincial town of Göttingen and spent four semesters there, once again. I took among other things, Max Abraham's course 'Theory of Electrons', the atomistic confirmation of Maxwell's Theory, and Karl Schwarzschild's Geometrical Optics, although the latter was too highly specialized for my taste. During this time I also passed my state examination for certification to teach in secondary schools. This I did on the side and I was really surprised when I received a 'good' in it.

Since I had chosen chemistry as a subject for examination I had to take an exam in mineralogy as well. Now, I had never studied mineralogy. During my first stay in Göttingen I had made a half-hearted attempt to attend a mineralogy course but had given up very soon. From books I then learned the rudiments of crystallography, that is to say, crystal classes, that was all. The exam was administered by the geologist, Prof. Könen, and I still remember how his amusement grew and grew in the face of my entirely obvious ignorance until he finally broke off the interview. Thanks to my knowledge of chemistry, unusually extensive for a candidate for a state teaching certificate, the committee declared the exam to be successfully passed. The impression that I would never make use of this certificate may have contributed to this decision.

But how did it happen that my training in physics had this obvious deficiency, which by the way, was shared by most physicists in those days, both those older and younger than myself. The reason for this lies probably in the fact that in University teaching crystallography had been pushed almost completely into the lectures on mineralogy where, for the most part, only the descriptive part of mineralogy was stressed. In physics lectures crystals were mentioned generally in connection with optics and occasionally with elasticity, that was all. Even Voigt, who did so much for crystal physics, acted not differently. His fundamental book on crystal physics did not appear until 1910. Perhaps in my case this complete lack of knowledge had the happy consequence that later, in 1912, I could approach a question of crystal physics with complete freshness of mind. Much later, during my years in Frankfurt (1914–18) I filled this gap, but I never achieved the perception of space which marks all those who do crystal physics in their youth. But I did acquire something of that dedication which every crystallographer has for his subject, as it appears with Voigt in an almost touching way in the preface of his book.

In the fall of 1905 Planck offered me the assistantship in his Institute for Theoretical Physics which was about to become vacant. I accepted with pleasure and remained there for more than three years. In addition to supervision of the library my duties consisted in checking the problems which Planck collected each week after his lecture and discussed next day in the problems period after the assistant had looked them over and brought to his atention anything that seemed noteworthy. As a student I had faithfully done these problems. But to correct the work of others was almost more instructive because in this way one could learn what mistakes and misunderstandings were possible and discuss them and many other things with Planck. At this time I also began to do scientific work of my own. It is true I had already published in Göttingen an investigation of the propagation of natural radiation in dispersive media, based on Planck's hypothesis concerning the nature of radiation. However, now, once more following Planck's lead, I attacked the more profound problems of the reversibility of optical reflection and refraction.

Planck's formula for the entropy of a pencil of light rays showed unequivocally that the division of energy from one ray into two geometrically equal rays, e.g. equally long ones, is accompanied by an increase of entropy if, as was then usual, one added their entropies. Then, according to the second law of thermodynamics, the separation of a beam into a reflected and a refracted part should be irreversible. However, a simple argument on optical interference showed that because of their coherence the two beams could be reunited into one, that differs in no way from the original one. This was a profound dilemma. Would one have to abandon the second law for optical processes?

The explanation lay with the already mentioned Boltzmann principle of entropy and probability. It showed clearly that while one could add the entropy of incoherent beams, this was not possible for coherent ones. The entropy of the two beams resulting from reflection and refraction is exactly equal to that of the entering beam.

An hour after leaving Planck's home in the Grunewald following our decisive conversation on this subject, I found myself at the Zoological Gardens without knowing what I wanted there or how I had got there. So overwhelming was this experience.

However, I do not want to enlarge on every phase of my researches. I know from reading the biographies of others that this gets to be rather dull, interesting at most only to a careful historian of science. But I think I may be allowed to mention some high points.

When I returned to Berlin in 1905 I heard Planck talk in one of the first colloquia of the winter term—or was it the very first?—on Einstein's work, which had appeared in September: *On the electrodynamics of moving bodies*. The transformation of space and time performed by the relativity theory, proposed in this paper, appeared strange to me and I was by no means spared the doubts later voiced by others. But the idea took hold of me, especially because Planck soon published several research papers of his own on the subject. Thus in 1907 I could show how Fizeau's famous interference experiment in moving bodies, which until then had been considered the unimpeachable proof for the existence of the luminiferous ether fitted into the new theory which denied the existence of such quasi-matter. What had been taken for granted until then, namely that one can add the velocity of light and the velocity of a body was simply not correct.

I do not know whether it was this work alone or other things as well which resulted in the inquiry made by the publishers, Fr. Vieweg & Sohn, asking whether I would write a monograph on relativity theory. I did, and thus became the author of its first comprehensive presentation. The book was generally well received and eventually reached four editions. I wrote it—I had moved from Berlin to Munich in 1909—in a little boat-house which stood on posts in the water at the shore of the Starnberger See in the ducal gardens of Feldafing and had a marvelous view of the Herzogenstand, Heimgarten, Benediktenwand and the mountains of the Karwendel. I never again had it so good.

At the University of Munich there worked at that time besides Röntgen also Arnold Sommerfeld who had been asked some years earlier to fill Boltzmann's former chair. A powerful inspiration emanated from this incomparable teacher. Among many other things he concerned himself with the theory of X-rays, and developed a theory of the creation of X-rays at the anticathode of the X-ray tube, which proved very useful. It was based completely on the wave theory for these rays, therefore stood in contrast to W. H. Bragg's corpuscular theory which was very popular in England. The basis for making a choice between the two was supplied by Walther and Pohl in Hamburg in their photographs of the diffraction of X-rays by a wedge-shaped slit, which P. P. Koch, Röntgen's chief assistant, was measuring photometrically. Sommerfeld applied the traditional diffraction theory, with the result that he obtained a rough but very useful estimate for the mean wave-length which he published in 1912. Furthermore, Barkla's proof of the polarization of X-rays was discussed in the Sommerfeld colloquium, and so were also the features of the characteristic X-rays of the elements. Thus, one lived there in an atmosphere saturated with problems concerning the specific nature of X-rays.

Röntgen himself began to withdraw at this time and in summer he no longer lived in Munich but in Weilheim, a small town 60 km to the south. He commuted daily by train for his lectures. He still directed his institute, and his assistants and doctoral students produced valuable research. He also still did some work himself, e.g. together with Joffé he investigated certain questions of crystal physics in which he had been interested for some time. He had grown, however, over-cautious in the publication of his results. Again and again new confirmation had to be collected and in this way nothing was really completed. He did not appear for the colloquium and seemed to take little interest in the newer researches. Some theories he favoured with mild ridicule: when the Friedrich-Knipping experiment was interpreted as showing interference, his instinct was for a long time to refuse to accept this interpretation. On the other hand in 1919 he immediately recognized the importance of C. T. R. Wilson's cloud chamber for research on radioactivity and he gave it high praise, as I know from a reliable source. As far as I am concerned I spoke to him only once at any length when, on a train ride to Feldafing, I happened to find the only available seat in a third class compartment opposite His Excellency. At that time I gained the impression that for people like me he could have been easy to talk to, had there been more opportunity to do so.

In 1919 I talked with Röntgen once again informally. Shortly after the Rätezeit in Munich* I went to visit him at his institute but found him ready to leave for the train to Weilheim. So I accompanied him on foot to the Starnberger Bahnhof. But instead of talking about science he expressed his pleasure at all the signs of returning order and observed with a certain delectation the really very delicate system of cracks which surrounded each bullet hole in the glass of display windows. People have often speculated about the reasons which drove this man into virtual retirement after his epoch-making achievement

* A brief period in 1919 when Munich was governed by a kind of Soviet, a council of workers and soldiers.

in 1895/6. Many motives have been suggested, some of them far from flattering to Röntgen. I think they are all wrong. In my opinion, the impact of his discovery was so overwhelming that he, who was 50 at the time, never recovered from it. For—as few people seem to realize—every great intellectual discovery is a heavy burden for the man who makes it. And this was not exactly alleviated when, like many other discoverers, he was forcibly made aware of the less admirable qualities of some of his fellow-men.

The magnitude of his exploit can be recognized particularly if one considers the large number of other physicists, some of them of great renown, who experimented with the same kind of apparatus before Röntgen and did not discover X-rays. For such a break-through into completely unsuspected territory one must have enormous courage and in addition a self-discipline which preserves mental calm and clarity in the midst of the great joy and excitement of the first findings. Many observations had to be made and correlated to make possible for Röntgen to write three treaties like those of the years 1895 to 1897, which exhausted the subject so completely that for almost a decade nothing new could be said about it. With what ingenious care they are written! I know only few accounts of discoveries which do not contain mistakes of one kind or another. Röntgen was correct in every detail.

It became of special importance to me that in Munich the hypothesis of space lattices for the explanation of crystal structure was still alive although elsewhere it was hardly mentioned any more. The reason for this was partly that the university had a collection of models from the times of Leonhardt Sohncke, who had taught in Munich until 1897 and had made considerable contributions to the mathematical theory of space lattices. Mainly, however, the credit must go to Paul von Groth, Professor of Mineralogy, who used the crystal structure models in his lectures. In February 1912 it happened that P. P. Ewald, then working on his doctorate under Sommerfeld, came to visit me in my apartment and asked for help with his problem: to investigate mathematically the action of light waves in a lattice of polarizable atoms. Although I could not help him, I said quite by chance in the course of the discussion that some time one should irradiate crystals with shorter waves, i.e. X-rays. If the atoms really formed a lattice this should produce interference phenomena similar to the light interferences in optical gratings. This was discussed among the younger physicists around Munich, who met every day after lunch at the Cafe Lutz. One of them, Walther Friedrich, who had just finished his doctoral thesis on X-ray scattering under Röntgen, and was now one of Sommerfeld's assistants, offered to test the idea experimentally. The only difficulty was that Sommerfeld did not think much of the idea at first and preferred to have Friedrich do an experiment on the directional distribution of the rays emanating from the anti-cathode. But this difficulty was overcome when Paul Knipping, another of Röntgen's doctoral students, offered his help. And so the experiment on the transmission of X-rays through crystals began around Easter 1912.

Not the very first, but already the second experiment showed a result.* The photograph of the X-rays transmitted through a piece of copper sulfate showed, besides the primary X-rays, a circle of spectra diffracted by the lattice. I was plunged into deep thought as I walked home along Leopoldstrasse just after Friedrich showed me this picture. Not far from my own apartment at Bismarckstrasse 22, just in front of the house at Siegfriedstrasse 10, the idea for a mathematical explanation of the phenomenon came to me. Not long before I had written an article for the *Enzyklopaedie der mathematischen Wissenschaften* in which I had to re-formulate Schwerd's theory of diffraction by an optical grating (1835), so that it would be valid, if iterated, also for a cross-grating. I needed only to write down the same condition a third time, corresponding to the triple periodicity of the space lattice, in order to explain the new discovery. In particular it was thus possible to relate the observed circular pattern of rays to the cones corresponding to the three interference conditions. The decisive day, however, was the one a few weeks later when I could test the theory with the help of another, clearer photograph.†

The theory proved valid far beyond my expectation. This was a surprise because, as had been clear from the beginning, it was only an approximation. Indeed the precision measurements done in Siegbahn's Institute in Uppsala (1920) showed minor deviations; the perfected theory required to explain them one owes to P. P. Ewald. However for the overwhelming majority of the many cases investigated since then, the first 'geometric' theory is perfectly adequate. Although it does not always work out as well as for X-rays, the theory can be used even for the electron interferences in crystals which were discovered in 1927 by C. C. Davisson and L. H. Germer, and by G. P. Thompson.

Standing on the very same spot as Planck, when he spoke for the first time in December 1900 about his theory of radiation and the quantum

* See Fig. 4–4(1).

† See Fig. 4–4(3) and (4).

theory, I presented my discovery on 8 June 1912 to a meeting of the Deutsche Physikalische Gesellschaft in the Institute of Physics of the University of Berlin. Since then a vast body of experimental and theoretical literature has grown up around it, and when in 1941, I wrote a book, *Röntgenstrahlinterferenzen*, to present merely the theoretical aspects of the field, it turned out to be 350 pages long. The first major step beyond my original work, which had appeared in the Bayerische Akademie, was taken by W. H. and W. L. Bragg.

This step, which consisted essentially in the investigation of individual crystal structures was one I could hardly have taken myself. I have always been primarily interested in the great underlying principles in physics—which is why Planck's lectures, emphasizing just these, had fascinated me—and the general principles, concerning X-rays on the one hand and crystals on the other, had after all been established with the help of the experiments of Friedrich and Knipping. The Braggs, father as well as son, had a love for the individual substance and could devote their labour to the structure of crystals from sodium chloride, diamond, and so forth, to the most complicated silicates. Science needs scholars with many different talents and would soon come to a standstill if all scientists were of the same intellectual type.

The history of the discovery of X-ray interference illustrates beautifully the value of scientific hypothesis. Many people irradiated crystals with X-rays before Friedrich and Knipping. However, their observations were limited to the directly transmitted ray which revealed nothing remarkable beyond the weakening produced by the crystal; they missed completely the less strong diffracted rays. It was the theory of the space lattice which provoked the idea of investigating the neigbourhood of the direct ray. Of course the diffracted rays would eventually have been discovered as stronger and stronger X-ray tubes were developed; some accident would have pointed them up. But it is hard to guess when this might have happened, and we can certainly say that the theory of the space lattice was absolutely essential to account for their presence.

As I said before, I don't want to present a complete list of my scientific works, nor do I want to go into the details of my contributions to the mathematical development of X-ray diffraction theory. But I do want to mention a series of publications influenced by my earlier studies in connection with Maxwell's theory. For several years (until 1934) I acted as a consultant on theory to the Physikalisch-

Technische Reichsanstalt and there came into personal and intellectual contact with Walther Meissner who was an old acquaintance from my days as assistant in Planck's problems periods. Meanwhile he had become a member of the Reichsanstalt and Director of its low-temperature section. He was particularly interested in super-conductivity, that strange disappearance of electrical resistance which some metals exhibit when cooled to liquid helium temperatures. It was known that a sufficiently strong magnetic field could reverse this phenomenon. However, the measurements that showed the required field strength had an incomprehensible dependence on its direction with respect to the axis of the wire, which at that time was the only form of superconductor used for experiments. It occurred to me that the super-conducting wire itself distorts the field in such a way that the field actually present at the wire surface is considerably greater than that measured at a distance. My conjecture that actually the same surface magnetic field strength was always needed to destroy the super-conductivity could now be verified quantitavely and applied to other shapes such as super-conducting spheres. I presented this theory at the physics meetings at Bad Nauheim in 1932 when I was awarded the Planck Medal. During the following years W. F. de Haas and his collaborators produced full confirmation of the theory with their measurements at the low-temperature institute at Leyden. Later I worked on the thermodynamics of super-conductivity and, following Fritz and Heinz London's lead, on an extension of Maxwell's theory to include super-conductivity. But not all the conclusions possible from this theory have as yet been checked by experiment; here many fundamental tasks remain for postwar times.

* * *

I wrote the above in 1944 for publication at that time. But the war and immediate post-war conditions interfered and only now (1951) is there a prospect for publication. In the meantime, however, much has happened which after all should also find a place in my autobiography and I have decided to continue the story. I considered for a long time whether this addition still belongs under the title 'My Development as a Physicist', or whether I had better choose for it the title 'Ausklang'

(Diminuendo). However the development of the true scholar ends only with his death, and in any case I have to go far back to make understandable what I have to tell.

As I said earlier in this story, when I left the Gymnasium I had to accommodate myself to military service no matter how much I considered universal military training to be an unjustified encroachment on personal liberty by the state and a waste of some of the most precious years of development. These were the years dominated by Bismarck and consequently very peaceful. Outwardly I did not fare badly after overcoming my initial difficulties and on the insistence of my father I even became a reserve officer and completed all the prescribed exercises. But inwardly my attitude to all things military remained wholly negative. I do not at this point want to go into the non-military circumstances which increased this opposition and finally brought on a severe illness. Suffice it to say that afterward I could request my discharge with a clear conscience. In 1914, at the outbreak of the First World War in which (as I thought at the time and still think) Germany suffered an injustice, I tried to rejoin the army. I even refused a position in Switzerland so that I might share the fate of the German people no matter how hard that would turn out to be. I was, however, rejected; the same old circumstances which I do not wish to discuss, stood in the way. Much as this pained me at the time, it turned out for the best, for when they wanted to put me back into uniform for the Hitler war, I could successfully request deferment by pointing to this previous rejection. Already in 1937 I had sent my only son to America so that he should not be compelled to fight for Hitler.

All my life I was prudent enough to remain, if possible, aloof from all political activities save voting in elections, even though I took a lively interest in politics; I knew my limitations. But who remained in a position to ignore politics after 1932 when absolutely everything was pulled into a political context and evaluated politically? My urgent sense of justice was particularly violated by the lawless capriciousness of National Socialism and my pride as a scholar was hit hard by Nazi interference in the freedom of science and of the universities. Although it could not touch me personally, I had—to put it mildly—an insurmountable aversion for anti-semitism ever since my schooldays. Before 1933 I never gave even a passing thought to the question of the 'race' of a new friend. Never before, not even in 1918–19 had I been therefore in such despair over the fatherland as I was during its death struggle in 1933–34 until it received, on 4 August 1934, the last fatal stab in the back.* Like many others I frequently quoted to myself the verse

> Denk ich an Deutschland in der Nacht
> So bin ich um den Schlaf gebracht.†

Often I asked myself upon awakening whether I had not been dreaming when I remembered the horrors of the preceding day. Unfortunately they were reality, cruel reality.

But I did not suffer my mood to paralyze me. As much as I could, I assisted those directly affected, for instance by warning them in time, especially those colleagues who had lost their positions. There were a few rare cases of colleagues who were able, of course not solely because of my assistance, to survive in Germany through that entire unfortunate period. Much more frequently I smoothed the way for those who emigrated by sending advance reports to the foreign aid organizations about their personalities, their domestic circumstances, their particular abilities and desires. Since the postal censor confiscated such reports, we found safer ways of sending them across the border. Once I even used my automobile to get a colleague who believed himself in danger, across the border into Czech territory. This was easier at the time than seems likely in retrospect.

All this had to be carried on as secretly as possible. But I declared my own attitude publicly as two documents appended here will substantiate. One contains the text of a speech which I gave as chairman of the German Physical Society at a physical society meeting in Würzburg on 18 September 1933, and had printed shortly thereafter in *Physikalische Zeitschrift*. The other is an obituary I wrote for the eminent physical chemist Fritz Haber, whose dismissal and banishment belong among the particularly inglorious achievements of Hitlerism. The obituary appeared in *Naturwissenschaften*, in the spring of 1934. Both earned me reprimands from the Ministry of Culture in Berlin. Apparently the ministry felt obliged to do something for my entertainment.

As professor at the University of Berlin and Acting Director of the Kaiser-Wilhelm-Institut für Physik I was tied to Berlin even after 1939. This situation eased up after the Ministry of Education declared me an emeritus on 1 October 1943, a year before I had reached retirement age, but with my consent. Remaining in Berlin I was exposed to the bombing raids on Berlin until the middle of April 1944. I witnessed

* After Hindenburg died, two days earlier, Hitler concentrated all power in himself.

† Thinking of Germany at night I find my sleep in headlong flight (Heine).

for example, in the unforgettable night of 15 to 16 February 1944, the burning of Otto Hahn's Kaiser-Wilhelm Institut für Chemie. A sea of flame burst forth in terrifying beauty from the framework of the roof and the shattered south wall of this monumental building. When the Kaiser-Wilhelm-Institut für Physik was moved away from Dahlem and bombs had made my house in Zehlendorf, if not uninhabitable, at least unpleasant, I went with the Institute to Hechingen. There at the foot of the Hohenzollern castle my wife and I had a peaceful year, and although fleets of planes would thunder by high over the town, they left it almost unscathed. And thus I recall these times as relatively happy despite the severe hardships of war. Toward the end I must admit the terrors of the Hitler regime came very close to us, as when a good friend was suddenly arrested in the street and we had reasons to fear that he would be shot. Even the preparations for the defense of Hechingen, childish as they were, would have meant great danger if there had actually been a struggle. Fortunately, the feeling prevailed that such hopeless resistance was criminal and it was with a sense of relief, in spite of the military measures that would inevitably accompany such 'conquest', that the inhabitants watched the French and red-Spanish troops enter the town without a struggle on 23 April 1945. All of this could surprise no one.

But it was a surprise when, on the following day, a squad of American and British soldiers appeared, and occupied and searched the Kaiser-Wilhelm-Institut für Physik. As it turned out later, this was part of the American 'Operation Alsos'. The name of this operation was a translation of the Commanding Officer's name, General Groves, into Greek (ἄλσος = grove).

Well-known scientists formed part of this mission, among them my good friend S. Goudsmit. To my amused surprise he suddenly appeared in our home, adorned with a steel helmet. However, he was carrying out his activities in deadly earnest. He was in deep mourning for his parents who had been taken from Holland to one of the concentration camps and there had been 'liquidated'. (Heisenberg and I had intervened as soon as we heard of such a possibility but we had been too late.) Well, as they departed the Americans took away with them a number of nuclear physicists including myself, thereby according me the completely undeserved honor of being accounted a nuclear physicist. With some additions this group increased to ten members. We were taken first to Heidelberg, then to Reims, then to the Paris suburb of Le Vésinet, next to the delightful country residence Faqueval south of Huy in Belgium and finally to Huntingdon in England. We had no complaints about the treatment accorded us and the military diet was most beneficial after the deprivations of the war period. At Huntingdon we had for our use not only the spacious, two-hundred-and-fifty-year-old country house 'Farmhall' but also parts of the adjoining gardens. We had a beautiful view of the park, stretching all the way to the river Ouse; we had English and American newspapers, journals and other literature as well as scientific books, and we had an excellent radio to bring us the many musical offerings of the BBC. Quite often one of the English officers guarding us took us for a ride in the beautiful surroundings of Huntingdon, to the famous cathedrals of Peterborough and Ely and even to London. In London, by the way, lived the only physician and dentist whom we were permitted to consult. We never went to Cambridge even though it was very close by, since presumably we would have been recognized in that university town. Our presence, after all, was a secret of the highest order. For the first four months we were not even allowed contact with our families and later on, when we could write them, we were not to disclose even the country in which we were being held. From these precautions one surmises a certain ambivalence on the part of the victors as to how they should deal with the German physicists whose competence on the one hand they esteemed greatly and yet, on the other hand, feared as potentially dangerous. The situation had its comical aspects but was emotionally oppressive for us. Nevertheless, I was able to write a paper in Huntingdon on the absorption of X-rays under the conditions of interference which later appeared in *Acta Crystallographica*. Frequent colloquia also helped to keep us mentally alert.

Early in 1946 we were allowed to leave England and, after a short intermediate period in Westphalia, most of us found ourselves in Göttingen, where I stayed until April 1951 and where my activities included first once again the post of Acting Director of the Kaiser-Wilhelm-Institut für Physik, and also soon thereafter an adjunct professorship at the University. I wrote a book *The Theory of Superconductivity*; revised my book on electron diffraction (*Materiewellen*) in parts for a new edition, and made several contributions to scientific journals. Also I was able to almost complete the manuscript of a revision of that very first book of mine, *Relativity Theory*, which I mentioned earlier and which had long been out of print. In April of 1951 I accepted an offer to become director of the Institute for Physical Chemistry and Electrochemistry of the Forschungshochschule in Dahlem, the former Kaiser-Wilhelm-Institut founded by Fritz Haber. That one should be offered and accept such a position at the age of 71 is certainly singular. But then, is not all of present day Berlin a singularity?

I have often been asked why I did not emigrate during the Hitler times. There were many reasons for this: for one thing, I did not want to take away any of the few posts available abroad from colleagues who needed them more urgently than I did. Above all, however, I wanted to be immediately available when the collapse of the Third Reich, which I had always foreseen and long hoped for, would make possible cultural reconstruction on the ruins created by that regime. This became the main direction of my activities after 1945. In 1946 it was my privilege to initiate the founding of the 'Deutsche Physikalische Gesellschaft in der Britischen Zone' (German Physical Society in the British Zone),* and in 1950 to participate in the creation of the 'Verband Deutscher Physikalischer Gesellschaften' with which the former affiliated under the name of 'Nordwestdeutsche Physikalische Gesellschaft'. From the end of 1946 to the middle of 1948 I worked on the reunification of the Physikalisch-Technische Reichsanstalt which had been widely dispersed during the war. In 1949 this institution was renamed Physikalisch-Technische Bundesanstalt. In both these enterprises the participation of the British Research Branch was decisive and one cannot be grateful enough to Dr. Ronald Fraser, its director at the time. It was he who procured for the Bundesanstalt its present large tract of land near Braunschweig with its undamaged German air force buildings, which offered undreamed-of possibilities for expansion. However the corresponding American and French organizations also deserve thanks. They appeared on the scene later when it had to be decided that the Bundesanstalt should be a joint enterprise of all three Western occupation zones, and they then acted with sincere goodwill toward German science and economy. Under the circumstances administration of the Bundesanstalt could be taken over by Germany only after the founding of the Deutsche Bundesrepublik; and it has been well supported ever since.

Not only inside Germany did gaps have to be filled after 1945, but also the relations between foreign and German scholars had to be gradually re-normalized. My personal knowledge in this field is limited to physics and crystallography but there, I am convinced, we will succeed within a few years if politics in general continue to be calm. Conditions are rapidly returning to normal as I myself could see during several trips abroad and as is confirmed by all the reports of

* The Allied Control Commission did not authorize organization across the boundaries of the occupation zones into which Germany had been split up.

other Germans. Foreigners, especially Englishmen and Americans, come in growing numbers to German scientific conferences. In this connection I must tell of two incidents which are simply unique.

Once, while we were still imprisoned in Huntingdon, on 2 October, 1945, to be precise, O. Hahn, W. Heisenberg and I were invited to tea by the President of the Royal Institution in London, Sir Henry Dale. One of our English guards took us there by car; punctually at 5 p.m. our host received us at the entrance and led us up the stairs to his private appartments where we were offered the usual afternoon tea. We met there a small group of famous members of the Royal Society, not only physicists, and discussed with them the future of science in the British Zone. To preserve at least the formalities of secrecy that surrounded us, our hostess, Lady Dale, was not permitted to be present, nor, of course, any of the servants. Promptly at 7 our host rose, since a longer visit was not permitted, and accompanied us Germans to the entrance door where the car was already waiting to drive us back to Huntingdon.

For me this tea held a special surprise. I received an oral but completely official invitation to attend a special session of the Royal Society on 9 November 1945, to be held in memory of Röntgen's great discovery, which according to his diary was initiated by an accidental observation on 9 November, 1895. Full of wonder I accepted the invitation expressing of course my doubts that the military authorities would allow me to attend since they were after all holding us in the strictest secrecy. My doubts were completely justified. I did not get the permission. To be consistent, the military authorities could not agree. But consider the implications of this invitation: exactly half a year after the most terrible fighting there had ever been, the Royal Society honours a scholar of the enemy nation with a Commemorative Session and invites another scholar of that nation to attend! I do not believe that any other scientific organization can boast of such an action.

My experiences in London in July 1946 reflect no less glory on the Royal Society and all its scholars. First there was an international conference on crystallography which dealt, as is usual today, mainly with questions of crystal structure and interference phenomena of X-rays and electrons. I was the only German invited and in this way returned to England only four months after the end of my imprisonment there. On the journey there and back I was escorted by a British officer and in London my charming host, a well-known English crystallographer, stayed with me for the first few days. After that I could wander freely in London and its suburbs, and I found that being a German (which I did not wish, nor indeed could conceal) I had no need to fear insults from the inhabitants. I often had to inquire about street directions or means of transportation and always received friendly and correct information. Once in a suburban train I happened to sit opposite a native Englishman, who was using the time on the train to read a German short story, I believe by Storm. Who in Germany would have dared to read an English book in a public conveyance during World War I or the Hitler period?

This is not the place to report on the scientific discussions of that meeting. At the official banquet the chairman made an after-dinner speech in which he remarked in extremely warm words on my attitude during the Hitler period. In my reply I stressed that I was most grateful for his praise but that thousands upon thousands of German men and women were at least equally deserving of it.

Immediately after the conference came the Royal Society's Newton celebration. Actually this should have taken place in 1942 on the anniversary of the day in 1642 when this great scholar died. The only German actually invited was Max Planck, who had long been a Foreign Member of the Royal Society and who came despite his 88 years. But I went with a bachelor member of the Society, who used the invitation he had received by mistake for his non-existing wife to bring me along to the festivities held in the reception rooms of the Royal Society as part of the Newton Celebration. There were gathered together scholars and scientists from all nations, with the possible exception of Russia. And they all came up to me—not only Englishmen, but also Americans, Frenchmen, Poles, Czechs, Hungarians, Scandinavians, Italians, Belgians, Dutchmen, etc.—sometimes without a word, to shake hands. It was not easy to hide how moved I was.

The freedom of science which we lost in 1933 we have not yet regained; not only such things as control commissions for nuclear research stand in the way. Research can also not survive when financial support is reduced below a subsistence level; and this is now the case in many universities. The oppression is worse in the East Zone than in the West Zones. The continuing escape of scholars from there is clear enough evidence for this. Like most other West-Berliners I feel as if I were occupying an outpost against the advance of anti-spiritualism. It is not an easy position, and there are moments when one doubts that this struggle has any hope of success. But then I read the great old Goethe:

Feiger Gedanken
Bängliches Schwanken,
Weibisches Zagen,
Ängstliches Klagen
Wendet kein Elend,
Macht Dich nicht frei.

Allen Gewalten
Zum Trutz sich erhalten,
Nimmer sich beugen,
Kräftig sich zeigen,
Rufet die Arme
Der Götter herbei.*

Appendix

In an address and an obituary note I have tried to illuminate in broader perspective some of the stages in the development of the physical world picture.

First, the *opening address at the physicist's convention in Würzburg, 18 September 1933:*

'When we convene tomorrow in the institute of physics of this university we will meet on historical ground. In this building, toward the end of 1895, Wilhelm Conrad Röntgen discovered the rays named after him. To discuss their importance for physics and the whole range of their application would be superfluous, if not in bad taste. But let us remember the grandeur of the achievement of this man who was the first to see consciously and to raise from the twilight of uncertainty into the full light of sure scientific knowledge what others before him had approached closely and passed by. We know from the Röntgen biography by Glasser that across the ocean there exists a genuine X-ray photograph, taken in 1890, which was recognized as such only after Röntgen's work was published.

'Then, in this place we remember Röntgen's successor Willy Wien who taught and did research in this building for 17 years. His classical theoretical work concerning heat radiation, which, in the form of Wien's displacement law, became an integral part of our science, dates back to his early years under Helmholtz at the Physikalisch-Technische Reichsanstalt. But nowhere else could he devote so many years with so many students and friends to quiet and highly productive experimental research as here. The knowledge of canal rays which was gained here forms no small part of the contents of those magnificent volumes of the *Handbuch* which are just coming out on this subject.

* Timid indecision, cowardly thought,
Unmanly hesitation, anxious lamentation
Bans not misfortune, makes you not free.
Stand up defiant of force!
Never bow under! Prove yourself strong!
This calls the arms of the gods to your side.

I want to mention especially two of Wien's papers of this period because they were truly fundamental: the first quantum theoretical determination of the frequency of X-rays, which gave only an approximate but basically correct value; then the experimental confirmation of the relativity of electromagnetic fields, demanded by relativity theory: light from the particles of canal rays moving in a magnetic field, shows the same spectroscopic Stark effect which is familiar for stationary light sources in an electric field.

'But this same building has also experienced entirely different things: During the World War [I] Wien transformed his institute and, in cooperation with Max Seddig, also the adjoining chemical institute, into a laboratory for the investigation of all the apparatus needed for modern communication technology, and especially for the production of amplifier tubes. The communications system of the army we put into the field in 1914, in contrast to other preparations and equipment, was not at all up to date; in this our enemies were far ahead of us. If we caught up, slowly but very substantially, in the course of the next four years, the physicists who worked in this building deserve a good part of the credit.

'Physics observed a memorial day of special significance on 22 June of this year. It was on this day three hundred years ago that the trial of Galileo before the Inquisition came to an end. The cause for this trial was, as is well known, the theory of Copernicus concerning the movement of the earth and all other planets around the sun, a theory which was in conflict with then accepted views and aroused as much attention and excitement in those days as relativity theory does in our century. Galileo was not its only but its most successful defender, because he was able to support it so convincingly with his own wonderful discoveries of the moons of Jupiter, the phases of Venus and the rotation of the sun around its own axis. Galileo was found guilty. He had to abjure the Copernican system and was condemned to life imprisonment. To be sure, this restriction of his freedom was administered rather gently; he was assigned a house which he could not leave and in which he could not even receive visitors without permission. Actually he was able to publish his principal physical treatise, *Dialogue concerning two New Sciences*, while in custody. Nevertheless, he remained a prisoner for the rest of his life.

'This condemnation has attached to it the well-known legend that Galileo, while renouncing the doctrine of celestial movement and signing the abjuration, said: "And yet it moves". It is a legend, historically unprovable and inconsistent, and yet it is ineradicable from the popular mind. What is the source of this legend's persistence? Most probably the fact that Galileo must have asked himself throughout the proceedings of the trial: "Of what importance is all this? Whether I, whether any human being now affirms it or not, whether political, whether ecclesiastical power is for or against it, this does not change the fact at all. Power may hold up knowledge of it for a while, but still, someday it will break through." And so indeed it did; the triumphal march of Copernican theory was irresitible. The Church itself which had damned Galileo, gave up its opposition with all due formality, even if only after two hundred years.

'Also later there occurred occasional bad times for science, as in Prussia under the otherwise meritorious Friedrich Wilhelm I. But under all oppression the representatives of science could find comfort in the victorious certainty which expresses itself in the simple sentence: *And yet it moves.*'

The obituary note of Fritz Haber (1934, *Naturwiss. 22*, 97) delineates in a few strokes his importance:

'On 9 December 1928, Fritz Haber's sixtieth birthday, a small group of friends and colleagues assembled in front of the Kaiser-Wilhelm-Institut für physikalische Chemie und Elektrochemie, *his* institute, to plant there the Haber-Linde* while he himself was sojourning in the south. This was the only birthday celebration; however, like other scientific journals, the *Naturwissenschaften* published a special issue with contributions from the most notable researchers in Haber's field. To it let the reader turn to discover to its full extent the loss which chemistry and physics, agriculture and industry both for war and for peace have sustained by Haber's death. But he will discover there too that Haber's greatness only in part finds expression in his own publications. His qualities as director were perhaps even greater—while his collaborators had full freedom to develop their own abilities, his was the all-pervasive spirit in the institute. His sixtieth birthday did not put a stop to his work. Oxydation and explosion processes and how they are influenced by atoms and radicals, auto-oxydation and reduction in solutions, especially in biological processes, are themes and projects of his own researches during the last years. Of work done by others in his institute let me mention Bonhoeffer and Harteck's separation of para- and ortho-hydrogen and their use for the exploration of complicated chemical processes and of magnetic

* A tree, called in English variously Linden- or Lime Tree or Basswood.

properties of substances. Throughout its existence his institute was known far and wide as a home for widely diversified scientific inquiry. On the 2nd of May, 1933, Haber submitted his resignation.

'Themistocles is known to history not as an exile at the court of the Persian king, but as the victor of Salamis. Haber will be known to history as the ingenious inventor of the process of the fixation of nitrogen with hydrogen which is the foundation for the industrial extraction of nitrogen from the atmosphere; as the man who thereby—as was said at the presentation of his Nobel prize—"has created a most important means for advancing agriculture and the welfare of man"; who created bread from air, and excelled triumphantly "in the service of his country and of all mankind".

'And what remains for those who knew him and now mourn him? Memories—"His friends and collaborators know the goodness of his heart, his loyalty, his chivalry and gentleness, his indignation at sordid self-interest and wickedness, and the happy equanimity with which he overlooks minor faults. For those younger than himself he has an untiring kindness, endeavouring constantly to give hope and to point out new approaches; he has no patience for narrow-mindedness and intolerance; while he appreciates what is excellent in other countries and cultures, his heart beats for Germany: for like no other man he helped this country to protect and feed its children in its time of greatest distress."

'Thus wrote Margrete von Wrangell in 1928. Thus we remember Haber. For he was ours.'

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